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The grebe–flamingo connection: A rebuttal.—The flowering of avian orders after the massive extinctions at the Cretaceous–Tertiary Boundary is one of the most difficult, yet critical, subjects to recreate in the evolution of the vertebrates. Ideally, one would begin by studying fossil birds from early Paleocene deposits. So far, there is insufficient fossil material to accomplish this, but going back from the present evidence is fraught with traps and stumbling blocks.

Information on phylogeny is obtainable from at least two present sources: the whole-animal biology of the organisms and molecular biology. The most accurate phylogenies will result from those sets of data in which there is the closest agreement. Especially desired is information that can help explain the stages through which organisms have passed to reach their current condition.

To a large extent, however, cladistic analyses of phylogeny overlook biology and paleontology and focus, instead, on the analysis of large numbers of characters, which are employed without regard to possible convergences. The presumption, evidently, is that if enough characters are used, any complications resulting from convergent evolution will be swamped out and thus not be significant. However, because the fewer convergent characters that are included, the more accurate the analysis will be, it follows that characters suspected of convergence should be omitted. The results of pruning may be surprising and may show why some phylogenies are far off the mark.

A recent cladistic analysis proposing a sister-group relationship between the grebes (Podicipedidae) and the flamingos (Phoenicopteridae) (Mayr 2004) exemplifies this problem. Mayr used characters described in two of my papers (Storer 1982, 2000). In the first (Storer 1982), I pointed out that in both groups (and also the tinamous, gallinaceous birds, pigeons, sandgrouse, ibises, spoonbills, some cormorants, some falcons, and four of the nine suborders of the complex order Gruiformes), variable numbers of the thoracic vertebrae are fused into a notarium. According to my understanding, this structure has arisen independently in at least 10 phylogenetic lines of birds, presumably to strengthen sections of the vertebral column. In falcons, a notarium might prevent damage from the hard jolt in striking prey on the ground. In heavy-bodied but poorly maneuverable flyers (e.g., tinamous and Galliformes—an accepted convergence—and grebes), a stronger vertebral column could be advantageous in hard landings. In flamingos, it could mitigate the

tendency of the downward pull of their long legs and neck to stretch gaps between the vertebrae during flight. Whatever the selective advantage might be, the presence of a notarium is not evidence for a relationship between flamingos and grebes.

The structure of the legs and feet and how these elements are moved are integral to understanding how locomotion evolved in grebes and flamingos. In most birds (and tetrapods), the joints between the phalanges and between these and the tarsometatarsus are hinge-like; those of the grebes are unique among living birds in having the beginning of a ball-and-flange type of joint, which provides rotation between these joints. This type of joint was carried to an extreme in *Hesperornis* (Marsh 1880), which provides another obvious case of convergence.

J. Fjeldså (pers. comm.) and I (Storer 2002) independently concluded that the foot structure of the proto-grebe arose as an adaptation for moving through dense stands of upright, hard-stemmed vegetation by rotating the toes 90° so that the side of the foot passed through the vegetation first. The unwebbed toes were then rotated back to their original position and separated and placed on the ground, permitting some of the reeds to pass between them. When the grebes moved to an aquatic habitat, the toes became lobed and the movements of the feet used in passing through vegetation became adapted for foot-propelled swimming. If so, the evolution of grebes' feet from those of other swimming birds would have been impossible. With regard to flamingos, Mayr (2004) proposed ancestry from a group of swimming birds. However, if the above explanation of the evolution of grebes' feet is valid, grebes and flamingos could not have come from a common swimming ancestor. If, on the other hand, flamingos were derived from shorebirds, as paleontological evidence indicates (Feduccia 1999), their webbed feet (as in some shorebirds; e.g., avocets) could have evolved primarily as support for foraging on a soft substrate.

The one character used by Mayr that is unique to both the grebes and the flamingos is the presence of a chalky covering of the eggs. Its function is unknown. Both groups build their nests in water—the grebes on floating vegetation, the flamingos on mud in shallow water—and in some cases, the eggs are subject to immersion and possible clogging of pores in the shell, which would suffocate the embryo. Other birds using similar habitats (e.g., jacanas) lack this shell structure. Its origin and primitiveness, then, are unresolved;

but if convergence is involved, the character has no use in phylogenetic studies. An analysis omitting this structure and the notarium would, *ipso facto*, diminish support for a sister-group relationship between the flamingos and grebes.

Data from parasites are also relevant. Usually, a parasite is acquired by a new definitive host when the latter ingests an intermediate host of the parasite and the parasite becomes able to reproduce in the new host. Mayr's (2004) notation that all the known definitive hosts of the cestode family Amabiliidae are grebes, with the exception of a single species (in a very distinct subfamily) in flamingos, cannot be used as evidence of a phylogenetic relationship. The intermediate host (or hosts) for the amabiliid species in flamingos is unknown, but of 13 intermediate hosts in grebes, 11 are dragonfly or damselfly nymphs (Odonata) and the other two are a mayfly nymph (Ephemeroptera) and a water boatman (Corixidae, Hemiptera); all of these insects are aquatic. In a phylogenetic study, the parasite character can be used only after crossing-over is eliminated as the means by which members of the same family of parasites came to parasitize members of both definitive host groups. Van Tuinen et al. (2001) were also in error in using this character as evidence of a phylogenetic relationship. In this case, the odds favor the likelihood that any crossover would have occurred after both grebes and flamingos became aquatic.

Several figures in Mayr's (2004) paper require discussion. In figure 2, character 46, the ratio of length to width of the basal phalanx of "major digit of the wing" is a proportion. Because its boundary can be set at will, the boundary should be justified. In figure 3, the tubercle on the cnemial crest of the grebe is not clearly depicted, and the structure connected to the tubercles in the three species shown should be identified to confirm that it is the same in all groups. In character 57, the crests on the hypotarsus are present in both grebes and loons as well as flamingos. This suggests that it is advantageous in diving birds and therefore convergent. Apparently, Mayr was unaware that the hypotarsal canal for *M. flexor perforatus digiti II* is present in the primitive genera of grebes but not in *Podiceps* and *Aechmophorus*. This figure should have illustrated the primitive rather than derived condition, which is irrelevant in this context. There are other inaccurate representations of skeletal characters that lead the reader to suppose that skeletons of grebes and flamingos are more similar than is the case. In grebes, for example, the fourth toe is the longest; but in figure 4, the fourth is shoved between toes two and three, making the proportions appear similar to those of flamingos and most other birds.

There are two basic problems with Mayr's (2004) paper. The first involves the author's failure to use the mass of basic natural-history information relevant to

the subject. He failed to appreciate that crossing-over is a common way for a parasite to move from one definitive host to another. Similarly, the inability to explain how the structure of the feet could change from webbed in flamingos to the far more complex structure of grebes' feet, or vice versa, indicates that any such connection must have occurred before either line took to the water. These points weaken his argument for any sister-group relationship between grebes and flamingos. The second problem is his not considering whether convergence might be involved in some characters, such as the notarium. Convergence is important, and not screening for possible examples before making an analysis is like failing to remove the rotten apples before filling a barrel for storage.—ROBERT W. STORER, *Museum of Zoology, University of Michigan, Ann Arbor, Michigan 48109, USA*. [Editor's Note.—R. W. Storer was unable to complete the revision of the manuscript of this letter. The revision was prepared by Joseph R. Jehl, Jr. and edited by Helen F. James.]

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The public perception of science and reported confirmation of the Ivory-billed Woodpecker in Arkansas.—Fitzpatrick et al. (2006a) took issue with my “Perspectives in Ornithology” article in *The Auk* (Jackson 2006a) on events related to the reported confirmation of at least one Ivory-billed Woodpecker (*Campephilus principalis*; hereafter “ivory-bill”) in the Big Woods of eastern Arkansas. What they presented is *ad hominem*, focused more on the messenger than on the message. The message was that the evidence given by Fitzpatrick et al. (2005a, b) presents an interesting hypothesis, but no confirmation of the existence of a living ivory-bill. This conclusion was reached by independent scientists within weeks of the announcement of the reported discovery (Nemésio and Rodrigues 2005; an unpublished manuscript by R. O. Prum, M. B. Robbins, B. W. Benz, and myself, which was shared with Fitzpatrick and his colleagues, was tentatively accepted by *PLoS Biology* following peer review but was withdrawn by the authors). In the past year, these authors have been echoed by others (e.g., Sibley et al. 2006) with tenable alternative hypotheses. The burden of proof for the existence of ivory-bills in Arkansas is with those who claim confirmation. Nothing in Fitzpatrick et al. (2006a, b) strengthens the argument that the woodpecker exists. The answer to this dilemma is not found in dissection of minor details, but with unequivocal evidence of a living ivory-bill.

In summer 2005, when news media were filled with stories of the reported rediscovery, I was invited by Spencer Sealy, Editor of *The Auk*, to write a “perspective” piece on the unprecedented events. A perspective is much like an editorial. I was asked to write it because of my more than 40 years of work focusing on the behavioral ecology of woodpeckers, and more than 30 years studying and searching for ivory-bills in both the United States and Cuba. These efforts resulted in several publications, including the *Birds of North America* account for the species (Jackson 2002) and a book, *In Search of the Ivory-billed Woodpecker* (Jackson 2004, 2006b). In addition to my expertise, I was also independent of the discovery efforts. The perspective of those involved in the discovery was, by then, well known. That I had a different perspective was also known.

Fitzpatrick et al. (2006a) and Fenwick et al. (2006) suggested that my “Perspectives” article was published without proper editorial protocols. That is not true. Such an article offers the point of view of an individual or individuals with recognized expertise on the subject. Perspectives are usually labeled as such and, indeed, my “Perspectives” article in *The Auk* (Jackson 2006a) was so labeled at the top of page 1. Perspectives in *The Auk* are usually invited and usually not subjected to the typical peer review system. Mine was

not and, contrary to Fitzpatrick et al. (2006a:587), I have not claimed that it was. Because I questioned aspects of the reported rediscovery of the ivory-bill, however, I would have been remiss not to seek peer review. Indeed, Editor Sealy was anxious that I have the article reviewed; I did, and I forwarded reviewer comments to him.

Thirteen colleagues reviewed my manuscript and provided criticism, insight, and suggestions for improvement. Ten are acknowledged in the article; three asked not to be listed. While I am indebted for reviewer advice and for the efforts of the editor, I take full responsibility for the final perspective. In spite of accusations of error presented by Fitzpatrick et al. (2006a), I stand firmly behind the substance of the arguments I made.

In their rebuttal to Jackson (2006a), Fitzpatrick et al. (2006a) dwelt on semantics rather than implications, used quotations out of context, exaggerated the significance of their data, and used untruths and half truths as weapons of mass deception seemingly targeted at a public audience. I will not dissect details of their rebuttal except to demonstrate examples of their approach.

The word that catches a reader’s attention here is “untruths.” I agonized over using the word, but it characterizes their approach (Fitzpatrick et al. 2006a:591) in referring to my citation (Jackson 2006a) of anonymous references: “He [Jackson] cites five ‘anonymous’ authors in arguing that science was compromised, but these were opinion pieces written by journalists and bloggers. None purported to be presenting a scientific case, and none was presented by anyone directly involved in scientific research.”

The truth is that I cited *seven* anonymous references and not one was used in a context arguing that science was compromised. The first (Anonymous 2005a) referred to a transcript of the *60 Minutes* television program that focused on Cornell’s search. I included it to provide an exact quotation of what Tim Gallagher said. The second, third, and fourth anonymous references (Anonymous 2005b, c, d) were to a U.S. Department of the Interior news publication from which I quoted a statement recounting the “dramatic discovery and confirmation of the Ivory-billed Woodpecker” and noted that “The U.S. Department of the Interior has done an exceptional job of ‘selling’ the Ivory-billed Woodpecker.” The fifth reference (Anonymous 2005e) was to a brief editorial in *World Birdwatch*, a publication of BirdLife International, the title of which proclaimed “Agreement over Ivory-billed Woodpecker Sightings,” though it was well known that there were scientists who did not agree that ivory-bills had been confirmed. *World Birdwatch* has since corrected that statement (Anonymous 2005f; see Nemésio et al. 2005). The sixth reference (Anonymous 2005g) was to an editorial in *North American Birds* that suggested specific applications

and amplifications of the “ABA Code of Ethics” with regard to birders searching for ivory-bills. The seventh reference (Anonymous 2005h) was to an article in *Audubon Mississippi* that described a pledge that local chapters had made to respect the ivory-bill and its habitat. None, except the transcription of the *60 Minutes* program, was from the mainstream media and, to be certain of the quotation, I used not only the transcript, but also the actual video of Tim Gallagher speaking. I cited no anonymous bloggers.

Fitzpatrick et al. (2006a) took issue with my use of the phrase “faith-based ornithology.” I stand by my assertion that the declaration of *confirmation* of the existence of ivory-bills in Arkansas was based on faith, because the data presented thus far have been subject to multiple interpretations by independent scientists.

In many ways, there has been more focus on the public perception of science than on science itself. This is not about whether or not ivory-bills could be in eastern Arkansas. We agree that they could be; we all want them to be there. This is not about the need to protect old-growth forest and link patches to create corridors of habitat that can provide for wide-ranging, old-growth species. We agree that this is an important conservation goal. This is about truth as defined by science, as opposed to truth defined by the perception of science. It is about the integrity of science.

Data and analyses presented have provoked legitimate, differing interpretations and professional opinions. These are not only acceptable but appropriate, healthy, and essential to the scientific process—and to good conservation. However, neither self-deception nor deliberate deception has a place in either science or conservation; in the end they can undermine the fabric of both. Whatever else they are, science and conservation are human endeavors, subject to all the foibles of humans: our biases, our desires, our emotions, and our interrelationships with others. They also are subject to errors of carelessness and misinterpretation but, through the processes of science, these are usually remedied. To an extent, the peer-review process so frequently mentioned by Fitzpatrick and his colleagues is a system that not only assures some control over the quality of scientific publications, but also tempers the influence of biases and other human frailties on the outcome of scientific endeavor. But the peer-review process sometimes fails.

Marketing campaigns—promoting everything from ideas to products to politics—often take advantage of the respect given to science. In advertisements for medications, for example, actors wear white laboratory coats and are shown in laboratory settings. Their appearances are often coupled with authoritative uses of numbers. If something is quantified, the thinking goes, it must be accurate. Perhaps not. I support the searches that have gone on in Arkansas and believe that more searches for this species are needed elsewhere. However, I still maintain that marketing was used to sell “confirmation” of

the existence of ivory-bills in Arkansas when data were inadequate to stand up to scientific scrutiny.

In defense of their assertion that observers were not in error when they said that the bird they observed at 100 m or more was “much larger than” a Pileated Woodpecker (*Dryocopus pileatus*), Fitzpatrick et al. (2006a:588) suggested that I had previously stated that ivory-bills were much larger. They quoted my description of two specimens laid side by side (Jackson 2004: 3): “By itself the Pileated was impressive; next to the Ivory-bill it was puny.” This out-of-context quotation amounts to deception. My next sentence (Jackson 2004: 3) clarifies my description: “It was not that the body of the Ivory-bill was so much larger than that of the Pileated, but rather that the bill of the Ivory-bill was so much larger and so different.” Observers of putative ivory-bills in Arkansas did not comment on the relative size of the bill or length of the neck of the bird they saw—two characteristics that a keen observer might have discerned. I stand by my assertion.

If the assumption one begins with is faulty, the outcome of the analysis, no matter how sophisticated, is also likely to be faulty. I submit that, because of low sample sizes and lack of details associated with mensural techniques, most of the numbers presented in the rebuttal by Fitzpatrick et al. (2006a) are relatively meaningless. I also hasten to add that I know ivory-bills are larger than Pileated Woodpeckers. That was never an issue. The issue that precipitated Fitzpatrick et al.’s (2006a) rebuttal was my contention that it is not scientifically defensible to state that a bird seen for a few seconds as it is flying away 100 m or so distant must be an ivory-bill because it was “much larger than a Pileated” as reported by several of the observers. This issue is discussed at length in Jackson (2006a) and I stand by my assertion.

In regard to the reported length of the seven observations of birds described as ivory-bills in Rosenberg et al. (2005), Fitzpatrick et al. (2006a) are correct that I erred in suggesting that none was reported as being longer than the four-second video taken by Luneau. One was reported as “about 7 seconds,” another as “just under 10 seconds.” The fact remains that both the sightings and the video are too short and have too many problems to inspire confidence.

Fitzpatrick et al. (2006a:590) suggest that I “dismissed” their evidence. To the contrary, I examined their evidence very carefully in light of our knowledge of the species and my experience as a scientist who has specialized in studying behavioral ecology and variation in woodpeckers, and I have come to a different conclusion. I have acknowledged the possibilities suggested by their reports. In Jackson (2006a) I referred to their “tantalizing reports” (p. 11), noted that their “use of ARUs has provided some tantalizing possibilities” (p. 12), and referred to the hope they have given us. I also stated my professional opinion that their evidence is inconclusive.

In regard to my interpretation of the Luneau video, Fitzpatrick et al. (2006a:590) again quoted me out of context. They suggested that I made only a "cursory comparison" of their photograph with known ivory-bill photographs and art work. My statement was that "even a cursory comparison of this figure with the photographs...or art...shows that the white...is too extensive..." (Jackson 2006a:8). They turned a figure of speech into a statement of fact. My own comparisons of the white on the bird in the Luneau video were frame-by-frame and were conducted in the context of having examined and measured more than 200 ivory-bill specimens.

Fitzpatrick et al. (2006a:588) asserted that their record of ivory-bills was "unanimously accepted by a state records committee." It was not (Anonymous 2006a). A recent news story (by the wife of one of the Arkansas searchers) revealed that the vote was four to one (Peacock 2006). The dissenter's reasoning included the following: "For something like an Ivory-billed Woodpecker, you have to be pretty sure.... If you're wrong, it's like crying wolf. You have to be exceptionally certain." The dissenter called it "'strange' that Cornell biologists interpreted every facet of the 4-second film as supportive of their interpretation that it is of an Ivory-billed Woodpecker" (Peacock 2006).

An error made by Fitzpatrick et al. (2005a) suggests that none of the 17 authors of the article read one of the references they cited. Fitzpatrick et al. (2005a:1460) asserted that the last ivory-bills documented in Cuba were photographed by George Lamb in the 1950s. If they had read the paper cited (Lamb 1957) as documentation, they would know that no photographs were mentioned. If they had read my account (Jackson 2004:200) of Lamb's work, they would have found reference to a letter that suggested that Lamb only saw ivory-bills at roost time when there was likely inadequate light to take photographs. Published records suggest that John Dennis took the first and last photographs of ivory-bills in Cuba, in 1948; one was published in *The Auk* (Dennis 1948). This error of citation, however, ends on a happy note. Fitzpatrick and his colleagues have apparently located a photo that includes an Ivory-billed Woodpecker—taken by Lamb without the benefit of a telephoto lens. I applaud their efforts and success in locating this photo.

I report here a factual error in my perspective (Jackson 2006a) that was not identified by Fitzpatrick et al. The error is of no significance to the substance of the article, but should be corrected. In discussing efforts to restore ivory-bills, I mentioned the possible role of captive breeding and used the Laysan Duck (*Anas laysanensis*) as an example of captive breeding allowing reintroduction of a species. Although this duck is bred in zoos, I erred in assuming that captive breeding was involved in the reintroduction. It was not. Birds introduced to establish new populations

were wild-caught individuals (Rebecca Woodward pers. comm.; Gummer 2006).

In addition to criticisms of my perspective on the science of the search for ivory-bills in eastern Arkansas, Fitzpatrick et al. (2006a) found fault with my assessment of the politics of the effort. They specifically took issue with my statement about the allocation of funds by the federal government for ivory-bill searches and conservation. I cited Dalton (2005), a science writer for *Nature*, a journal comparable in stature to *Science*, who indicated that funding for the efforts was not new money, but that it was money re-allocated from other projects, including projects focusing on other endangered species. Fitzpatrick et al. (2006a) quoted Department of the Interior administrators as saying this is not true, but their denial seems to rest on the definition of "allocation." Perhaps somehow the money had not been "allocated," but this is semantics. The bottom line is that projects concerned with other endangered species received less funding than biologists had anticipated as money was put into the ivory-bill effort. I confirmed this with federal biologists involved with other endangered species, as did Dalton (2005) and more recently Crewdson (2006). Crewdson noted that biologists spoke only on condition of anonymity for fear of retribution. Biologists I spoke with expressed similar misgivings.

Certainly the federal effort in support of the ivory-bill recovery effort is unprecedented. An initial federal funding package of \$10.2 million was announced in the spring of 2005 (Anonymous 2005i). More funding has been announced since, including a postdoctoral position and a senior biologist position (Anonymous 2006b). It is instructive to compare this level of funding to the median of all federal and state expenditures for an endangered species in 2002 of about \$14,000 (Trombulak et al. 2006). While I acknowledge that many species will benefit from these expenditures, the focus of the spending is on the ivory-bill. The conservation efforts are good, but does the end justify the means of achieving them? The focus of the efforts should be on the ecosystem rather than on the bird. As one journalist commented: "Conservation dollars are too precious and too hard to come by to be tied to the tail feathers of a bird that may not even exist" (Hendershot 2006).

I have long championed the possibility that ivory-bills have survived into recent decades (Jackson 1989, 2006a), but if they have not, the conservation momentum gained by announcement of their "discovery" should not be squandered (Jackson 2006b, c). We must capitalize on growing recognition of the (1) importance of old-growth forests, (2) positive roles of seasonal flooding in riverine forests, (3) need for extensive areas of forest, and (4) wisdom of creating habitat corridors linking these extensive forests, these green pearls of life. We must also focus on the restoration of integrity to science.

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Response to letter by J. A. Jackson.—We regret Jackson's choice of words in reference to our scientific integrity and ethics. We made every effort to strictly limit our letter (Fitzpatrick et al. 2006) to correcting errors made in his earlier commentary (Jackson 2006a), and we stand by every one of the points we made. In his response here, Jackson (2006b) wonders if we have read Lamb's work, yet acknowledges his own error on the point we raised about Lamb's findings. We are confused by this. As we mentioned in our original article (Fitzpatrick et al. 2005), George Lamb did indeed photograph Ivory-billed Woodpeckers (*Campephilus principalis*) during his expedition to Cuba in 1956. We are in possession of one of these unpublished photographs (courtesy of Lamb), and we are investigating the whereabouts of several others known to exist.

With respect to the persistence of Ivory-billed Woodpeckers in North America, responsible scientists differ in their interpretations of the evidence to date, and all hope that more will be forthcoming. We are pleased that Jackson agrees that conducting a systematic search for Ivory-billed Woodpeckers, in Arkansas and in other promising areas across the historic range of the species, is both essential and long overdue. Properly surveying the many extensive areas of regenerating forest in the southeastern United States where the species is rumored to

exist is difficult, labor-intensive, and expensive. Federal funding for this search continues to be modest, and in no way jeopardizes the funding of any other endangered species recovery efforts. We will continue to augment this funding with whatever state, local, and private resources we can secure, and we encourage others to do the same. We continue to cooperate with numerous colleagues in this scientific effort, and will do so as long as we encounter evidence that one or more breeding pairs of this magnificent woodpecker could exist. Finding breeding pairs has been our goal since we began our searches in Arkansas in 2004, and our criteria for proof of such do not differ at all from those of Jackson or others who have criticized our work. We adhere to the tenet that investigating a possible future for the Ivory-billed Woodpecker is not justifiably separated from active efforts to restore large tracts of old-growth southern forest. We encourage a vigorous and united focus on both tasks.—JOHN W. FITZPATRICK, *Cornell Laboratory of Ornithology, 159 Sapsucker Woods Road, Cornell University, Ithaca, New York 14850, USA (e-mail: jwf7@cornell.edu)*; MARTJAN LAMMERTINK, *Cornell Laboratory of Ornithology, 159 Sapsucker Woods Road, Cornell University, Ithaca, New York 14850, USA*; M. DAVID LUNEAU, JR., *Department of Engineering Technology and Department of Information Technology, University of Arkansas at Little Rock, Little Rock, Arkansas 72204, USA*; KENNETH V. ROSENBERG, *Cornell Laboratory of Ornithology, 159 Sapsucker Woods Road, Cornell University, Ithaca, New York 14850, USA*; TIM W. GALLAGHER, *Cornell Laboratory of Ornithology, 159 Sapsucker Woods Road, Cornell University, Ithaca, New York 14850, USA*; and RONALD W. ROHRBAUGH, *Cornell Laboratory of Ornithology, 159 Sapsucker Woods Road, Cornell University, Ithaca, New York 14850, USA*.

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