70508

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Anne Secord
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Dear Anne:

Thanks for sending me a copy of GE's Evaluation of the Hudson River Tree Swallow study. I've made a few comments on the following pages that point out some of the more egregious misrepresentations they include. The biggest difficulty in some cases was in deciding if the statements they made were intentionally misleading or simply errors in basic biology. The list that follows is by no means an exhaustive summary of the problems in GE's document but should give you a flavor of the overall quality of the work.

Best wishes,

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Note that my work address will change as of 1 July 2001 to:

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Comments from John McCarty (regular text) and Anne Secord (italics)on General Electric Company's Evaluation of U.S. Fish and Wildlife Service Tree Swallow Studies along the Hudson River.

General Electric's evaluation starts off in the first paragraph of the executive summary with an attempt to summarize the conclusions of the Hudson River Tree Swallow project. This attempt is both incomplete and inaccurate. The report states that we:

> "concluded that PCB exposure negatively affects reproductive success for tree swallows breeding on the Hudson River. They also concluded that PCB exposure causes premature development of plumage in subadult females. Lastly, these researchers indicated that behavioral effects in the parents result in inferior quality nests, which in turn, may affect reproductive success (Secord and McCarty 1997; McCarty and Secord 1999b)." [page vii]

The most important mistake Exponent makes in this section is in stating that the Hudson River Project concluded that PCBs *caused* the patterns we reported. One of the first lessons scientists are taught is the importance of using terms such as "cause" carefully and precisely. Establishing a cause-effect relationship is an important goal of many research projects but conclusions about such relationships need to be supported by the correct type of evidence. The papers we published in the peer reviewed literature explicitly state that we have not established a causal link between PCBs and any of the anomalies we describe in Hudson River tree swallows. Indeed, we clearly state that the design and goals of our study: "makes it impossible for us to unequivocally assign PCBs a causal role in any of the patterns observed here." (McCarty and Secord 1999b, page 1438). The fundamental error represented by General Electric's misuse of the idea of causality and their misrepresentation of our conclusions in this opening summary pervades much of the evaluation.

Study Design Critique

The critique of our reference site at Lock 9 in 1994 does a fair job of summarizing the shortcomings of that site. This is apparently taken directly from our publications but for some reason General Electric fails to point out that we did not use reproductive success data from that site in our peer reviewed publications.

GE criticizes our use of the Champlain Site because it differed in characteristics from other sites. We acknowledged the limitations of the site and as John McCarty indicated, we did not use reproductive data collected from the Champlain Site in 1994. I want to emphasize that it is impossible in a field investigation to select sample sites with identical conditions of e.g., topography, slope, soils, vegetation, and size. Any reputable field scientist would understand this fact. GE mentions that eggs at the Champlain sites had higher than expected concentrations of PCBs. An important point to make is that analysis of daily PCB accumulation rates by nestling tree swallows showed that nestlings from the Champlain Sites were receiving very low concentrations of PCBs in their diet (indicating low PCB concentrations in nearby habitat), supporting a hypothesis that adults nesting at these sites arrived with high PCB body burdens that were transferred to eggs. These high body burdens in adults may have been accumulated during migration up the Hudson River.

The critique of the Ithaca site is also puzzling. The General Electric report complains that "very limited information" about this site is available. This is an interesting complaint since the site is described in my Dissertation (McCarty 1995: which is cited by GE) and in numerous papers published in the scientific literature. The authors at Exponent apparently also missed the fact that we reported levels of PCBs in Ithaca tree swallow eggs (103ng/g) and nestlings (6ng/g) in McCarty and Secord 1999b (page 1434).

General Electric's criticism of our comparisons between Tree Swallows from Ithaca and the Hudson River can not be reconciled with their subsequent use of data on tree swallows collected at sites as distant as British Columbia and as long ago as 1932 (GE's Appendix A).

GE comments several times about concentration-response relationships expected as though all biological changes would respond in a linear or at least consistently predictable manner to an outside factor. To our knowledge, no one has characterized the dose response relationship for PCBs vs. reproductive success in tree swallows. We do not know the shape of any dose-response curve, nor where on any hypothetical curve our study population may lie. See also Dr. McCarty's discussion of Type II errors (page 5).

Effects on Reproduction

The General Electric report compiled published and unpublished data on tree swallow reproduction from across the species range. Unfortunately, there is insufficient information presented in this report to thoroughly critiques GE's analysis. For example, Appendix A does not provide complete references for the majority of studies it cites so it is not always clear what the source of these data are. In addition, the biological basis for the criteria used to select studies is not clear (e.g. what is the biological basis for including a study site located 1.9 km from a wetland but excluding one 2.1 km from the ocean?). It is also unclear how the authors determined that study sites were "not located on an industrially polluted waterway." Some would consider Lake Erie polluted while many of the northern lakes (such as around Sudbury) have elevated mercury levels. These cases are of course not as severe as the Hudson River but where is the cutoff?

A careful review of their comparative studies used in Appendix A shows that 90% of those studies violated or may have violated their own screening criteria. They used data collected adjacent to sewage lagoons, in spite of their criterion that they did not consider studies conducted adjacent to polluted waterways. They also used data from studies where it was unknown if the birds were nesting adjacent to water bodies even though a criterion was that the site was located within 2 km of a water body.

The approach used by General Electric in this section seems to be based on comparing the mean values of reproductive parameters from Hudson River sites to the distribution of mean values from a variety of years and populations studied elsewhere. There may

be a few problems with the analysis itself. For example, it appears that in some cases General Electric included means for different years from the same study site (e.g. 17 yearly means for Chapman's study at Princeton). These observations may not be independent and thus probably should not all be included in analyses as independent observations. Unfortunately they do not present sufficient information to thoroughly evaluate their analysis.

What does appear clear is that General Electric is not aware that their analysis is asking a subtly different question than that asked by the Hudson River project. Their approach of looking at the distributions of means of reproductive parameters for populations is addressing a question about how population averages vary. In contrast, where McCarty and Secord use reproductive success data from other populations (e.g. McCarty and Secord 1999b Table 2) we have focused on the underlying distribution of variation among individuals. While GE's approach may be valid for the question it is actually asking, it will not be able to evaluate the variability among individuals that seems to drive some of the patterns seen in Hudson River birds.

It is clear that this section of General Electric's evaluation focuses primarily on mean "clutch size, number of chicks hatched per nest and number of chicks fledged per nest" [page 8]. Since our published reports have already noted that these variables are similar for Hudson River birds and tree swallows breeding elsewhere (e.g. McCarty and Secord 1999b page 1435) it is hardly surprising that the analysis conducted by GE failed to find significant differences between the Hudson and other populations.

The failure to find a relationship between PCB concentration and reproductive success for the Hudson River sites is hardly surprising. The General Electric report concludes that this "clearly illustrates the lack of an adverse or negative relationship between PCB exposure an reproduction." This conclusion is not supported by the data. One of the other lessons all beginning scientists are taught is that "lack of evidence for a relationship is not the same as evidence of no relationship." Simply put, the failure of General Electric to find a simple relationship between PCB exposure and hatching or fledging success is not evidence that no such relationship between PCBs and reproduction exists. Rather, it merely means that there is no evidence from the currently available sample to support such a relationship. This conclusion is further weakened by the small sample size and low statistical power available for an analysis. Mainstream ecologists have become increasingly aware of "type II" errors in making these comparisons (that is failure to detect a relationship that in fact exists). A variety of measures of statistical power are widely available and could be applied here.

We also specifically noted in our publication (McCarty and Secord 1999b) that there was a high incidence of unexplained nest abandonment and egg burial among Hudson River tree swallows studied. This is an important point given the pattern established in the literature between chlorinated hydrocarbons and aberrant reproductive behavior in a variety of bird species. That this aberrant behavior did not always affect overall reproductive success was noted in our publication.

GE refers to data from the Saratoga Inland Site (page 13), data that were not used in any of our publications.

Effects on Plumage Development

The evaluation of the patterns of plumage color in female tree swallows focuses solely on the pilot data from 1994 presented in Secord and McCarty 1997. Since GE's analysis of the 1994 data is no longer relevant, I won't critique that part of their report. General Electric does not present any criticism of the analysis of the full data set presented in the peer reviewed article (McCarty and Secord 2000) that supercedes the discussion of female plumage in Secord and McCarty 1997, and I assume they agree that the our analysis does a satisfactory job of demonstrating that the pattern of plumage color in female tree swallows from the Hudson does differ from that seen in the rest of the species' range.

Some of the general criticisms of the plumage color work presented by General Electric are addressed in McCarty and Secord 2000 so I won't repeat them here. For example, we note that age of the females is not known, but that this is fact is not relevant to the conclusions we draw (page 990). General Electric does focus extensively on the question of female age but doesn't seem to understand that whether second year Hudson River females have advanced plumage or whether older females have retarded plumage development is a secondary question. The suggestion General Electric that we should have used "skull pneumatization [sic]" to measure female age is misleading. Skull pneumaticization is a useful technique for determining the ages of passerines at certain times of year. However, careful reading of the standard reference book by Pyle (Pyle 1997 which updates and corrects the Pyle et al. 1987 cited by GE) reveals that this would not, in fact, lead to reliable aging of females in the breeding season. This mistake on GE's part is understandable since the information in Pyle is sometimes difficult for the non-ornithologist to understand. However, Pyle 1997 does clearly state that skull pneumaticization in tree swallows may be complete in individuals as young as 6 months or may not be complete even into the third year.

The amount of effort General Electric put into the discussion of Control of Plumage Development suggests that they accept that the pattern of plumage color is abnormal in Hudson River females but that they question the mechanism behind this change. We are explicit in discussion of these patterns (McCarty and Secord 2000) that the mechanism behind plumage development in Tree Swallows has not been studied but based on studies of other species is almost involves the endocrine system:

> "The hormonal basis for subadult plumage in Tree Swallows has not been studied, but sex-specific differences in plumage generally are under hormonal control" (page 993)

That said, the discussion of the mechanisms guiding plumage development in birds presented by General Electric has some basic flaws that I am compelled to discuss.

First, the focus of General Electric on estrogen misses the basic fact that the endocrine system produces numerous biologically active compounds and chemicals such as PCBs

that interfere with the normal functioning of the endocrine system. Thus, any number of hormonal pathways could be abnormal in the Hudson River birds.

Second, General Electric makes much of the categories of plumage dimorphism presented in Owens and Short 1995. I'm afraid the authors at Exponent may have misunderstood some of the material in Owens and Short. While Owen and Short do review some of the evidence for mechanisms that produce plumage dimorphism in birds, their attempt to categorize different species is directed primarily at understanding differences between male and female plumage, not delayed plumage maturation, and certainly not the unusual case of delayed female plumage maturation seen in Tree Swallows. General Electric states that: "it has already been established that tree swallows fit in [Owens and Short's] category #2" [page 29] but this conclusion is simply not defensible. Still, the lack of empirical evidence does not prevent GE from concluding that plumage color in tree swallows is not in any way influenced by hormones but is the result of the "difference in chromosomal balance between the homogametic male and the heterogametic female, rather than hormone production" [page 29].

General Electric's conclusion that the subadult plumage of female tree swallows is under purely genetic control (without, I assume, the endocrine system acting as the mechanistic link between DNA and plumage color) raises some interesting questions. To be generous I will assume that GE is proposing that there are sets of genes on the female sex chromosomes that are under control of a clock that causes them to produce brown plumage the first time a female molts and blue green plumage during subsequent molts. While this mechanism is not outrageous, it does raise the question of why such a high percentage of Hudson River females have plumage intermediate between the two types. Perhaps General Electric is suggesting that something special in the Hudson River valley is producing a high incidence of a very specific mutation in the genes for female plumage color?

General Electric also raises doubts about our assumption "that tree swallows breeding along the Hudson River are contaminated with PCBs" [page 30]. While it is true that we do not have data on PCB body burdens for the females used in the plumage study, it is reasonable to assume based on our other published work that these individuals are exposed to PCBs along the Hudson.
I would re-emphasize John McCarty's point that throughout most of their discussion of plumage development, GE failed to consider the paper published on this subject (McCarty and Secord 2000), although they appear to be aware of it since they reference it once in the 8 pages of comments on this subject. Without consideration of this paper and the data within it, their arguments are incomplete. They also offer a convoluted conclusion that implies that if plumage does not affect reproductive success, then PCBs are not likely to be responsible for advanced plumage development. These are two separate phenomena. I reiterate John McCarty's point about GE's misuse of the term "estrogenic", as though all endocrine disruptions are due to alteration of this hormone alone. This demonstrates a poor understanding of endocrinology and avian physiology.

Effects on Parental Behavior

General Electric's discussion of the Tree Swallow project's paper on nest quality (McCarty and Secord 199a) seems to accept that behavior of adult swallows on the Hudson is abnormal and only quibbles with whether measuring behavior via one endpoint (amount of nest material) or another (time-activity budgets) is more appropriate (page 36). Most of General Electric's critique focuses on implications of the behavioral differences for reproductive success (e.g. whether the abnormal nests built by Hudson River Tree Swallows are actually of lower "quality"). This critique contains a number of small mistakes that in sum impact the overall quality of their analysis.

Page 31 Paragraph 2: General Electric seems to suggest that there is not a direct relationship between nest volume and nest mass. This would be of more concern if the materials used to construct nests varied widely in density (i.e. across orders of magnitude), but since grass is used as the primary material for almost all tree swallow nests it is misleading to suggest that Lombardo's results are not relevant to the Hudson River study.

Page 32, paragraph 3: Winkler 1993 was not conducted at the same site as used in McCarty and Secord 1999 but at a larger site approximately two km away. David Winkler did provide the nest quality data for the Ithaca site used in both McCarty and Secord 1999a and 1999b.

Page 33 bottom to top of page 34: General Electric criticizes Winkler and Lombardo et al. for using an experimental approach. I suggest that the vast majority of mainstream ecologists and ornithologists would advocate using experiments to determine what the effect of feathers on reproductive success actually are. In the following paragraph, General Electric states that "it is reasonable to expect that the experimental adults would not be able to feed their nestlings as much as control birds if they are spending time searching for replacement feathers." This idea is actually presented by Winkler in his paper (though GE fails to acknowledge that). In addition, GE fails to report that Winkler also shows data that indicate that adults do not search for feathers when they have nestlings to feed.

Page 35 paragraph 1 and page 36 paragraph 2: General Electric notes that we did not quantify feathers at our sites. GE suggests that the mechanism behind the lower numbers of feathers in the nests of Tree Swallows at more contaminated sites may be that fewer feathers are available at highly contaminated sites. General Electric's hypothesis that high levels of PCBs in the environment near their plants may result in lower numbers of birds and hence fewer feathers available to Tree Swallows is plausible, especially since many species of birds are much more sensitive to PCBs than swallows and may not be able to survive in the conditions created along the upper Hudson. However, we believe that GE is misrepresenting the strength of the data to be found in Austin and Low's 1932 paper and in Schaeffer 1971 paper and think it is more likely that the lower number of feathers in Tree Swallow nests at contaminated sites are a function of altered behavior.

page 35 paragraph 2: General Electric implies that thermoregulation is only important during the nestling phase. This is not true. Feathers may provide important insulation both to the incubating female and to eggs left unattended earlier in the breeding cycle.

PCB Bioaccumulation in Hudson River Tree Swallows
General Electric notes that PCBs accumulated in nestlings even at sites that are not adjacent to commonly recognized deposition zones. Insects emerging from the Hudson at these sites also had high levels of PCBs. General Electric's observation emphasizes that PCBs released from their plants are at high concentrations over a much wider area than is generally discussed. This is especially evident at the remnant site where adult insects were collected after they emerged from the river but prior to their first flights.

We routinely observed tree swallows foraging in large numbers over the Hudson River adjacent to the Remnant 4 Site. From a food habit study, we know that 98% of the insects eaten by tree swallows from this site were of aquatic origin (Secord and McCarty 1997), and the Hudson River was the only aquatic habitat within 2 km of the study colony. We can therefore conclude with a high degree of certainty that the Hudson River is a significant source of food for our Remnant 4 Site tree swallows. It seems reasonable and likely that sediments in the vicinity of the Remnant 4 Site are highly contaminated with PCBs, either from the significant upstream sources or from leakage at the Remnant 4 Site itself.

GE maintains that it is speculative to extrapolate measured TEQ concentrations in swallows to other bird species. It is scientifically appropriate and defensible to use existing data in the literature to estimate PCB uptake by species that are not directly studied. For example, a study was conducted at Green Bay to evaluate PCB uptake by closely located tree swallows, red-winged blackbirds, common terns, and Forster's terns (Ankley et al. 1993). Although the PCB congener patterns differ between the Hudson River and Green Bay, we are confident that we can use PCB uptake relationships demonstrated in this study to estimate PCB uptake by fish eating birds along the Hudson River when data on tree swallow PCB accumulation is known. As with all modeling efforts, there is acknowledged uncertainty associated with these estimates.

It is not surprising that other studies have failed to show a relationship between PCB concentrations and biological effects in tree swallows since the Hudson River tree swallows are more highly contaminated than those from most other studies.

We did, in fact, detect some deformities that have not been reported in the literature. These include one cross-billed adult female in 1995, and in 1998, two nestlings with deformed legs, one nestling with small eyes, and one nestling with a crossed bill. We also detected obvious abdominal edema in two nestlings in 1998 and there may have been more subtle manifestations of edema in other nestlings in all years of our study.

<u>Conclusion</u>
General Electric's final conclusion that " this report refute[s] the three hypotheses s

proposed by McCarty and Secord in their publications" [page 47] is simply not defensible, based simply on the fact that this publication does not demonstrate an accurate understanding of our papers or conclusions. The failure to accurately describe our conclusions by itself is sufficient to question the reliability of this report. More compelling is the failure of the report to address, much less refute, the fundamental conclusions of our publications.

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