Comments on the Proposed Listing of Nine Distinct Population Segments of Loggerhead Sea Turtles as Endangered or Threatened

prepared by

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Summary

- Loggerhead sea turtles have been listed under the ESA as “threatened” since 1978. It is beyond doubt that they are under threat and require conservation. Yet that is of no present concern. The **only** issues at hand are whether loggerhead should be listed as “endangered” (*versus* “threatened”), whether the listing should be for the species as a whole or for multiple DPSs and, if for DPSs, then their boundaries.
- NMFS and the USFWS (the “Services”) have concluded that the global loggerhead population should be divided into nine DPSs and that seven should be “up-listed” to “endangered” status. **The arguments advanced in support of those conclusions are not credible and do not even address the issues**.
- **The proposed division of the species should be rejected** unless and until a case has been made for it. **The proposed “endangered” status would be entirely inappropriate**, either for the species as a whole or for the population nesting around the western North Atlantic and its marginal seas – a population with tens of thousands of breeding females, an overall abundance in the millions and no current downward trend in numbers.

- The global loggerhead population is undoubtedly spatially structured – as are those of almost all species. The issue at hand is whether the structure is relevant to the loggerhead’s listing under the ESA. By the Services’ long-standing policy and by Congressional direction, the authority to recognize DPSs is to be used “sparingly”. It is not sufficient to show that a species is spatially structured. The degree of discreteness must be sufficient to justify the sparing use of DPSs. The Services’ proposed nine DPSs would be, literally, unprecedented for any non-salmonid species – no other such species having been divided into more than three. Yet, far from arguing that such extreme subdivision was consistent with the “sparing” use of DPSs, the BRT never even used the word.
- By the Services’ policy, a DPS must be discrete and “markedly separated”. The BRT described assorted differences among proposed DPSs, though the evidence presented was variously over-stated, misinterpreted and misquoted. What the BRT did not do was to show that the quantitative degree of distinction amounted to marked separation. It did not so much fail to show that the proposed DPSs are discrete to the degree required as it failed to provide any relevant argument whatsoever. Indeed, the BRT claimed that the proposed DPSs are separated by vicariant barriers. Examination of known loggerhead movements shows that the barriers are actually behavioral and hence of unknown permeability.
- The policy also requires that a DPS have sufficient significance to justify the sparing use of the Services’ authority. The BRT offered claims of uniqueness and genetic difference but no credible argument of the marked differences required by policy. Some of the proposed DPS would, however, rise to a level of “significance” based on the gaps in loggerhead distribution that would arise if the

unit were extirpated. The Services' policy does not, however, contemplate a DPS which is not both discrete and significant.

- The suggestion that the DPSs be defined geographically is a major mistake, which would lead to individual loggerhead changing their DPS as they progress through life. Any subdivision should use DPSs based on the location of hatching and nesting, not on an individual's current position.

- Three arguments have been raised for declaring seven DPSs as "endangered".
- The first was "Computation of Susceptibility to Quasi-Extinction" – an approach designed for the IUCN's definition of endangerment (which focuses on relative decline in abundance) and entirely irrelevant to the ESA concepts. It uses complex mathematics to extrapolate over an untenable 100 years an oversimplified model loggerhead nesting data (ignoring other life stages and hence the conservation efforts made to date). The model is incapable of responding to recent trends and is disproven by the very data to which it was fitted. So clearly deficient is it that the BRT itself declared its results "not indicative of a true probability of quasi-extinction" – and yet the probability and imminence of extinction are the sole relevant issues when judging between "threatened" or "endangered" under the ESA. This analysis was not applied to representative data on each DPS. Rather, each available time-series was analyzed individually – though some were truncated and at least one used old, raw data, rather than available corrected values. The BRT used the analysis to consider risks of quasi-extinction over a 100-year span which is irrelevant to the imminence of extinction implied by the ESA's definition of "endangered". Further, the Team arbitrarily changed the critical value of the "SQE" metric, increasing the probability that loggerhead would be deemed "endangered".
- For the loggerhead of the western North Atlantic, the analysis actually indicated (entirely unreliably) that the population is safe from quasi-extinction any time in the next hundred years. The BRT did not draw that conclusion.
- The approach was applied to just five out of nine proposed DPSs. Two of the five were deemed to show increasing trends and thus were classed as "threatened". The remaining three, plus the four DPSs for which there were no time-series data, were classed as "endangered".
- The second modeling effort was a "Threat Matrix Model". The BRT concluded that "The analysis does not provide estimates for the likelihood or probability of extinction" and it should thus have been rejected as irrelevant to the question at hand. The BRT nevertheless sought to use this model to determine both trends in population abundance and the principal anthropogenic contributors to any declines. The Threat Matrix Model did not consider abundance and thus even if it could reliably have tracked the trends (which it did not), it could not provide guidance relevant to the BRT's task.
- The central problem with this model lay in the absence of knowledge required for parameterization. There is limited empirical evidence for the overall annual survival rate of adult female loggerhead. Unfortunately, the BRT linked that to an

unsupported assumption of 5% natural deaths and hence arbitrarily set anthropogenic deaths at two to three times the natural ones – a clearly unsustainable level. The results of the modeling were thus founded on an assumption alone. Indeed, the key inputs to this model were values for anthropogenic mortality rates. Those were based solely and explicitly on "expert opinion", without the experts being named, their expertise revealed or the means by which their opinions were gathered being stated. As a direct result of its complete reliance on opinion, the Threat Matrix Model must be dismissed as non-scientific and thus inadmissible as evidence in an ESA-listing decision.

- Even the BRT could not endorse the opinions, admitting that the model "may not represent the actual mortalities and may overestimate the anthropogenic mortalities" and that their own examination of the model "indicated potential overestimations by the experts of anthropogenic mortalities". Nor were these merely methodological deficiencies. In the few cases where data exist for scientific estimates of anthropogenic mortality rates, the values used by the BRT can be seen to be simply wrong – absurdly so in some cases. In short, the model was badly biased. It was entirely unsuited to its sole purpose as a direct consequence of the inadequacies in parameterization.
- The Services did not rely on their BRT's modeling but, in their *Federal Register* notice, gave primacy to discursive presentations of the time-series of nesting and of the dangers faced by loggerhead. Those were attempts to use words as substitutes for the quantitative modeling. Yet, the decision between "endangered" and "threatened" listings is one of degree and hence quantitative. Without numbers, the Services had no argument at all. Without an examination of the balance between growth, reproduction and death, a list of the many hazards is as meaningless as a list of all the prey items available to loggerhead.
- The Services' own policy demands "convincing information" before "up-listing" a species from "threatened" to "endangered". For loggerhead are concerned, they not merely failed to offer a convincing case, they have not even offered a credible one.

- **Reconsideration of the available data on the loggerhead of the western North Atlantic and its marginal seas confirms that this is not an endangered population and hence that loggerhead are not endangered as a species.**
- The population has nesting at many points between Guiana and Virginia, promising a resilience to threats based on broad distribution and intra-populational genetic diversity. It includes tens of thousands of adult females, a total female abundance (excluding eggs and hatchlings) of a few million and an overall population of several millions. Such a population may well be under threat but the threat would have to be dire indeed to raise the risk of imminent extinction required for "endangered" status under the ESA.
- From Florida to Mexico, slow rebuilding of depleted loggerhead was interrupted by a severe decline in nesting from 2000 or 2001 to 2004, with indications of a further drop in 2005–06. Nest numbers in peninsular Florida dropped by half,

those in Mexico by a third. It is not certain that numbers of females dropped by an equal amount but that would be a conservative assumption. Data from 2008 and 2009 (not used by the BRT) indicate that nesting has at least stabilized and may be increasing again.

- The sudden onset of the decline rules out most potential causes and shows that it was a loss of adults and older juveniles during the period that nest numbers fell, not the delayed effect of deaths of young juveniles long before. The absence of contemporary reports of a massive die-off of large loggerhead and an equal absence of any sharp increase in human activities threatening the turtles makes an anthropogenic cause unlikely, leaving the only probable cause as an epizootic. Unusual deaths of loggerhead, mostly of adult and near-adult sizes, were noted in southern Florida from fall 2000 through 2004 and tentatively attributed to a trematode infestation. Strandings data suggest epizootics in Florida waters in 2002–03 and again in 2006.
- Proof is not to be expected but few major mass-mortality events in marine species, exploited resource populations perhaps excepted, have better documented causes. Conservation-management of loggerhead should proceed, cautiously, on the basis that the observed declines in loggerhead nesting were caused by a trematode infestation – pending the emergence of contrary evidence
- The survey data suggest that the initial rapid decline in nesting ended some years ago. While the possibility of a repeat event remains, epizootics typically strike hard only once. Thus, it is likely that the cause of the decline has ended also.
- Juvenile abundance should have increased over recent decades, as the many conservation measures have taken effect – effects which will yet to have made much appearance amongst the adults. There are, however, no adequate in-water surveys capable of confirming that expectation. A number of available indices from the southeastern U.S. show the expected rise, as do some offshore indicators from areas further north but indices from the Chesapeake and Long Island Sound show major declines. Where the numbers have increased, so have the average sizes, suggesting the expected effects of relatively-abundant year-classes growing towards maturity. However, some of the indices showing decadal-scale increasing trends dipped from 2004 to the most recent available data in 2007 – corresponding to the second wave of the epizootic indicated by the strandings data. While the recruiting juveniles remain vulnerable to the pathogen, future trends in the loggerhead population will be unclear.
- While recovery of the loggerhead of the western North Atlantic cannot be guaranteed, with the sharp drop in nesting ended there is no reason to anticipate any on-going decline. Meanwhile, the abundance of the population is high enough to provide a buffer against short-term losses. Thus, **there is no prospect of imminent extinction and no justification for an "endangered" listing under the ESA**.

Introduction

Sea turtles are very special, charismatic animals. Each of the seven extant species is depleted below its natural abundance, some of theme very severely so, and all six which occur in the Northern Hemisphere are listed under the U.S. Endangered Species Act ("ESA")[1]. Five are listed as "endangered"[2] but the loggerhead turtle (*Caretta caretta*) is comparatively abundant and, since 1978, has been listed as "threatened". There are both legal and moral obligations on us all, collectively and individually, to rebuild the populations of the seven species to levels that can ensure their continued survival into the future. Achievement of that goal will face many conservation challenges – some of which will require considerable sacrifice on the part of the people, corporations and communities whose lives, work and businesses cause them to interact with the turtles.

Within the array of conservation tasks, in 2007 the Center for Biological Diversity, the Turtle Island Restoration Network and Oceana (hereafter "the petitioners") between them submitted two petitions to the National Marine Fisheries Service and the U.S. Fish & Wildlife Service (respectively "NMFS" and "USFWS", hereafter collectively termed "the Services") calling for loggerhead turtles in the North Pacific and those in the western North Atlantic to be "up-listed" from "threatened" to "endangered" status under the ESA. In response, early in 2008 NMFS assembled a thirteen-person Loggerhead Biological Review Team ("BRT") to undertake a status review of the species, the Team being composed mostly of staff of the Services, though with one member from each of the Florida Fish & Wildlife Research Institute and the North Carolina Wildlife Resources Commission. The BRT was charged with reviewing all relevant scientific information relating to loggerhead population structure, determining whether the global loggerhead population should be divided into Distinct Population Segments ("DPSs") and, if so, assessing the status of each DPS. The Team did not, however, respond within the statutory deadlines for twelve-month findings on the petitions (which fell due in July and November 2008) and in March 2009 the petitioners filed notice of an intent to sue, followed by the filing of a suit in May of that year.

The BRT's findings finally emerged in August 2009 as *Loggerhead Sea Turtle (*Caretta caretta*) 2009 Status Review under the U.S. Endangered Species Act* (Conant *et al*. 2009 – hereafter cited as "*Status Review*"). In October, the petitioners and the Services agreed to a February 19, 2010 deadline for the "twelve-month" findings required under the ESA. That was subsequently extended to March 8. The Services' findings eventually appeared in the *Federal Register* on March 16 (*Federal Register*, vol. 75, pp. 12598–12656). Following the conclusions of the BRT, they proposed that the global loggerhead population be divided into nine DPSs, two of which, in the opinion of the Services,

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[1] The flatback turtle occurs only in the waters north of Australia and breeds only on the beaches of that nation. It is not listed under the U.S. ESA.

[2] Green and olive ridley turtles have certain breeding populations listed as "endangered" while the remainder of each of those species is listed as "threatened".

should be listed as "threatened", with the other seven (including the only two whose ranges include portions of the coastlines and waters of the United States) being listed as "endangered". The *Federal Register* notice solicited public comments with a deadline of June 14, 2010 but, following requests to do so, that was subsequently extended to September 13[3].

The current document comprises comments arising from an independent scientific review of the BRT's *Status Review*, prepared for a consortium of fishing-industry interests and intended for submission to the Services along with the other responses to the *Federal Register* notice. As science-based comments, this document largely addresses the contents of the *Status Review*, which was itself the only formal analysis underpinning the *Federal Register* notice. I have, however, also considered that notice itself, particularly where the arguments it presents in support of the BRT's conclusions differ from those that the Team offered in the *Status Review*.

While the conservation of sea turtles must consider many factors and many approaches, the core of the Services' responses to the petitions should have been very much more focused. The importance of protecting turtles and ensuring their continued survival is not in question. Indeed, it is beyond doubt. The specific conservation measures needed to rebuild the turtle populations are of vital concern but were not the subject of the petitions. There is not even a question of whether loggerhead should be listed under the ESA. They already are and have been since 1978. The **only** issues at hand are whether loggerhead turtles should be listed as "endangered" (*versus* their current status as "threatened"), whether that determination should be made for the species as a whole or for multiple DPSs individually and, if loggerhead are to be subdivided, then what should the subdivision be.

Comments on the Services' announced findings can be even more focused. While it might be desirable, it is not necessary for a commentator to demonstrate that an "endangered" listing would be inappropriate for loggerhead before objecting to "up-listing". As a matter of long-established policy of the two Services, the 1996 *Endangered Species Petition Management Guidance*[4] states that:

> The responsible Service will make a "not warranted" finding if the status review does not provide convincing information to conclude that the species now meets the definition of an endangered species under the ESA. [*Endangered Species Petition Management Guidance*, Section III.C.2.b.(1).Situation 2.]

The onus thus lies with the Services to provide a convincing case that "endangered" (rather than "threatened") status is justified and, before objecting to the proposed "up-listing", it is sufficient for a commentator to show that no such case has been made.

[3] The information on the course of events leading to these comments has been extracted primarily from the *Federal Register* notice.

[4] Available as http://www.nmfs.noaa.gov/pr/pdfs/laws/petition_management.pdf

That is not simply a matter of policy but also one of logic: For conservation purposes, the principal loggerhead nesting area, on a global scale, comprises the beaches of peninsular Florida. As will be outlined later in these comments, during the last dozen years the annual counts of loggerhead nests on selected index beaches in that State plunged by 50%, strongly suggesting an equal drop in the numbers of adult breeding females (Witherington *et al*. 2009). That decline seems to have ended but its causes remain unknown and, while they do so, the prospects for the species will be uncertain. Moreover, within weeks of the publication of the *Federal Register* notice, an explosion aboard the drill ship *Deepwater Horizon* led to uncontrolled flow of oil and gas from a well off the coast of Louisiana – resulting in the worst anthropogenic environmental disaster to ever strike U.S. marine waters. It is too soon to say what the consequences of that event for loggerhead turtles may be. They may turn out to be negligible but they could prove to be dire. Potentially, when responding to the two petitions the Services might have built a case for an "endangered" listing from the decline in loggerhead nest numbers. Alternatively, they might in future build such a case from the unpredictable consequences of the *Deepwater Horizon* tragedy. Commentators cannot, however, be expected to counter hypothetical arguments which the Services might have been able to raise but did not (or have not yet). Nor can we be expected to comment on hypothetical cases supporting "endangered" status which might be prepared by third parties. The onus for preparing the case for a change in status lies with the Services and it is the case presented by the BRT in the *Status Review* and by the Services in the *Federal Register* notice which alone requires a response here.

The comments presented below thus address primarily the arguments that NMFS and the USFWS have raised in support of their conclusions that the global loggerhead turtle population should be divided into nine DPSs and that seven of those should be "up-listed" to "endangered" status. In brief outline: I find that those arguments offer no credible support for the conclusions and, indeed, that the analyses prepared by the BRT did not even address the relevant questions of whether any subpopulations of loggerhead merit DPS status and whether any DPS or the species globally merits "endangered" status. In presenting my review of the Services' arguments, I offer only limited comment on any of the proposed DPSs other than those in the western North Atlantic and in the North Pacific – those being the only two that occur in U.S. waters or on U.S. beaches Furthermore, while a large population of loggerhead nests on the beaches of Oman, data on its abundance are scarce and the unstable politics of the Middle East pose risks to its long-term prospects. Hence, the global conservation status of loggerhead hinges on the status of the western North Atlantic population, giving that one portion of the species exceptional importance.

While these comments are thus essentially confined to the Services' case, as set out in the *Status Review* and the *Federal Register*, I do offer a perspective on the status of the western North Atlantic loggerhead in order to show that the arguments in support of "endangered" status do not merely fail on technicalities. The current situation of loggerhead is **not** an example of a species collapsing towards extinction while the appointed BRT fails to adequately document an inescapable conclusion which it had itself reached. Rather, there is a very credible case to be made that loggerhead turtles are already correctly listed under the ESA as a single population with "threatened" status. If others have counter arguments to make, they need to be made and evaluated on their merits before "up-listing" is considered.

Distinct Population Segments

Along the Atlantic and Gulf of Mexico coasts of the United States, it makes little difference (to either turtles or humans) whether loggerhead are considered as a single unit for the purpose of an ESA listing or else divided into multiple DPSs: As noted above, the abundance of the populations nesting in peninsular Florida is so much larger than those elsewhere (Oman excepted) that the conservation status of Floridian loggerhead will determine the status of whichever unit it is included within, be that a single one for the entire species or a western North Atlantic DPS[5]. The number of loggerhead nesting in Japan is, however, very much lower and a North Pacific DPS, should such a unit be formally adopted, might well one day be classed as "endangered" when the species as a whole would not even be listed under the ESA. Hence, the question of subdivision of the species for ESA-listing purposes is important, at least to U.S. interests in the Pacific and to turtle conservation there[6].

Of perhaps greater importance here, the sections of the *Status Review* dealing with the DPS question advance a number of precedents which could have very unfortunate consequences if applied more generally. The DPS provisions of the ESA have been little used to date, their first application having only become formal in 1996, while their meaning and implications remain in dispute (e.g. Rosen 2007, Schiff & Thompson 2010). In a scenario of evolving understanding, the precedents offered in the *Status Review*, and echoed in the *Federal Register* notice, should not be allowed to stand without comment.

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[5] The *Status Review* uses the label "Northwest Atlantic DPS". The term "northwest Atlantic", however, is usually applied to the waters from Cape Cod to Cape Farewell, Greenland and eastward to mid-ocean – a region which sees few loggerhead, save for migrants passing in the Gulf Stream, and no nesting by them. By the most extreme interpretations, the "northwest Atlantic" extends only as far south as Cape Hatteras, while almost all the loggerhead nesting in the region is further south. The proposed DPS is thus better labeled as the "western North Atlantic" or perhaps, following international precedents, as the "west central Atlantic" one. The former is used here.
With the exception of its "Mediterranean Sea DPS", the *Status Review* also applied oceanic labels to the various loggerhead populations without mentioning those oceans' marginal seas. For convenience, I have followed that lead here but it should be understood that, for present purposes, the "western North Atlantic" includes both the Gulf of Mexico and the Caribbean Sea, plus lesser but still substantial water bodies such as Chesapeake Bay or Pamlico Sound. Likewise, the BRT's "north Indian Ocean" should be read as including the Arabian Sea, plus potentially such other areas as the Persian Gulf or the Gulf of Aden.

[6] Whether the loggerhead in other areas are or are not treated as DPSs would have direct and practical effects primarily only in so far as U.S. laws have extra-territorial implications. Those are not trivial but they are secondary to the effects considered in this commentary.

Sparing Use of the DPS Provision for Markedly Discrete Subpopulations

I would not in the least dispute the overall point that the global loggerhead population is structured – as is certainly normal (if perhaps not quite literally universal) for the populations of all other species. Turtle taxonomists are agreed that loggerhead form a single, monotypic, circumglobal species but that does not mean that they are members of one homogeneous population. The continents divide the sub-tropical and warm-temperate ocean basins inhabited by loggerhead, as do the equatorial regions, producing six major subdivisions of the loggerhead's environment – though some of the six may have more interchange than do others. The major nesting areas are near the western margins of each of those basins but some have additional nesting in the east, while the Mediterranean Sea adds a special case. Where loggerhead remain abundant enough, and sufficient research has been done, it is clear that there are finer subdivisions, at least among the nesting females (e.g. Bowen *et al*. 2004, 2005). The western North Atlantic population, for example, has been divided into five "Recovery Units" for conservation purposes [NMFS & USFWS 2008 (hereafter cited as *Recovery Plan*), pp. II-5 – II-6] and yet it is known that there is still-finer structuring within the major Units, with individual females showing some fidelity to particular nesting beaches and perhaps to particular parts of a beach – as is entirely typical of migratory animals when returning to the places where their young enter the world. All of that spatial structuring of the population is simultaneously expected, biologically interesting and important to loggerhead conservation efforts. The immediate issue at hand, however, is none of those. Rather, that issue is whether any of the structure is relevant to the loggerhead's listing under the ESA.

The Act is, of course, the Endangered Species Act and exists to prevent extinction of species. Its coverage extends to recognized subspecies (of which there are none in loggerhead) and, for vertebrate animals only, to Distinct Population Segments. However, by long-standing policy of the two Services, founded on an instruction in a Senate Report from the 96th Congress, the authority to recognize DPSs is to be used "sparingly" [*Federal Register* 61(6): 4725, February 7, 1996]. That direction from the Congress was not merely a matter that arose in debate during the 1978 reauthorization of the ESA, when the DPS provisions were added. Rather, in 1979 the GAO recommended to Congress that those provisions should be eliminated from the Act since they could potentially be used to list very local populations. Congress rejected the GAO advice but the Senate Committee acknowledged the dangers and hence added the direction that the authority to declare DPSs be used "sparingly" (Rosen 2007, Schiff & Thompson 2010).

Current listings under the ESA total 1953 species or subspecies, of which 947 are vertebrates. Only 17 of those have multiple listings, though some others comprise a single listed DPS from a species, the remainder of which is unlisted. Five of the 17 are Pacific salmons – a very special case, with their notorious population subdivisions among river systems, which required the development of an additional kind of unit, the "Evolutionary Significant Unit" or "ESU", which is considered to comprise a DPS for listing purposes. The remaining dozen, the entire set within the 947 listed vertebrates for which DPS designations have been applied as they might be with loggerhead, comprise grizzly bear (the Yellowstone population formerly being a DPS though its status is now subject to litigation), gray wolf (now just two DPSs, following the recovery of a third), Stellar sea lion (two DPSs), California tiger salamander (three DPSs), piping plover (two DPSs), roseate terns (two DPSs), green turtles (two DPSs), and five foreign species (two DPSs each). In almost every case, the DPS separation has been used to distinguish portions of a species which differ in their conservation status. Of the above dozen, only the dugong is divided into two DPSs both of which are endangered (one being the population in Palau, the other the remainder of the species throughout the Indo-Pacific basin), while two of the three California tiger salamander DPSs are endangered and the third threatened[7]. Without endorsing all of those designations, which I am not competent to do, the totals do appear to be appropriately sparing – a maximum of three DPSs per species (salmonids excluded) and usually only as many as are needed to accommodate the range of status of a species (i.e. one for any endangered portions, another for those threatened, while the rest of the species, if any, is not listed). Outside of the Pacific salmonids, carving up a species into multiple DPSs would be unprecedented.

Yet that is precisely what the BRT has proposed, and the Services have endorsed, for loggerhead, suggesting a division into no less than nine DPSs, seven to be separately listed as "endangered" while the remainder receive "threatened" designations. In arriving at that recommendation, however, instead of following the *Policy Regarding the Recognition of Distinct Vertebrate Population Segments*, the BRT never even used the word "sparing" in its *Status Review*, let alone offering any discussion whatsoever of whether its conclusions met that standard. The *Federal Register* notice (p. 12599) did use the word, just once, in stating the gist of the Service's policy. Its authors made no attempt to supplement the BRT's arguments with any *post hoc* justification that the extraordinary nine DPSs constitute a sparing application of the ESA's authority to designate such segments of a species.

Selection of Potential DPSs

The first phase in determining whether a species should be divided into multiple DPSs for ESA-listing purposes must be the delimitation of the subunits to be considered. Unfortunately, the BRT provided very little information on how they went about that process, though it seems that they did not attempt any formal analysis such as might have shown that their selection of nine particular units was the most efficient choice. They did claim to have "considered a vast array of information" and to have "discussed" the issue, when "the physical separation of ocean basins by continents was first considered" (*Status Review*, p. 16). They also reported that they chose the nine potential DPSs "Through the evaluation of genetic data, tagging data, telemetry, and demography" (*Status Review*, p. 18)[8] but they then seem to have plunged into examining the discreteness and

[7] Details of existing ESA listings taken from http://ecos.fws.gov/tess_public/TESSBoxscore#ij.

[8] Had their examination of discreteness progressed as far as statistical analysis, they would have faced a serious problem of circular reasoning: Having apparently used all available information in selecting the units to compare, tests of the efficiency of those chosen array of DPSs would have been badly distorted by

significance of the resulting units. Perhaps there is no reasonable way in which any more-formal analysis could have been conducted and maybe selection of the most obvious, large-scale geographic units was the only viable approach to selecting DPSs. It should however be noted, as did one of the external reviewers (Reviewer #2), that the IUCN has recently divided loggerhead into eleven units, splitting the BRT's proposed "South Atlantic" DPS into eastern and western units, while doing the same for the "north Indian Ocean" DPS. I am not able to say whether that extra division is or is not justified but the *Status Review* should have provided sufficient explanation for the BRT's choices that their reasons for rejecting both the IUCN arrangement and any number of other potential alternatives are clear to the reader. It does not – even though the BRT did cast doubt on the existence of any active loggerhead nesting on the African coast of the South Atlantic or on any coasts of the northern Indian Ocean aside from the principal areas around the Arabian Sea, other than a few nests in Sri Lanka.

Within this topic of delimitation of potential DPSs, the BRT fell into a grave error of quite another kind: It is entirely sensible to define DPSs with regard to geography but it is a mistake to give them geographic definitions. That is: A unit might sensibly be defined as, for example, the loggerhead of the western North Atlantic, based on the locations where the animals hatched and where the adult females subsequently nest, but the BRT was misguided in its decision to define the DPS as all those loggerhead currently in the western North Atlantic (*Status Review*, p. 31, cf. *Federal Register* notice, p. 12611). As a direct result of that choice, a juvenile loggerhead which hatches on a beach in Florida becomes a member of the "Northeast Atlantic Ocean DPS" when drifts across the 40°W meridian, as most surviving hatchlings from Florida do, and a member of the "Mediterranean Sea DPS" if it later swims through the Straits of Gibraltar to feed around the Balearics – as do many loggerhead from those same Floridian beaches. This postulated juvenile loggerhead would once more be a member of the "Northeast Atlantic" DPS when it died as bycatch on a pelagic longline off the Azores. Defining DPSs in such a way as to create these unnecessary changes in membership cannot be helpful to conservation efforts. It cannot be useful to deny the vulnerability of U.S.-hatched turtles to bycatch mortality in the eastern Atlantic through an arbitrary re-allocation of those turtles to a different DPS when they are on the longline grounds. Nor can it be helpful to imply that the Mediterranean loggerhead population is invulnerable to extinction because the western basin of that sea is continuously being re-colonized by Florida-hatched juveniles, which stay only long enough to feed before migrating back into the Atlantic.

Indeed, when the BRT came to parameterize their own Threat Matrix Model (discussed below), they ignored this aspect of their own definitions of their proposed DPSs and instead produced estimates of mortality rates based on where turtles hatched, rather than where they died – which was and is the only sensible way to group the animals. When reviewing the factors affecting the populations, however, the BRT resorted to having it both ways: Section 5.2.6 "pertains to all loggerhead turtles that may be found in the

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using the same data in the tests as in the design phase. Since the BRT never attempted to demonstrate that its choices were optimal, that particular problem did not arise.

Northwest Atlantic" but readers are told to also consult the sections on the northeast Atlantic and the Mediterranean (*Status Review* p. 129)! That introduces serious and quite unnecessary confusion.

This major error of the BRT's should be corrected immediately.

Discreteness of Proposed DPSs

Once potential DPSs have been delimited, the *Policy Regarding the Recognition of Distinct Vertebrate Population Segments* calls for two tests to be applied, in sequence, before considering the status of the unit for listing under the ESA. Those tests are, firstly, for the discreteness of the unit (in relation to the rest of its species) and, secondly, its significance to its species. The discreteness can be biological but may alternatively be a matter of international boundaries, where there are significant differences across the boundary in terms of the inadequacy of regulations. That option was not invoked by the BRT. In the case of biological discreteness among potential DPSs, it is not enough that the units are somewhat distinct. The *Policy* requires that they be "markedly separated", though it is broadly inclusive of the nature of the separation and the kinds of evidence which may demonstrate it.

Unfortunately, the BRT showed little overt recognition of this requirement that the separation be marked, which requirement carries an implied but consequent demand that the quantitative degree of separation be considered. Rather, the *Status Review* dealt with the discreteness broadly, while generally over-stating the case for a distinction among the proposed DPSs. The *Federal Register* notice did nothing to tighten the argument.

The best kind of evidence for discreteness of potential DPSs would be genetic but the *Status Review* offers little in the way of published information on nuclear DNA. It does cite unpublished studies and is correct to do so but only limited weight can be given to them: It is a commonplace of research to find that final analyses do not support the investigators' initial impressions, while "unpublished" means that work has not yet been subjected to peer review. The *Status Review* does have much to say of mitochondrial DNA ("mtDNA") but, while that is a very valuable source of information for many purposes, its strictly maternal inheritance limits the conclusions which it can support. The differences in observed mtDNA haplotype frequencies among loggerhead nesting populations certainly show that there is not free exchange of individual females amongst them but that has never been a plausible hypothesis – and it is not clear that much else of present relevance can be drawn from the existing mtDNA analyses[9].

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[9] At one point, the BRT went so far as to suggest that the "unique [mtDNA] haplotypes could represent adaptive differences" (Status Review, p.31). That misunderstanding of the nature of variations in the mtDNA control region should be passed over without comment, save to note that, were it true, all of the BRT's own use of haplotype variations as indicators of discreteness would be invalidated.

The BRT repeated past errors in the turtle literature by supposing that the age of the most recent exchange of animals between nesting populations can be judged from the age of the unique mtDNA haplotypes seen within them (e.g. *Status Review* p. 17). In reality, because of the absence of gene flow resulting from exclusively maternal inheritance, new mtDNA haplotypes arise sympatrically and could exist for very extensive periods (potentially millions of years) within their original populations before moving into a new area, all the while diverging from other haplotypes through the accumulation of mutations. Meanwhile, the small population sizes which have attracted ESA-related interest to loggerhead mean that founder effects and the continual narrowing of the numbers of founding ancestors which have surviving genetic offspring, which is inherent to exclusively maternal (or paternal) inheritance[10], will magnify the differences in haplotype frequency. Those complications are potentially amenable to population-genetic modeling and the existing mtDNA data might be used to estimate the probabilities of various rates of exchange of females among the breeding populations. The BRT, however, has not offered anything of the sort. Rather, they have casually used such misleading platitudes as "nesting colonies of Japanese loggerheads are found to be genetically distinct based on mtDNA analyses" (*Status Review*, p. 20), when they also report that recent, unpublished research shows that an archetypically South Pacific haplotype, found amongst turtles taken as bycatch in the North Pacific, has now also been found amongst females nesting on Japanese beaches (*Status Review*, p. 19). It may well be that that haplotype has long been present at low frequency in the North Pacific population but its presence in Japan might alternatively be the result of a low rate of exchange of breeding females between the two oceans. Until evidence and argument are assembled to reject that possibility, the presence of that "South Pacific" haplotype on Japanese beaches invalidates the claim of genetic distinction. Likewise, the BRT declared that "Brazilian rookeries have a unique mtDNA haplotype" (*Status Review*, p. 26), which they do, in the data from current studies of nesting females. However, as in the Pacific, the southern haplotype has been found amongst juveniles in the North Atlantic (*Status Review*, p. 26) and it may yet be found in breeding females there as sampling expands – with the same implications as the South Pacific haplotype has when found on the beaches of Japan.

Based on haplotype ages, the BRT declared that North Pacific loggerhead have been isolated from those in the South Pacific for one million years (*Status Review* p. 19) and that the Oman colonies supposedly last exchanged individuals with those in the Atlantic and the Mediterranean some quarter-million years ago (*Status Review* p. 25), while the

[10] This narrowing is most famously seen in humans, with all of us being descended from a single "mitochondrial Eve" a mere 200,000 years ago – despite the human breeding population at that time numbering tens of thousands. Its cause is that "mitochondrial lineage" of any adult female whose surviving offspring are all male is as surely extinct as the lineage of a female that has no young which survive to reproduce themselves. Amongst the surviving mitochondrial lineages, those in which the daughters fail to produce any granddaughters, the granddaughters fail to produce any great-granddaughters and so on likewise expire. When a population passes through an abundance bottleneck, as most turtle populations have over the last half-dozen generations, the diversity of mitochondrial lineages within them narrows swiftly.

Brazilian nesting population has been isolated from those in the North Atlantic for at least that long (*Status Review* p. 26). Such prolonged isolation is inherently unlikely, considering that the BRT's suggested isolating mechanisms are founded the interplay of continental barriers with the climatic zones and the subtropical oceanic gyres, both of which have been much altered by glacial cycles through the Pleistocene. It was only when considering, first, recolonization of North Atlantic beaches during the current interglacial and, second, the likelihood of a colonization event passing around the Cape of Good Hope that the BRT acknowledged the profound changes in loggerhead distributions which must have occurred in recent millennia (*Status Review* pp. 25–26). There is no indication in the *Status Review* that the Team noticed the inevitable invalidation of their other conclusions which must arise from such climate change.

In a further dangerous development, the BRT noted that recent mtDNA research using new primers "suggest[s] finer scale population structure" (*Status Review*, p. 19). I do not doubt the result nor the existence of the fine-scale structure. However, with maternally-inherited mtDNA, every female is the founder of her own discrete lineage. In a mitochondrial sense, every female is discrete and unique within her own generation, though linked in one direction to her mother and in the other to potential offspring. That uniqueness is certain without any data being collected and remains so even if current testing capabilities are incapable of detecting the differences in the mtDNA of different individuals – even if, indeed, the full sequences of the mtDNA are actually identical among the descendants of a recent common ancestor.

Meanwhile, laboratory capabilities to discern the variations among these lineages are continuously increasing. When it comes to human mtDNA, consumer-level tests can now distinguish such fine-scale subdivision as to be of interest to genealogists and it is likely that equally-powerful markers will be found for many ESA-listed species in due course. The rate of change in sea turtle DNA is much slower than that in other vertebrates (Bowen 2003), including humans, and even the finest variations in turtle mtDNA may prove have conservation relevance, though that remains to be demonstrated. Of greater concern, no precedent should be casually set through a loggerhead decision that would in future allow fine differences in mtDNA to be justification for DPS status, lest we be faced with listings of hundreds of distinct lineages in each species – contrary to the "sparing" use of DPSs, as required by the Services' *Policy*.

While highly-precise mtDNA studies will be immensely valuable in conservation efforts and should be fully utilized in developing recovery plans, close attention to the requirement that DPSs be "markedly separated" is essential when considering division of a species for listing purposes. In its *Status Review*, in contrast, the BRT gave no indication that they understood the problem. They certainly offered no solutions.

Another useful form of evidence of discreteness would be tagging data, which the BRT indeed made use of – though the Team again over-stated its case. The *Status Review* declares, for example, that "individuals from Japan remain in the North Pacific for their entire life cycle, never crossing the equator or mixing with individuals from the South

Pacific" (*Status Review*, p. 18). Aside, however, from the genetic evidence pertaining exclusively to the selection of breeding beaches by adult females, all that is offered in support of that statement is that "extensive satellite telemetry" [*sic*: only 200 turtles having been tracked, most likely for short periods] failed to detect a single crossing of the Equator, while no loggerhead tagged in the North Pacific have yet been recovered in the South (*Status Review*, p. 19). The latter argument is boosted by a note that more than 30,000 individuals have been marked (*Status Review*, p. 19) but the relevant statistic is not the number recovered south of the Equator (currently zero) divided by the number released but rather the number recovered south of the Line divided by the total number of recoveries – which the BRT summarized as "at least three" juveniles plus "several" adults (*Status Review*, p. 19). Indeed, any such ratio should be weighed by the efforts made towards having recoveries reported, which are likely to be larger in regions where the investigators expect success, and by the likelihood of anyone noticing the presence of a tagged individual, which is much higher on a nesting beach than elsewhere. With such small numbers, any conclusion then needs to be supported by statistical analysis, probably leading to estimates of the probability that various levels of exchange of individuals across the Equator might have occurred and yet not have been detected. Until such an analysis has been completed, which it has not, claims based on tagging results that loggerhead "never" cross the Equator in the Pacific must be rejected as lacking any scientific foundation whatsoever. Should the analyses be done, I fear that they would show that the probability of detecting of small, but not insignificant, rates of interchange is very low indeed – meaning that the BRT's claims would still be unsupported.

In the case of the discreteness of Indian Ocean populations, the BRT did not even offer that questionable level of evidence. Instead, the Team appealed firstly to "observations of tag returns and satellite telemetry" [when all they cited of that kind comprised six recoveries of tagged females and some hundreds of scale-clipped juveniles from Tongaland[11] (Baldwin *et al.* 2003), plus two dozen satellite-tracked individuals (*Status Review*, pp. 23–24)], secondly to "multiple lines of evidence", without stating what they were, next to an unsupported proposition "that significant vicariant barriers exist" and ultimately to a unanimous conclusion that three DPSs exist in that Ocean (*Status Review*, p. 25). The same reliance on unanimity was invoked for the separation of the Atlantic and Mediterranean loggerhead into four units (*Status Review*, p. 30). Among scientists, such an appeal to unanimity of opinion can usually be seen as an indicator of the absence of the supporting empirical evidence which alone could provide a foundation for scientific conclusions. Such facile arguments as those might suffice to confirm the obvious, that the global loggerhead population has a geographic substructure, but they cannot be sufficient to show that the level of "discreteness" rises to the "marked" separation required of a DPS.

[11] The name "Tongaland" for the northeastern-most coast lands of South Africa, abutting the border with Mozambique, is long established in the turtle literature and is used here. That area is now correctly called Maputaland.

Nor is this simply a matter of the BRT having failed to marshal the evidence for a high level of separation among the six subtropical oceans, nor indeed just a weakness in the currently-available data which might have been marshaled in that cause. Rather, there is evidence of considerable exchange of individuals among the nine proposed DPSs. The routine presence of juvenile loggerhead which hatched on the shores of the western North Atlantic in the eastern half of that ocean, and indeed in the Mediterranean, is well established and fully acknowledged in the *Status Review*. The mtDNA evidence for juveniles from Southern Hemisphere beaches being taken in fishery bycatches north of the Equator has likewise been admitted by the BRT. The cases of the Pacific and the Atlantic have already been referred to above[12]. In the Indian Ocean, one satellite-tracked adult female from Tongaland has been observed moving to waters off Somalia (*Status Review* p. 23). She is, to date, a lone example but the total number of satellite tags applied and successfully tracked has not been high.

It thus appears that the only major physical barriers to loggerhead movement are those created by the continents. It seems reasonable to assume that there is no direct exchange between the Pacific and the Atlantic, neither around Cape Horn nor *via* the Bering Strait and the Arctic Ocean. The minimum temperatures along either route are far too low for loggerhead. Significant direct exchange between the Mediterranean and Indian Ocean basins, *via* Suez and the Red Sea, also seems unlikely though for the reverse reason: excessive heat and salinity, particularly in the Great Bitter Lake. Whether there are exchanges between the Pacific and Indian Oceans, either between the Southeast Asian landmass and Australia or else to the southward of the latter remains unclear – not least because loggerhead are so scarce on either side of the Indonesian archipelago that it would be difficult to detect even exchanges of a significant fraction of the few animals there. With the Malacca Strait lying barely north of the equator and Torres Strait at 10° South, the presence of loggerhead (normal thought of as a subtropical and warm-temperate species) might seem unlikely but individuals which nest on the east coast of Australia have been captured in eastern Indonesia (Limpus & Limpus 2003b) and both Queensland and Western Australian loggerhead have been taken on foraging grounds on the Arnhem Land coast of the Northern Territory (Limpus & Limpus 2003a, Baldwin *et al*. 2003). The alternative route south of Australia would have to lead through the Bass Strait but, at 40°S, that is well within the observed Northern Hemisphere latitudinal range of loggerhead, while typical water temperatures are no lower than those tolerated by the species elsewhere.

The more interesting example of exchange between ocean basins concerns loggerhead movements around the Cape of Good Hope, between the Indian and South Atlantic Oceans. The BRT, citing Baldwin *et al*. (2003), declared that the range of the proposed southwest Indian Ocean DPS extends to Cape Agulhas, "the southernmost point of

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[12] The BRT sought to deny the relevance of the distinctive South Pacific mtDNA haplotype amongst North Pacific bycatches by invoking "unpublished data" on nuclear DNA. Any such denial would need the support, not of data but of analysis. "Unpublished" in this case appears to mean "incomplete" and thus not evidence at all.

Africa" (*Status Review*, p. 23) and even "There is also no evidence of movement of adult Southwest Indian Ocean loggerheads west of 20ºE longitude at Cape Agulhas" (*Status Review* p. 32). Perhaps the Team was unaware that the Agulhas Current flows westwards past the Cape. Although most of its water turns through the Agulhas Retroflection and returns to the Indian Ocean, retroflection eddies or "Agulhas rings" are periodically pinched off and enter the flow of the Benguela Current in the South Atlantic. That movement of ocean water westwards around the Cape was long-since revealed by plankton distributions and became an accepted fact of marine biogeography half a century ago. Any oceanic species with a range extending to Cape Agulhas must, therefore, see bodies of water suited to its survival regularly passing between the Indian and South Atlantic oceans, with the organisms themselves moving that way also unless they have behavioral patterns preventing their translocation. None of that should be any surprise to the BRT: It was outlined in Bowen's (2003) chapter on loggerhead genetics, which is repeatedly cited in the *Status Review*, while the BRT also cited Bowen & Karl (2007), who noted "two effective transfers of matrilines between the Atlantic and Indian Oceans […] One of these appears to be very recent". Indeed, Baldwin *et al*. (2003), the BRT's chosen source for this topic, actually described the foraging range of the Indian Ocean loggerhead as being "southwards as far as Cape Agulhas <u>and into the Atlantic Ocean</u>" [emphasis added]. They also noted that the Tongaland hatchlings enter the Agulhas Current "and are swept southwards, some even rounding the Cape <u>and entering the Atlantic Ocean</u>" [emphasis added]. That was not new information in 2003. Hughes (2010) has recently noted that recoveries of marked hatchlings from the Tongaland beaches showed "an annual leak of loggerhead genes into the Atlantic Ocean", while citing a source published in 1978, and the confirmation of that movement by DNA methods as early as 1994. Indeed, the *Status Review*'s External Reviewer #6, Dr. Ronel Nel, specifically warned the BRT of her expectation that Tongaland loggerhead regularly enter the Benguela Current. What she was not yet able to say in her review was that two adult females satellite-tagged on the beaches of northeastern South Africa in January 2009 by her program proceeded to swim into the South Atlantic – one reaching a point off Namibia and the other a mid-ocean position west of Angola before their transmitters failed[13]. Such very recent research should not be relied upon until its principal investigators have had a chance to consider the meaning of the data and have prepared a formal report. However, it would appear that Dr. Nel's expectation, Baldwin *et al.*'s (2003) broad-brush summary and the earlier studies running back 30 years have all been confirmed, while the BRT's insistence that southernmost Africa is an effective barrier to loggerhead movement has been refuted[14].

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13 http://www.ioseaturtles.org/pom_detail.php?id=85

14 Curiously, the final text of the *Status Review* made no acknowledgement whatever of External Reviewer #6's points, unless that was the self-contradictory comment on p. 17 that "some dispersal from the Tongaland rookery in the Indian Ocean into feeding and developmental habitat in the South Atlantic is possible via the Agulhas current". The BRT did not acknowledge that such dispersal undermined one of the key tenets of their argument. That blatant disregard of a reviewer's rebuttal calls into question whether the *Status Review* can be regarded as a peer-reviewed, scientific document at all.

The BRT, meanwhile, insisted upon a date of *circa* 12,000 years ago for the most recent exchanges of loggerhead between the Atlantic and Indian oceans, based on the earlier misapplication of the ages of mtDNA haplotypes as *termini ante quem* for the most recent exchange. They went so far as to repeat speculation by Bowen (2003) that the movements between the two oceans occurred during a warm period and became rare as the region changed into a cold-temperate one (*Status Review* p. 25). Unfortunately, what Bowen (2003) actually wrote, while citing his own earlier work, was that the most recent exchange had occurred "as recently as the current interglacial period (< 12,000 years before present)" – a *terminus post quem* which is fully consistent with on-going exchanges today. His speculation about the effects of climatic changes on loggerhead movement around the tip of Africa did not relate to passage being possible around 12,000 but subsequently closed off. He was thinking of much longer timescales, with the route being effectively closed to loggerhead some 2.5 million years ago, though with some exchange during subsequent warm periods (Bowen 2003). The BRT might also have considered that its preferred limiting date of *circa* 12,000 years ago was the mid-point of the (Northern Hemisphere) Younger Dryas – the principal cold period since the end of the last glaciation. It was also a time of transition in the Southern Hemisphere from the Antarctic Cold Reversal into a warm period which lasted from 11,500 to 9,000 years ago. Thus, exchanges of loggerhead around Cape Agulhas were <u>less</u> likely 12,000 years ago then than they are in the warmer climate today. Besides, the *Status Review* would have us believe that there was an interchange of loggerhead between the North Atlantic and the Indian Ocean 12,000 years ago, when there has been no exchange between the breeding populations in the North and South Atlantic in 250,000 years and no female descendants of the migrants of the Younger Dryas have crossed the Equator in the Indian Ocean and become established on the shores of Arabia. That, while not strictly impossible, stretches credulity.

In reality and under existing climatic conditions, the limited available evidence is thus sufficient to at least indicate, and in most cases to confirm beyond doubt, that there is regular and routine intermingling of loggerhead between the proposed North and South Pacific DPSs, between the latter and the Southeast Indian Ocean DPS, between the North and Southwest Indian Ocean DPSs, between the latter and the South Atlantic DPS, between the latter and the two proposed North Atlantic DPSs and most especially between the three units proposed for DPS status in the North Atlantic and Mediterranean basins. The sole linkage within the species which has yet to be demonstrated, east to west across the Indian Ocean, has likely been obscured only by a lack of turtles and of turtle research in most of that basin. These are not the rare events across geological time that the *Status Review* and the *Federal Register* notice admit to. They are an entirely normal part of the lifecycle of the turtles, occurring in current times and expected to continue for the foreseeable future.

Where the distinction between the proposed DPSs may arise is among the breeding adults or at least the breeding females. The overwhelming majority of those do home to their natal regions to nest and perhaps return, in not to the beach where they hatched, then to the same stretch of coast (Bowen *et al.* 2004). That form of distinction is indeed important to the DPS question. Where the BRT erred was not in claiming a partial separation among ocean basins but in concluding that the barriers are oceanographic, continental or "vicariant". Those barriers are, very clearly, behavioral, resulting from behavior which either cause intermingled turtles to return to their natal regions before nesting or else prevent displaced individuals from nesting at all. That error on the part of the BRT was critical because, while the Americas form a barrier between the Atlantic and Pacific which is impenetrable to loggerhead, behavioral barriers have some failure rate. Even the *Status Review* admits that the known distribution of the species can only be explained by post-glacial colonizations, which require that some individuals resist the urge to home and instead seek out new beaches.

That being so, the key question of the degree of distinction between the proposed DPSs becomes a question of the failure rate of the homing behaviour: What percentage of loggerhead females, when reaching sexual maturity, fail to return to their natal regions and instead join some other rookery? It is clear that that percentage is small or else the observed differences in mtDNA haplotype frequencies among the ocean basins could not be maintained. It is also clear that the percentage is not zero or the species would long since have dwindled to extinction. Indeed, loggerhead are known to hybridize with other turtle species (Bowen 2003), so the behavioral mechanisms keeping different populations apart cannot be very strong – at least in the males.

What is not known is the one thing that matters to the distinction among proposed DPSs: Whether the rate of exchange among rookeries is of conservation significance or, conversely, whether the populations in the various basins are "markedly separated" and hence rise to a level of discreteness that might justify DPS status. Having failed to recognize that the barriers are behavioral, the BRT made no attempt to examine their failure rate. In truth, it is unlikely that they could have proceeded far: There has been some examination of the extent of interchange on a much more local scale, such as within Florida (Bowen 2003), but neither tagging of hatchlings nor genetic studies have yet been attempted on anywhere near a sufficient scale to quantify the low levels of inter-ocean exchange of females which survive to breed. Without that information, no meaningful determination of the level of distinction among the proposed DPSs is even possible[15].

In summary, the BRT did not so much fail to show that its nine proposed DPSs are discrete to the degree required of a DPS by the *Policy Regarding the Recognition of*

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15 The BRT's declaration that "There is no evidence or reason to believe that female loggerheads from South Pacific nesting beaches would repopulate the North Pacific nesting beaches should those nesting assemblages be lost " (*Status Review* p. 31), and the repetitions of the same sentence with respect to other regions, is literally true. Should an entire regional nesting population be wiped out, it is unlikely that a future colonization event would see adoption of exactly the same as had been used before. That, however, is not a relevant consideration when determining whether proposed DPSs are "discrete". The question at hand is whether or not the existing population utilizing the beaches in one region receives recruiting adults which hatched on beaches outside that region in sufficient numbers to be of conservation relevance. There very much is reason to think that they may do so, whatever the beliefs of the BRT members, while there is insufficient evidence to answer the question in either direction.

Distinct Vertebrate Population Segments as it failed to provide any relevant argument whatsoever. The *Status Review* quite simply fails to offer any support for the proposal that has any bearing on the requirement that the units, which are undoubtedly partially separated from one another, be “markedly discrete”.

Significance of Proposed DPSs

Should more developed analyses of the existing data, or else knowledge from later studies, one day establish that the loggerhead in the different ocean basins are indeed discrete in the meaning of the *Policy*, it would still be necessary to show that the proposed DPSs have sufficient “biological and ecological significance”, “considered in light of Congressional guidance […] that the authority to list DPS’s be used […] sparingly”. The *Policy* is explicitly open-ended in the types of significance which might be invoked but mentions occupation of “an ecological setting unusual or unique for the taxon”, a distribution such that loss of the DPS would create “a significant gap in the range” of the taxon, and genetic characteristics which differ “markedly” from other populations.

For the population nesting in Japan, the BRT argued that the North Pacific Ocean is a distinct ecological setting from those occupied by other loggerhead (*Status Review*, p. 20). It is, of course: The five great sub-tropical oceans are each different from the others, the three southern ones inter-connecting at mid- to high-latitudes, the North Atlantic being open to the Arctic Ocean and only the North Pacific being effectively closed towards the pole – firstly by the Aleutian chain and then, more definitively, by the narrow, shallow Bering Strait. The northern Indian Ocean is even more unusual, barely reaching beyond the tropical zone at all, while being divided into two by the Indian Subcontinent which stretches south almost to the Equator. With those and other profound differences in basin morphology, it is no surprise that the Kuroshio, the Gulf Stream and the Tasman, Agulhas and Brazil currents are not exact analogs of each other, while the northern Indian Ocean has no true western boundary current at all. Nor are the biotic communities in the various oceans the same, the North Atlantic being notably depauperate, while the Indo-Pacific has the richest fauna. Those differences are very real but they are also no greater than the differences among adjacent river systems or nearby lakes. The differences among the five sub-tropical gyres simply do not rise to the required level of significance for the authority to invoke DPSs to be used “sparingly”. If the North Pacific ecosystem was to be considered a unique setting for loggerhead, then every geographically-delimited ecosystem would have to be considered unique for every species living there and the *Policy’s* test for significance would be rendered meaningless.

The BRT likewise sought to claim a genetic difference between North Pacific loggerhead and those elsewhere (*Status Review*, pp. 20–21). That too is real but no evidence is brought forward suggesting that it is any larger than would be expected among the discrete populations of any other species – while there would be no point in the test of significance if every population which passed the test of discreteness was automatically deemed "significant". The BRT did not advance any argument that the genetic characteristics differ "markedly" among the proposed DPSs, as suggested by the *Policy*.

Elsewhere, the BRT also sought to invoke the *Policy's* suggestion that a potential gap in a species range could be grounds for "significance". The Team raised no logical argument, however, merely offering the bald statement: "The loss of the North Pacific population segment would result in a significant gap in the range of the taxon" (*Status Review*, p. 31). Obviously, the loss of any geographically-defined, discrete portion of a population would result in some part of that population's range being lost but, if that were to be the definition of a "gap" then every discrete unit of any population would be considered "significant": A range contraction implicit to the loss of any sub-unit cannot rise above the commonplace, while a test of significance that every discrete unit would pass would be no test at all.

There seem to be two logical situations under which a range gap might be deemed "significant". Most importantly, I would suggest that the *Policy's* focus on gaps relates to the very real conservation concern, albeit more with terrestrial species, of habitat fragmentation. A "gap" in that sense would involve breaking a connection between other portions of the population's range – though whether the connected portions could be considered discrete enough to be potential DPSs must then be doubtful. That form of gap cannot, however, apply to the North Pacific loggerhead. Their loss, tragic though it would be, would involve only a withdrawal from the extremity of the species' range, not a break within that range. The other possible interpretation of the significance of a range gap is that the potential loss of some population segment would cause a range contraction covering a substantial fraction of the species range. It is not clear whether that is the intent of the *Policy* nor whether it was the intent of the BRT but the *Federal Register* notice did make repeated reference to the great size of the ranges occupied by each proposed DPS – going so far as to term most of them "hemispheric" (e.g. "The North Indian Ocean population segment encompasses an entire hemispheric ocean basin", *Federal Register* notice, p. 12610), which was a most curious abuse of that term[16]. Still, the North Pacific Ocean most certainly covers a substantial fraction of the loggerhead's total range (indeed of the total surface area of the planet) and perhaps a North Pacific DPS, should its discreteness ever be established, might be judged "significant" on that basis alone.

The "significance" other eight proposed DPSs are of less immediate concern, for the reasons already stated. Suffice to say that the BRT's arguments were generally similar to those offered for its North Pacific DPS, which included a restatement of the ill-founded notion that each unit is genetically distinct, though the validity of some of those arguments may be somewhat greater for some of the other proposed DPSs – potential loss

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[16] A hemisphere is half of a sphere and a reasonable description of the entire Pacific Ocean, North and South together, if something of an exaggeration even then. The small fraction of the Indian Ocean lying north of the Equator is the fraction within the Northern Hemisphere but it is not a discrete ocean basin bathymetrically and it most certainly does not cover a hemisphere.

of any of the Southern Hemisphere populations more plausibly creating a gap in the distribution, for example, while the northern Indian Ocean does have an environment occupied by loggerhead that is rather more unique than any other.

Summary of DPS Proposal

In summary, the BRT entirely failed to address the "discreteness" requirement for a DPS. It is beyond doubt that there are barriers to free exchange of adult females among the proposed nine units but no analysis has been performed and no argument advanced which might have indicated that the quantitative degree of distinction rises to the requirement that they be "markedly discrete". Furthermore, while the BRT has made claims about their nine proposed units meeting the ecological, genetic and range-gap criteria for "significance", the first is clearly false, the second at best unproven and the third only applicable to some of the proposed units, unless "gap" is to be interpreted in merely quantitative terms.

The proposed separation of loggerhead into nine DPSs would be, quite literally, unprecedented for any non-salmonid species. Such a dramatic change in the use of the Services' constrained authority to declare DPSs would not necessarily be wrong but it would require a particularly well-founded justification. Instead, the BRT offered no credible support whatsoever for its proposal. I can only conclude that the proposal should be rejected unless and until a much better-founded case has been made for it.

I would further recommend that the Services ensure that the BRT's arguments do not become precedents for similar analyses of other species. The existing *Policy* requirement that DPSs be "markedly discrete" is a quantitative one and, if addressed through genetics, requires quantitative population-genetic modeling – modeling specifically suited to mtDNA if that is the source of the data examined. Tagging data, which will usually be scarce with a species rare enough to be considered for ESA listing, likewise requires quantitative analysis leading to estimated probabilities of various levels of exchange, not mere self-serving, off-the-cuff interpretations. Claims of barriers in the ocean need to be based on a right understanding of both current and palaeo-oceanography, not on guesses that support *a priori* choices of units to designate as DPSs. Tests of significance need credible arguments that the differences claimed are indeed significant. The BRT failed in all of those, rendering its *Status Review* simply a waste of paper.

Susceptibility to Quasi-Extinction

The BRT's primary task was to determine whether loggerhead, treated as a single, globally-distributed species or as multiple DPSs, should correctly be listed under the ESA as "endangered" or, conversely, whether they should remain as "threatened". When it came to their final "Conclusions", presented in Section 6 of the *Status Review* (on a DPS-by-DPS basis only: *Status Review* pp. 161–166), the BRT raised some unsupported speculations that assorted sources of anthropogenic mortality cannot "be sufficiently reduced in the near future", "will become an even more substantial threat" or are "likely to continue and may increase". Only for protection of nesting females in the northeast Atlantic did the BRT see hope of an improving trend. Those extraneous thoughts were grossly inappropriate: new ideas introduced as "conclusions", without the presentation of any supporting evidence or even discussion. Otherwise, the Team's conclusions rested solely on the results of two exercises in mathematical modeling, the first entitled "Computation of Susceptibility to Quasi-Extinction" (here abbreviated as the "SQE" approach or model) and the second termed a "Threat Matrix Model"[17]. As is explained in the next section below, by the BRT's own admission the latter can give no support to any conclusions about whether loggerhead are "threatened" or "endangered". Hence, the central question facing the BRT, "Do loggerhead turtles merit being listed as 'endangered' rather than 'threatened'?", is actually addressed in just those 17 of the *Status Review*'s 166 pages of text which concern the SQE approach. Unfortunately for the Services' conclusions, the argument developed there is so deficient that it offers them no support whatsoever.

The presentation of the SQE approach in the *Status Review* is so opaque as to defy comprehension while Snover & Heppell's (2009) original proposal of the technique provides little clarification[18]. That paper does, however, cast some light on its authors' intent for their approach. Most importantly, it is clear from Snover & Heppell's (2009) work that the SQE approach was designed for application with the criteria for "endangered" status used by the International Union for Conservation of Nature ("IUCN") in an international, advisory context, which are fundamentally different to the definition of "endangered" under the ESA. Whether the approach actually has any utility in the IUCN context remains to be determined: Its authors failed to demonstrate its value (presenting instead only a circularity of reasoning) and it has been too recently proposed for other scientists to have yet investigated its properties. What is certain, following from the BRT's ill-considered attempt to apply it in the very different national context under

[17] So stylized were the BRT's "Conclusions" in their Section 6 that the use of the "two approaches" was claimed for each of the nine proposed DPSs – including the four for which no data exist for fitting the SQE model and hence to which that approach was not applied.
An earlier "Synthesis" (*Status Review* Section 4.5, pp. 83–85) presented much the same conclusions, though in a less-stylized format.

[18] It may be noted that Dr. Snover was a member of the BRT. Whether her natural interest in her own published work contributed to an inappropriate choice of analytical approaches by the BRT is a matter for speculation that cannot be addressed here.

the ESA, is that this SQE approach has no utility within the regulatory regime implemented under U.S. law[19].

Model Structure

The mathematics of the SQE approach uses a "diffusion approximation" to include variability and uncertainty in the calculations but the underlying model is a very simple exponential model of the year-to-year change in population size: $N_{t+1} = N_t e^{\mu}$ (*Status Review* p. 37). With such a model structure (and ignoring the effects of variability, which the SQE approach does not), any positive value of μ will inevitably result in projections of population size which, over time, accelerate towards infinity. Conversely, any negative value of μ will produce population abundances declining ever more slowly, reaching zero only after infinite time. (In such an exponential model, and still ignoring variability, the rates of increase or decrease are constant – in the sense of a constant annual fraction of the existing abundance at any time.) Such a model can have great utility in some situations, particularly when a depressed population is released from whatever pressures pushed the numbers down, but it also has grave limitations. For one thing, no trust can be placed in the outputs of such a simple model if it is extrapolated far into the future since it necessarily assumes that present conditions (and hence present trends in abundance) will continue unchanged – which is the one thing that will <u>not</u> happen[20]. Secondly, the model cannot reliably be extrapolated through so many years as to radically change population abundance, since it ignores the inevitable density dependence in the dynamics of any population (the error being most obvious when the model projects an impossible, infinitely large abundance). Then again, the SQE model does not incorporate any biological information whatsoever, save for some abundance estimates used as input data. The mathematics are entirely blind as to whether the input counts represent numbers of turtle nests or numbers of whooping cranes. Indeed, they could just as well represent numbers of radioactivity decay events (for which the exponential model would be appropriate), numbers of road traffic accidents, the retail price index or anything else. The SQE model is not a model of a biological system but merely a way of representing temporal change, with the trend over time forced to conform to the expectations of a constant rate of proportionate increase or decrease (aside from inter-annual variability).

To apply the approach to loggerhead, the authors needed time series of abundances to which they could fit the model. There are no really adequate in-water population estimates of loggerhead and the BRT necessarily confined itself to data from nesting beaches – counts of either nests or breeding females. There are a number of such data sets, some of which are of quite high quality, most notably the Florida index-beach

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[19] The use of an IUCN-oriented analytical approach appears to have led the BRT towards an implicit application of the IUCN's listing criteria in what was an ESA listing decision. Besides being inappropriate under U.S. law, that was a most unfortunate step: A recent careful and sober review of the issue has concluded that "the current [IUCN] Red Listing criteria have resulted in biological assessments of marine turtles that are inaccurate and misleading for conservation priority setting" (Seminoff & Shanker 2008).

[20] The IUCN's criteria for classifying species as endangered explicitly consider risks of extinction if present conditions continue unchanged. There is no such expectation under the ESA.

surveys which cover the principal nesting areas of the western North Atlantic loggerhead (Witherington *et al*. 2009). Unfortunately, the blindness of the model's simplistic mathematics leads to results which blithely ignore this limitation to data on adult females and their nesting. The adult males can perhaps be passed over for present conservation purposes (since a few male loggerhead seem fully capable of successfully mating with many females). The juveniles cannot be. Loggerhead mature, on average, at ages of some 30 years, while much of the conservation effort over recent decades has been directed towards the accessible life stages of the species – particularly the eggs and hatchlings. The late maturation of the species creates a very long lag time between those conservation efforts and any observable response in numbers of nesting females or their nests. There is a further lag before the full effect of will be seen, since the numbers of mature females reflect not just current recruitment from the juvenile population but also past recruitment and adult survival rates – which, in long-lived turtles, are high by the standards of most marine organisms (estimates running around 0.85 annually). Hence, none of the efforts to protect loggerhead nests and hatchlings which have been made since 1990 have yet been reflected in the nesting data used to fit the SQE model. Even conservation measures (focused on nests and hatchlings) introduced when loggerhead was first listed under the ESA in 1978 will only have begun to affected the nest counts on the Florida Index Beaches before the 2007 data cut-off used in the BRT's SQE modeling and will not have their full effect on those data for at least another two decades. There are shorter lag times between the introduction of measures to protect older, neritic juvenile loggerhead (such as requirements for fitting shrimp trawls with Turtle Excluder Devices: "TEDs") and the appearance of the consequences of those measures on the nesting beaches. Indeed, the first appearance of the benefits from modern, large TEDs should follow almost immediately. The full effect will not be seen, however, until all surviving year-classes of loggerhead have experienced only TED-equipped nets since their first return from oceanic waters. That will not be for another decade at least. In short, and even were there no other deficiencies, the BRT's application of the SQE approach could not have extrapolated current trends in loggerhead conservation but only a melding of the consequences of current conditions acting on adults with those of very different past conditions, approximately a quarter- to a half-century ago, acting on juveniles – a decidedly meaningless exercise. The BRT itself was not ignorant of this problem and noted the limitation to data on nests and hence nesting adults (*Status Review* p. 83). They misunderstood the deficiency in their work, however, and suggested that mortality on younger turtles "will not yet be reflected" in nest counts when the issue is one of the <u>changes in</u> that mortality rate since the 1970s not being reflected – a matter of the entire machinery of existing conservation measures and efforts being ignored by the Team's chosen SQE approach.

Worse still, the very data that the BRT used to fit the SQE approach invalidate that model, at least where the two proposed DPSs of principal interest are concerned. The sole time series of abundance for North Pacific loggerhead comes from the Japanese nesting beaches. As selected by the BRT, that index has shown an eight-year decline in numbers, followed by an increase of equal duration (*Status Review* Fig. 1, p. 40). Had the Team considered a longer run of data from the sub-set of Japanese beaches for which counts are available, those would have shown that the post-1990 decline was preceded by a marked increase in nesting during the 1980s. Indeed, the limited available data suggest that there may have been an increase in numbers in the 1950s followed by a decline in the 1960s (Kamezaki *et al.* 2003), suggesting the possibility of some sort of cyclic behavior. For their part, the peninsular Florida index-beach data show a dozen years of increasing numbers, followed by a sharp decline (*Status Review* Fig. 4, p. 44) which seems to have now leveled out, though that is not so evident in the truncated version of the time series used by the BRT. Thus, those two data sets (the only two used by the BRT in SQE modeling which really matter to the question of the status of loggerhead under the ESA) both show "non-monotonic" behavior, meaning that they neither progressively increase nor progressively decrease. Rather, each data set shows at least two stanzas, with very different trends in abundance indicating very different underlying factors driving the populations during the different periods. What each data set is struggling to tell the investigators is that they cannot usefully be modeled by any equation which assumes a steady trend – yet that is exactly what the BRT did. The results of such efforts can be nothing but absurd.

The BRT was not unaware of some of these limitations. Citing Snover & Heppell (2009), the *Status Review* declared that "SQE values are not indicative of a true probability of quasi-extinction […] Rather, the index serves as a tool for classifying populations by relative status" (*Status Review*, p. 36). Unfortunately, the BRT then proceeded to ignore that limitation and used the model to conclude that the probability of extinction of loggerhead surpasses the limit for "endangered" status. To do so, they followed an IUCN guideline and extrapolated their calculations through 100 years – stretching their model far beyond its breaking point.

Selection of Data

Had the approach been appropriate, which it was not, the BRT would have had to fit their SQE model to data representative of the abundance of either loggerhead turtles as a species or of the population in each DPS. The Team made no evident attempt to do so. Instead, the BRT seems to have used every time series of counts of either loggerhead nests or else breeding females of the species known to its members, subject only to a constraint that the series extend over at least 12 years[21]. The *Status Review* offers no discussion whatsoever of whether or not the counts used were representative of trends in nesting in their respective DPSs. Where more than one time series was available for a single DPS, the BRT neither combined them into one representative index, nor merged them during analysis (perhaps in some sort of weighted average), nor selected the one series showing the greatest abundance, which might perhaps have best defined the

[21] The originators of the SQE approach, Snover & Heppell (2009), suggested that increasing the length of a data run above 15 years did not have a large impact on the accuracy of 100-year extrapolations – though their Figure 4 suggests quite otherwise, even within the circular reasoning that those authors employed in judging accuracy. There does not seem to be any analytical support for the BRT's decision to use even shorter data sets.

conservation status of the DPS. Instead, each time series was analyzed separately. Whatever its biological merits, that choice rendered the analytical results inapplicable to the question at hand, since each DPS can only be listed once under the ESA and the decision on whether it should be "threatened" or "endangered" must be made at the DPS level, not on some subunit for which data are available.

Nor were the full available data sets used. For the western North Atlantic, the BRT cut off the Florida Index-Beach nest-count time series at 2007, which was the lowest count on record, without offering any explanation or even a statement that later data existed but had not been used. In contrast, the loggerhead *Recovery Plan*, published in December 2008 (eight months before the *Status Report* emerged), managed to include the 2008 data point (*Recovery Plan*, p. I-9). It was the 6th from lowest count in the time series, approximating the counts of 1989, 2002 and 2003, and its inclusion would have changed the output of the SQE model – not sufficiently to create an appearance of a positive trend from 1989 to 2008 but quite enough to materially alter the quantitative degree of the model's extrapolated decline in abundance. As will be shown below, it is the extent of that decline which is critical to the choice between "threatened" and "endangered" status (or would be if the rest of the approach were valid)[22].

Another interesting example of the BRT's unacknowledged and unexplained data selection emerges in the case of their proposed southwest Indian Ocean DPS. For that region, the only data suitable for the Team's SQE analysis come from the beaches of Tongaland, South Africa, where nightly foot and vehicle patrols count all adult loggerhead coming ashore to nest. The BRT used a time series presented by Baldwin *et al*. (2003) which covered the period 1963 to 1999 (*Status Review* Fig. , p. 42) – and continued to do so despite External Reviewer #2 drawing attention to the obvious deficiency in the 1963 data point. The surveys have, however, continued through the past decade and are currently overseen by Dr. Ronel Nel who, in her capacity as External Reviewer #6 for the *Status Review*, provided the BRT with graphs showing the entire time series from 1965 to 2008 – a version of which has since been published by Hughes (2010). The Team, which could and should have sought out those data before commencing its SQE analyses, made no response and the final *Status Review* continued to present a model fit to data truncated at 1999 without offering any explanation or even acknowledgement that a longer time series exists. In this case, the *Federal Register* notice did acknowledge External Reviewer #6's information (p. 12614), though without admitting to any incompatibility with the data used by the BRT.

In truth, the new data would not have altered the BRT's conclusions: Baldwin *et al.*'s (2003) counts showed an increasing trend and hence pointed the Team towards a conclusion that their proposed southwest Indian Ocean DPS is merely "threatened".

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22 The authors of the *Federal Register* notice (p. 12614) noted the 2009 nest count in this on-going time series, correctly reporting it as the fourth-lowest to date but saw it as "reinforcing the assessment of nesting decline". They did not remark on the 2008 nest count, nor did they note the general upward trend in the series since 2004.

Dr. Nel's more recent data show a rather nice exponential increase, at about 5% per year, in Tongaland loggerhead numbers since the mid-1980s. If her time series had been subjected to the SQE approach, that increase would have confirmed the BRT in its conclusions. What is more interesting is that Dr. Nel's series shows loggerhead numbers falling from about 1970 until the mid-1980s – a period when Baldwin *et al*. (2003) showed, and hence the BRT saw, a gradual increase. Dr. Nel's numbers are also approximately 50% higher than those used by the BRT. Closer examination of the published account shows that Baldwin *et al*. (2003) provided the raw counts of turtles observed. Dr. Nel, it must be assumed, has applied some sort of correction or calibration, perhaps to account for differences among years in survey coverage, though she does not seem to have yet explained her calculations in print. The point of present importance is not that one data set is more or less reliable than the other, nor would I argue that Dr. Nel's time series should be accepted before her calibrations have been subjected to peer review. Rather, the message is that the BRT naively used uncorrected raw data, apparently without consideration of their deficiencies, when one alternative for the corrections serves to reverse the direction in the abundance trend – while the BRT also quite unnecessarily cut the last decade off the Tongaland time series. Such severe failures undermine whatever tattered credibility the rest of the SQE analyses might have retained.

Quasi-Extinction Threshold

Having applied the wrong model to the wrong data sets, the BRT then compounded their errors with an interpretation of "quasi-extinction" that is inappropriate under U.S. law. The ESA is, of course, concerned with the danger of <u>extinction</u>, not merely severe depletion. It would, nevertheless, have been appropriate for the analysts to focus on <u>quasi</u>-extinction, which the *Federal Register* notice (p. 12641) correctly defines as "the minimum number of individuals (often females) below which the population is likely to be critically and immediately imperiled"[23]. It was appropriate firstly because the BRT's choice of model (random variations aside) necessarily predicts that zero abundance (i.e. extinction) cannot occur in less than infinite time, rendering their analysis futile if pushed to its logical conclusion. Moreover, in reality, rather than in the world of a simplistic, abstract model, quasi-extinction is important because it is likely that, should the loggerhead population be driven below some minimum number of individuals, it would be either unable or at least unlikely to ever recover – perhaps because the chance of males and females finding one another to mate would drop away, maybe because the resulting genetic bottleneck would result in such inbreeding as to weaken the survivors, while there would be too high a probability of the inevitable chances of the world knocking out the few survivors. To put that in population-dynamic terms: Below some minimum abundance the population can be expected to show "depensatory" behavior and hence conservation management must ensure that the abundance never falls below the quasi-extinction threshold..

[23] A definition conspicuously absent from the *Status Review*.

The proper response to those concerns would have been to review the available literature on loggerhead and other turtles to look for evidence of depensation and so to derive an estimate of the abundance corresponding to quasi-extinction. There are available data on breeding populations comprising anything from a few dozen adult females (e.g. Coral Cays, South Pacific: *Status Review*, Fig. 2, p. 41) to tens of thousands of them (breeding in peninsular Florida). Such data should have allowed estimation of an appropriate, perhaps precautionary, level representing quasi-extinction. The BRT, however, made no such attempt. Instead, they defined what they termed the "Quasi-Extinction Threshold" ("QET") in purely relative terms, with a range of values being considered all the way from a 2.5% decline below current abundance to a fall of 97.5%. Thus, a 10% decline in abundance (for example) was accorded the same level of concern whether the population before that decline included 50, 500 or 50,000 breeding females. A QET thus defined can have no relevance to the question of whether or not a species falls into the "endangered" category under the laws of the United States.

To determine whether a species is "endangered" in the meaning of the ESA, that is whether it is "in danger of extinction", should involve more than a mere examination of both current abundance and trends in abundance but most certainly neither of those should be examined in isolation. Particular circumstances aside, a severely depleted population should not be considered "endangered" if its numbers are increasing quickly and have been doing so over a prolonged period. Nor should a species be rated as "endangered" simply because its numbers are dropping swiftly, if they are dropping from a high level. In contrast, a swift decline in a moderately-depleted species or a slow decline in a severely-depleted one might well be grounds for "endangered" status, depending on the other circumstances of the case. Yet, by setting their QET in strictly relative terms, the BRT removed current abundance from their consideration of the status of loggerhead, just as their focus on the SQE approach had removed all factors other than adult female numbers. The Team thus made a mockery of its own analysis[24].

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24 The BRT's interpretation of "quasi-extinction" in terms of relative decline alone, while so bizarre on its face, probably arose straightforwardly from the Team's direct and naïve adoption of the SQE approach from Snover & Heppell (2009), without modifications to suit it to application under the ESA. The IUCN's concept of endangerment does indeed consider a rapid rate of relative decline as sufficient evidence to declare a species endangered – though the IUCN's primary (not sole) focus is on a decline which has already happened, over three generation times of the species in question (or a minimum of 10 or a maximum of 100 years), rather than one projected to happen in the future. [The IUCN criteria also consider absolute abundance and the extent of the geographic range but as alternatives to the relative rate of abundance change, not in conjunction with it in a single criterion (Seminoff & Shanker 2008).] Whatever its merits for the IUCN's purposes, that concept of past proportionate declines is not relevant to the ESA's definition of "endangered" and a model designed to address the IUCN concern, such as the SQE model, is unlikely to be applicable to decisions made under the ESA.

Neither the IUCN nor Snover & Heppell (2009) have employed the sort of free-form QET that the BRT opted for. Rather, the IUCN has set specified amounts of relative decline as criteria for different levels of endangerment and depending on whether the cause has been determined, whether it has ceased to act and whether its effects are reversible (Seminoff & Shanker 2008). Why the BRT opted not to use those criteria and to substitute the range from 2.5% to 97.5% is unclear.

100-Year Extrapolation

That analysis was then run to see whether a QET (itself anywhere from 2.5% to 97.5% below current abundance) would be breached within the next 100 years. That was a time scale based on IUCN recommendations and, apart from inability of the SQE model to provide meaningful projections over such a long time period, it is a sensible one for conservation planning when dealing with a species as long-lived and late-maturing as loggerhead. It is equally sensible for use when generating advice on a species' conservation status in an international context, as the IUCN does. The task of an ESA Status Review, however, relates to the application of national legislation. The United States Congress has seen fit to pass an Endangered Species Act which provides for very strict regulations to prevent the extinction of species, even at the price of major costs to human interests, and has defined "endangered" accordingly. Few would disagree with the necessity of such an ESA but it is important that the sometimes-draconian measures needed to avert imminent extinction are only applied where they are required – only, that is, where extinction would otherwise be imminent. To extend such measures to other cases would not only increase the burdens on human interests beyond those mandated in law but would also distort the allocation of conservation resources, directing them away from where they are most needed, while further weakening protection for all species which require it by bringing the ESA itself into disrepute. In short, when applying the strictures of the ESA, it is important to use the ESA's own definition of "endangered" status and not to import a different definition, developed by a different organization for its own, and very different, purposes.

The ESA defines an "endangered species" as one "which is in danger of extinction", as distinct from a "threatened species", which is one "likely to become an endangered species within the foreseeable future". Even with long-lived species like loggerhead, it is difficult to foresee the future as far as one hundred years out but the time scale selected for the BRT's SQE analyses would perhaps not be inappropriate when determining whether a species is "threatened". It is, however, quite simply irrelevant to the question of whether a species is in danger of extinction now – and hence "endangered" in the meaning of the Act. A decline towards low abundance over the next century, that is, might be a reasonable representation of a species approaching endangerment and hence a justification for "threatened" status. It is not and cannot be an indication of an existing state of endangerment. Yet the sole task confronting the BRT, after selecting DPSs, was to determine whether loggerhead are "endangered", since they are already listed as "threatened" and there has been no petition to "de-list" them entirely.

Analytical Results

Thus far, this account of the SQE analysis has presented it as though it uses deterministic calculations, producing only a single answer from each data set. That presentation makes the underlying deficiencies easier to understand. The actual modeling, however, makes allowance for the variability in the data around the fitted exponential relationship and projects the uncertainty illustrated by that variability into the future. With each loggerhead data set, the BRT made 5,000 replicate projections, allowing examination of the probabilities of various outcomes. What is important to understand about this modeling of variability, however, is that it did nothing to correct the major deficiencies identified above. The simplistic extrapolation of recent trends far into the future, with a constant percentage rate of increase or decline (random variability aside), the inability to cope with non-monotonic data[25], the ignorance of the biology of the species of interest (loggerhead in this case), the limitation to a single life stage, the failure to consider the major time lags in loggerhead conservation, the lack of any attention to whether or not the data sets used were representative of their respective DPSs, the analyses of units smaller than DPSs, the definition of QETs in relative rather than absolute terms, and the hundred-year projections when the question at hand concerns imminence of danger were all unaffected by whether the analyses used deterministic or stochastic methods and yet those deficiencies, singly or in combination, where quite fatal.

What the stochastic modeling did do was to greatly complicate presentation of the results – exacerbated by the authors declining to settle on a single value for the QET. Just two of the data sets analyzed showed increasing numbers of nests: the Tongaland series that has already been mentioned above and a 1988–2003 series from Brazil. For those two, the probabilities of quasi-extinction are estimated to be negligible – unsurprisingly given the sort of naïve extrapolations inherent to these SQE analyses[26]. All other data sets analyzed led to estimates of declining abundance, though with a single exception (data from one of the minor South Pacific nesting areas) the confidence intervals around the estimates included the possibility that each data set was actually showing an increase in abundance (*Status Review*, p. 47, Table 2). For each of the 5,000 extrapolations for each data set, the analysis determined a probability that abundance would drop below each possible level of

[25] The specific problem of the non-monotonic trends in the only two data sets of major importance might have been partially addressed by stochastic modeling but was not by the analyses used in the *Status Review*. Those analyses modeled uncertainty with a simple random variable. Had an auto-correlation term been included, which was not done, then the analyses might have better mimicked the Japanese and peninsular Florida nest-count data. In the case of the North Pacific loggerhead, it might have been possible to model the apparent cyclic behavior. For the western North Atlantic population, however, the model would not have been any the more mechanistically realistic than the one the BRT did use and thus would have been no more suited for extrapolation. The Florida time series suggests not the cyclic behavior that auto-correlation would mimic but rather separate stanzas, with different factors dominating abundance trends at different times. Such pronounced temporal differences in the underlying processes defy any attempt at extrapolation.

[26] The BRT took these negligible probabilities of quasi-extinction as indications that the two proposed DPSs are "threatened" for ESA-listing purposes. In contrast, Snover & Heppell's (2009) own application of their SQE approach to three data series on leatherback turtles concludes that a population with just such a trend of increasing numbers merits no IUCN listing at all. (They made no mention of the ESA's listing criteria.) For a species as depleted as leatherback, that can be seen as illustrative of the folly of the IUCN's definitions of endangerment. Nevertheless, the BRT was less than even handed in suggesting that almost any declining trend is grounds for "endangered" status under the ESA, while DPSs with increasing trends should remain "threatened". If low current abundance was grounds for "threatened" status despite a rising trend, then high current abundance should be grounds for the same status, despite a declining one.

QET by the end of the 100 years of the extrapolation (*Status Review* p. 37)[27]. The proportion of the 5,000 runs for which there was a greater-than-90% probability of breaching the QET was computed and used as a metric of the "Susceptibility to Quasi-Extinction" or "SQE". In their proposal of the approach, Snover & Heppell (2009), recommended assessing these SQE against a critical value of 0.4 but the BRT arbitrarily opted to reduce that to 0.3, thus reducing the probability that "a DPS [would be] considered to be not at risk when in fact it is". While a degree of precaution is clearly appropriate in the management of any species listed under the ESA, the decision that loggerhead turtles are at risk was made in 1978 and nobody has suggested reversing the judgment made at that time. The question before the BRT was whether or not the degree of risk was such as to justify an "endangered" listing – yet the Team offered no argument as to whether 0.3, 0.4 or some other value was appropriate for that purpose[28].

These various deficiencies notwithstanding, the results of the SQE analyses are presented in the *Status Review* as Figures 6 to 10 (pp. 48–52), with some limited accompanying text on p. 46. Those results can best be approached through examination of the graph for the "Peninsular Florida Recovery Unit" (*Status Review* Fig. 8, top right panel, p. 50), which is actually the outcome of applying the SQE approach to the loggerhead nest counts from the Florida index beaches. That graph concerns the bulk of the western North Atlantic nesting and hence relates (in so far as the SQE approach can be said to relate to anything in the real world – which is not far) to the conservation status of loggerhead both on the eastern seaboard of the United States and as a global species.

The graph shows that the analysis computed an SQE greater than the selected critical value, 0.3, if the QET was chosen to be about 8% or greater. Since the current number of breeding females utilizing the beaches of the peninsular is of the order of 35,000, the analysis thus suggests that the population is safe from quasi-extinction (in the commonly-accepted meaning of that term, which is also the one defined in the *Federal Register* notice) any time in the next hundred years provided that the QET is not greater than some 2,800 breeding females. Since the steadily-increasing numbers of nesters on the

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27 With SQE approach, it was not simply a matter of each replicate run either leading to quasi-extinction, at each QET, or not. The analysis produced 5,000 probability distributions for each data set. That is inherent to Snover & Heppell's (2009) diffusion-approximation approach.

28 Snover & Heppell (2009) used projections of the SQE model over 100 years to examine the error rates resulting from projecting it on the basis of 30 years of data. There is a circularity in such reasoning which causes it to ignore errors in model structure and the like. The projections used QETs only over the range 50% to 90% but those were expressed as the converse of the numbers used in the *Status Review* (the former's 90% was the latter's 10%). Snover & Heppell's (2009) Figure 5 suggests that a critical value of 0.3 and a QET of 10% would result in about a 0.05 probability of a real risk of extinction being missed but about a 0.10 probability that a species not likely to drop below the QET would be misclassified as being at risk. Whether that would be an appropriate balance is perhaps debatable.
The authors of the Federal Register notice appear to have misunderstood this aspect of the BRT's work. The notice (p. 12641) stated that the analyses indicated that three of the proposed DPSs are "at risk of declining to levels that are less than 30 percent of the current numbers of nesting females (quasi-extinction thresholds < 0.30)". That is true but the BRT invoked "0.3" with reference to the SQE, not the QET.

Tongaland beaches has risen from only a few hundred individuals in the 1960s[29], it seems safe to say that the true, biological QET for loggerhead turtles is substantially below 2,800. To put that in simpler terms: If the BRT's SQE approach had any validity at all (which it does not), it would have confirmed what was always intuitively obvious – A species or a population which has tens of thousands of breeding females, as the western North Atlantic loggerhead does, is not in imminent danger of extinction unless its numbers are falling very quickly and thus does not merit "endangered" status under the ESA. That "confirmation" is groundless, of course, since the SQE analysis has no credibility but it is sure that the only science-based argument advanced in support of the Services' conclusion that "endangered" status is justified for loggerhead actually shows the reverse[30].

The same cannot, of course, be said of all of the other proposed DPSs. If the species were to be divided into nine units for ESA-listing purposes, then some might well merit "endangered" status – an observation that does not need the support of the SQE analysis. Before leaving the status of loggerhead in other regions, however, it may be noted that the BRT saw its proposed South Pacific DPS as declining, based on applying the SQE analysis to data from four rookeries (*Status Review* Fig. 2, p. 41). Yet, most curiously, the recent NRC Committee on Sea Turtle Population Assessment Methods (one of the nine members of which was also a BRT member) reached the contrary conclusion. Having declared that the available data on the southern Great Barrier Reef loggerhead provide "a sound foundation for assessing" the risks of anthropogenic hazards, that Committee judged that recent conservation measures have mitigated the worst problems "resulting in an apparently recovering stock" following earlier declines in the 1980s and 1990s (NRC 2010). Their cited source (Chaloupka *et al.* 2008) drew no such conclusion, attributing trends in nesting numbers to climate change, though Dr. Chaloupka was himself a member of the NRC Committee and perhaps contributed more recent, unpublished information.

Whatever the causes of the up-tick in loggerhead nesting on eastern Australian beaches, which began in the late 1990s, and however one evaluates the likelihood of it continuing, this disagreement between the BRT and the NRC Committee throws a key point into focus: The BRT concluded that the numbers are declining because it fitted a model to the data which assumed, *a priori*, that there could be no reversal in the underlying trends. Thus, short increases or brief declines in nesting were entirely ignored, save as they affected the average trend in each time series. A conservation success, resulting in rebuilding from past declines, would not alter the BRT's classification of a DPS as "endangered" if the increase in abundance only spanned a few years, as it should not. But even prolonged, continuous rebuilding would not change that classification until the

[29] Some 300 nests annually (Hughes 2010), with a clutch frequency of 5 and a remigration interval of 3 years (*Status Review* Table 1, p. 39).

[30] Arguably, the SQE analysis indicated that even a "threatened" listing is not justified for loggerhead: A species that will not approach quasi-extinction during the next hundred years can hardly be said to be likely to reach "endangered" status within the foreseeable future. It would not, however, be wise to build an argument for de-listing loggerhead on anything as unreliable as the BRT's SQE model.

(inevitably slow) increase in abundance finally reversed the (sometimes swift and severe) declines of former decades – with longer data sets tracking declines from earlier periods of higher abundance needing even more prolonged successful rebuilding before the model would perceive a net increase and cause the BRT to declare a DPS as merely "threatened". An analysis which, like the SQE approach, demands decades of successful rebuilding to pre-modern levels of abundance before admitting that a population is not in imminent danger of extinction is simply valueless for classifying species or DPSs under the ESA. An analysis which, again like the SQE approach, demands longer periods of rebuilding when there are better historical data, rather than because of deeper depletion before recovery began, is doubly useless for the purpose.

The underlying problem with these SQE analyses of loggerhead was, of course, that the BRT opted to use elegant mathematics, obscured by a surfeit of verbiage, to fit a model, at one inappropriate and grossly simplistic, to just one kind of data, rather than applying intelligent thought to all that is known of loggerhead turtles. The *Status Review* does not even demonstrate the application of any thinking to the selection of data fed into the model, nor to the interpretation of the results that it generated. That casual and careless strategy allowed the same naïve approach to be mechanically applied to each proposed DPS (or rather to just the five out of nine for which time-series data were available) but it negated the value of all the research that has been done on the conservation of turtles, all the published scientific literature and even the intelligence of the BRT's members. The Team thereby failed to engage with the task that it had been set and ended by providing no information or guidance pertaining to the key question of whether or not loggerhead should be "up-listed" to "endangered" status under the ESA[31].

[31] As has been noted above, an earlier draft of the *Status Review* was circulated to eight external reviewers. Most made no mention of the modeling and largely confined their comments to details of the literature on loggerhead from their own regions – literature which should have informed the analyses of conservation status but did not. External Reviewer #5 made some detailed comments on the use of the time series of nesting counts, questioned the arbitrary choice of 0.3 for a critical value of SQE but made no other comment on this modeling. It was only External Reviewer #7 who offered more. He had himself worked on "diffusion-approximation" models of loggerhead and with that background raised a number of penetrating comments on the BRT's SQE approach – comments which seem to have been ignored during the production of the final *Status Review*. External Reviewer #7 did not remark on the sorts of underlying weaknesses identified in the present comments.
External Reviewer #7 was self-identified in his comments as Dr. Jeffrey Moore who, at the time, was a post-doctoral research scientist in the Center for Marine Conservation of Duke University Marine Laboratory. He is now a wildlife biologist working for NMFS's Protected Resources Division at their Southwest Fisheries Science Center and thus is no longer external to the Services.

Threat Matrix Modeling

Following the SQE estimations, the *Status Review* offers a "Threat Matrix Model". With considerable further development, that might have potential as a tool in planning loggerhead conservation tasks. What it could never be, however, is more than peripheral to the issue at hand: The question of whether or not to list loggerhead as "endangered" rather than "threatened". The BRT seems to have been confused, or perhaps conflicted, about the relevance and utility of the model for the task.

Purpose & Objectives

The Team offered no comment on its purpose when introducing the model (*Status Review*, p. 36) but later concluded: "The analysis does not provide estimates for the likelihood or probability of extinction" (*Status Review*, p. 53). I concur. It does not. But since it does not, it provides no information whatever pertaining to the question that the BRT had been set[32]. The Team even admitted that the model "may not represent the actual mortalities and may overestimate the anthropogenic mortalities" (*Status Review* p. 58) and again that their own examination of the model "indicated potential overestimations by the experts of anthropogenic mortalities" (*Status Review* p. 67), which was one way of saying that the model was (a) wrong and (b) biased. Another such was the admittance that "we used the precautionary principle for characterizing the threat level" (*Status Review* p. 77). The BRT did suggest that the biased values allowed for comparisons of mortalities among the life stages of a DPS or between the DPSs for the same life stage (*Status Review* pp. 54, 77). Unfortunately, for that to be true the input assumptions would, at a minimum, need to have had the same biases across life stages and across DPSs. Since they patently did not, the Threat Matrix Model can be dismissed as useless even for such comparisons – which would in any case be irrelevant to the issue at hand[33]. Elsewhere (*Status Review* p. 59), the model was discussed in terms of its relation to management goals, which may be fine but has no connection to the issue of an "endangered" listing. The same can be said for the claim that the "compilation of known threats […] provided an opportunity to evaluate the potential negative impacts […] in the future" (*Status Review* p. 77) – a laudable goal and one that the exercise of creating the model might actually help with but again nothing to do with the purpose of the *Status Review*.

Those deficiencies should have dissuaded the BRT from making any use of the Threat Matrix Model. They did not. Instead, the BRT concluded that: "According to the threat matrix analysis, […] all loggerhead turtle DPSs have the potential to decline in the future" (*Status Review* p. 83). That notion could only be entertained if the model used reasonably-accurate estimates of the various forms of mortality, which even its authors had admitted that it did not. Indeed, in the same paragraph in which they drew that

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[32] The BRT's assigned tasks are specified on p. 6 of the *Status Review*.

[33] Because of the non-linearities that necessarily arise when combining or varying the mortality rates that arise from different causes, meaningful comparisons would be unlikely if the estimated rates are badly wrong, even if the biases could be held constant.

conclusion, the BRT stated: "We emphasize, however, that these indices were used to measure the negative effects of known anthropogenic mortalities on the overall health of each DPS and not to estimate the actual population growth rates of these DPSs" (*Status Review* p. 83). Even setting aside the awkward reality that the anthropogenic mortality rates are not known, but merely guessed, that statement is confusing, since the "overall health" of a population would seem to be synonymous with its "growth rate"[34]. The statement appears, however, to be an acknowledgement that the model cannot be used to support deductions about declines (or increases) in loggerhead abundance and thus equally cannot be used to support decisions concerning conservation status under the ESA since the Threat Matrix Model has no other bearing on the matter.

In the end, the BRT drew from this model the revelation that "precise assessment of the status of any DPS" is impossible without accurate knowledge of the age at first reproduction and of mortality rates, both natural and anthropogenic (*Status Review* pp. 83–84). It did not need a model to reach those conclusions but perhaps, when combined with a realization that the required knowledge is unavailable, they are the final statement of the uselessness of the Threat Matrix Model for determination of the status of loggerhead.

When it came to supporting final conclusions (*Status Review* pp. 161–166), the BRT sought to use this model to determine both trends in population abundance and the principal anthropogenic contributors to any declines. The former could not be evidence of endangerment, even if the trend was a decline, unless it was also shown that that decline, if continued, would lead to quasi-extinction in the immediate future – for all of the reasons already discussed above. Yet, the Threat Matrix Model did not consider abundance and thus could not provide guidance relevant to the BRT's task. Besides, any usefully-accurate estimate of abundance trends would require that the model's input absolute values of mortality estimates were reasonably reliable, which even the BRT did not claim (*Status Review* pp. 58, 67). As for identifying the principal sources of anthropogenic mortality: That would require, at the very least, trustworthy relative rankings of the various causes of loggerhead deaths. It is most doubtful whether those were available to the BRT.

A threat-matrix model (but not the BRT's Threat Matrix Model) might have provided relevant answers in any of three areas. Firstly, such a model might have shown that loggerhead mortality rates are so high, relative to the life-time fecundity of the females, that numbers must be dropping swiftly towards extinction. That might have been sufficient to demonstrate endangerment, either in the absence of time series of abundance data or as independent confirmation of the results of analyses of such series. Unfortunately, a threat-matrix model intended for that purpose would need to have very much more precise estimates of current vital rates, including mortality rates, than are yet

[34] I here reluctantly accept the BRT's abuse of biological terminology – an abuse too commonly seen in the work of other authors also. Both "growth" and "health" are, correctly speaking, properties of individual organisms, not of populations.

available for loggerhead. Secondly, and with rather less demand on non-existent data, a threat-matrix model might have been used to show that current anthropogenic mortalities of juveniles are so high as to swamp the benefits from existing conservation measures, such that the year-classes which will recruit to the adult population over the next decade are no more abundant than the currently-adult year-classes were at the same age. If that could have been demonstrated, the concern over long lag times between the introduction of conservation measures and observable responses in the SQE analyses could have been set aside. However, the parameterization of the BRT's Threat Matrix Model was not remotely adequate even for that purpose. Finally, if anything were really known about the relative importance of different sources of anthropogenic mortality of loggerhead and if reasoned conclusions could be drawn about future trends in related human activities, inferences might have been developed concerning the prospects for bringing death rates down to the point that the abundance of the turtles could increase at a comfortable rate. The wording of the *Status Review* hints at all three of these applications of the BRT's model without ever fully confronting any of them. Since that text does not, its authors avoided the need to examine whether their modeling was adequate to any of those purposes. It was not[35].

Model Structure

The Threat Matrix Model itself appears to be a rather straightforward one, if a bit simplistic[36]. The BRT, aware of the simplicity, noted that the Model "did not consider environmental or demographic stochasticity, density dependence, autocorrelations in vital rates, or sampling variations" (*Status Review* p. 57). It also did not consider long-term climate change, the effects of still-evolving political perceptions of the necessity for conservation of threatened species, the consequences of growing human wealth in areas adjacent to the nesting beaches nor those of demographic change, the implications of unpredictable events (such as major disasters) and much else besides. Whether such a limited model can actually have much utility in planning the management of a long-lived species like loggerhead is a matter than can be passed over here but might merit some thought in other fora.

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35 Interestingly, for the two proposed DPSs that the BRT rated as "threatened" based on their SQE analysis (i.e. the southwest Indian Ocean and the South Atlantic DPSs), the Team trusted in the fallacious output of their Threat Matrix Model sufficiently to declare that, while each DPS "is likely not currently at immediate risk of extinction, the extinction risk is likely to increase in the foreseeable future" (*Status Review* pp. 163, 166). For the South Atlantic DPS, they went so far as to say that that model indicated that the abundance "is likely to decline in the foreseeable future" (*Status Review* p. 85). How those conclusions were thought to be consistent with the previous statements that "The analysis does not provide estimates for the likelihood or probability of extinction" (*Status Review*, p. 53) and "these indices were used to measure the negative effects of known anthropogenic mortalities […] and not to estimate the actual population growth rates" (*Status Review* p. 83) must be left to the imaginations of readers of this commentary.

36 Because the Threat Matrix Model has no relevance to the issues at hand and anyway cannot be parameterized without resort to unscientific opinions, I have not devoted the time that would be needed to check the matrix modeling itself. The absence of further comment here should not be taken as endorsement of that modeling.

There is some confusion in the way that the turtle mortalities resulting from various causes were combined. The best approach would have been to use exponential mortality rates, which are simply additive – as External Reviewer #5 noted and as any of the Services' fishery stock-assessment scientists could have explained[37]. The BRT, in contrast, opted to combine the mortalities from disparate sources by using the product of the survival rates (*Status Review* p. 57). That would only be exact if the various sources of mortality acted in sequence through the year, though the error in applying it to simultaneous, on-going threats (as the BRT did) need not be serious in the present context. It might amount to about 15% annually in the numbers of survivors – enough to be serious in stock-assessment work but probably not with this highly-uncertain Threat Matrix Model.

However, when combining their estimates of the different types of anthropogenic mortality (before merging the resulting combination with the rate of natural losses), the BRT appears to have ignored its own simplified mathematics and simply summed four mortality rates (*Status Review* pp. 68–76). In the case of neritic juveniles and adults of the western North Atlantic population, for example, the BRT produced a "Worst" case combined anthropogenic mortality rate estimate of 0.50, when their approach of multiplying survival rates would have given 0.42. Perversely, the summation could have corrected the error implicit in using the product of survival rates. Whether it did or not depends on exactly what was meant by the four estimates of source-specific mortality rates – themselves based on expert opinion (as discussed below). If the experts considered the proportion of loggerhead dying of, say, "overuse" <u>given that others were dying of other causes</u> (including natural mortality), then the BRT's addition would have given the correct answer. If, as seems more likely, the experts were less sophisticated in their understanding of population dynamics, then addition would have been an incorrect approach. Whether any resulting error was more than insignificant in comparison to the errors in the experts' estimates of the rates is unlikely.

Otherwise, the BRT seems to have had a poor understanding of its own model. The *Status Review* declared that "the lack of precise estimates of anthropogenic mortalities resulted in a wide range of possible status" (*Status Review* p. 83). In fact, the range in "status" arose purely from the wide spreads between the thresholds that the BRT had set for the four mortality-rate classes into which expert opinion placed its best guesses. The effect of the absence of any usefully precise knowledge of anthropogenic mortality rates was quite different: It simply resulted in erroneous selection of the classes and hence gross errors in the supposed mortality rates.

[37] This was but one of several comments on the Threat Matrix Model from External Reviewer #5 which the BRT chose to ignore or, worse, to obfuscate when finalizing their *Status Review*. In a number of cases, the wording which External Reviewer #5 objected to was removed but the underlying defects in the Model were thereby merely concealed, not corrected. External Reviewer #5 was one of only two who provided any substantive comments on the Threat Matrix Model.

Parameterization: Opinion is not Empirical Science

The consequences of these and other weakness in the structure of the Threat Matrix Model were likely minor. The greater problems lay in the Model's parameterization.

It required input values of both loggerhead vital rates and the anthropogenic mortality rates acting on the turtles. The former (set out in *Status Review* Table 1, p. 39) were given some basis in science, though it is very poorly documented in the *Status Review*. Remigration intervals, clutch frequencies, clutch sizes and rates of emergence success are all potentially estimable using field data and the lack of clear citations of the sources used by the BRT for its chosen values can be passed over here. The Team set sex ratios at an arbitrary 50:50 but the Threat Matrix Model need have been no different if it had been declared as a model of females only. The Model also required estimates of the durations of various life stages. For those, the BRT unfortunately resorted to but a single citable source (though an unpublished thesis), which provided only a generalized estimate of 30 years for the average age at first reproduction. The Team then divided the latter 29 of those years (i.e. excluding the eggs and hatchlings) between oceanic and neritic-juvenile stages, with the former estimated as anywhere from 10 to 27 years depending on the ocean in which the loggerhead live[38]. The basis for those supposed durations of the oceanic stage is nowhere stated but the BRT's estimate of an average 10 years for that stage in western North Atlantic loggerhead (*Status Review* Table 1, p. 39) corresponds reasonably well (though not precisely) with the estimate of an 8.2-year stage duration in the *Recovery Plan* (p. I-26) – an estimate which was supported by citations of evidence. It is clear, however, that the average durations of the oceanic and neritic-juvenile stages are of no greater importance to the dynamics of the species than are the variability around those averages. For that, the BRT could do no better than suggest a standard deviation of age at first reproduction of 5 years, implying 95% confidence limits of 21 and 41 years, plus the application of the same coefficient of variation (5/30 = 0.17) to the shorter durations of individual stages – a guess on which the "experts of the team agreed" since the resulting "values were deemed reasonable" (*Status Review* p. 54). That would be fine for an academic exercise in modeling to explore the "what if" implications of various hypotheses. It is not, of course, an acceptable basis for generating conclusions to guide the application of the ESA, which demands that decisions be based on the "best available scientific and commercial information", not on guesswork.

The proportion of adults remaining in neritic waters after mating and (for females) nesting was set at anything from 0.30 to 0.95 (depending on the ocean in which they

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[38] The notion that juvenile loggerhead live through discrete oceanic and neritic stages has had long currency, though the contrary evidence has been mounting for some years. That idea has now been thoroughly refuted by McClellan & Read (2010), though perhaps too recently for the BRT to be expected to have responded.

It is unclear whether the BRT's Threat Matrix Model was a four-stage one, which does not allow for older juveniles and adults to return to oceanic habitats after a neritic period, as stated on p. 53 of the *Status Review*, or a six-stage model which does include such return, as stated on p. 56. The Model did provide for a proportion of adults to return to oceanic waters after their first reproduction.

live), with only the Indian Ocean loggerhead being given different proportions in nesting and non-nesting years (*Status Review* Table 1, p. 39). The reasoning behind those estimates is nowhere explained in the *Status Review*.

The remainder of the Model's parameterization concerned survival rates (or their complements: mortality rates). That selected for the hatchlings, 0.4 survival, was based on a single cited study which actually considered Kemp's ridley turtles and not loggerhead at all (*Status Review* p. 55). For adult female loggerhead, average annual total mortality rates (including natural and anthropogenic components) can be estimated from the rates of return to nesting beaches of marked individuals, the results generally being greater than 0.8 (*Status Review* p. 55), which sets a lower bound on what the survival rate in the absence of anthropogenic deaths would be. The BRT seems to have settled on an adult "natural" survival rate of 0.95 (*Status Review* Table 1, p. 39), though the Team nowhere said why. That assumption immediately implied that anthropogenic deaths of adults are two to three times the natural ones, which means that it went a long way towards being an assumption of an answer to the question that the BRT had been set. The Team then proceeded to examine the rates of increase or decrease in modeled population abundance that would arise under a range of possible values of overall mortality rates for the hatchings, older juveniles and adults (*Status Review* p. 55 & Figs. 11–17, pp. 60–66). They seem to have arbitrarily decided that, should anthropogenic mortality cease immediately, the abundance of the population would increase by 5% or 10% – though the *Status Review* offered no supporting logical argument nor even an admittance that such an assumption had been made[39]. With those assumed rates of increase combined with the estimates for egg, hatchling and adult survival, it was possible for the BRT to extract juvenile (excluding hatchlings) natural survival rates that would allow the Model to conform to the Team's expectations. The juvenile survival rates thus arrived at ranged from 0.787 to 0.956, depending on the relevant ocean area and habitat (oceanic *versus* neritic: *Status Review* Table 1, p. 39).

The resulting model, termed the "Base Population Model" in the *Status Review*, thus depended for its parameterization on some well-founded estimates (e.g. of clutch size), some that were less-certain but empirically-based (e.g. average age at first reproduction), some values drawn from expert opinion (e.g. duration of the oceanic juvenile phase), some that were simply arbitrary assumptions (e.g. rate of population increase in the absence of anthropogenic mortality), and some founded in circular reasoning and a desire for an internally-consistent model, given all of the other input values and their places in the model structure. Such a mixture may be entirely adequate in an academic modeling exercise, designed to scope out the range of plausible population dynamics of loggerhead and to identify the more important gaps in existing knowledge, prioritizing them for future research support. What it is entirely inadequate for is a foundation for a model which purports to represent real loggerhead populations, the behavior of which model

[39] The *Status Review* (p. 59) referred to these rates of population increase as "the maximum population growth rates". They are nothing of the kind, though discussion of their true nature must be deferred to later in this commentary.

will be taken as indicative of the responses of those real populations and used as such when making management decisions. Yet the latter was, of course, the sole role of the BRT's Threat Matrix Model. That Model was entirely unsuited to its sole purpose as a direct consequence of the inadequacies in parameterization of the "Base" version.

Those were not, however, the sum total nor even the major part of the deficiencies in parameterization of the BRT's Model. Rather, when it came to the critical step of quantifying the rate of anthropogenic mortality, the Team reported that they had relied exclusively on agreement among "The experts of the team" (*Status Review* p. 54), "experts' knowledge" (*Status Review* p. 56), "experts" (*Status Review* p. 57), and "experts' opinions" (*Status Review* p. 83). Expert opinion has its place when management decisions call for scientific advice and properly-documented knowledge is lacking. Embarking on that road, however, starts an analysis down a slippery slope: Placing too much reliance on opinion divorces the advice from empirical foundations, which means that it is not "science" at all. Basing the value of a single parameter on opinion is probably acceptable, provided that the model's output is rather insensitive to the value chosen. Within limits, providing opinion-based qualitative interpretations of model results is reasonable. Building an entire model on opinion is not – not if the results are to be considered "science" and used under the ESA's rubric of "best available scientific and commercial information". As a direct result of its complete reliance on opinion when setting these anthropogenic mortality-rate values, the BRT's Threat Matrix Model must be dismissed as non-scientific. It does not even come close to meeting the required standard.

It is not simply the reliance on expert opinion alone, though that would be damning enough. The *Status Review* nowhere names the experts who were consulted, nor does it state the fields of their expertise: expert turtle biologists are not necessarily expert in turtle conservation and are most unlikely to have expertise in, for example, temporal trends in fishing effort by the pelagic longline fleets – the cause of much of the anthropogenic mortality on oceanic-juvenile loggerhead. One vital skill for constructing a threat matrix should be an understanding of population dynamics, since that should guide the untangling of proportions of a population dying from numbers of observed deaths, while allowing for the influence of the rate of mortality caused by one factor on the numbers dying as a consequence of a different cause. That is not a simple matter and, on the internal evidence of the parameterizations selected by the BRT, one can be confident that it was an area of expertise that was insufficiently developed among the experts consulted.

Even if appropriate experts were gathered, there is nothing in the *Status Review* which indicates that any particular methodology was used in extracting their considered opinions, despite the body of extant literature on appropriate techniques for that purpose. For all that the *Status Review* indicates, a couple of individuals could have scribbled a few numbers on a piece of paper after pondering their own personal interests. That is no foundation for technical advice to decision-makers.

The *Status Review* does reveal that the experts consulted with regard to threats on Pacific loggerhead gave opinions which included natural mortality, whereas those providing opinions on the turtles in other areas excluded that factor and suggested only anthropogenic mortality rates (*Status Review* p. 59) That would appear to indicate both that different experts contributed opinions on the threats to different proposed DPSs (thus eliminating any credible inter-DPS comparisons) and that there was a marked lack of coordination in the opinion-gathering process. The latter alone casts much doubt on the whole affair.

There is a particular weakness in the *Status Review's* presentation of the means by which the experts' opinions were quantified. I suspect, based on the wording offered on pages 56 and 77 that the experts were asked to rank threats into four classes, "high", "medium", "low" and "very low", each of which labels was accompanied by a numerical range of corresponding mortality rates (e.g. "high" was taken to be an additional mortality, on a particular life stage and caused by a particular threat, exceeding 0.2). External Reviewer #7, in contrast, appears to have thought that the experts merely selected nominal labels (e.g. "high" or "very low") and that quantitative ranges were subsequently applied to those nominal classes. Some credence is given to that possibility by the *Status Review* discussing the effects on the model of using different mortality-rate values for the same four nominal classes (*Status Review* p. 59) – in what may be additional wording provided in response to External Reviewer #7's comment. If that is what was done, however, it would reduce the gathering of opinions to farce: No expert can offer a meaningful opinion on whether a particular threat poses a "low" or "medium" risk of death unless she knows what the compiler of opinions means by those terms. Indeed, as External Reviewer #7 pointed out, the names used for the four nominal classes do not correspond at all well with the quantitative thresholds that the BRT applied. (For example, a mortality rate between 0.10 and 0.20 caused by a single anthropogenic factor was labeled "Medium" by the BRT when the natural mortality rate for adults was supposed by the Team to be around 0.05. Even total anthropogenic mortality, not just that resulting form a single factor, of twice, let alone four times, the natural level have to be regarded as "very high", not "medium".) If the experts were not informed of that offset between class names and the quantitative thresholds, they would be expected to have selected the wrong mortality-rate classes. Moreover, if the experts were pressed to rank factors without any statement of quantitative thresholds, no two of them would use the terms in the same ways.

The formal *Status Review* is not, however, the sum total of the available information on how the opinion-based estimations of anthropogenic mortality rates were made. There is additional detail in comments appended to an Excel spreadsheet available through the same website as is the *Status Review* itself. That is hardly an appropriate format for archiving key foundations of the technical advice supporting a major public decision. Unfortunately, in presenting the material in that unconventional format, the comments' authors seem not to have seen any need to explain how they derived their estimates in sufficient detail for their reasoning to be checked – as they would have done had the information been prepared as a scientific report. Nevertheless, the comments do hint at something of the thinking that went into the numbers. They are primarily brief summaries of some of the issues considered by the experts. Many do cite published sources but only a few offer citations supporting quantitative estimates of mortality rates. The expertise of the experts is not in doubt and hence it is no surprise that they were familiar with the turtle-conservation literature. It is the means by which that qualitative knowledge was converted into quantitative inputs to the Threat Matrix Model that is in doubt and the spreadsheet comments do nothing to add clarity to or confidence in that process.

As an example, the reasoning supporting a decision that oceanic juveniles of the western North Atlantic loggerhead are subject to "Medium/High" anthropogenic mortality (meaning anything up to 25% per year) was said in the corresponding spreadsheet comment to be:

> Substantial mortality occurs in longline fisheries operating in the western Mediterranean, Gulf of Mexico, and North Atlantic. Although total mortality has not been estimated for all fisheries, the combined mortality is likely significant. Entanglement in and ingestion of marine debris is common in oceanic juveniles (Witherington 2002; Witherington, unpublished data) and is an additive mortality factor. Cumulatively, mortality from these sources is believed to be in the medium to high range.

That comment offers no indication that the key longline-bycatch mortality was considered quantitatively by the consulted experts. Notably, it does not cite Lewison *et al.*'s (2004) estimates of the magnitude of that bycatch – estimates inconsistent with the "Medium/High" ranking. More generally, it offers nothing on why the decision favored that combination of mortality-rate classes, rather than some other alternative, beyond a "belief" (which was a fine statement of faith but is not even as dubiously-credible a foundation for scientific conclusions as is expert opinion).

External Reviewer #7 made a point of this over-reliance on expert opinion, though he couched it in more moderate terms, expressing concern that "using opinion provides an illusion of knowledge" and noting that the opinion-based estimates led to model outputs "so uncertain […] as to be arguably of little value". In his detailed comments on the wording of the *Status Review* draft, he stated "The assignment of these rates is, I think, the most dubious aspect of this analysis" and added "the experts' estimates, being opinions, are probably not credible", asked "Considering that many of these rates are simply not estimable (or at least haven't been estimated), so how is an experts' guess going to improve on our lack of knowledge?", observed "I feel like these guesses give a false sense of knowledge where there is none", and noted "the rates […] are so unrealistically pessimistic as to be only marginally helpful". External Reviewer #5, the only other one of the eight to provide substantive comment on this modeling, was more restrained but noted: "I am a bit concerned about the level of background that the experts had with regards to thinking quantitatively. My guess is that these estimates are mostly relative rather than simply quantitative […]. What were the experts asked specifically?" There is no indication in the final *Status Review* that the BRT responded to these damning criticisms of their Threat Matrix Model, unless it was by the addition of the mutually-contradictory caveats, noted above, with which they attempted to colours their statements about the Model's utility for their purposes[40].

There was one final oddity in these parameterizations: the manner in which the various causes anthropogenic of mortality were grouped. Whoever consulted the selected experts apparently started from the five factors to be considered when listing a species under the ESA (*Status Review* p. 57) – certainly one foundation for classifying threats but hardly the most convenient. Inadequacy of existing regulations was dropped from the five on the reasonable grounds that it does not directly kill any turtles but merely allows their death by other means. Of the four remaining factors, destruction or modification of habitat or range also does not usually kill individuals. Its effect is more often to limit carrying capacities or vital rates, though sometimes it can limit an individual's ability to avoid other forms of death. The experts' reasoning which lay behind their opinions can only be deduced from the cryptic spreadsheet comments and it is not clear whether they considered only directly-caused mortality (as was required for the Threat Matrix Model) or also indirect effects (which they had declined to consider when inadequate regulation was the indirect cause). In practice, however, habitat-induced mortality rates were graded as "Low" or "Very Low" (and hence of little consequence to the analysis) throughout, with the sole exceptions of the impacts on eggs and hatchlings in the Mediterranean and in the North Pacific (*Status Review* pp. 68–76). For the latter region, the experts' habitat-related considerations and reasoning were summarized as:

> Includes beach armoring; beach cleaning; beach debris; beach sand placement; beach vehicular driving; beach erosion (washouts) and accretion; boat anchoring; coastal construction including deterioration of structures; eutrophication; explosives for construction, military operations (land mines on beaches), channelization, underwater explosives, and fishing; human presence; light pollution; noise pollution; ocean acidification; other shoreline stabilizations; pile driving; recreational beach equipment; sand fences; sand mining (beach and seabed); trophic changes from benthic and beach habitat alteration; trophic changes from climate change; trophic changes from fishery harvest; and water quality including toxicants, agricultural runoff, and sewage eutrophication.

And for the eggs and hatchlings specifically as:

> Substantial problem of beach armament throughout Japan, coastal development and construction, erosion control structures, beachfront lighting, vehicular and pedestrian traffic, sand extraction, beach pollution, removal of native vegetation and planting of non-native vegetation (Kamezaki *et al.* 2003, Kudo *et al.* 2003, Suganuma 2002). Beach debris (H. Peckham, ProPeninsula, personal communication, 2009). No real

[40] In one farcical fragment of wording, which the BRT may have intended as a rejection of External Reviewer #7's points, the *Status Review* declares that "our threat levels were determined from available information on anthropogenic mortalities" (*Status Review* p. 67). If information had been available, the Team would have had no reason to resort to opinion-based estimates.

> quantitative studies have been conducted, but low hatchling success has been documented at important beaches in recent years.

While that is all most unclear, it strongly suggests that the BRT considered a muddle of declining habitat extent and habitat quality (leading to reduced hatching success and hatchling survival), with limited mortalities directly caused by sand removal or dumping, vehicle wheels and the like. Such a muddle risks dual counting of the same deaths under two or more of the BRT's four categories, or else an accounting as "mortality" of losses already incorporated into the Threat Matrix Model via its input value for hatching success.

Their next category of anthropogenic threat was "overuse", which is surely a sensible factor to consider when listing a species but was at best misnamed in the present application. What the BRT and its experts seem to have used this category for was not "overuse" but rather purposeful, directed harvest – a very different thing. This factor was ranked "Very Low" or "Low" in almost all cases but as "Medium" for South Pacific adults and "High" for those in the northeastern Atlantic. In that region, the factor was interpreted as:

> Includes legal harvest, illegal harvest, and management/research activities.

For the adults specifically the experts' reasoning was summarized as:

> Nesting female loggerheads and eggs are still harvested at Boa Vista, Cape Verde (Cabrera *et al*. 2000, López-Jurado *et al*. 2003). In 2007, 36% of the nesting turtles were hunted, which is about 15% of the estimated adult female population (Marco *et al*. in press). In 2008, the military protected one of the major nesting beaches (50% of the island's nesting) on Boa Vista where in 2007 55% of the mortality had occurred; with the additional protection, only 17% of the turtles on that beach were slaughtered (Roder *et al*. in press). On Sal Island, 11.5% of the emergences on unprotected beaches ended with mortality, whereas mortality was 3% of the emergences on protected beaches (Cozens *et al*. in press). The slaughter of nesting turtles is a problem wherever turtles nest in the Cape Verde Islands and may approach 100% in some places (C. Roder, Turtle Foundation, Münsing, Germany, personal communication, 2009; Cozens in press). It is estimated that today more than 20% of the nesting emergences throughout the archipelago results in the slaughter of the females (J. Cozens, SOS Tartarugas Cabo Verde, Santa Maria, Sal Island, Cape Verde, personal communication, 2009). The meat and eggs are consumed locally as well as traded among the archipelago (C. Roder, Turtle Foundation, Münsing, Germany, personal communication, 2009). Additionally, free divers target turtles for consumption of meat, often selectively taking large males (López-Jurado *et al*. 2003). Turtles are harvested along the African coast and, in some areas, are considered a significant source of food and income due to the poverty of many residents along the African coast (Formia *et al*. 2003).

> Loggerhead carapaces are sold in markets in Morocco and Western Sahara (Fretey 2001, Tiwari *et al.* 2001, Benhardouze *et al.* 2004).

That comment, which provided considerably more quantitative information than most of those on the spreadsheet, confirms that the "overuse" factor was actually used to include mortalities resulting from directed harvest, whether over-harvest or otherwise, but not those resulting from other kinds of "use".

It also indicates a substantial quantitative error: If published and "in press" estimates of the proportion of emergences ending in slaughter range from 3% to 36%, it is difficult to see how a rating of "High", meaning a mortality rate in excess of 20% can be sustained. The average clutch frequency in the northeastern Atlantic is said to be 5 (*Status Review*, Table 1, p. 39), such that a female on a protected beach could be exposed to a 3% risk five times but, with a remigration interval of 3 years, that is only a 5% average annual mortality rate. Those emerging on beaches hunted at a rate of 36% per emergence would stand a 90% chance of being taken over five emergences in the year, for an average annual mortality rate of 30%. The proportion of the turtles using beaches which experience different levels of hunting are not stated in the above text and perhaps are not known but the midpoint of the range of inferred mortality rates is 17.5% per year – s "Medium" level of mortality, according to the rankings adopted by the BRT, but not "High".

The ESA-listing factor labeled by the BRT as "Disease/Predation" is equally appropriate as something to be considered when listing a species but, as External Reviewer #5 noted, is a curious category when examining anthropogenic mortality. Probably in consequence, it was rated "Very Low" or "Low" throughout. The intent may have been to consider only anthropogenically-enhanced predators and diseases, though the factor is described in a spreadsheet comment as:

> Includes disease and parasites, egg pathogens, exotic dune and beach vegetation, predation by exotic species, predation by native species, and red tide and other harmful algal blooms.

One important consequence of this strange use of ESA listing factors as categories of anthropogenic mortality is that almost all of the human-caused loggerhead deaths, in reality as well as in expert opinion, are lumped into an undifferentiated "Other" category. That cannot have helped the experts in their task and does not help the reader of the *Status Review* understand what is happening to the model turtles.

The lumping had another curious, though probably beneficial, effect: The BRT sensibly did not want to claim annual mortality rates exceeding 100%. There need have been no fear of that if the Team combined mortality rates resulting from different causes by calculating the product of the corresponding survival rates. Instead, as has been explained above, the BRT simply summed the anthropogenic mortality rates. To prevent the sums going above 100%, the Team set an upper limit on the "High" mortality-rate class at 25% – reasoning that there were four factors and all four could, in theory, be set to "High"

(*Status Review*, p. 57). In practice, the first three factors very rarely summed to a "worst case" mortality rate greater than 30%, limiting the overall anthropogenic mortalities of any one life stage in any one region to a maximum of 0.55 (*Status Review*, pp. 68–76). That oddity served to moderate, to some degree, the overestimation of mortality rates, though it entirely undercut the logic of setting the 25% limit. It would also prevent the Threat Matrix Model from having any value in studies of the relative importance of different sources of anthropogenic mortality, if other deficiencies had not already prevented such use: One cannot compare the relative magnitudes of different things when one of them has been given an arbitrary upper bound.

The criticisms raised thus far in this section have been essentially methodological. They show that the parameterization of the Threat Matrix Model was so uncertain that its output does not merit any credence whatsoever. With perhaps the exception of the last paragraph above, these criticisms have not, however, shown that the parameter values selected by the BRT were actually wrong. For most, that cannot be shown because the true values are simply unknown – which is why the BRT resorted to opinion-based estimates in the first place. There are a few points at which it is possible to go further.

For the western North Atlantic loggerhead, the experts thought that anthropogenic mortality on oceanic juveniles is "Medium/High" (*Status Review* p. 73), which in the BRT's rankings means an annual mortality rate of anything up to 25%. In reality, there are some 35,000 loggerhead nests on Florida index beaches annually, with those comprising some 70% of all nests in the State (Witherington *et al*. 2009). There is additional nesting elsewhere on U.S. coasts and those of Mexico and some Caribbean islands for a total of over 70,000 nests as an annual average through the decades either side of the turn of the millennium (cf. *Recovery Plan* p. vii). With an average clutch size of 115 (*Status Review* Table 1, p. 39), that means an annual total of 8.0 million eggs (to a precision of two significant figures). The BRT favored a first-year survival rate of 0.4 following an emergence success rate of 0.54, which should mean 1.7 million western North Atlantic loggerhead entering their second year of life every year. Subsequent oceanic juvenile natural survival rates are estimated as 0.794 or higher, meaning a total mortality rate of 40% if anthropogenic mortalities reached 25%, while there is supposed to be an average 10-year oceanic-juvenile phase (*Status Review* Table 1, p. 39). With those numbers, the deaths from a 25% annual anthropogenic mortality rate would approximate one million per year. In practice, loggerhead bycatch on pelagic longlines (the only major source of anthropogenic mortality on oceanic juveniles yet identified) in the Atlantic has been estimated as 150,000 to 200,000 (Lewison *et al*. 2004), not all of which will be western-hatched turtles, while some proportion of those hooked are released alive. The numbers of deaths implied by the BRT's experts' opinions are thus an order of magnitude greater than the empirically-observed figures[41]. Even a "Medium" ranking would have been too high as its lower mortality-rate threshold (0.1) implies more

[41] External Reviewer #7 came to the same conclusion, stating: "There is no way that oceanic turtles (especially juveniles) incur an added 10–30% mortality due to bycatch", though he does seem to have been thinking of bycatch by U.S. fisheries only. His comment appears to have been ignored by the BRT.

than 500,000 deaths annually. Quantitative estimation, on the empirical foundation provided by Lewison *et al*. (2004), in place of the BRT's expert opinions suggests that a "Low" rank (mortality rate 0.01–0.1, annual deaths 70,000–500,000) would have been appropriate for "Other" anthropogenic mortality on oceanic juvenile loggerhead of the western North Atlantic population – not the "High" used in the Threat Matrix Model. That would have changed the range of perceived overall anthropogenic mortality rates on that one life stage from the BRT's 0.10–0.28 (*Status Review* p. 73) to 0.01–0.13, as the calculations for the *Status Review* were performed, which may be compared with a natural mortality rate of 0.21 – meaning that anthropogenic losses would be somewhere between negligible and minor.

Of all of the survival or mortality rates of loggerhead, the only ones that are directly estimable using foundations of empirical evidence, with useful precision and at affordable cost, are the hatching success of eggs and the survival of breeding females across remigration intervals. The latter can be tagged and returns of tagged females to their nesting beaches monitored. As has been noted above, the BRT recognized that those studies produce adult female survival rates of 0.81 to 0.88 (*Status Review* p. 55)[42] for overall mortality rates on the life stage of 12–19%. The Team chose to set the natural component of that mortality at 5% (*Status Review* Table 1, p. 39), leaving some 10% anthropogenic mortality – a simple additive calculation being accurate to this level of precision when mortality rates are so low. The Threat Matrix Model, in contrast, was given opinion-based rates of 13% to 50% annually for those western North Atlantic females which remain in neritic habitats (or a little higher in a nesting year) and 10% to 28% for individuals in oceanic habitats (*Status Review* Table 8, p. 73). With an estimated 95% neritic (*Status Review*, Table 1, p. 39), those figures translate into population-wide anthropogenic mortality rates of 13% to 49%. When compared to the BRT's own empirically-based estimate, those opinion-based numbers range from an over estimate to an absurd error – which does nothing to build confidence in those of the experts' guesses which cannot be directly checked .

Considering the dubious nature of its parameterization, the BRT was very sensible to include a sensitivity analysis within its examination of the Threat Matrix Model (*Status Review* pp. 85–99). Bizarrely, however, they only looked at the sensitivity of their results to estimates of the loggerhead's age at first reproduction and the percentage of the juvenile phase that is spent in oceanic habitats. Those were among the parameters for which there was at least some empirical foundation for the values used and hence some small confidence. Sensitivity of the Model's outcomes to the many ill-founded, opinion-based inputs was not examined at all, unless one counts the arbitrary re-assignment of the mortality thresholds for each class (*Status Review* p. 59) – for which no results appear to be reported anywhere in the *Status Review*, despite its authors promising to do so (*Status Review* p. 67).

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[42] The TEWG produced estimates of 0.73 to 0.85 (*Assessment* p.78).

Model Results

So deficient was the parameterization of the Threat Matrix Model that its results can be given no credence at all. As has been noted above, even the BRT recognized the problem, if not its severity nor its implications, when they wrote that the model "may not represent the actual mortalities and may overestimate the anthropogenic mortalities" (*Status Review* p. 58) and that it had "potential overestimations […] of anthropogenic mortalities" (*Status Review* p. 67). The only External Reviewers who commented on this modeling likewise denounced the reliability of its parameterization. That being so, there is little point in discussing what the outputs were. Suffice to say that the BRT concluded, on the basis of this deficient model, that the abundance of each of its proposed DPSs could currently be declining (some swiftly), while only those in the Pacific and Indian oceans could be increasing – and that only slowly. Those conclusions cannot be taken seriously, as they measure the effects of over-estimates of anthropogenic mortality and do not represent trends in real loggerhead populations. It must be noted, however, that (as with the SQE approach) the BRT sought only to show increasing or decreasing tends in loggerhead abundances, not the imminence of extinction – which would require information on current abundances as well as trends. As the Team itself wrote: "The analysis does not provide estimates for the likelihood or probability of extinction" (*Status Review*, p. 53). Thus, nothing in the output from the Threat Matrix Model could have had much relevance to the objectives of the *Status Review*, even if it had been possible to parameterize the Model with reasonable accuracy – which could not be done.

While the Model's results must be thus dismissed, some aspects of their presentation merit further discussion. The Threat Matrix Model was initially run as a "base population model", without the estimated anthropogenic mortalities. That is a rather strange beast. In their reviewed draft, the BRT said that it was intended "to represent the plausible pristine condition of the DPS". External Reviewer #5 correctly objected and the final wording became "to represent the maximum plausible vital rates and population growth rate, which may be attained for a recovering DPS" (*Status Review*, p. 53) – a suggestion repeated several times in the *Status Review's* Conclusions (*Status Review* pp. 161–166). Unfortunately, that is not correct either. What the "base population model" represents (or would if it credibly represented a loggerhead population at all – which it does not) would, save for one complication, be the consequences of eliminating all anthropogenic mortality following depletion of the population to the level seen *circa* 2010 (which is not necessarily the level that would see the maximum vital rates nor the maximum rate of population increase). The complication is that the "base population model" assumes stability in mortality rates over recent decades, before the hypothetical lifting of anthropogenic pressures, whereas at least some real loggerhead populations have experienced major changes in mortality as increasing human pressures have been interwoven with emerging conservation efforts. Given the lag times inevitable to the dynamics of such a long-lived species, the real populations are not in equilibrium with the current mortality rates. The "base population model" is, therefore, an abstraction which could never have more than limited relevance to the real world. Given reliable parameterization (which the Threat Matrix Model has not been), comparison of model runs incorporating anthropogenic mortality with the "base" case might prove informative but it would be the comparisons rather than the "base population model" itself, that would provide the information.

That being so, one would not expect to see many useful results extracted from the "base" model. The *Status Review* provides none, the supposed results (*Status Review* p. 59) offering little more than methodological information. The "base" runs did suggest that if the loggerhead were now left alone, their abundance would climb at less than 10% per year but that was hardly a surprise with such a species. The Model gives no indication of how long such a rate of increase might continue before density dependence and habitat carrying capacity (both ignored in the Model) began to have a noticeable effect.

The Model runs incorporating anthropogenic mortality served to summarize the experts' opinions (and that poorly) but it is not clear that they did anything else. Attempts to compare among DPSs (*Status Review* p. 77) fail because the parameterization was based on disparate opinions, not comparable data. The *Status Review* (pp. 80–82) found that the western and eastern North Atlantic, Mediterranean and South Atlantic DPSs had the worst prospects, with the northern Indian Ocean DPS not far behind. Perusal of the threat rankings (*Status Review* pp. 68–76) shows that the first four were the only DPSs given any "Medium/High" or "High" threats, most of which arose out of expert opinions concerning levels of bycatch. Whether that reflects more severe losses in the Atlantic Ocean and its marginal seas than in the Pacific and Indian basins or, conversely, whether it merely indicates the BRT's experts' awareness of bycatch in the former area, coupled with their ignorance of the Pacific fisheries remains a moot point[43]. The reasons for the poor showing of the northern Indian Ocean DPS are less obvious but may be related to the assumed higher levels of natural mortality in that ocean.

The Model was used to calculate "best case" and "worse case" results, the former being determined by using as input the lower threshold for each class of anthropogenic mortality rate selected by expert opinion (for each of four factors, for each of six life stages and for the region, or proposed DPS, for which the model was run), whereas the "worst case" used the upper threshold. That is a very crude way to apply the uncertainty inherent to the use of the four nominal rankings of mortality rate: If the experts had selected the correct class in every case (which they did not), it would be extremely improbable that all 24 mortality rates lay near the lower bound of their classes or that they all lay near the upper. Even if the probability of each estimate was uniformly distributed between the thresholds for its class (a not-unreasonable assumption), the probability of the combination of two dozen of them would be concentrated towards the middle. Thus, the "best" and "worst" cases may be the extreme values that the Model can produce, given the inputs provided, but they are not themselves plausible indicators of

[43] Lewison *et al*. (2004) estimated loggerhead bycatch on pelagic longlines at some 50,000 in the Pacific, compared to 250,000 in the Atlantic and Mediterranean combined. However, the Pacific loggerhead populations only total a few thousand breeding females, compared to several tens of thousands in the Atlantic. Thus, bycatch mortality rates may well be higher in the Pacific, even though the number of loggerhead taken is lower.

what is happening to loggerhead – and would not be even if the Model's parameterization were vastly improved.

The presentation of "results" from the Model made much use of a measure denoted as P_λ, which the BRT suggested "can be considered as an index of the future population health" (*Status Review* p. 59). Whatever P_λ may represent, it has little to do with the future since the Model considers only present conditions and the current rate of change in abundance. What P_λ is, as a matter of calculation, is a ratio: the difference between the model's "best case" rate of population increase and population stability, divided by the difference between the "best case" increase and the "worst case" decrease – all of the regional populations being projected to decrease in the "worst case" scenarios (*Status Review* pp. 81–82). That makes P_λ some sort of measure of where current expert opinion places current mortality rates within each DPS relative to the "break even" point between a recovering trend and further decline. Even if the opinions could be trusted (which they cannot be), such a P_λ would still be a poor measure since it implies a uniform distribution of the probabilities of anthropogenic mortality rates between the two cases – when that distribution is most certainly not uniform.

The BRT claimed (*Status Review* p. 80) that its estimates for the North Pacific were more precise than those for other areas when what they really meant was that expert opinion produced almost entirely "Very Low" and "Low" threat rankings for the North Pacific. Since the mortality-rate thresholds for "Very Low" and "Low" were 0–0.01 and 0.01–0.10 respectively, the Model's numerical outputs necessarily spanned a narrower range than those from models with many "Medium" (0.10–0.20) and "Medium/High" (0.10–0.25) rankings. That too was unhelpful.

Nothing more need be said of the Threat Matrix Model. It could only ever have had limited relevance to the question of the whether loggerhead merit listing as "endangered" and even that would have needed acceptably-precise, empirically-based parameterization. Instead, it was given vague and opinion-based inputs that, where they can be checked against data, are seen to be seriously wrong. Such a model cannot be accepted as a work of science and cannot be given credence as any indication of current trends in loggerhead populations. Some more-developed variant of the model might be useful in future if it were possible to parameterize it with adequate confidence. The present version, however, has no utility whatsoever. There may have been academic and educational value in the exercise of the model building, but the parameterization renders the resulting Model entirely useless as a foundation for any form of management decision. Most especially, the Model is utterly unsuited for evaluation of the quantitative degree of risk to loggerhead survival, which must lie at the heart of a choice between "threatened" and "endangered" status.

The *Federal Register*'s Argument

The authors of the *Federal Register* notice may have had some awareness of the deficiencies in the SQE approach and the Threat matrix Model. They certainly should have: External Reviewers #5 and #7 had left little room for doubt in the matter. What is sure is that the notice played down the significance of both quantitative analyses, suggesting that they only "provided additional insights into the status of the nine DPSs" (*Federal Register* notice p. 12640). The problem with that position is that elimination of the two models leaves the Services' central conclusion, that loggerhead are endangered across most of their global range, with not even the pretence of a foundation.

The *Federal Register* consideration of the statuses of the proposed DPSs began with a discursive treatment of the various time-series of nests and nesting females (pp. 12613ff). If the BRT had started with that, proceeded to quantitative modeling of current trends and if that modeling had shown the trends leading to quasi-extinction within the immediate future, a case for "endangered" status might have been constructed for some of the DPSs. The Team, however, only attempted its irrelevant SQE approach, while the authors of the *Federal Register* notice made no quantitative projections and thus could not step from past observations to future consequences. Their discussion of the past thus gave no indications of the future and so no evidence of endangerment.

They did proceed to an examination of the five factors to be considered when listing a species under the ESA (*Federal Register* notice pp. 12615ff). That examination was based on Section 5 of the *Status Review* and described in great detail, supported by copious citations of the literature, all of the dangers besetting loggerhead[44]. It was, however, only a discursive alternative to the BRT's Threat Matrix Model, as the discussion of trends in nest numbers was an analog of the SQE approach. Neither discussion could support quantitative conclusions and hence could not indicate whether the turtles are "endangered" or merely "threatened". Every biological population exists in a dynamic equilibrium, or perhaps disequilibrium, between factors tending to increase its size (primarily growth and reproduction) and the deaths which reduce its abundance. Listing all of the many reasons that loggerhead die is no more evidence of their decline to extinction than a list of all of their prey species would be an indication that their numbers will increase without limit. Decisions about "up-listing" require quantitative information on the balance between reproduction and death. Without that, there is no scientific foundation for determining the status of a population or a species.

When it came to the final "Finding", the *Federal Register* notice (pp. 12649ff) sought to rely on all four arguments: the SQE approach, the Threat matrix Model, the discussion of trends in nesting and the list of dangers faced by loggerhead. The BRT's unsupported declarations that the sources of anthropogenic mortality are unlikely to be alleviated in the foreseeable future were also trotted out once again. Unfortunately for the Services'

[44] There is no space here to examine the *Status Review's* Section 5 in detail, nor is that necessary to a rejection of the Service's arguments. The lack of specific comment should not be taken as an endorsement of that Section. It does not merit any.

position, those five bear no more credibility when combined than any of them can carry alone.

The Services' own *Endangered Species Petition Management Guidance* demands "convincing information" before "up-listing" a species from "threatened" to "endangered". Where loggerhead are concerned, not only have they failed to produce a convincing case, they have not even offered a credible one. Indeed, the BRT has shown very little attempt to do so. In the absence of any better argument, the proposed listing of loggerhead as "endangered" can only be rejected as without merit.

A Reconsideration

The above comments have shown that the *Status Review* and the *Federal Register* notice contain nothing that amounts to the case needed to justify an "up-listing" of loggerhead. Under the onus set by the *Endangered Species Petition Management Guidance*, there is no requirement for a commentator to proceed further. A question may reasonably be raised, however, as to whether this is simply a matter of the BRT failing to marshal a credible case documenting real endangerment of the turtles or, conversely, whether the conservation status of loggerhead truly falls within the range labeled as "threatened" under the ESA. While not necessary, it is appropriate to attempt an answer to that question here.

I only offer a consideration of the status of the western North Atlantic loggerhead population and indeed primarily of those loggerhead nesting on the beaches of peninsular Florida. For all of the reasons previous outlined, only the conservation status of that population is of immediate concern to ESA-listings of loggerhead on the eastern seaboard of the United States, while its status will determine the listing of the entire species, unless and until loggerhead are divided into separate DPSs.

In my examination, I draw on a new assessment which was prepared by the Turtle Expert Working Group ("TEWG") contemporaneously with the BRT's work on the *Status Review*. The report on that work (TEWG 2009 – hereafter the *Assessment*) was released in July 2009, just a month before the *Status Review* was finalized[45]. The tasks of the BRT and the TEWG were not identical. The former was required to look globally and the latter regionally, though the BRT chose to run analyses only on its nine proposed DPSs (or on finer subdivisions of them) and the loggerhead of the western North Atlantic formed one such unit. Furthermore, the BRT was charged with determining whether the status of loggerhead falls above or below the line separating "endangered" status from "threatened" under the ESA, whereas the TEWG considered status and trends in the population more generally. One might nevertheless anticipate that two contemporaneous conservation assessments of the same population, prepared by the same agency (albeit

[45] The TEWG was convened in December 2006 (*Assessment* p. vii). The BRT was convened in February 2008 (*Status Review* p. 6).

USFWS was only involved in the *Status Review*), would apply similar methodology to similar data sets and would arrive at similar conclusions – doubly so since four of the thirteen members of the BRT were also among the sixteen members of the TEWG. Reality proved quite otherwise. Where the BRT naively applied a single, over-simplified model to but one data type and built a second model on non-scientific foundations, the TEWG carefully examined a wide range of information, including size frequencies and strandings data as well as abundance indices. Where the BRT offered conclusions which admit of no doubts, the TEWG explored the major uncertainties surrounding the status of loggerhead. Where the BRT concluded that the western North Atlantic population is "endangered", the TEWG stated that there can only be a limited ability to assess that population until better data are available – though the Group noted the decline in nesting, as it also foresaw increased recruitment to the adult population in the next number of years (the latter implicitly denying any risk of imminent extinction). If there was any commonality between the two, it was that the TEWG, like the BRT, modeled the time series of nest numbers using constant rates of proportional decline (though the two groups used quite different model formulations) and both thereby missed the messages in those data[46].

I also draw on the recent work of the loggerhead Recovery Team (NMFS & USFWS 2008: here cited as the *Recovery Plan*), three of the eight members of which were also on the BRT. Discrepancies between the conclusions of that Team and those of the BRT can, however, perhaps be attributed to the passage of time and the resulting increase in understanding over the eight months between completion of the *Recovery Plan* and that of the *Status Review*.

Resilience of the Population

A first point to note is that, while the numbers of nests on many beaches are low, loggerhead nesting around the western North Atlantic and its marginal seas stretches from Virginia via Mexico to Guiana (Ehrhart *et al*. 2003), spanning more than 30° of latitude. Such a broad distribution gives hope that the population can survive through climate change and more local adverse environmental perturbations. It also carries the promise of reasonable levels of intra-population genetic diversity. This broad spread of the population is no guarantee of long-term survival but it does promise more resilience than if nesting was confined to a handful of beaches in one small area.

Overall Abundance

The Recovery Team concluded that the total number of loggerhead nests on U.S. beaches alone fluctuated between 47,000 and 90,000 annually between the mid-1990s and the mid-2000s, with the available data suggesting an annual average of some 80,000 since

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[46] The BRT did cite the TEWG's assessment in their own *Status Review* and did so four times. All four, however, used the *Assessment* as a secondary source of information on turtle migrations. The BRT made no mention of the TEWG's own work on the status of the western North Atlantic loggerhead.

1989 – to which can be added far smaller numbers of nests scattered through the "Greater Caribbean" (*Recovery Plan*, pp. I-3 – I-11). Applying the *Status Review*'s values for remigration interval and clutch frequency, such nest numbers suggest an adult female population of something less than 50,000 individuals. In view of the trends to be described shortly, that can be interpreted as several tens of thousands around the turn of the century and a few tens of thousands by 2005.

If, for purposes of illustration, annual survival rates of juveniles (after the first year) and those of adults are taken as 0.85, inclusive of the effects of anthropogenic mortality, and if the age at first reproduction is taken as 30 years (*Status Review*, Table 1, p. 39), 50,000 adult females would imply a total female abundance (excluding young of the year) of approximately 3.3 million in the western North Atlantic population, assuming a stable population[47]. Such an estimate cannot be considered accurate, of course, but it is indicative. It may also be an under-estimate: If the total mortality rate is judged to be higher (and hence the survival rate lower than 0.85), either overall or for the juveniles alone, still greater total numbers would be needed to sustain the estimated adult abundance. Sex ratios in loggerhead are most unlikely to be even but there seem to always be sufficient reproductively-active males to mate with the females that are ready to nest[48] and hence, for comparison with conservation of species which do maintain even ratios, the effective size of the loggerhead population in the western North Atlantic population (excluding young of the year) can be placed at several million individuals. To that can be added, seasonally, several million more eggs and hatchlings, for an annual peak abundance perhaps around 15 million.

Conservation status cannot be judged by abundance alone but populations enumerated in the millions, with tens of thousands of reproductively-mature females do at least constitute a considerable buffer against extinction. Such a population may well be under threat (and few would doubt that the loggerhead population is) but the threat would have to be dire indeed to raise the risk of imminent extinction required for "endangered" status under the ESA.

Trends in Abundance

Much the best dataset on the abundance of western North Atlantic loggerhead comprises the time series near-comprehensive censuses of nests on index beaches in peninsular Florida, which extends from 1989 to date (Witherington *et al.* 2009). The index-beach survey, like any other, is not above question but all signs are that it returns more reliable

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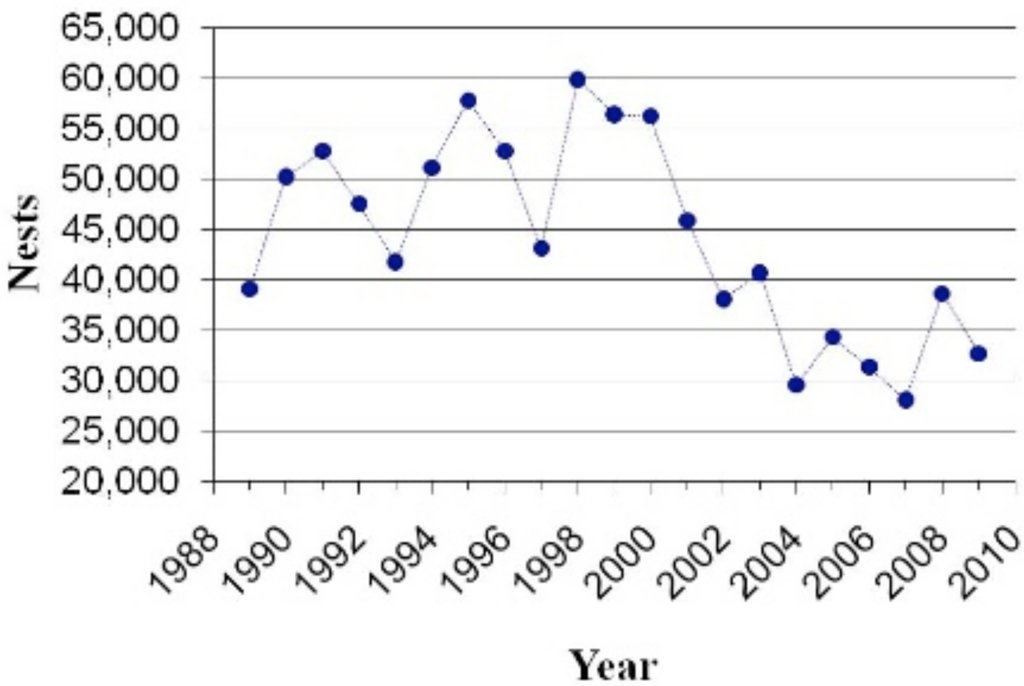
47 The calculations need only a simple spreadsheet model of relative abundance declining with age under a constant mortality rate.
Most of the millions of loggerhead would, of course, be very young oceanic juveniles. Confining the calculation to animals of 10 or more years of age suggests a population of around 1.0 million females in the "neritic" stages.

48 Bowen (2003) argued that the high rate of multiple paternity of the eggs in loggerhead nests in Florida shows that the number of males is not a limiting factor, despite the hatchlings from warm, southern-Florida beaches being 93% female.

data than most, while it appears to give a fair indication of trends in the total loggerhead nesting in Florida, only some 70% of which is on the index beaches[49]. Through to 2009, the most recent data yet available, the series of counts can be represented as:



Nest Counts on Florida Core Index Beaches 1989–2009
[Source: http://research.myfwc.com/images/articles/10690/20100521_135508_18669.jpg]

The severe decline in nesting through the earliest years of the present century has already been noted above. Before examining it in detail, however, it is well to consider the increasing trend through the 1990s. With ages at first reproduction of 20 to 40 years (*Status Review* p. 54), the late-juveniles newly recruiting to the adult population in those

[49] This approval of the index-beach time series as a measure of loggerhead nesting activity in Peninsular Florida is based on an examination of scientific issues, particularly as those have been documented by Witherington *et al.* (2009). I know of no evidence for deliberate misrepresentation or casual error affecting the reported nest counts but neither have I made any examination of the surveys to confirm the absence of such factors. I have requested an outline of the quality-control processes used by FWRI to ensure the reliability of the data but no explanation has been forthcoming. Thus, I am reduced to accepting the counts at face value – an inadequate position for a dataset so central to an important public decision.
It may, however, be noted that Weishampel *et al.* (2010) have compiled the nest-count data for just one portion of the east coast of Florida and found a very similar temporal pattern to that illustrated for all of the index beaches by Witherington *et al.* (2009). To the extent that those analyses were independent of one another, they serve as mutual checks and give some enhanced confidence in their respective results.

years had mostly hatched on Florida beaches between 1950 and 1980. That was not an era when one would have expected decreasing anthropogenic stresses on those beaches. Specific conservation efforts only began towards the end of that period, with loggerhead being listed under the ESA in 1978. The young turtles of those year-classes would have returned to neritic waters from about 1960 onwards, where they would have faced increasing levels of fishing effort and other dangers well into the 1990s – hazards that the adults would also have faced. Thus, it appears that loggerhead nesting in Peninsular Florida increased for a decade in despite of trends in human activity, rather than because of them – even though the years of increase saw strenuous efforts to improve egg and hatchling survival (efforts yet to bear fruit because of the long lag times in loggerhead dynamics). The only conservation measures that might have materially aided the observed increase were protections for adult females on the nesting beaches (slightly aiding overall adult survival rates) and perhaps the introduction of TEDs in the 1980s. The latter might have allowed an increase in juvenile survival sufficient to drive the trend in adult abundance and hence nesting, with the on-going rise as more of the recruits to the adult population had enjoyed the protection of TEDs for a greater span of years. However, the early designs of TEDs had only limited benefits for larger juvenile loggerhead, necessitating new regulatory requirements in 2003. Most of the recruits to the adult population during the era of increasing nesting will already have been well grown before TED use became universal. Thus, the expansion of nesting through the 1990s speaks to environmental, rather than anthropogenic, drivers of abundance – much as Chaloupka *et al*. (2008) have suggested for loggerhead in the Pacific. That is a warning against over-interpreting the nest-count time-series in terms of human activities[50].

Unfortunately, following those years of increase, to a peak of some 60,000 nests in 1998, nesting fell and fell severely – dropping to under 30,000 in 2004. While those numbers concern only the index beaches, there is some survey work elsewhere in Florida and it gives no indication of any large-scale re-distribution to other beaches (Witherington *et al*. 2009). Thus, the crash seems sure to have been an actual decline in nesting. Whether it represented a decline in the number of adult females (*versus* a decline in clutch frequency or an increase in the average remigration interval) is less sure[51] but management of a threatened species needs to err on the side of caution and hence should seriously consider the likelihood that the drop in nest numbers represented a decline in adult females. The TEWG concluded that "the decline is likely real and should not be ignored" (*Assessment* p. 54), though they were thinking more broadly than just the beaches of Florida. I would endorse that conclusion.

The TEWG also saw the decline as on-going, which was a fully reasonable interpretation of the data used by that Group in their analyses, using data which only extended to 2007

[50] Witherington (2003), who likely wrote before the 2002 index-beach nest counts were available, noted the signs of recovery in loggerhead nesting and stated "It is reasonable to assume that the Endangered Species Act has had a profound, positive influence on loggerhead abundance within the United States." I would argue that that was an unreasonable assumption.
[51] The question is well reviewed by Witherington *et al*. (2009).

(*Assessment* Fig. 17, p. 58). Through to that year, the nest counts show an almost-linear decline from 1998. The addition of the 2008 and 2009 data points (which were not used by the BRT in the *Status Review* either) changes the picture considerably and gives an impression not only of renewed rebuilding, albeit surrounded by inter-annual variability, but also of the major decline having ended by 2004 (see above graph). It would be too much to claim that loggerhead nesting numbers are now increasing once more but they do appear to have stabilized – though inter-annual variability makes short-term trends difficult to discern. The 2007 count was the lowest in the series but that for 2008 returned to the level of 2002, which was on a par with 1989. The 2009 survey found numbers below those of 2005 and 2008 but above 2004, 2006 and 2007. The survey results for 2010 are awaited. They may (but may not) go far towards resolving whether nor not loggerhead nesting really is now increasing. The data through to 2009 at least leave little doubt that the major decline is over and indeed ended some years ago.

There is no other dataset of western North Atlantic loggerhead abundances of comparable quality to the Florida index-beach time-series but there are other nesting surveys which can offer some geographic context for the decline. Data from beaches north of the Florida / Georgia line show a steady decrease in nest numbers but no sudden drop after 2000 (*Recovery Plan* Fig. 3, p. I-7). Various subregions within peninsular Florida have shown remarkably similar patterns of decline, though the most northern and most southern beaches on the east coast saw rather lesser rates of decrease in nesting (Witherington *et al*. 2009). There is a parallel time series from the Florida panhandle but it only extends from 1997. For what it is worth, that series does show a decline of more than one half in nest counts from a peak (of nearly 250 nests) in 1999 to the lowest point in the time series in 2003, followed by apparent stability to 2009[52]. The swift decline is not, however, been seen in the limited nesting data from the Dry Tortugas (*Recovery Plan*, Fig. 6, p. I-10), though it is not impossible that data limitations have obscured a real drop there. Further to the west, nesting on the beaches of Quintana Roo, Mexico arguably increased from 1989 to 1999, though it would be better to say that nest numbers were stable through most of that time, aside from an anomalously-high count in 1995. The number of nests in 2000 and 2001 were divergent from one another but a sharp decline in nesting was evident by 2002. One might see 2001 as the last year of the increasing trend (with 2000 as anomalously low) or 2000 as the first of the decline (with 2001 anomalously high). Either way, nest numbers dropped approximately simultaneously with the decline seen in Florida, though not as far: By 2004, the nest totals were down by about a third (*Assessment* Fig, 21, p. 62). It is not clear from the data available to the TEWG (which extended only to 2006) whether that decline ended in 2004 or is on-going[53].

[52] The time series to 2008 is illustrated in the *Recovery Plan* (Fig. 7, p. I-11). Its extension to 2009 is available at: http://research.myfwc.com/images/articles/10690/20091012_092415_16265.jpg

[53] The TEWG attempted a comparison of temporal trends in the Floridian and Mexican nest counts (*Assessment* Fig, 22, p. 63) but perhaps missed the significance of the coincident timing because they did not set aside the 1995 Quintana Roo data point as being anomalous.

In summary, the sharp decline in western North Atlantic loggerhead nesting after 2000 was recorded broadly from the Atlantic coast of Florida westwards to Mexico. It was, however, weaker or absent from northeastern Florida northwards and perhaps in southernmost Florida, including the Dry Tortugas.

Causes of the Decline

The important aspects of this decline in nesting are that it was almost-certainly real, undoubtedly severe, likely represented a collapse in the numbers of adult females, and that (for now) it appears to have ended. The key remaining questions are: What caused the decline and will it recur?

The TEWG's conclusion that "the cause of this [decline] is still unknown" (*Assessment* p. 54) was unfortunate. The cause was evidently unknown to the TEWG, and final proof is lacking, but evidence pointing very strongly to one cause was in front of the Group, had they chosen to look for it[54].

The key characteristic of the decline, after its severity, was its rapid onset. It is, however, unclear just how sharp the fall really was. The Florida survey only has small uncertainty in the numbers of nests on the index beaches but there is considerable inter-annual variability, likely primarily related to the proportion of adults which come ashore in any given year[55]. 1998 saw the highest recorded nest count, so far, in the time-series but the slight dip to 1999 and 2000 was well within the normal range of variability. There is no reason to suppose that any special decline began before the nesting season in the latter year. Indeed, it may not have begun before the fall of 2001: From the start of the current index-beach surveys in 1989, there was a pronounced inter-annual pattern in numbers of nests, with every fourth year seeing substantially fewer. That might have been a chance effect, with three of twelve years seeing fewer turtles nest, while the gap between the second and third just chanced to equal that between the first and second. If so, the reduced numbers of nests in 2001 represented the first phase of the major decline. Alternatively, the relatively-low 1989, 1993, 1997 and 2001 nest numbers may have been part of a real biological cycle, most probably linked to loggerhead remigration intervals. (Similar cycles, though of two-year periodicity, are typical of green and leatherback turtle nest counts: *Assessment* p. vi) That is not to suggest that the normal interval was four

[54] The TEWG's further statement that "because we have now experienced a period of ten years of declines in nesting we can be sure that we will see another cycle of decreased nesting as the hatchlings from this time period reach sexual maturity" (*Assessment* p. 54) displayed a disturbing lack of comprehension of population dynamics in a group charged with such an assessment, besides its misrepresentation of the past trends in the data.

[55] Unlike many surveys, the inter-annual variability is not caused by sampling error: The surveys aim for a complete census of nests on the index beaches during the survey season. Nor is it likely to result from rapid year-to-year changes in adult numbers, rare events aside, since turtles are long-lived. While there are other contributing factors, inter-annual variability in the nest counts seems likely to be primarily driven by variations in the proportion of mature females migrating to shore to nest.

years, which would be considerably greater than the accepted estimates[56]. Rather, most individual females follow either a 2-, 3- or 4-year remigration cycle (Miller *et al*. 2003). If, hypothetically, there were an equal number of 2- and 3-year animals, if every individual followed an exact cycle and if there were no 4-year nesters on the beaches in 1989, the apparent cycle (showing some 80% as many nests in the low years as in the average of the others) would be consistent with a mean remigration interval around 2.7 years – well within the range of empirical estimates for loggerhead (*Status Review* Table 1, p. 39). Clearly, that hypothesis is not realistic but it does illustrate that a four-year cycle in nest numbers driven by remigration behavior is not inconsistent with the observed average intervals. If that four-year pattern is projected forwards through the data, the last pre-collapse year would be identified as 2001.

At the opposite end, the current lowest data point in the time-series comes from 2007, though (inter-annual variability aside) the graph suggests stability or renewed rebuilding since 2004. Thus, roughly half the adult female loggerhead seem to have disappeared over a period of three or four years (2000 or 2001 to 2004).

The rapidity of that decline, and even more the rapidity of its onset, have much to say about the cause. In the various discussions of the fall in nest numbers, there has been a subtext (rarely explicitly stated) that the apparent fall in adult numbers in the early years of this century arose from reduced survival of young turtles one or more decades earlier. That cannot be: Recruitment to the adult nesting population is not simply delayed until decades after hatching, it is also delayed by a variable number of decades – while the impact of reductions in recruitment is itself not instantaneous but gradual, as the accumulated mass of existing adults slowly dies off. Following the BRT's estimates of loggerhead dynamics, if all production of hatchlings was immediately reduced by some substantial fraction and that reduction maintained, the change would show 5% of its eventual effect on adult abundance 25 years later, 25% of it after a delay of 30 years, 50% after 35 years, 75% after 40 years but 95% not until 50 years after hatchling production was cut[57]. If the additional mortality acted on oceanic juveniles, so that it

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56 Though Schroeder *et al*. (2003) noted unpublished data from peninsular Florida suggesting a mean remigration interval of 3.69 years – all estimates of shorter intervals quoted by them having come from studies completed long before the index-beach surveys began. Likewise, the TEWG (*Assessment* p. 78) found a best fit with models of adult survival if those assumed a four-year remigration cycle.

57 Those results are based on the BRT's average age at first reproduction of 30 years, with a standard deviation of 5 years, along with an overall survival rate of 0.85 for both adults and juveniles.

It is, however, unclear how the BRT modeled the variability in ages at first reproduction. Aside from the standard deviation (5 years) and 95% confidence limits (21 to 41 years), all that the *Status Review* reported was that a negative binomial distribution was assumed (*Status Review* p. 54). It might be expected that that would be the distribution of the probabilities of individual juveniles of various ages transitioning to adulthood. However, if modeled that way, the average age at first reproduction would be dependent on the mortality rate acting on older juveniles. With high mortality (likely meaning high anthropogenic mortality added to natural losses), the average age of first reproduction observed on the breeding beaches would be much reduced since the year-classes would be thinned by deaths (as well as prior maturations) before they reached advanced ages. Yet the 30-year value was derived from just such empirical observations. To make the average independent of mortality rate, I have chosen to apply the BRT's mean and standard deviation to appeared as a reduction in the numbers recruiting to the "neritic stage" (were there such a thing), then its effects on adult numbers might be a little less drawn out, since fast-growing individuals likely both return to coastal waters and reproduce sooner than the slow growers, meaning that the variation in the duration of the "neritic stage" may be a little less than that in the age at first reproduction. Should the extra mortality act on younger neritic juveniles, however, its impacts on adult numbers would be more drawn out: It is implausible that any source of mortality would affect only turtles in their year of return to coastal waters. Some older juveniles would be killed also. Hence, over a decade or two, successive cohorts would reach adulthood after experiencing progressively greater numbers of years of heightened mortality.

Clearly, none of those prolonged appearances of effects on adult numbers bears any resemblance to the sudden onset of the decline in nest numbers on Florida beaches. It follows that the primary causes of that decline cannot have involved reduced survival of young loggerhead.

The only way to explain the rapid onset of a steep decline in adult female loggerhead numbers is to suppose that the primary cause involved the deaths of either the adults themselves, older juveniles that were about to reproduce for the first time or a combination of the two[58], <u>and</u> that those deaths occurred through the same years that the numbers of adults fell. Hence, if the 50% drop in nest numbers between 2000 or 2001 and 2004 was paralleled by a decline in adult females, then some 25,000 large loggerhead died during those years – over and above the several thousand that must die in a typical year, given observed mortality rates[59]. No such mass die-off of these large, charismatic

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the numbers of juveniles reaching adulthood at each age, rather than to the chance that a juvenile of that age would reach adulthood. It is not known whether the BRT also followed that path. (To simplify my calculations and since they are only intended to be illustrative, I used a Gaussian, rather than negative binomial, distribution.)

58 Direct estimations of overall adult female survival rates have found them to be in the range 0.81 to 0.88 (*Status Review* p. 55) or 0.73 to 0.85 (*Assessment* p. 78). If the rate was 0.85, halting all recruitment to the reproductive population would result in adult abundance falling 15% or a year or 48% in four years, which is consistent with the observed rate of decline in nesting on the index beaches. Thus, the deaths <u>could</u> all have been those of older juveniles about to reproduce for the first time. However, it is difficult to envision a source of mortality that would eliminate all older juveniles over a number of years without killing any adults.

Besides, for the one nesting area for which suitable data exist, Quintana Roo, Mexico, there has been an increase in the proportion of first-time nesters. The trend was very slow from 1998 until 2003 but 2004 saw a substantially higher proportion of new adults (*Assessment* Table 15, p. 80). That was also the last year of notably-declining nest counts on the Mexican beaches, at least in the data available to the TEWG (*Assessment* Fig. 21, p. 62). Thus, the decline in that area, if nowhere else, included a substantial loss of adults and little sign of reduced recruitment. Recruitment failure cannot explain any decline in adults nesting in Quintana Roo – nor, by extension, can it explain the parallel decline in Florida.

59 The TEWG undertook a new analysis of tagging data to generate estimates of annual adult survival rates, producing best estimates of 0.73 for Melbourne Beach, FL, 0.81 for Wassaw Island, GA and 0.85 for both Quintana Roo, Mexico and Bald Head Island, NC (*Assessment* p. 78). The *Assessment's* Executive Summary declared those figures to be "current" (*Assessment* p. xi) and, if they were, they might negate the conclusion developed here that approximately half the adult female loggerhead died during the early years

animals was reported at the time but that is only a comment on our inability to monitor ocean ecosystems. The only way to explain the index-beach nest counts without invoking the deaths of tens of thousands of adult or near-adult loggerhead is either to suppose that nesting declined without a decline in adult females or else that the survey counts declined without the real number of nests doing so. Having already set aside those possibilities, the task of identifying the causes of the decline becomes one of finding a source of mortality that could have hit large loggerhead swiftly and hard, beginning in 2000 or 2001. Somewhat more tentatively, it should be a source that ended only a few years later.

The TEWG concluded that "no single mortality factor stands out as a likely primary factor. It is likely that several factors compound to create the current decline" (*Assessment* p. xii). The rapidity of onset argues otherwise: While loggerhead face many causes of death and the risk of each is constantly varying, the sudden and unique event of a severe decline in nest numbers must have had either a single, predominant cause or else multiple major causes were closely synchronized in time. The latter, while not impossible, would be improbable enough that it can be set aside here.

Lastly, it is clear that the cause of the decline was specific to loggerhead: Green and leatherback turtles, which use some of the same nesting beaches in Florida, remain much less abundant than loggerhead but their numbers continued to increase as loggerhead nesting fell (Witherington *et al.* 2009).

Witherington *et al.* (2009) have reviewed a wide range of possible causes for this decline, including direct harvest of turtles or eggs, fishery bycatch, disease, boat strikes, pollution, climate change and a decline in food resources[60]. The first can be rejected: Large scale turtle harvests or nest raiding simply do not occur on Florida beaches. Environmental change may well have played a part, as it likely did in the preceding period of increasing nesting. However, the rapid onset of a single, massive loss of adults and older juveniles is simply inconsistent with gradual change in the loggerhead's environment as the primary cause. Thus, the only plausible primary factors in the decline were either an epizootic or a temporary increase in one of the major, known anthropogenic causes of mortality: fishery bycatch, boat strikes or pollution.

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of this century. Closer examination of the TEWG's report, however, indicates that the Quintana Roo data were from tagging which has been extensive since 1996, the Wassaw Island data spanned 1973–2008, while the Melbourne Beach tagging was mostly in the mid-1980s, though continued through to 2007. The data from Bald Head Island spanned 16 years and finished not later than 2007 (*Assessment* pp. 10, 77-78). Thus, none of the estimates was specific to the years of nesting decline. Indeed, it appears that no program is routinely monitoring time-specific mortality rates of adult female loggerhead and the recent NRC committee had to urge even development of new time-generalized estimates (NRC 2010).

[60] Witherington *et al.*'s (2009) discussion of the causes of the decline was unfortunately compromised firstly by a failure to recognize that the losses of loggerhead were simultaneous with the fall in nest numbers and secondly by the authors' repeated explicit reliance, in their logical arguments, on what they "believed" to be true. Belief being a matter of faith and directly opposed to empiricism, their discussion cannot be accepted as a work of science. Witherington *et al.*'s (2009) listing of the possible causes of the loggerhead decline is, nevertheless, useful.

Fishing effort has markedly declined in U.S. waters since the mid-1990s, notably following the Sustainable Fisheries Act of 1996, while at least some progress has been made with reducing turtle takes per unit effort (with, e.g., TEDs in shrimp trawls, circle hooks on longlines and extensive, seasonal area closures in a number of fisheries). Any sudden increase in loggerhead bycatch would require an equally sudden increase in, or change of, fishing operations or else a redistribution of the turtles onto existing fishing grounds – and specifically in an area where loggerhead were vulnerable but other turtle species were not. Nothing of the kind should have escaped notice if it acted on a sufficient scale to remove 25,000 large turtles within a few years, in this era of increasing intensively monitoring[61]. It would be a sad reflection on the existing observer programs if tens of thousands of adult loggerhead had been killed without anyone noticing. Thus, and contrary to Witherington *et al.*'s (2009) conclusions, it seems most improbable that fishery bycatch made a major contribution to the swift decline in adult abundance after 2000, even if it is the principal contributor to on-going anthropogenic mortality, as so often claimed.

Boating activity around Florida almost certainly increased during 2000 to 2004 but it was increasing before then and the increase continued until economic recession struck in 2008 – a pattern inconsistent with the decline in loggerhead nesting. Besides, while there is no intensive monitoring of boat strikes on turtles, it seems unlikely that a massive and sudden increase in annual deaths would have passed unnoticed in these environmentally-conscious times. Deaths from pollutants might be more easily missed but the sudden drop in numbers suggests a specific pollutant event, rather than on-going discharges. It would have to have been a very large-scale event to have had such dramatic effects, yet there seems to be no record of such a thing during the years in question – while the recent *Deepwater Horizon* tragedy shows the scale of media coverage that eventuates when a regionally-important pollution event occurs. It would also have to have been a rather particular pollutant to have affected loggerhead so severely without apparently harming leatherback or green turtles.

Thus, while it is not possible to be definite as to the causes of the observed decline in loggerhead nesting on the beaches of the western North Atlantic after 2000, the most plausible primary cause must be an epizootic with a causative agent specific to loggerhead and perhaps one specific to adults – maybe a sexually-transmitted disease or one acquired by females while ashore. While unexpected, such an event would be by no means unprecedented: Mass-mortality events have been seen many times in many kinds of marine organisms, including turtles.

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[61] It may be noted that the swift development of a much-smaller loggerhead-bycatch problem in the sea-scallop dredge fishery of the Mid-Atlantic Bight during the same timeframe was detected, mitigating measures were developed and implemented by regulation in 2005 (Smolowitz *et al.* 2010). An equally-minor bycatch issue in the reef-fish longline fishery of the eastern Gulf of Mexico was likewise detected and led to an expansion of an existing area closure, with effect from 2009.

Indeed, while the existence of an epizootic has been inferred above from the absence of other plausible explanations for the observed decline in loggerhead nesting, Jacobson *et al.* (2006) have provided entirely independent confirmation that a loggerhead epizootic did occur in southern Florida waters at about the time indicated by the nest counts – though there is no direct evidence that that event killed sufficient turtles for it alone to account for the decline. Between October 2000 and March 2001, 189 dead and debilitated loggerhead were recorded, most of them in the Keys. Their sizes ranged from under 30 cm to over 100 cm (Curved Carapace Length, "CCL") but most of the affected individuals, or at least most of those which stranded, were 70 to 100 cm – meaning larger neritic juveniles and mature adults. Despite careful study of many specimens, Jacobson *et al.* (2006) were unable to provide a satisfactory identification of the cause of the deaths. The affected turtles showed clinical signs of a neurological disorder which appeared to be associated with an infestation of the trematode *Neospirorchis*. That, however, could not be confirmed as the cause[62].

The epizootic event drew the attention of veterinarians and others who were concerned to describe a particular disorder, not to explore its implications for turtle populations. In consequence, the course of the epizootic was not monitored as it might have been and the number of stranded turtles documented in the published report was limited to the number contributing to the study, plus those which the authors could confidently report had similar symptoms. Jacobson *et al.* (2006) did note that, after the initial event, a further 18 loggerhead with similar symptoms stranded between April 2001 and September 2004 – the terminal date perhaps owing more to scientific publication schedules than to anything else. They also estimated the mean number of recorded strandings for the October to March season, through the previous decade, at 33, suggesting that some 150 of the observed dead and dying individuals were over and above normal losses (Jacobson *et al.* 2006).

It is clear that only a small and variable proportion of the turtles which die at sea end up being recorded as strandings on the shoreline (e.g. Epperly *et al.* 1996; Hart *et al.* 2006; Tomás *et al.* 2008). Jacobson *et al.* (2006) took their estimate of 150 excess strandings in six months and extrapolated it to some 2,200 deaths by applying a ratio of strandings to deaths based on the results of Epperly *et al.* (1996). Those authors had observed 12 strandings on North Carolina beaches during a period when an estimate of 181 turtles were killed by the summer flounder fishery operating offshore. Such an extrapolation was all very well for veterinarians seeking a conservative indication of the severity of the infestation that they were describing. It is not sufficient as an estimate of the magnitude of the epizootic for the purposes of an assessment of conservation status. Indeed, Epperly *et al.* (1996) did not themselves recommend the use of their observed ratio for estimating

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[62] Besides the trematodes, loggerhead are susceptible to the effects of red tides. Indeed, that was the only cause of a potential epizootic mentioned by Witherington *et al.* (2009), who did not cite the work of Jacobson *et al.* (2006). The TEWG also gave primacy to red-tide deaths (though they did note the trematode infestation), going so far as to claim a temporal association between red tides and loggerhead strandings (*Assessment* pp. 81–82), though it is hard to see in the data.

mortalities from recorded strandings. Rather, their conclusion was: "Strandings, therefore, demonstrate only that mortalities have occurred. They are not a reliable indicator […] of the number of mortalities that have occurred. And significantly, the absence of strandings does not imply that at-sea mortality, even of great magnitude, has not occurred."[63]

Turtles would be negatively buoyant without the air in their lungs and thus they sink when they die. As their corpses decompose, they bloat and may float back to the surface, sinking once again when decomposition proceeds far enough to release the gases (Epperly *et al.* 1996). It may be expected that turtles which die of disease, rather than drowning in fishing gear, tend to be emaciated at death, meaning a higher proportion of dense bone and a lower amount of buoyant lipids than in healthy animals. Thus, all else being equal, turtles that succumb to an epizootic likely sink faster and float for less time than those lost to anthropogenic causes. Those which sink into deep water may never float back to the surface, nor will those which are encountered by large scavengers able to eat the flesh before it decomposes. A turtle which sinks onto a smooth, sandy seabed may move with the currents – towards the beach if the water flows that way at the time. To strand at all, a dead and bloated turtle will usually have to float up reasonably near a beach during a time of onshore winds (Hart *et al*. 2006). To have a high probability of being recorded, the beach in question must be one that sees regular surveys by the Sea Turtle Stranding and Salvage Network ("STSSN") and the stranding must occur during a season when the surveyors are active. Epperly *et al.*'s (1996) study was conducted on the ocean beaches of North Carolina, where long stretches of sand slope gradually to a wide continental shelf. Most of the strandings were thought to have been discards from a flounder fishery that operated largely within five miles of the tideline. It is not to be supposed that the ratio of standings to deaths observed in that situation would also apply to loggerhead dying of a different cause (trematode infection), in a different place (southernmost Florida), with very different bathymetry (deep water lying close inshore, with reefs between it and the beaches), very different oceanography (the Gulf Stream flowing swiftly along-shore), different scavenger communities consuming dead turtles and a quite different environment for recording strandings (the Keys *versus* the linear barrier beaches of the Carolinas).

If fluctuations in the strandings data offer some kind of uncertain, binary indicator of mass-mortality events, interpreting them is not straightforward. Short-term variations may result merely from meteorological and oceanographic events which bring a higher

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63 The decline in loggerhead nesting goes far to confirm Epperly *et al.*'s (1996) point. If the fall in nest numbers was matched by a drop in adult female numbers, then some 25,000 adult and near-adult females died in a period of about four years, over and above the loss of several thousands per year through the normal mortality rate, plus however many males and younger juveniles may have died. Yet less than 10,000 loggerhead strandings were reported through that period (see below), with no particular indication of any special increase in mortality.

Whatever happened to the lost adults, very few of them stranded and hence the lack of strandings cannot be used to argue against any particular cause of the decline – save perhaps to suggest that nest numbers fell faster and further than the abundance of adult females did.

proportion of dead turtles to shore. Long-term trends in the data might confidently be said to represent real change but whether an increase in strandings follows from a rising population, increased mortality rates in a declining population or merely an improved efficiency in reporting can be unclear. Even an increase in the proportion of strandings with some particular characteristic, such as evidence of disease, may only point to a decline in other causes of death. When both the absolute and relative abundances of particular classes of strandings spike upwards, however, one might begin to find meaning in the data: Local variations aside, the relative increase shows that it was not merely weather conditions stranding more dead turtles, while the absolute increase shows that there really were more turtles on the beach.

Total loggerhead strandings (all States from Texas to Maine, all cases of death) have varied from year to year but increased slowly from 1986, when about 1,200 were reported, to a peak of nearly 2,500 individuals recorded in 2003, though there were some years in the early 1990s with only about 1,000. Since approximately 2004, there was an apparent decline through to at least 2007, though subsequent data may show that it was more of a leveling-off at about 1,750 per year. Reported strandings in Florida alone account for about half the total and have followed a similar trend to total strandings, though 2006 saw the highest number of reports on record and, in Florida, the dip to 2007 looks more like a minor inter-annual fluctuation than the beginning of a decline[64].

Within the total strandings, a proportion of the turtles are recorded as emaciated, which has been taken as an indication of the individual's death having resulted from a disease (e.g. Witherington *et al*. 2009). The number of stranded emaciated loggerhead recorded on Florida beaches was very low in 1986 but increased thereafter, reaching about 75 in 2001 (*Assessment*, Fig. 28, p. 83). The increase was roughly linear, with fluctuations generally tracking the total number of strandings – suggesting the incidence of weather events conducive to turtle carcasses coming ashore. The percentage of Florida loggerhead landings that were emaciated rose from very low levels in 1986 to nearly 11% in 2001 (*Assessment*, Fig. 28, p. 83). While other explanations are possible, those trends are consistent with an increasing population of neritic juveniles, resulting from assorted conservation measures, meaning more natural deaths from a generally-constant background rate, coupled to a reduced number of deaths caused by fishing gear, as TEDs were introduced to the shrimp fleets. After 2001, however, both the number of emaciated loggerheads stranded in Florida and the proportion of strandings that were emaciated spiked upwards. In 2003, they peaked at 170 and 16% respectively. Both measures then dropped back to close to the long-term trend lines, before spiking even higher in 2006 – reaching 200 and about 16.5% (*Assessment*, Fig. 28, p. 83). Such data cannot yield definitive answers but they do strongly suggest that epizootics struck the loggerhead in Florida waters in 2002–03 and again in 2006. The strandings are recorded by calendar year, not by the period from one nest census to the next, while weaknesses in the data mean that there may be some distortion of apparent timing between an epizootic and the

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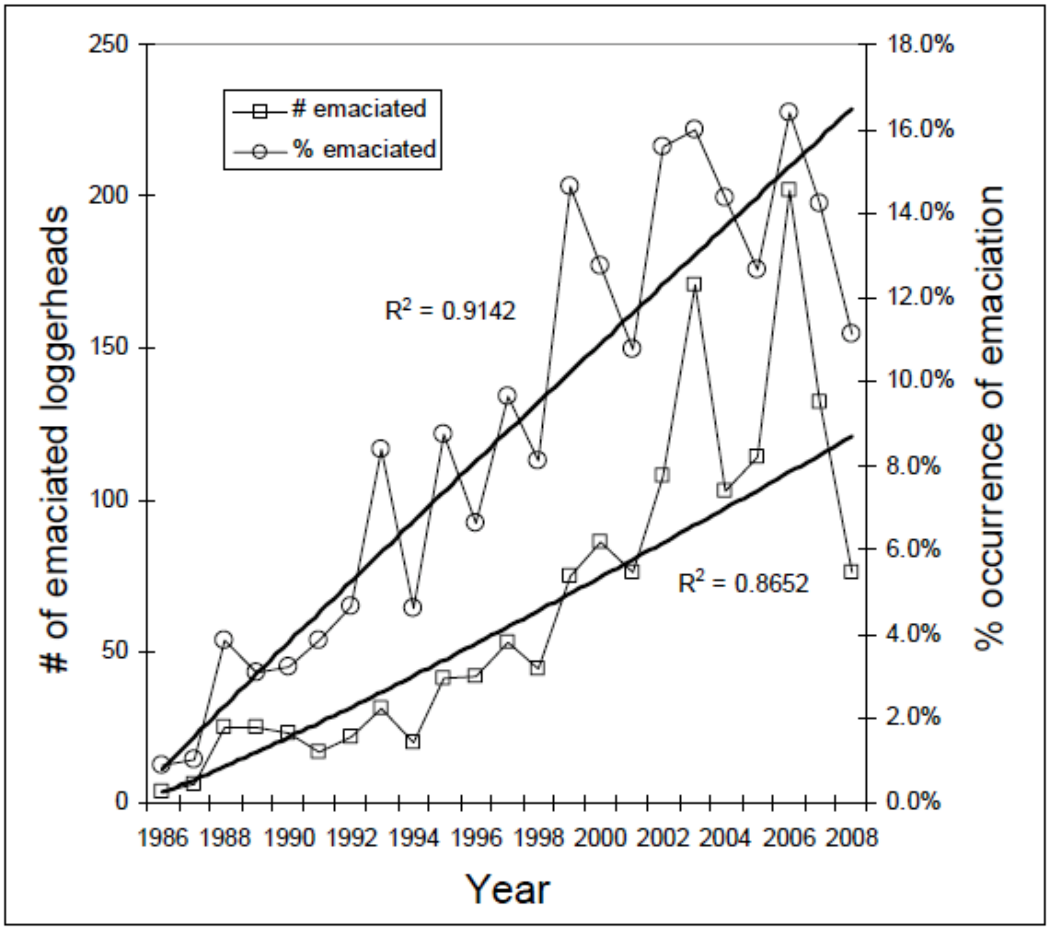
64 Data on total loggerhead strandings were downloaded from the STSSN website: http://www.sefsc.noaa.gov/seaturtleSTSSN.jsp

recording of emaciated strandings. The suggested epizootics do, however, match the apparent declines in nest numbers, the major one falling between 2001 and 2004, with a secondary outbreak reversing recovery from 2005 to 2007.



Time Series of Numbers of Emaciated Loggerhead Stranded on Florida Beaches and of Emaciated Strandings as a Percentage of All Loggerhead Strandings
[Diagram originally published as Figure 28 in TEWG (2009)]

If numbers of stranded emaciated loggerhead have been low, the numbers of them that are adult-sized (≥82 cm) have necessarily been even lower. In Florida, they were zero until 1989 but then rose swiftly to about a dozen annually from 1992 until 2001, though numbers declined slowly through those years. The proportion that those adults represented of all Florida loggerhead strandings followed a very similar trend to the raw numbers, with maximum values of around 8%. Both the number of emaciated adult strandings and the proportion of all strandings that they represented then spiked upwards, reaching a peak of 33 (about 13%) in 2003, before dropping back by 2005 and then peaking again (though slightly lower than before) in 2006 (*Assessment*, Fig. 29, p. 84). The early zeros look to be recording deficiencies at the beginning of the STSSN program, while the decade of near stasis argues for approximately steady adult numbers (slowly increasing if the nest counts be trusted) and equally steady (perhaps slowly declining) disease levels. The later data, however, strongly suggest that the apparent epizootics (dated as 2002–04 and 2006–07 by the adult strandings) struck the adults particularly hard. Numbers stranded nearly tripled between 2001 and 2003, whereas those of juveniles little more than doubled during the same years.

To summarize: The index-beach nest counts show a pattern of rapid decline which is at least strongly suggestive of a mass-mortality event affecting adult and older juvenile loggerhead during the period fall 2000 to spring 2004 and likely more specifically from fall 2001 onwards. (The data are consistent with a further decline between fall 2005 and spring 2007, though that could be normal inter-annual variability in the percentage of adult females migrating to the nesting beaches.) Meanwhile, the STSSN strandings data indicate a mass-mortality event caused by an epizootic during calendar 2002 to 2004, with a repeat event in calendar 2006–07, adult loggerhead having been especially affected. Finally, veterinary research by Jacobson *et al*. (2006) has generated a description of a particular, and novel, neurological disorder of loggerhead which first emerged amongst the strandings from fall 2000 to spring 2001 but persisted at least until 2004. They described an infestation of *Neospirorchis* in affected turtles but could not confirm that that was the cause of the disorder. Individually, those three independent sources of evidence are only indicative. Even in combination, they fall far short of proof. However, there can be few major mass-mortality events in marine species, exploited resource populations perhaps excepted, for which the causes are better documented.

I will not claim that the observed declines in loggerhead nesting were caused by a trematode infestation but the evidence for that is persuasive and I do claim that the conservation-management of loggerhead should proceed, cautiously of course, on that basis pending the emergence of contrary evidence.

The argument developed here has been founded on an assumption that the observed decline in loggerhead nesting was paralleled by one in adult female abundance. The available data can be fully explained without deviating from that assumption. Any epizootic will, however, spare some individuals while it kills others. A proportion of the survivors will be weakened by the disorder. The paresis described by Jacobson *et al.* (2006) would, in non-lethal cases, be debilitating. Meanwhile, remigration intervals in loggerhead are thought to be determined by the period necessary for a female to gather enough surplus energy to support vitellogenesis of a new batch of eggs (e.g. Chaloupka *et al*. 2008). A debilitating but non-fatal disorder would slow an individual's uptake of energy, thus delaying her return to the nesting beaches. It is thus possible that the 50% decline in nesting resulted from an epizootic which killed far less than one half of the adult female loggerhead but which also increased the remigration intervals of the survivors, substantially reducing the percentage of the reproductive adults which returned to shore in each year since 2001.

There is no way to test that hypothesis in the short term and I do not recommend that much reliance be placed upon it. However, the numbers of nests may show rapid recovery if surviving adults are able to overcome their health challenges and return to normal remigration intervals. Should some future index-beach survey show exceptionally high numbers of nests, the data ought to be viewed cautiously: A sudden spike in nesting might result from multiple females whose migrations had been delayed all returning to the beach together – their behavior effectively synchronized by the after-effects of the epizootic.

The remaining key question is whether the decline in the loggerhead has ended. The data from the index-beach surveys is equivocal. It does strongly suggest that the initial rapid decline ended some years ago but there is, as yet, no convincing sign of renewed recovery. Identification of the likely cause as an epizootic does suggest that the worst is over: Like human epidemics, epizootics can return for a second round but the first is usually the worst, in that it afflicts the host population when it has the least resistance, whereas subsequent rounds face a population already shorn of its most vulnerable members. The strandings data, however, point to a second event in 2006–07 and the possibility of a third, albeit likely minor, round in the future cannot be dismissed.

After that, the trematode (or whatever other causative agent may have driven the recent epizootics) can be expected to persist at low levels in the loggerhead population, perhaps slowing recovery below the rate that might have been achieved but likely having little other effect. Loggerhead, like every other wild species, will of course continue to be vulnerable to other epizootics in the future but there is no reason to suppose that it is any more troubled by them than other species are. There is no reason to suppose that severe mass-mortalities of loggerhead are anything but very rare events, one of which happened in the western North Atlantic during the earliest years of the 21st Century.

Juvenile Abundance

The future of the loggerhead population lies less with the existing adults than with the current juveniles – turtles which have enjoyed the benefits of a range of conservation measures on the beaches where they hatched and subsequently on their neritic feeding grounds. Perhaps more specifically, that future depends on what effects the apparent epizootic has had on those juveniles or will have as they recruit to the adult population. Unfortunately, very little is known of the abundance of juvenile loggerhead nor of trends in that abundance.

Their numbers should have increased over recent decades. There have been extensive efforts to protect nesting beaches, restrict beach nourishment, reduce barriers to turtle movement across the beaches, light pollution, nest predation, vehicle movement on beaches, and to eradicate exotic plants. In coastal and some oceanic waters, there have been major programs to reduce bycatches through technological means (e.g. TEDs, circle hooks) and by extensive seasonal or year-round closures of fishing grounds, while fishermen receive training in the safe release of captured turtles. Some gear types, notably gillnets, have simply been made illegal across wide areas – albeit not primarily as a turtle-protection measure. There have also been restrictions on channel dredging, military training and the offshore petroleum industry. Meanwhile, efforts to rehabilitate stranded turtles have been developed. Some initial steps have even been taken to reduce vessel strikes. (*Recovery Plan* pp. I-67 to I-79 outlines some of these measures, though it is notably weak in its coverage of others, such as fishery closures.) With so much conservation effort and such high costs to certain economic sectors, one would certainly hope that there has been a positive effect for the loggerhead. However, the net effect of all of those programs cannot be sure and might have been overwhelmed by a reversal of the positive trend in the turtles' environment which appears to have acted in the 1990s. Determining whether the increase in juvenile turtles which should have happened actually did needs evidence.

No really satisfactory method for in-water surveys of turtles has yet been developed (cf. NRC 2010) but, even was there a suitable technique, its comprehensive application would be prohibitively expensive. Nesting females necessarily come ashore, where they are relatively easy to count, and they also home to a relatively-restricted number of beaches. Some survey effort is needed to ensure that major new colonies are not emerging elsewhere and to account for occasional strays but counts of nests or crawls on a finite number of known beaches can provide a reliable index of loggerhead nesting and hence a good (though still imperfect) indicator of adult female abundance. In contrast, the in-water population ranges widely and there is no guarantee that its distribution is stable from year to year. Indeed, there is persuasive evidence of major inter-decadal change. It used to appear possible to disregard the younger, "oceanic" juveniles and consider (though not count) a discrete "neritic" stage that would at least be confined to the continental shelves. However, McClellan & Read (2010) have now shown conclusively what others had begun to suspect (cf. Watson *et al*. 2005; Brazner & McMillan 2008): A substantial fraction of larger, older juvenile loggerhead do not confine themselves to neritic waters but forage over deep ocean, eastward of the Grand Banks of Newfoundland. There is no reason to suppose that that fraction will remain stable over decades.

Unless it one day becomes possible, both technically and economically, to maintain routine turtle surveys throughout the western North Atlantic and its marginal seas, from the Mid-Atlantic Ridge to the coast of Mexico, all surveys of juvenile loggerhead are and will remain confined to limited portions of their range. Without some improbable confirmation that the distribution of the turtles within that range is stable, the observed trends in survey results will always be a confounding of changes in abundance and shifts in distribution, while we will have no means to disentangle the two.

The BRT barely mentioned in-water turtle surveys in its *Status Review* and offered no comment on the available data. The *Recovery Plan* (pp. I-13 to I-17), in contrast, recognized nine available time-series of in-water abundances. The Recovery Team saw two as showing increasing numbers of loggerhead, those being bycatches in pound nets in Pamlico Sound, NC and turtles taken from the cooling-water intakes of the St. Lucie nuclear power plant (Hutchinson island, FL). That Team was unable to perceive any trend in the loggerhead catches in trawl surveys off the southeastern U.S. (though comparison of survey catches to those of shrimp trawlers around 1980 indicated a major increase – possibly an effect of different gear and fishing practices)[65] or in data from a shipboard visual survey in Florida Bay. Pronounced declines were seen in pound net bycatches in Long Island Sound, NY and in observations during aerial surveys of Chesapeake Bay. Tangle-net surveys in Mosquito Lagoon and Indian River Lagoon, FL showed no trends in catch rate across recent decades but there had been a decline since early work, using similar but not identical methods, in the 1970s[66]. Epperly *et al*. (2007), besides providing the primary report on the Pamlico Sound bycatches, reviewed a few of the same datasets. They noted that the Indian River lagoon time series had shown a marked increase in its latter years, catch rates in 2002–05 being about double those of earlier years, even though (as the Recovery Team had found) there was no statistically-significant trend over the entire time series. Furthermore, where the Recovery Team in 2008 considered the South Carolina SEAMAP trawl surveys only from 1990 to 2000 and did not recognize any trend in loggerhead catches, Epperly *et al*. (2007) noted a 5% annual increase over 1990–2006. More recently, the TEWG has re-examined some of the same time series (*Assessment*, pp. 64–73). They effectively endorsed the readings of Epperly *et al*. (2007) but went further into the sizes of the turtles captured, which had increased over time. The TEWG saw increases in average sizes and concluded "there appears to be increasing trends in catch rates through 2008 in the southeast U.S. This, coupled with the shift in median size of neritic juveniles, may indicate there is a relatively large cohort that will be reaching maturity in the near future" – a conclusion the Group labeled "good news" (*Assessment* p. 69). The TEWG did trouble over an apparent decline in the smaller size-classes of neritic juveniles, which seemed to warn of recruitment failure. Closer inspection of the data (*Assessment* Fig. 24, pp. 71–73) suggests, however, that only the relative proportion of small juveniles fell, while their absolute numbers remained approximately stable. That is, increased survival of neritic juveniles (perhaps a consequence of the introduction of TEDs) has led to greater abundances of larger, older juveniles, reducing the proportion of the population represented by turtles newly returned from the ocean, even though the numbers returning have not changed. Those trends are fully consistent with the introduction of conservation measures over the past three decades, coupled with the lag times inherent to the life history of loggerhead.

If that news is good, however, the very recent data on juvenile abundances (*Assessment* Fig. 23, p. 67) is not. The TEWG only examined those data for long-term trends, finding

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[65] The Recovery Team considered data from two trawl-survey series (*Recovery Plan*, p. I-15). The SEAMAP surveys have been on-going and data from them through to 2007 were examined by the TEWG (*Assessment* p. 67). A shorter survey series extended only from 2000 to 2003. That work resumed in 2008, when a non-significant increasing trend in loggerhead was detected. The increases in 65 cm to 85 cm (Straight Carapace Length, "SCL") loggerhead were statistically significant, as was a decline in the very few 45–55 cm animals (Arendt *et al*. 2009). While interesting, that data series is too short to be taken very seriously.

[66] The authors of the *Federal Register* notice (p. 12614) transposed those conflicting results, suggesting a declining trend in the recent data but no trend over the long term. That was erroneous and misleadingly appeared to negate the critical point that the decline may have resulted from alterations in the survey protocols between the early work and the current survey series.

increases. The swift decline of nesting after 2000, however, demands attention to shorter-period changes. Sadly, the catch from the cooling-water intakes of the St. Lucie power plant peaked in 2004 and had declined by more than half by 2007. The tangle-net survey of Indian River Lagoon likewise saw a peak catch rate in 2004, before dropping by around three-quarters to 2007. Recent SEAMAP and Pamlico Sound data are more equivocal but not inconsistent with such declines. It is not impossible that the second round of the epizootic, suggested for 2006–07 by the strandings of emaciated loggerhead, hit the juvenile population along the east coast of Florida, which seems from inspection of the time-series of juvenile abundance to have been spared the first round.

The TEWG also drew a tentative distinction between the increasing trend which they saw in multiple data sets from south of Cape Hatteras and a drop in numbers to the northward that was illustrated by the sharp declines in Long Island Sound pound-net catches and sightings by aerial surveys of Chesapeake Bay. While that distinction was consistent with the datasets which the Group examined, none of the recent reviews of in-water loggerhead abundances have used all of the indicators available. For over 40 years, NMFS ran standardized trawl surveys of northeastern waters, using the research vessel *Albatross IV*. No formal report on turtle catches in those surveys seems ever to have been produced but it is known that some loggerhead were taken and there have been suggestions that the data indicate stability or a slightly increasing trend through to the recent retirement of the ship. From still further to the north and east, NMFS has data from bycatches in the distant-water swordfish longline fishery. Those too have never been formally analyzed to produce a trend in an indicator of loggerhead abundance and (in contrast to the trawl surveys) changes in the fishery over time may preclude such an analysis. It is known that experimental results in 2002–03 suggested that using circle hooks would reduce turtle take rates by 75%. In the event, loggerhead takes in 2004–08, when the use of circle hooks was mandatory, averaged only 18% lower than they had in 1998–2000[67]. That is consistent with the effect of the hooks being overwhelmed by a tripling in the abundance of oceanic juveniles and adults along the edge of the Gulf Stream, south and east of Newfoundland (equivalent to a steady annual increase of about 17% a year) but it cannot be said to be proof of such an increase. The authors of the study could only say that the lack of deeper declines in takes was “due to other factors not measured”. Loggerhead bycatch per unit effort in the Canadian pelagic longline fisheries showed a similar increase, being significantly higher in 2005–06 than in 1999–2000 or 2004 (Brazner & McMillan (2008). There are other fisheries with observer data which might be used to develop alternative indications of trends in loggerhead abundance, though such data sets are hard to interpret, in part because any data showing sufficient takes for usefully-precise statistical analysis usually lead to conservation measures which reduce the number of takes relative to turtle abundance. Meanwhile, fishermen’s

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[67] S.P. Epperly, L.P. Garrison & L.W. Stokes (2009) *Sea Turtle Bycatch Reduction in the U.S. Atlantic Pelagic Longline Fishery – preliminary analysis*. PowerPoint presentation to NMFS Advisory Board, February 17, 2009 and to the 29th Annual Symposium on Sea Turtle Biology and Conservation, February 18, 2009, Brisbane, Australia, 27p.
Available at http://www.sefsc.noaa.gov/PDFdocs/AP_Epperly_etal_pre_post_regulation_ISTS_2009.pdf

impressions of changing frequencies of turtle encounters, while both honestly presented and valuable as indicators, cannot be reliably expressed in the quantitative terms needed when examining temporal trends. There are, however, occasional cases of near-qualitative change, with retired fishermen reporting that they almost never saw a turtle while captains with recent experience report seeing many. One such example is the sea-scallop fishery of the Mid-Atlantic Bight, which unexpectedly began taking substantial numbers of loggerhead around the turn of the century[68] where essentially none had been seen before. That scatter of observations cannot amount to evidence for an increasing trend in in-water loggerhead abundance but it does cast doubt on the TEWG's conclusion of a decline in waters north of Cape Hatteras. More importantly, it serves as a powerful reminder that none of these datasets measure loggerhead abundance alone. They also respond to the distribution of the animals and it is fully possible to see severe declines in numbers inside such areas as the Chesapeake and Long Island Sound simultaneous with swift increases a few tens of miles offshore – particularly if the loggerhead have reason to shift their foraging from declining inshore oyster beds to newly-abundant offshore scallops.

In summary, the TEWG saw grounds for guarded optimism in the medium term, as abundant juvenile loggerhead south of Cape Hatteras reach sexual maturity, though the Group expected decline in later years resulting from what they saw as lower numbers of young juveniles. I am unconvinced of the latter but troubled by the possible recent losses of older juveniles to the putative epizootic, with that mortality perhaps being on-going as young loggerhead not previously exposed to the pathogen reach ontogenetic stages in which it cannot be avoided. Given the gross uncertainty surrounding any attempt to extrapolate from an in-water index to the abundance of juvenile loggerhead, it may be best to reserve judgment on likely future trends in recruitment to the adult population.

Conclusions

Loggerhead nesting on the beaches of the western North Atlantic and its marginal seas has dropped precipitously in the past decade, the numbers of nests being cut by about half. Conservation-management should proceed on the assumption that there was a corresponding loss of some 25,000 adult females, beginning after the 2000 nesting season and perhaps in the fall of 2001, though it remains possible that many of them are still alive but have had to extend their remigration intervals. The cause of the decline cannot be confirmed but it appears to have been an epizootic, perhaps associated with infestations of the trematode parasite *Neospirorchis*. The initial epizootic seems to have run its course by 2004 but there is evidence of a subsequent event, with less impact on adults but perhaps more on juveniles, around 2006. Further losses of adults, over and above normal mortality rates, may occur but are not expected – unless and until some unrelated calamity befalls the loggerhead. The western North Atlantic population is left

[68] Endangered Species Act Section 7 Consultation on the Atlantic Sea Scallop Fishery Management Plan [Consultation No. *FINER/2007/00973*], March 2008.

with tens of thousands of breeding females and millions of individuals (even if the eggs and hatchlings are excluded from that count).

Through the 1990s, nesting was increasing – at the slow rate which is all that can be expected of a long-lived species but nevertheless showing steady progress. Conservation measures protecting adults on the beach will have contributed to that trend, as will the initial effects of the use of TEDs. However, the primary driver of the increase may have been an improvement in environmental conditions on the foraging grounds used by adults and older juveniles.

There are grounds for optimism that, with the worst of the epizootic now over, rebuilding of the adult population will resume and will be more rapid than it was in the 1990s as the recruiting year-classes will have enjoyed the benefits of enhanced conservation measures – perhaps most notably through reduced exposure to shrimp trawls lacking TEDs. There is even a chance of a very rapid recovery if surviving adults that were debilitated by the trematode are able to purge the parasite and recover their normal remigration intervals. The optimism should be tempered, however, since the state of the foraging environment may have declined over the past decade, while there is a risk that the cohorts of maturing juveniles will be decimated by renewals of the epizootic as they become exposed to the pathogen. There is also the small but ever-present risk that some other disaster will strike.

While recovery of the loggerhead of the western North Atlantic cannot be guaranteed, with the sharp drop in nesting ended there is no reason to anticipate any on-going decline. Meanwhile, the abundance of the population is high enough to provide a buffer against short-term losses. Thus, there is no prospect of imminent extinction and no justification for an "endangered" listing under the ESA.

Seminoff & Shanker (2008) recently declared: "we see no plausible scenario by which anthropogenic impacts, either direct or indirect, could wipe out an entire [marine turtle] species within the foreseeable future". That was a considered judgment, rather than a conclusion based on any explicit analysis, but the examination of the available empirical data presented above confirms their point, at least as regards loggerhead and specifically the western North Atlantic population.

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