

Appendix 1. The complete in-house review that Jeff Podos (repeatedly) requested of me, and that he now uses as proof of my harassment.

Introductory notes:

I offer some introductory thoughts here, giving some context. First, with full disclosure, I want to point out the long-standing difference of opinion with Podos (and his advisor Nowicki, and Nowicki's frequent collaborator William Searcy) about what constitutes science; it is absolutely true that we have these long-standing differences. What is not true is that there is any personal vendetta against these authors. I ask anyone who reads this material to keep in mind only issues of science. Someone might claim that I am disqualified as an objective analyst of these matters because I have been thinking about these things for far too long (published critiques of research begin in 1989, countered immediately by William Searcy, in the journal *Animal Behavior*); I think a more cogent argument is that, given how long I have fought for integrity in science, I am eminently qualified to address these matters.

I confess to having a large folder in my file cabinet with articles written by Steve Nowicki, together with my critical notes on each. When asked by Duke University to write a 'letter of promotion' for Nowicki, I said I would be willing to write a letter, but it would be a letter recommending demotion rather than promotion; Duke then decided they didn't need my letter, as they were constructing their promotion case only with supporting evidence (not mentioning negative evidence), much like Nowicki was doing in his research (not mentioning, e.g, alternative explanations or nonsupporting evidence), much as his student Podos now does, just as he now trains his graduate students. For Podos, and now his students, there's a book title here, something like "All I Really Need to Know I learned in Graduate School." I find this culture of advocacy distressing, as it produces reams of publications with mistruths about how the world actually works.

For an illustration of this cultural transmission of "research" strategy from graduate mentor to student, I suggest two publications for consideration. In the first, mentor Nowicki anchors the paper by his student Podos. In the second, mentor Podos anchors the paper by his student Goodwin (the second paper, of course, is the main focus of my allegations of misconduct).

1. Podos, J., S. Peters, and S. Nowicki. 2004. Calibration of song learning targets during vocal ontogeny in swamp sparrows, *Melospiza georgiana*. (See critical review of this paper in item #4g on the website.)
2. Goodwin, S. E., and J. Podos. 2014. Team of rivals: alliance formation in territorial songbirds is predicted by vocal signal structure.

Podos' manuscript that he asked me to review was submitted to some journal (I forget which), and the journal asked me for a review. With full disclosure, I sent the same review to the journal that I sent to Jeff (all copied below). I don't know what happened to that manuscript, but I would never see another manuscript (or grant proposal) from Jeff. I have always wondered what he told journal editors and granting agencies about me so as to avoid my evaluation of his work. I would assume it is something along the lines of what his graduate mentor Nowicki says, as I have never, to the best of my recollection, ever seen a manuscript or grant of his either.

OK, enough. Here's the 2004 in-house review of the Podos paper:

8 October 2004

Hi Jeff:

“Intraspecific *divergence* in many animal taxa is facilitated by geographic *variation* in mating signal structure, and by the *tendency* for individuals to discriminate among local and distant signal variants.”

There’s the first sentence of your abstract. I read that sentence at least 5 times before I thought I knew what you were saying (ok, perhaps I’m both senile and grumpy). Here’s what I think it says:

“Mating signals that *vary* geographically cause many animal taxa to *diverge* because individuals *tend* to discriminate between local and distant signals.”

What I’ve done is take your nouns and convert them to verbs (italicized; see lessons for clear writing by Joseph Williams in his book *Style* . . .).

It seems to me that there are three (true) statements in your sentence:

1. Mating signals vary geographically
2. Animal taxa diverge
3. Individuals discriminate between local and distant signals

But I object (rather strongly) to the way that you’ve put these three statements together. Isn’t it just as likely that (especially learned) signals vary because of lack of dispersal between even local sites? In the first sentence of the abstract, where the author has what I would call the responsibility and expectation to place his/her work in a broader context so that the readers know the general subject area in which the author is writing, you have chosen a very narrow and, I think, misleading statement. I feel that you are exploiting reader expectation by stating an untested idea as fact in a place where the average reader expects a general, true statement.

Sorry, but you’ve pushed one of my big red buttons. It is this kind of statement that I think misleads and manipulates rather than informs readers. It is the kind of opening statement that provides the story around which the article will be written, but the hidden assumptions in the opening statement mislead the reader into thinking that you’ve provided the general, accepted framework for the paper but instead have provided a narrow story line that tells only a tiny bit of what might be a cool story, presumably the story that you want to tell.

It reminds me all too much of the last Nowicki paper I read that started this way (to get this information I go to my extensive commentary on Nowicki in my file here at home): “Females of many songbird species show a preference for mating with males that have larger song repertoires . . .” And I would ask that we name one species in which it is shown that females make a mating decision based on repertoire size.* With your statement I would ask you to name one bird species in which it is shown that local populations diverge because individuals discriminate between local and distant signals.

In my view, science is the search for truth regardless of how good the story is, whereas “marketing or advertising” is the search for a good story regardless of the truth, or regardless of how good the data are. [emphasis added in 2014] Given what Nowicki [graduate advisor to Jeff Podos] does in his papers, and given his seminar here at UMass and the climax when he blatantly lied his way through the answer to a question in the Q&A period (presumably to protect his story and self-image), I’ll never read another paper of his; I simply don’t trust him, and I find his approach to science reprehensible. To me, his work is advertisement and self-promotion masquerading as science. Steve markets his ideas and himself, and he does it very well, but I wouldn’t call what he does science. You and I had a brief conversation in which you were eager to defend him, but we didn’t go into details, and we probably never will, especially as I am moving on. Perhaps all this is why your first sentence distresses me so, because I like you, and I expected otherwise from you.

Having spent this much time on the first sentence, it is perhaps no surprise that I’m not in the mood to read the details of the rest of the manuscript. I read it rather quickly, trying to get the main ideas, and had just a couple of more quick thoughts:

I’m concerned that you’ve measured 5 aspects of the songs and, finding no consistent differences between two populations, feel that you have adequately characterized the signals and can conclude that there’s an “absence of site-specific diagnostic song features.” A sonagram is such a crude rendition of a song, and taking only 5 rather simple measurements of each song from such a crude representation seems inadequate to me. And so much of your paper and ensuing discussion seem to hinge on this result. From my skeptical (cynical?) point of view, you have made a weak attempt with little power for finding population differences, so you now have (weak) negative evidence for population differences that you then seem to turn into strong support for the story that you have chosen to tell.

Although I believe your playback results, I think a better design would have been for the observers not to know what songs they were playing to the birds. If you know the hypothesis you are testing and the story that would be most interesting when you are estimating distances and such (and especially given what I detect as a strong bias toward wanting to support your story in

your first sentence in the abstract), it's all too easy to bias the numbers that are collected in the field. That's why all good experimental designs call for the observers to be "blind." Perhaps you were "blind" and I just read the ms too quickly to read that. If so, sorry I overlooked it.

Sorry I'm in such a grumpy mood tonight. I read the paper rather quickly as Anaheim scored a bunch of runs, capped by a grand slam, in the third game of the series, tying the game at 6-6. Perhaps if the Sox had won easily my commentary above would have been entirely rosy.

Best regards . . . Don

PS—If you value your career and the high esteem of your peers, perhaps I should encourage you to disregard all of my above comments. My approach to science might be all too old-fashioned.

*Yes, I know that Nowicki didn't actually say that females choose on the basis of repertoire size, but the entire paper is about female choice, so the reader is misled into concluding what Nowicki wants them to conclude even though he didn't say it himself. Clever, but . . .