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Janet D. Stemwedel (whose *nom de blog* is Dr. Free-Ride) is an assistant professor of philosophy at San Jose State University. Before becoming a philosopher, she earned a Ph.D. in physical chemistry. Email her at dr.freeride@gmail.com.

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Posted on: January 31, 2008 12:51 PM, by Janet D. Stemwedel

A [recent news item](#) by Rex Dalton in *Nature* [1] caught my attention. From the title ("Fossil reptiles mired in controversy") you might think that the aetosaurus were misbehaving. Rather, the issue at hand is whether senior scientists at the New Mexico Museum of Natural History and Science were taking advantage of an in-house publishing organ (the *NMMNHS Bulletin*) to beat other paleontologists to the punch in announcing research findings -- and whether they did so *with knowledge of the other researchers' efforts and findings*.

From [the article](#):

The disputed articles name and describe different aetosaurus, and detail how the 220-million-year-old reptiles are related to crocodiles and dinosaurs. In one instance, [NMMNHS interim director Spencer] Lucas, [former NMMNHS director Adrian] Hunt and Justin Spielmann, the museum's geoscience collections manager, are accused of rushing to publish a new name for an aetosaur (*Rioarribasuchus*) when they allegedly knew that palaeontologist William Parker of the Petrified Forest National Park in Arizona was soon to publish an article naming the species (as *Heliocanthus*).

The International Commission on Zoological Nomenclature says scientists must not name species if they know a competing scientist is in the process of doing so. Lucas denies knowing of Parker's plans.

Those of us who are not in the paleontology biz may not see the official name given to an extinct reptile as a big deal. Nonetheless, there's an international commission that sets the rules on this sort of thing. If you're part of a professional community, you're supposed to abide by the rules set by the commissions and institutions governing your professional community.

If you don't think they're good rules, of course, one of the things you should do as a member of that professional community is make a case for changing them. However, in the meantime making yourself an exception to the rules that govern the other members of your professional community is pretty much the textbook definition of an ethical violation.

Did Lucas actually know of Parker's plans? I don't know. *Should he have known?* In a professional community whose members communicate with each other about the work they're doing, it's quite possible that he should have.

Indeed, [some scientists who have a horse in this race](#) allege that the relevant work by Parker was known to Lucas and his colleagues:

The first paper (Lucas et al., 2006), which is less than two pages long, provided a new name for a fossil animal two weeks before a much longer paper (Parker, 2007), which had been in the works for over a year, did the same. The name suggested by Parker had already appeared in his unpublished thesis (2003), which was known to Lucas and his colleagues. It appears that Lucas and his colleagues did not agree that a new name for the animal was needed until they became aware of Parker's work, that they knew of Parker's intention to provide such a name, and that they rushed their paper into press at the last minute in order to take credit for an insight that was not their own. Under Article 23.1 of the rules of the International Commission of Zoological Nomenclature (ICZN), strict chronological priority must be followed, so that the name proposed by Lucas and his colleagues is the one that must be used from now on, denying Parker credit for his insight. Under the ethical guidelines of the same (# 2 in Appendix A), such behavior would also be unethical. Mr. Parker communicated with Dr. Lucas by phone, and Dr. Lucas claimed that he had independently reached the same conclusions as Mr. Parker. Given that Dr. Lucas has clearly been aware of Mr. Parker's conclusion for years, and openly disagreed with it, this claim appears to make no sense.

Again, I don't know what Lucas knew and when he knew it. But if one has read enough of a thesis to disagree publicly with certain of its conclusions, it's not unreasonable to assume

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that one has read other parts of the thesis as well.

Back to the [Nature article](#):

[L]ast July, Jerzy Dzik of the Palaeobiology Institute at the University of Warsaw sent Lucas an e-mail in complaint after Lucas published an article in the *Bulletin* describing Polish aetosaur fossils. The article appeared shortly after Lucas had visited the Warsaw Institute, when the fossils were close to being described by scientists there. Such a thing had not occurred in the past 50 years at his institute, Dzik wrote, adding: "Your action was harmful to many young researchers."

In an e-mail response to Dzik, Lucas blamed the Polish researchers for not being more explicit about their fossil-examination rules, but he did apologize for what he called "a misunderstanding".

This isn't an issue about naming (where ICZN rules might be invoked) but rather about description of fossils. Lucas is alleged to have used the access to the fossils (which the Warsaw Institute was kind enough to provide) plus the super-speedy manuscript-to-published article pipeline of the *NMMNHS Bulletin* to get *his* description of the fossils published first -- ahead of the Polish scientists working on the fossils and presumably relying on standard peer reviewed scientific journals to get their findings published.

Maybe it would have been prudent for the Warsaw Institute to be more explicit about "fossil-examination rules", but my guess is that they were operating under the assumption that members of their professional community ought to provide each other access to fossils. They were probably also operating under the assumption that members of their professional community would not act in such a way as to screw each other over.

Possibly Lucas was working with a different set of assumptions here.

Well, you may sigh, professional science is a shark-tank, and people need to watch their backs. Still, there are rules that are clear enough that their violation cannot be written off as one scientist seeing an advantage in the race to the finish line and taking it. Plagiarism, for example, is an action that can't be excused by "a misunderstanding".

From the [Nature article](#):

Another article published in the *Bulletin* by Spielmann and his bosses involves a reinterpretation of an aetosaur called *Redondasuchus*. Jeff Martz, a palaeontology doctoral student at Texas Tech University in Lubbock, says this reinterpretation -- involving bony spikes along the animal's back -- failed to properly credit his own similar description in a master's thesis, an act akin to plagiarism.

If you fail to credit the sources of your words or ideas, that's plagiarism. Plagiarism is one of the "big three" (along with fabrication and falsification) in official definitions of scientific misconduct. Responsible members of professional communities don't do that kind of crap to other members.

Could Spielmann have come to the same reinterpretation independently? It is possible, but [Martz gives reason to think it unlikely](#):

In his thesis, Martz had corrected a mistake made previously by Lucas and his colleagues regarding the orientation of a particular bone. In their paper, Spielmann et al. take credit for this correction without attributing it to Martz, including a figure (fig. 1) showing a reconstruction identical to the one in Martz's thesis (2002, fig. 3.1c). Spielmann et al. discuss Martz's thesis in great detail on other matters that they disagree with, making clear that they were familiar with its contents. It is therefore difficult to understand how this oversight could have been accidental. Being able to openly and freely disagree with another researcher's work is a normal and necessary part of science. Taking credit for another researcher's insights is considered plagiarism by most researchers.

It's not impossible that the NMMNHS group only read pieces of Martz's thesis. But given the presentation of a reinterpretation that turns out also to have been presented in a thesis you knew existed (because you read and debated with other portions of it), wouldn't it be reasonable to acknowledge that existing interpretation once you *do* know about it? Wouldn't respect for your professional community and its members make it reasonable to share credit in such a case rather than hoarding it?

There are a lot of aspects of this situation which cast NMMNHS in an unflattering light. These researchers don't seem much concerned with fostering good relations within their professional community, but rather with being first to as many results as they possibly can, no matter what that might mean for other researchers (especially junior ones) in their field. Rather than being part of a conversation in the peer reviewed journals others in their

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field use, they have a special in-house publication. Yes, it cuts out the waiting associated with peer review, but it also cuts the rest of their professional community out of the conversation. Finally, Lucas's response to the allegations (from the [Nature article](#)):

Lucas is known in the palaeontology community for his desire to publish a high volume of papers. He acknowledges that his "tough" approach has brought him into conflict with researchers before. "They are obviously angry," he says, but the complaint "doesn't have any substance".

To me, this is a pretty clear indication of the attitude Lucas takes toward members of his professional community. If stuff that he does makes them mad, they're the ones with the problem. It couldn't be that anything he did was inappropriate. He's tougher than the rest of them, and they're just jealous of his success.

Since the Ethics Education Committee of the Society of Vertebrate Paleontology is currently investigating the allegations, there may be more to say about this case before too long.

[1] Rex Dalton, "Fossil reptiles mired in controversy." Published online 30 January 2008 | *Nature* **451**, 510 (2008) | doi:10.1038/451510a

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What? It isn't just the biomedical crowd?

This one stinks like tar pits, my friends...

Posted by: [DrugMonkey](#) | [January 31, 2008 1:15 PM](#)

Sadly, no, DrugMonkey.

My husband spent 6 agonizing years getting his Ph.D. in paleontology and had some of his work stolen several times.

The really sad part is, despite the fact that paleontology is "sexy", there's not a lot of money or professional positions to be had. That makes it all the more cut-throat.

Posted by: Jen G. | [January 31, 2008 1:46 PM](#)

Hi Janet,

Thank you for the thorough coverage and for being so clear about the seriousness of what has *allegedly* (ahem) happened in these cases (plural!). Some folks are shrugging this off; on the Vert Paleo listserve the Nature article has even been denounced as tabloid journalism. But anyone who cares about how science works ought to be concerned about this, as you clearly are. Thanks again.

Posted by: [Matt Wedel](#) | [January 31, 2008 3:39 PM](#)

As a practicing paleontologist from New Mexico, I feel safe in saying that Mr. Lucas is well known in the field for deceit of every stripe, including theft of work, rush to publication, and misrepresentation of sources. Through the 80s and 90s, he was renown for taking on foreign graduate students and taking credit for their work. I personally witnessed him raiding cabinets at a competing (now defunct) museum and breaking up specimens in order to increase the number of catalog references bearing his name. I have also witnessed him dismantling posters at a professional conference to hijack lists of references, from which it would be possible to reconstruct work and ideas and present them as his own. He has a personal grudge against paleontology faculty at Texas Tech University (who have blown the whistle on him both scientifically and behaviorally numerous times) and will not miss an opportunity to set students there back.

Sadly, he is not the worst our field has to offer; at least Mr. Lucas has kept his hands off his female students, unlike the gentleman recently released by Washington University.

Posted by: Will S. | [January 31, 2008 4:22 PM](#)

If I were in the field, I would be very wary of 'collaborating' with Lucas in the future. Way to bury your career in science, dude!

Posted by: [Tlazolteotl](#) | [January 31, 2008 4:25 PM](#)

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f you don't think they're good rules, of course, one of the things you should do as a member of that professional community is make a case for changing them.

You can't change the rules in taxonomy without invoking the ghost of Linnaeus himself. People have been trying that for years with only the occasional glimmering of success.

Posted by: [Greg Laden](#) | [January 31, 2008 4:57 PM](#)

This case sounds familiar to me not only because of the scientists who allegedly committed the scientific misconduct, but for their institute's officials who in their efforts to cover up the misconduct have chose to "investigate" those allegation and declare them "groundless."

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Posted by: S. Rivlin | [January 31, 2008 5:11 PM](#)

You can't change the rules in taxonomy [sic -- nomenclature!] without invoking the ghost of Linnaeus himself.

That's an exaggeration, but mostly because Linnaeus didn't make much in terms of rules. In zoology, the rules began in earnest in 1843, and in botany only in 1867. However, yes, both Commissions are very conservative bodies. So much so that the head of the botanical one has called an action by his Commission "a revolution of the druids against the Enlightenment" in a published paper.

Incidentally, of the "some scientists who have a horse in this race", only Parker and Martz do. Naish, Taylor and Wedel don't.

Posted by: David Marjanović | [February 2, 2008 8:03 AM](#)

I must admit there are some things I don't understand about this affair, and it makes the present stonewalling inexplicable to me, as it is worrisome to many graduate students and younger workers. I have known Spencer Lucas for 30 years, and I have tremendous respect for the breadth and range of his knowledge and his productivity. It was painful for me, as for many others, to read the report in Nature, even though it was not the first wind I had of these complaints. So I don't understand how a curator and productive researcher of 30 years, who has worked on fossils from several continents and from the Permian to the Pleistocene, does not know that before you publish on specimens in another museum that are clearly undescribed and under study by others, you have to ask and receive explicit (written) permission. That's unquestionably the responsibility of the scientist who has been trusted to see the materials, not the fault of the museum for allegedly not clarifying it. I don't understand how it could happen that a graduate student, as Jeff Martz has claimed, could have his unpublished thesis repeated almost point by point and its figures duplicated by the same people in an in-house publication before he could get his results into print. I don't understand how Bill Parker could face a similar situation with his determination of a new taxon of aetosaur, particularly if the people who scooped him were on record several times in the recent past denying that it was a new taxon (which would seem to imply that they knew of his idea). Nature is not exactly the National Inquirer. Judgment of these issues is not up to me, but there is a sliding scale from bad judgment to outright transgression. If these charges are true, then that last line has been crossed several times. How can these be simply a "matter of opinion" or a misunderstanding? The evidence has been submitted publicly and through appropriate channels, and it seems clear, at least, that the New Mexico people need to present their side. Their failure to do so hurts themselves and their museum. There has to be a rational explanation here, and due process. I don't understand why this is apparently being stonewalled by New Mexico administrators and officials. But I suspect that most of them are not academics, and it would be difficult for them to determine impartially what constitutes problematic action in an academic field.

Posted by: kevin padian | [February 4, 2008 11:38 PM](#)

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