

Emails regarding Podos and Biology Letters and Goodwin and Podos (2014)

(as complete an accounting as I could find on 30 September 2015—some relatively unimportant, non-informative back-and-forth emails are missing)

12/1/2014	<p>Kroodsma to cited authors in the document entitled “Honest signaling in birdsong and the motor constraints hypothesis of Podos (1997): A contrary view.</p> <p>Hello birdsong enthusiasts (cited authors and a few “interested parties”—Group 2):</p> <p>Attached is a document that I began working on shortly after hearing the oral presentation of Goodwin and Podos (2014) at the ornithological meetings in Rhode Island during May of this year. Over the following weeks and months, I attempted to communicate with the authors about their paper (by both email and U.S. mail), but received no replies, and as week after week went by, I found myself studying more and more of the literature on this topic. By the time I finally said “enough,” the attached document had evolved into something far larger than I had ever considered at the outset.</p> <p>In the end, I realized that I was reading for the most part what Richard Feynman (1985:340) calls, to put it bluntly, “science that isn’t science.” Such publications lack his measure of “scientific integrity,” and are instead largely “advocacy” for favored ideas (Gitzen 1987). I was learning practically nothing about the behavior of nature but instead almost solely about the behavior of those who publish these papers.</p> <p>I don’t know the eventual outlet of my document, but I am aiming for a Forum article in Animal Behavior. Before it finds some public expression, however, I thought it appropriate to seek any feedback that cited authors or others might like to provide. A dialogue would be welcomed, and any response you care to provide will be added to the document for others to read.</p> <p>Feel free to forward this document to anyone you wish, especially any coauthors, with the same invitation for contributed commentary.</p> <p>If you plan to reply, I would appreciate hearing from you before 5 January 2015, at which time I’ll decide the next step for this document.</p> <p>Regards . . . Don Kroodsma</p>
1/24/2015	<p>Kroodsma to Association of Field Ornithologists</p> <p>Hello Vicki, Greg, Gary:</p> <p>I’m writing to each of you because you know something about birdsong, you all have an official status with AFO, and Vicki especially because I think you served as best paper chair at the 2014 meetings.</p> <p>I have become involved in the strangest set of circumstances that I could ever have imagined. I</p>

went to the AFO/Wilson meetings in a joyful mood, but left troubled by the “clean sweep” of the best student papers by the Podos lab at UMass. I was especially troubled by the Goodwin paper that won AFO best student paper, because I know chipping sparrows, and that paper was not about the birds that I knew. I have since studied the published version of the paper, and find that it is, well, how should I say it . . . it is fiction, with no science. There is not a shred of truth in the study.

I have tried to communicate with Goodwin and Podos about the study, but they refuse to respond to any of my inquiries (professionally unethical by any standards, I believe). After Mike Beecher and his student began inquiring about the paper, and went so far as to suggest a retraction of the published study, Podos stopped all communication with them as well.

The first bit of communication I received from Podos was indirect, by way of the University of Massachusetts police, who threatened me with criminal harassment if I sent them one more email. Pretty sad.

I have since reviewed all of the papers on the topic of “honest signaling” and the Podos graph of trill rate and frequency bandwidth, finding no science that supports the widely held notion that the quality of the song honestly tells of the quality of the male. The idea of honest signaling has been marketed well, but fails any close scrutiny. That review is taking shape, and will be published eventually, someplace, though the path to publication has also been one of the strangest stories I could ever have imagined being involved in.

So why specifically am I writing to you? Given all that I now know about the Goodwin and Podos paper that won the AFO’s best paper award, I’d sickened by the award, as I feel that it not only cheapened the entire scientific meeting, but also cheated some graduate student doing real science out of the award that he or she properly deserved. And given how medals of all kinds are retracted after cheating is discovered, I would advocate retracting the award from Goodwin and Podos and giving it to the next student in line.

That seems pretty harsh and unprecedented, I know, and you might claim that I’m no longer an impartial judge in this entire matter. All that may well be true.

I send this letter to alert you to what I feel was a great injustice at the AFO meeting in Rhode Island. If you wish to pursue the matter, I could provide you with the documentation that I have accumulated, and you could be the judge. You could, if you wanted, ask Goodwin and Podos to respond to the charges; that would, of course, be the only fair approach, and perhaps under that kind of pressure they would feel compelled to respond to you, when they have refused to respond to me and others.

A happy 2015 to each of you. I remember fondly my involvement with AFO—it’s a fine organization, one to be proud of.

Best . . . Don

2/1/2015

Kroodsmas to Biology Letters

Dear Editor of Biology Letters:

What should I, as a reader of *Biology Letters*, do when I discover that a paper published in the journal has no truth whatsoever in it? It is entirely fiction, and I believe it should therefore be retracted. What is the procedure that I should follow to propose this retraction?

Thank you.

	Sincerely . . . Donald Kroodsma
3/2/2015	<p>Biology Letters to Kroodsma</p> <p>Dear Don,</p> <p>I hope this email finds you well and please accept my apologies for the delay in responding to your message. I have discussed your comments on the article published by Drs Goodwin and Podos the Editor-in-Chief of the journal. I am writing to let you know that we have recently accepted a comment article on the Goodwin and Podos article which makes very similar points to the ones you have made. Our policy on Biology Letters is to have one comment only on any given article and one response from the authors. We will let you know when the accepted comment and authors' response is published and if you still have concerns that are not addressed in the comment you may wish to submit an online eletter. These are published alongside the online article and are more informal than comments. They are moderated in-house, sometimes with advice from the journal editorial board.</p> <p>Many thanks and best wishes, Surayya</p>
7/2/2015	<p>Kroodsma to Biology Letters</p> <p>Hello Surayya:</p> <p>I see now that the articles by Ackay and Beecher as well as Goodwin and Podos have been published. I do have very serious concerns about the original Goodwin and Podos paper, in ways that Ackay and Beecher did not address. Those concerns involve what I would call serious scientific and ethical misconduct, and I would very much like to submit an online letter, as you suggest in your email of March 5.</p> <p>Could you please tell me what my constraints are in writing such a letter? There is undoubtedly a word limit, for example? Is a figure allowed? Or a time frame in which this must be accomplished?</p> <p>Thank you.</p> <p>Kind regards . . . Don Kroodsma</p>
7/3/2015	<p>Biology Letters to Kroodsma</p> <p>Dear Don,</p> <p>Thank you for your reply. Unfortunately, our eLetter service is not currently working and so this option is not available. May I ask if your concerns are the same as those from your email dated the 5th February and if not please do provide a summary of what these are. May I also ask if the authors have contacted you regarding your concerns since your last correspondence with them?</p> <p>I look forward to your reply.</p> <p>Many thanks and best wishes, Surayya</p>
7/7/2015	<p>Kroodsma to Biology Letters</p>

Dear Surayya, for Biology Letters:

Thanks for your recent email, inquiring why I find Goodwin and Podos (2014, and their 2015 response) so disturbing.

In the attached document, I do my best to explain why.

Thank you.

Sincerely . . . Donald Kroodsma

7 July 2015

To: Biology Letters

From: Donald Kroodsma

Re: Goodwin and Podos (2014)

Hello Surayya:

Thank you for writing back, telling me you eLetter service is down. For something this important, I can be patient. If your eLetter service will be working in the near future, I can wait to submit a letter critiquing Goodwin and Podos (2014).

My concerns with their paper are quite simple, but serious. If I forego the nuanced language that I'd use in a published eLetter, I'd simply say the following (and for one simple reason, discussed at the end, I provide you with more detail than you initially need):

Goodwin and Podos (2014) is entirely fiction. The critique of Ackay and Beecher doesn't address key shortcomings of this paper because they 1) don't know the study animal (chipping sparrow) and the relevant published literature like I do, and 2) they don't live in the same university department as Goodwin and Podos (I am an emeritus professor in the same Department of Biology, University of Massachusetts, Amherst).

The most serious problems result from Goodwin and Podos omitting reference to two key, published facts of chipping sparrow biology that fatally undermine their entire study. These facts are well known, and were published by another graduate student in the same department, working on the same chipping sparrow population that Goodwin and Podos worked on.

The first matter is that Goodwin and Podos assume (and then confirm) that the trill rate of a male's song is an indicator of how well he can perform, i.e., his quality. That simply cannot be true, given how a young male chipping sparrow acquires his song: He settles next to an older adult and learns the song of that older adult. Trill rates in the population vary from roughly 6 to 36, and trill rate (and all other characteristics of a male's song) are thus determined by the adult next to whom he settles, not by any intrinsic quality of the young male himself. I can readily demonstrate this phenomenon with a figure or two, supplemented by reference to published literature, thus falsifying the idea that trill rate reflects male quality and also negating the confirmation by Goodwin and Podos that trill rate is used to indicate a male's quality in forming coalitions. (And the far-fetched, post-hoc explanation that a young male might only settle next to a male whose trill rate he is capable of learning is readily refuted by a published laboratory study showing how chipping sparrows show no difficulty learning whatever trill rate is randomly assigned to them.)

The second problem is that chipping sparrows do not defend the all-purpose territories on which Goodwin and Podos depend. Especially in the pre-sunrise hour, often when it is too

dark to see them, males range widely over geographic space, displaying in lek-like arenas often far from their day-time territories. These gatherings are almost certainly competitive, not cooperative, and one would no more label such lek-like gatherings a “coalition” than any gatherings that Goodwin and Podos incite when they play back songs within a daytime territory.

Based on these two unmentioned facts of natural history, from the very outset Goodwin and Podos (2014) is relegated to fiction, and then the true value of their paper becomes a primer on how the authors are able to confirm a falsehood. Ackay and Beecher (2015) address many (but not all) of these matters, only to have Goodwin and Podos (2015) staunchly defend their original publication (even though I had pointed out to Goodwin and Podos several times the fictitious nature of their study).

You ask if the authors have contacted me. Goodwin and Podos refuse to communicate (just as I have learned they refused to communicate with Ackay and Beecher). When I pointed out to them the serious problems in their paper, they went to the University of Massachusetts Police, and had an officer call me and threaten me with criminal harassment charges if I tried to talk to them about their work. The same officer told me that I had to tell the ~50 other scientists with whom I was communicating about this subject that none of them were allowed to communicate with Goodwin and Podos either, as all of them would be subject to the same criminal harassment charges (to save Goodwin and Podos the embarrassment, I did not comply with that last police request). As a result of these developments, I am not allowed to speak to another faculty member or his graduate students in my own university department.

These issues are about as serious (and also absurd, given involvement of the police) as we encounter in science. About the only worse thing, I suppose, is outright fabrication of data, but when one has so many other methods at one’s disposal, fabrication isn’t necessary to create fiction. This kind of fiction disguised as science undermines the integrity of all scientists, from those who study birdsong to those who study climate change, and has highly serious ramifications.

At the risk of diluting focus on Goodwin and Podos (2014), I’d like to point out that this paper is only the culmination of a series of papers by Podos and his colleagues on this topic, all of which use many of the same faulty methods to “confirm” the performance hypothesis of Podos (1997). The titles of these papers tell the storyline, much as the title does for Goodwin and Podos, but none of the titles conveys the true nature of the data, as careful analysis shows. Here I offer a sampling, with my comments in italics:

Goodwin, S. E., and J. Podos. 2014. Team of rivals: alliance formation in territorial songbirds is predicted by vocal signal structure. *Biology Letters* 10:Article Number: 20131083. *There is no team of rivals, no alliance formation; this title is outright false, with an interesting hypothesis presented as a conclusion.*

Moseley, D. L., D. C. Lahti, and J. Podos. 2013. Responses to song playback vary with the vocal performance of both signal senders and receivers. *Proceedings of the Royal Society B-Biological Sciences* 280. Issue: 1768 Article Number: 20131401. *Title is true, but only partially so, as two other trivial alternative explanations for the data are not mentioned.*

Responses also vary with 1) how abnormal the playback songs are and 2) how much song is played to the birds, both of which could readily explain the results without resorting to the nonparsimonious ideas of performance.

Lahti, D. C., D. L. Moseley, and J. Podos. 2011. A tradeoff between performance and accuracy in bird song learning. *Ethology* 117:802-811. *Again, here is an interesting hypothesis stated as a conclusion. There is no tradeoff between performance and accuracy as the authors intend the meaning; all of the results are simply explained by swamp sparrows innately knowing the characteristics of a normal song, and striving for that target.*

Dubois, A. L., S. Nowicki, and W. A. Searcy. 2009. Swamp sparrows modulate vocal performance in an aggressive context. *Biology Letters* 5:163-165. *The title is true, but highly misleading, because swamp sparrows modulate vocal performance in all contexts, no more in aggressive than in nonaggressive contexts, rendering the partial truth in the title highly deceptive. The apparent support for Podos' performance hypothesis (1997) again disappears upon close inspection.*

Ballentine, B. 2009. The ability to perform physically challenging songs predicts age and size in male swamp sparrows, *Melospiza georgiana*. *Animal Behaviour* 77:973-978. *The title might be true, but only partially so, because other aspects of song also predict age and size, and are more readily apparent to a listener and more likely to be used by a listener than the complex metric of "vocal deviation" required by the performance hypothesis.*

Podos, J., S. Peters, and S. Nowicki. 2004. Calibration of song learning targets during vocal ontogeny in swamp sparrows, *Melospiza georgiana*. *Animal Behaviour* 68:929-940. *Another fascinating hypothesis is presented as a conclusion, but there simply is no calibration; there is no evidence that a young swamp sparrow takes a tutor song and calibrates it to what he as an individual is capable of performing, in the context of the performance hypothesis.*

Ballentine, B., J. Hyman, and S. Nowicki. 2004. Vocal performance influences female response to male bird song: an experimental test. *Behavioral Ecology* 15:163-168. *The hypothesis is again stated as a conclusion in the title, but numerous alternative explanations for the data are ignored.*

Kroodsma, D. October 2004. An intra-departmental review provided by me to Jeff Podos. *I refer to this document, because the friendly relationship I had with Podos stopped here. In this intra-departmental review, I pointed out to Podos the difference between "science" and what I felt he was doing, in the following words:*

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 (8 October 2004)

During the following ten years, from 2004 to 2014, the marketing continued unabated, culminating in Goodwin and Podos (2014), in which I cannot find one iota of truth. The primary reason I refer to my ten-year-old document is to counter any claim by Podos that I should be dismissed because I have a personal vendetta against him. On the contrary, my disagreements with Podos are 100% science-related (see also Appendix).

Podos, J. 1997. A performance constraint of the evolution of trilled vocalizations in a songbird

family (Passeriformes: Emberizidae) *Evolution* 51:537-551. *A fascinating hypothesis, worthy of testing, but over the years it has only been confirmed, never tested; with some simple descriptive graphs, this hypothesis is readily falsified, but the rush to experiment and confirm over the years has led to a literature of mistruths.*

I would hope that neither Ackay and Beecher's published critique nor a temporary electronic problem with eLetters will prevent these serious issues from being addressed. This literature has been accumulating for more than a decade now, and Goodwin and Podos offer a most transparent view into how this literature has flourished.

And here's the reason I have supplied more detail than you might think necessary. Anyone reading this document will inevitably do so in disbelief, and might want to ask Podos himself about all this. You are welcome to do that, sending this document directly to him (I would, but can't, under threat of criminal charges), but I would also ask that no response from Podos be taken at face value, just as none of the publications I have referenced can be taken at face value. If the situation is appropriate, please give me a chance to address whatever defense Podos offers.

Thank you.

Sincerely . . . Donald Kroodsma

Appendix. Excerpts from the eight emails to Podos and his students that earned me the threat of criminal harassment charges. I can supply the entire emails, but these excerpts show a sincere attempt to engage Podos on the nature of his work, with no trace of a personal vendetta.

- 1) 9 July 2014 "I wondered if you could help me understand why in the paper you focused only on trill rate."
- 2) 1 October 2014 ". . . if you'd like to talk some of these things through, let me know, and I'll come over to UMass"
- 3) 8 October 2014 "I have no idea what is in your head . . . I'm still available to talk"
- 4) 15 October 2014 "JEFF, HERE'S ONE LAST ATTEMPT TO ENGAGE YOU"
- 5) 16 October 2014 "something well worth reading, from half a century ago" (Feynman 1985, Cargo Cult Science; on the "utter honesty and scientific integrity" needed to do science)
- 6) 9 November 2014 "I'd welcome any dialogue with you"
- 7) 1 December 2014 (early in day) "If you want to talk about any of this . . ., I can be reached at 413-247-3367, just across the river from you. Or email me"
- 8) 1 December 2014 (late night) "I thought it appropriate to seek any feedback that cited authors or others might like to provide. A dialogue would be welcomed"

7/8/2015

Kroodsma to Association of Field Ornithologists
Hello Vickie, Gary, and Greg; and Reed, for AFO:

cc: Bob, of WOS

Vickie, Gary, and Greg received the email copied below on January 24. Here is an update:

The courts of justice turn slowly, but in this case rather surely, I believe . . . the published version of Goodwin and Podos (2014), which won best AFO student paper award in Rhode Island, has now been firmly critiqued by Ackay and Beecher (2015) in *Biology Letters*. Goodwin and Podos (2015) responded, staunchly defending their fiction.

I have in turn responded to *Biology Letters*, because Ackay and Beecher's critique, as damning as it was, was rather lame, and *Biology Letters* has told me that I will be allowed to address the more severe shortcomings that Ackay and Beecher missed (see my attached draft to *Biology Letters*).

Given that there was not a word of truth in the Goodwin and Podos presentation at AFO, I am asking that the AFO consider retracting the award given to Goodwin and give it to the next student in line. That is an unprecedented action, I realize, but the paper was just as unprecedented in its lack of truth, and I would hope that AFO would have zero tolerance for this kind of behavior at its scientific meetings. It would not only send a message to Goodwin and Podos, but would also send a message to every graduate student who attends AFO meetings that the AFO is a serious, scientific society.

Podos on his website brags about a "clean sweep" of the student awards at the Rhode Island meetings, as if it were some kind of game being played (and it is, a marketing game, with little attention to truth). I am confident that close study of the WOS award given to Moseley would also reveal little truth to her study (I have not followed up on her paper, as I am unsure which, if any, published paper is based on her WOS award; I have studied other papers of hers and found them also seriously wanting). I'll leave it to WOS if they want to pursue it.

I am going to leave it at that. As a former councilor/officer of AFO, I'd be proud of AFO if it were to take a leadership role in promoting solid science among its graduate students.

Sincerely . . . Don Kroodsma

7/10/2015

Kroodsma to Dean Goodwin at UMass

Hello Steve:

A letter for you, attached.

Regards . . . Don Kroodsma

17 June 2015

To: Steve Goodwin, Dean, College of Natural Sciences (sgoodwin@cns.umass.edu)

From: Don Kroodsma

Re: Jeff Podos and science

Repeat sentence from the end: I would appreciate your acknowledging that you have received this letter. With no response from you, I will proceed to address these issues in the manner I can best devise (a line that I repeatedly told Podos).

Back on 26 January, I sent you an email message on this topic, but did not hear back. A few days ago I copied you on a message that I sent to *Biology Letters*. Not hearing back from you

could mean a variety of things (e.g., you never got those emails), but the situation is intensifying, and I thought I'd offer you a few thoughts directly.

It was a little over a year ago that I attended the ornithological meetings in Rhode Island, where Sarah Goodwin and Jeff Podos delivered an oral paper on their "Team of Rivals." I immediately recognized it is pure fiction, as given what was already known (but never acknowledged to the audience) about their chipping sparrow subjects, not a word of their paper could possibly be true. Yet, deceived by a polished presentation and impressive-sounding, confident results, the committee on student awards honored Sarah with a best-student paper award (which the Society is now considering retracting—an unprecedented action in itself).

A lot has happened since then. From the University of Washington, a major rebuttal of their published paper appeared in *Biology Letters*. As damning as that rebuttal was, there are even worse problems with the Goodwin and Podos paper that are not addressed, and *Biology Letters* has assured me that I will be allowed a follow-up in which I expose these additional ethical and scientific matters.

Despite repeated attempts to engage Podos or his students about their science, I have received no response from them, and instead Podos has enlisted the UMass police to threaten me with criminal harassment charges if I make any further attempt to communicate with them. Perhaps you were even consulted by Podos when that happened—I don't know, as that entire episode remains shrouded in mystery (and absurdity).

In my emails to Jeff, I have given him repeated opportunities to address these matters directly, in a style that he chooses. His refusal to communicate in any way whatsoever about his research data (itself a damning ethical issue in science) means that these matters will be taken to a public forum. In addition to the rebuttals that will appear in *Biology Letters*, I have prepared an extensive review all of the papers by Podos and his colleagues that have appeared on his topic of "performance" in the last 15 years.

The results are not pretty. They reveal Jeff's relentless marketing of a sexy, appealing idea that, upon close scrutiny, has no merit. Permeating these publications are an "utter dishonesty" and lack of "scientific integrity" (sensu Feynman, "cargo cult science"; see http://neurotheory.columbia.edu/~ken/cargo_cult.html), and all manner of pseudo-scientific methods are used to promote these ideas of performance (and Jeff's career). Once these issues are aired, I do not believe that any research Jeff Podos has ever done will have any credibility. He has, in my opinion, made a mockery of the scientific process, duping wide audiences into believing that his work has great significance.

These are among the most serious issues that we as scientists face, so I make one last offer to have Jeff address these matters on his own terms. I choose not to risk the (ridiculous) charges of criminal harassment and legal proceedings, so I send this letter to you (with my blessing to forward everything on to Podos himself—you do so at your own risk, however, as your own police have told me that *anyone* who tries to engage Podos about his science is subject to criminal harassment charges). If you wish to engage Jeff, and try to control the fallout on these issues, I leave it up to you.

This entire process has been no fun over the past year, but I have been unable to sit idly by as a field of science that I cherish is so abused. Science is "self-correcting," I've been told, and I am compelled to be part of the correction process. It's possible that you feel the same—I'd like to think so, even though Podos is one of "yours." For me, the correction process would exact swift and severe penalties against anyone who engages in this kind of false science (see Appendix).

One final thought. I am sure that the motivation for my efforts will be challenged. As scientists, let's list the possible explanations/motivations:

1. I loathe what is called "junk science," especially in my cherished field of birdsong. **True.** There's abundant evidence for this explanation. See previous papers I have published that challenge scientists in my field to do better work, such as the following:

Kroodsma, D. E. 1989. Suggested experimental designs for song playbacks. *Animal Behaviour*. 37:600-609.

Kroodsma, D. E. 1990. Using appropriate experimental designs for intended hypotheses in song playbacks, with examples for testing effects of song repertoire sizes. *Animal Behaviour*. 40:1138-1150.

Kroodsma, D. E. 1990. How the mismatch between the experimental design and the intended hypothesis limits confidence in knowledge, as illustrated by an example from bird-song dialects, p. 226-245. *In: Interpretation and explanation in the study of animal behavior*. M. Bekoff and D. Jamieson (eds.). Westview Press, Boulder, Colorado.

Byers, B. E., and D. E. Kroodsma. 2009. Female mate choice and songbird song repertoires. *Animal Behaviour*. 77:13-22.

2. I would like to undermine the career of anyone who publishes pseudo-science and who, with every publication, removes us farther and farther from a true understanding of what birds actually do. **True.** This is not personal. This is science, and a frontal attempt to stop pseudo-science.

3. I'd like to give Jeff Podos the opportunity to deal with these issues on his own terms. **True,** but I've given up (my naïve) hope that he'll come to terms with his ways unless forced to. I've given him every opportunity to do so in the past (I could send you copies of my previous emails to him), only to be threatened with charges of criminal harassment. Meanwhile, Podos continues, defiantly: Goodwin and Podos repeated their same flawed paper at a second scientific meeting last year, even after all of the flaws were pointed out to them; and they have defiantly defended their original publication in *Biology Letters*. If you want to try to convince Jeff to come to terms with these issues on his own, be my guest. I believe it will be far better for him in the long run if he himself addresses these issues candidly, rather than my (and others) taking him to task in a public forum for his misconduct.

4. This is a heads-up for you, in a leadership position, as to what is just over the horizon, to deal with in whatever way you wish, professionally or personally. **True.** My review has considerable momentum, solid support from leaders in the field of avian bioacoustics, and is nearly finished (awaiting data collected during 2015 spring/summer), but if some way is proposed to avoid a publication of my review, and these matters can be addressed in some satisfactory way without my public thrashing of the style of 'research' published by Jeff Podos, I'd be willing to consider any proposal from you.

5. I want to stop Podos from destroying any more graduate students. **Absolutely true.** I have studied the papers of only Goodwin and Moseley, and both are highly flawed, but excellently marketed. Science will be far better off without someone like Jeff training graduate students to work as he has.

6. I fret over abuse and waste of tax-payer money. **Not so much.** But I think that UMass would fret about this matter considerably, given that Podos has used over a million dollars of federal monies in these pseudoscientific endeavors.

7. This is a long-standing personal vendetta against Jeff Podos. **NONSENSE. FALSE.** I could send you my 2004 intra-departmental letter to Jeff in which I encourage him to do "science" rather than "marketing and advertising." There's nothing personal about that, but our friendly

relationship stopped with that review, as he's never communicated with me since. *This is about science. Period.* It's about people like Jeff systematically and relentlessly undermining a field of endeavor that I cherish, i.e., learning what birds actually do. It is my opinion that we would know more about what birds actually do if Jeff Podos never published a 'scientific' paper. And, if this were really a personal vendetta against Jeff, I would have followed the demand of the UMass police (and presumably Podos) and told all 50 people with whom I was communicating that all of them were also liable for criminal harassment charges if they attempted to communicate with Podos about his science; it was at that point that I told Officer Liptak of the UMass police that I would not comply with her demand, because following her orders would have made Podos appear as a complete fool to a large international audience.

I would appreciate your acknowledging that you have received this letter. With no response from you, I will proceed to address these issues in the manner I can best devise (a line that I repeatedly told Podos).

And if you want to invite both Jeff and me to your office for a no-holds-barred dialogue, that would be fine with me. I think that is the only way you are going to come to understand the breadth of the problem here.

Sincerely . . . Donald Kroodsma

An Accounting

Each of us as scientists has our own standards by which we measure the ethical and scientific behavior of others. My accounting for Goodwin and Podos, and especially for Podos alone based on his other publications as well, would be swift and severe, as they undermine not only science as a way of knowing but also the public trust in scientists of all fields, from scientists who study birdsong to scientists who study climate change. In my accounting, I'd ask for the following:

1. A full retraction of Goodwin and Podos (2014, 2015) in *Biology Letters*.
2. The Association of Field Ornithologists retracts its best student paper award to Goodwin, for her oral presentation of Goodwin and Podos (2014) at their 2014 meetings in Rhode Island.
3. All papers published by Podos are now questionable, not only for their obvious flaws, but also for practices in data manipulation or selection and analysis that cannot be detected; until Podos himself convinces scientists that some of his papers qualify for the test of "utter honesty" and "scientific integrity" that Feynman advocates, there would be a moratorium by all responsible scientists for accepting and citing these papers at face value.
4. The Animal Behavior Society, if it is a serious society that wishes to promote the scientific study of animal behavior, requests (or demands) that president-elect Jeff Podos resign that position.
5. The million or so dollars of tax-payer money that Podos has already spent on this "research" and graduate student training has done enough damage, and the Grants and Contracts Office at the University of Massachusetts, Amherst, should restrict all future grant applications from Podos until he has been rehabilitated as a scientist.
6. Graduate students in the Organismal and Evolutionary Biology program at the University of Massachusetts, and especially those under Podos' training, should use these documents (including Kroodsma in prep a, b) as a case study in how not to

	<p>conduct oneself professionally. And no students would be allowed to train with Podos unless they are co-advised by another faculty member.</p>
7/10/2015	<p>Dean Goodwin to Kroodsma</p> <p>I have received your letter and will look at it today.</p> <p>steve -- Steve Goodwin Dean, College of Natural Sciences University of Massachusetts, Amherst</p>
7/13/2015	<p>Biology Letters to Kroodsma</p> <p>Dear Don,</p> <p>Thanks for sending your letter through. We will be in touch again once we have considered your new comments.</p> <p>Before contacting us have you been in touch with the authors about these new concerns?</p> <p>Best wishes, Raminder</p>
7/13/2015	<p>Kroodsma to Biology Letters</p> <p>Hello Raminder:</p> <p>Thank you for acknowledging receipt of my letter.</p> <p>You ask if I've contacted the authors Goodwin and Podos. I would like to talk to the authors about this, and have made repeated attempts (8, to be exact) to do so, but they will never communicate with me. Using the University of Massachusetts police, the authors have threatened me with criminal harassment charges if I try to communicate with them about their work. The situation is beyond bizarre, but it is clear that they will not talk about their science, which to me is only one of many unpardonable, unethical offenses they are committing.</p> <p>You, on the other hand, are free to send to them what I have sent to you, as I spell out in my letter.</p> <p>Thank you.</p> <p>Kind regards . . .Don</p>
7/16/2015	<p>Kroodsma to Biology Letters, Dean Goodwin at UMass</p> <p>Hello Raminder:</p> <p>Thank you for acknowledging my email. Please note that Goodwin and Podos have not seen my letter to you because they threaten me with criminal harassment; I would gladly copy to them anything I have written about them, including this email to you.</p> <p>Perhaps I need to be rather blunt in my assessment of this situation: In my opinion, there is serious ethical and scientific misconduct in Goodwin and Podos (2014, 2015), and the paper should simply be retracted. It is not science, and Biology Letters should rise above these kinds of pseudoscientific publications.</p>

	<p>1) For starters, the authors knowingly and intentionally omit reference to two key biological facts that would fatally undermine their story. And those facts were published by another graduate student in the same graduate program, in the same university department, on the same study populations, so these are not obscure, unknown facts.</p> <p>2) Second, the authors discard 2/3 of their data and don't mention that in the publication; they got no statistically significant relationships with those data (data discarded were on 1) "performance" as measured by "vocal deviation" and 2) frequency bandwidth), so the authors reported only on the 1/3 of the data where they got a statistically significant result. To quote a recent paper on this topic: ""The omission of nonsignificant results from publications is undesirable for both scientific and ethical reasons."</p> <p>3) Third, the binomial statistical test that was reported on the retained data was done wrong, and was not at all significant. Even when tutored by Ackey and Beecher about how to do the test properly, Goodwin and Podos did the test wrong again in their 2015 rebuttal, again squeezing out another seemingly statistically significant result. One can perhaps attribute this inability to do a test properly to incompetence, but an explanation of intention seems just as likely, given items 1 and 2 above.</p> <p>4) and a multitude of other problems plague this study as well, some addressed in the critique by Ackay and Beecher, but I'll stop (for now) at the above 3 issues.</p> <p>I look forward to resolving these issues with Biology Letters.</p> <p>Kind regards . . . Donald Kroodsma</p>
7/16/2015	<p>Biology Letters to Kroodsma</p> <p>Hi Don,</p> <p>I just wanted to acknowledge this email.</p> <p>Thanks for letting us know about the situation and that the authors have not seen these comments from you. We'll be in touch.</p> <p>Best,</p> <p>Raminder</p>
9/2/2015	<p>Kroodsma to Biology Letters</p> <p>Hello Raminder:</p> <p>I last communicated with you on July 16 about Goodwin and Podos (2014), but I have not heard back.</p> <p>Could you please update me as to where these matters stand?</p> <p>Thank you.</p> <p>Kind regards . . . Don Kroodsma</p>
9/17/2015	<p>Association of Field Ornithologists to Kroodsma</p> <p>Hi Don, I sent an e-mail to you immediately after the AFO meeting in Nova Scotia and I'm</p>

	<p>sorry if you did not receive it. We appreciate you keeping us updated on the status of this issue. At the meeting, I provided the council with a copy of the letter you sent to Biology Letters, the original Goodwin-Podos paper, as well as the commentary by AcKay and Beecher and Goodwin and Podos' response. Bernie Lohr, one of our Council members noted that he had previously worked in Steve Nowicki's lab, but was asked to provide some background on the cast of characters. The issue was discussed in the context of the AFO having awarded Sarah Goodwin a presentation award based on the paper in question. A general discussion by the Council of this issue followed. The consensus of the group was that from the materials provided, we could find no evidence of malfeasance on the part of Goodwin and Podos and that this appeared to be a normal disagreement among scientists in methodological approaches and the interpretation of results. As a result, a motion was put forth that the 2014 AFO Best Student Presentation Award to S. Goodwin stand as awarded and that motion was unanimously approved by Council. Council did note that the decision of Biology Letters would be critical to further consideration of this issue. At the time of our evaluation, no decision had yet been made. Should Biology Letters formally retract this publication, we would certainly revisit this decision.</p> <p>Sincerely</p> <p>Reed Bowman</p> <p>President, AFO</p>
9/18/2015	<p>Biology Letters to Kroodsma</p> <p>Dear Donald,</p> <p>Thank you for your detailed correspondence on this matter. We take all complaints seriously and have investigated your concerns. We note that the authors' institution have conducted a thorough independent investigation into this matter and deemed this to be a difference of scientific opinion, of the kind that is very common in this field and across all scientific research. We are satisfied with the rigour and findings of this institutional investigation and therefore will not be taking any further action on this occasion.</p> <p>Many thanks and best wishes,</p> <p>Surayya</p>
9/18/2015	<p>Kroodsma to Biology letters</p> <p>Hello Surayya:</p> <p>Could I ask you to please share with me this investigation, so that I can see what the findings were?</p> <p>Thank you.</p> <p>Regards . . . Don Kroodsma</p>
9/22/2015	<p>Biology Letters to Kroodsma</p> <p>Dear Donald,</p> <p>Thank you for your reply. Dean McCarthy should be able to provide these findings accordingly.</p>

	<p>Many thanks and best wishes, Surayya</p>
<p>9/23/2015</p>	<p>Kroodsma to Dean McCarthy, Biology Letters</p> <p>Dear Dean McCarthy:</p> <p>As you perhaps know, I am an emeritus professor in your Organismal and Evolutionary Biology graduate program.</p> <p>Biology Letters has informed me that you supplied them with information that led them to dismiss my concerns about the research of Jeff Podos. I asked them for a copy of your report, but they have referred me to you (see copy of email below).</p> <p>Could you please forward to me a copy of what you sent to Biology Letters?</p> <p>Thank you.</p> <p>Kind regards . . . Don Kroodsma</p>
<p>9/24/2015</p>	<p>Dean McCarthy to Kroodsma and Biology Letters</p> <p>Dear Dr. Kroodsma,</p> <p>I did not send anything to any journals, nor could I have done so legally. My communications with students, when I act in my capacity as Dean of the Graduate School, are student records covered by the Family Educational Rights and Privacy Act (FERPA).</p> <p>John McCarthy</p> <p>Senior Vice Provost for Academic Affairs and Dean of the Graduate School</p> <p>Distinguished University Professor</p> <p>http://works.bepress.com/john_j_mccarthy/ 413-545-5271</p>
<p>9/24/2015</p>	<p>Kroodsma to Dean McCarthy, UMass Graduate School; and Biology Letters</p> <p>To John McCarthy, Dean of the Graduate School at UMass:</p> <p>Thank you for your reply. I am clearly confused about a communication that Biology Letters received from a "Dean McCarthy" at UMass. I think there's only one Dean McCarthy at UMass, but perhaps not. I'm sorry to have taken your time on this. I hope Biology Letters can help clarify the confusion.</p> <p>Kind regards. . . Don Kroodsma</p> <p>To Surayya, at Biology Letters:</p> <p>I am confused, and a little embarrassed that I bothered Dean McCarthy. I somehow misunderstood your email to me, about your statement that "Dean McCarthy should be able to provide these findings accordingly." He apparently knows nothing about this situation, and did not communicate with Biology Letters.</p> <p>Please clarify how I can get a copy of the correspondence that you received.</p>

	<p>Thank you.</p> <p>Kind regards . . . Don Kroodsma</p>
<p>9/30/2015</p>	<p>Kroodsma to Biology Letters</p> <p>Hello Surayya:</p> <p>I am of course eager to obtain a copy of the University of Massachusetts institutional report regarding Goodwin and Podos (2014). To learn that Dean McCarthy knew nothing about this report, when you seemed to think that it came from him, is more than a little intriguing. Could I ask you to please clarify from whom this report came, or perhaps send me the report yourself?</p> <p>Thank you.</p> <p>Kind regards . . . Don Kroodsma</p>
<p>9/30/2015</p>	<p>Biology Letters to Kroodsma</p> <p>Dear Donald,</p> <p>Thank you for your email, and apologies for the delay in responding and any confusion caused. The report findings were sent to me by the authors and this was signed off by the university review panel who investigated the case.</p> <p>Following on from the email below and reading over the Dean's reasons for not sending on this information, I cannot send a copy of the findings on to you. I understand that this may be frustrating but I am conscious of the rules as laid out by the university. We have scrutinised the situation very carefully and are satisfied with the results of the investigation, and hope you understand our position on this matter.</p> <p>Many thanks and best wishes,</p> <p>Surayya</p>
<p>9/30/2015</p>	<p>Kroodsma to Dean McCarthy, Biology Letters, Dean Goodwin</p> <p>Dear Dean McCarthy (copy to Biology Letters, and Dean Goodwin):</p> <p>I am confronted with a most confusing, not to mention frustrating, set of circumstances.</p> <p>Here is a small bit of the history:</p> <p>With Biology Letters, I have exchanged emails over the past half year or so, pointing out that one of their published papers, that by Goodwin and Podos (2014), simply cannot be true, for a variety of reasons.</p> <p>I told Biology Letters that they could forward any of my correspondence directly to Jeff Podos, with the understanding that Biology Letters would then share with me any response that they received. (I could not send anything directly to Podos because he has threatened me with criminal harassment charges if I try to communicate with him—a most bizarre circumstance in itself.)</p>

Podos in turn supplied Biology Letters with the results of an “*institutional investigation,*” which Biology Letters summarized as follows: “*the authors’ institution have conducted a thorough independent investigation into this matter and deemed this to be a difference of scientific opinion, of the kind that is very common in this field and across all scientific research. We are satisfied with the rigour and findings of this institutional investigation . . .*”

When I asked Biology Letters for a copy of this letter, they referred me to a “Dean McCarthy”: “*Dean McCarthy should be able to provide these findings accordingly.*”

When I asked you for a copy of the findings, you wrote the following: “*I did not send anything to any journals, nor could I have done so legally . . .*”

So I returned to Biology Letters and asked them to help me understand what is going on. Their response: “*The report findings were sent to me by the authors and this was signed off by the university review panel who investigated the case . . . Following on from the email below and reading over the Dean’s reasons for not sending on this information, I cannot send a copy of the findings on to you . . . [as] . . . I am conscious of the rules as laid out by the university.*”

There are a host of comments I could and should make about the above, but I’m going to simply ask if you can get to the bottom of this and send to me a copy of this secret investigation, or authorize Biology Letters to do so.

Thank you.

Kind regards . . . Don Kroodsmas

9/30/2015

Kroodsmas to Bernie Lohr, and Association of Field Ornithologists

Hello Bernie:

I have a challenge for you.

AFO apparently relied on your assessment to relegate the issues I raise to matters of "normal disagreement" among a "cast of characters" (copy of Reed's email below). I find that conclusion unsettling, about as unsettling as the claims that I hear about the earth being flat, or the climate not changing--both matters of opinion, one might say (though who else cares about birdsong?).

Given your role in this decision, especially given that you have spent time in the Nowicki lab and might therefore be expected to be an ally of both Nowicki and his student Podos, I am going to offer you a challenge. Attached is a partial manuscript that addresses Goodwin and Podos (2014), together with figures in a second attachment.

In the Summary, I ask that anyone who defends Goodwin and Podos (2014) as science must explicitly address several matters. Here's the challenge for you: To address those matters and still conclude that Goodwin and Podos (2014) is science and not marketing and advocacy.

You are welcome to share this document with Podos himself. I would do so, but his University Police prevent me from doing that. Get his help in defending his work. I cannot get a peep out of him.

Yes, I am somewhat relentless on this matter. I cherish knowing what birds actually do, the little discoveries about how the world works. I abhor the pseudoscience on birdsong that markets novel, exciting, cutting-edge claims with no truth behind them.

	<p>regards . . . don</p>
<p>10/1/2015</p>	<p>Kroodsma to Bernie Lohr and AFO representatives Hello Bernie (copy to relevant AFO officers):</p> <p>I have been called many things, and persistent is one of them (inconvenient, and a nuisance are cousins to persistent). Yesterday I offered you a challenge with a relatively long manuscript, but I'd like to simplify that challenge. Attached is a far shorter manuscript focused on just one reason why Goodwin and Podos (2014) is entirely false. My manuscript is based on science, not on a cast of characters who have differences of opinion about how science is done.</p> <p>If you still think that it is just a matter of opinion, as Reed summarized, I would simply like to have you tell me so. As before, you are welcome to share this document with Podos himself. I dare say the only way you will get a response from Podos, however, is if you tell him that AFO is considering retracting their best student paper award—but I realize that you could not do that without the blessing of AFO council, of course.</p> <p>Podos has gone to great lengths to avoid responding to these scientific issues (he knows, and I know, that his career and credibility are on the line, as Goodwin and Podos is only the culmination of a series of such papers on his performance hypothesis). First, he engaged the University Police to try to silence me, by threatening me with criminal harassment charges. Then he sent the results of an “institutional investigation” (“independent, thorough, rigorous,” according to <i>Biology Letters</i>) to <i>Biology Letters</i>, again closing off all lines of communication; the findings of this investigation are top secret and they cannot be shared with me. Never has Podos communicated with me directly, in spite of my repeated attempts to establish a dialogue.</p> <p>In the interest of science, I would like to see Podos pressed on these matters. He is president-elect of the Animal Behavior Society, for example, which has a strong ethical statement on its website about authors corresponding about their work. AFO was duped into awarding Goodwin and Podos one of its top honors, and AFO therefore, in my opinion, has some responsibility as a credible scientific organization to inquire into matters like this.</p> <p>Kind regards . . . Don Kroodsma</p> <p>Attached: <i>Goodwin and Podos (2014) refuted in One Figure</i></p>
<p>10/1/2015</p>	<p>Bernie Lohr to Kroodsma</p> <p>Hi Don,</p> <p>I'm happy to give you my version of what transpired at our AFO Council meeting this past summer. Reed can confirm or elaborate on any of this if my memory isn't completely accurate.</p> <p>First, I admitted at the start of the Council discussion, in full disclosure, that I knew Jeff Podos and had been a fellow grad student in the Nowicki lab. As I'd spoken with Jeff about the Ackay and Beecher critique, and was familiar with the song performance literature (to a certain extent - I haven't published articles in that context myself, nor have I reviewed any, to the best of my recollection), I was asked to</p>

provide some background on the topic. It's an overstatement, however, to say that the AFO relied on my assessment to form their decision. My comments were part of a longer discussion that revolved primarily around the material we'd been provided with in our packet not too long (a week or so?) before the meeting. Other members of the Council, I'm sure, formed their own impressions based on the overall discussion and material we had in front of us.

My own discussion with Jeff, roughly a year or so earlier, involved much of what was published in the exchange with Ackay and Beecher. I had heard Jeff's side of the story, and relayed it at the meeting, but it should also have been obvious from his rebuttal. At no time did he mention you to me in our discussion, or that you might have concerns about the article as well. There was clearly some history there (which was obvious from the contents of your letter), but I was unaware of your disagreement with Jeff until we got the packet of material for our Council meeting. I mentioned at our discussion that it was inappropriate of me to comment on your interactions with Jeff (or attempts to interact with him) as I knew nothing about that.

The question before us was one of retracting a student award that had been given the previous year. I personally disagree that retracting a student award would be an appropriate way to get a response from Jeff on this issue (as I said, others formed their own views on whether that would be appropriate). Whatever the result of this disagreement, and however his reputation might suffer if he's shown to be wrong on the issue, Jeff's job isn't in jeopardy - he's a tenured professor. Sarah Goodwin on the other hand is a PhD student, just getting started in a career.

That said, you raise some good points in your critique about the way in which chipping sparrows learn song, and about their behavior more generally. I agree for example that tests of song performance are more appropriately made with species that either don't learn their vocal signals, or learn by improvisation or innovation rather than imitation. I would ask (in terms of the second figure you, sent which shows relatively faithful copies of a specific neighbor by young birds), how chipping sparrows decide which of their neighbors they'll copy, and whether that might be related to that male's song performance? (with the understanding that song "performance" might be measured in a number of different ways). All questions I would be curious about, but all that is to say that I haven't studied chipping sparrow song or behavior myself. I'm not qualified to decide whether the Goodwin and Podos work is "fiction." As far as I know, however, (and this was a conclusion drawn independently by other members of the Council as well), there was no malfeasance regarding their data. I guess I take the longer view that if Goodwin and Podos' interpretation of their data wasn't correct, or their conclusions didn't adequately account for what's known about the species, that the truth about that situation will be revealed in time.

I'm wondering if the appropriate venue for your views wouldn't be the literature, as I think it would generate a vigorous and interesting discussion. Why not publish your article (like Ackay and Beecher did)? That's certainly something you've done before. I remember the

pseudoreplication debate as a young grad student. You were right about that, and I don't know anyone now who doesn't take pains to replicate their playback design appropriately. Likewise, Byers and Kroodsma 2009 is widely cited, I think, in papers that address the evolution of large repertoires, even if there continues to be a debate about that issue. (And to be honest, I likely put more weight on the value of laboratory choice tests with females than you do in that context, though I agree that it's problematic at best to extrapolate to female preferences in the field). It sounds as if you have similar criticisms of the song performance literature. Jeff and others, depending on the scope of your critique, would likely feel compelled to respond there. Perhaps a broader review of the issues surrounding the song performance literature would be the appropriate place for your specific criticisms of the Goodwin and Podos work?

Regards,

- Bernie

10/2/2015

Kroodsma to Bernie Lohr, and AFO

Hello Bernie:

Thank you for your thoughtful response.

There is a larger picture that I haven't conveyed. I have reviewed all of the performance literature, in excruciating detail, and have a lengthy manuscript on it all. The rather inevitable conclusion is that there is nothing there, and that it is promoted relentlessly by all manner of pseudoscience. My manuscript may eventually see the light of day, but it will face stiff opposition because of its message.

I called Podos on his style of 'research' nearly 11 years ago, when I described to him the difference between science and marketing: "Science is the search for truth, regardless of how good the story is; marketing and advertising is the search for a good story, regardless of the truth" (October 2004). Podos is marketing, putting it simply, searching for the good story, the hook that will get attention. He has refused to communicate with me ever since, now even resorting to using the University Police to threaten me with criminal harassment charges, and hiding a university investigation into his work, provided to Biology Letters, that remains top secret—I am not allowed to see it.

So, when I look at Goodwin and Podos (2014), and the award given by the AFO, given the larger context it was for me the last straw, because that paper is the culmination of a long history of marketing the performance hypothesis. There simply is no truth in that paper, and to see AFO duped to honor it a major award is disheartening. Below is my logic in refuting the paper, the quotes taken from my updated version of "Goodwin and Podos (2014, 2015) Refuted in One Figure":

Discussion

1) The trill rate of a male is determined by the song of his adult tutor, *not* by his relative prowess or "performance ability" (sensu Podos 1997).

2) There is no evidence for song learning in any songbird species or especially in chipping sparrows (Liu and Kroodsma 1999, 2006) that a male is in any way limited in what naturally occurring trill rate he can learn.

3) The trill rates of ~7 syllables/second for birds 1A and 1B (Figure 1) and trill rates of ~25

	<p>for birds 14A and 14B were determined by where the males settled on their first territory and do not reflect a measure of male quality.</p> <p>4) Goodwin and Podos (2014, 2015) omit all reference to how chipping sparrows actually acquire their songs (Liu 2001; Liu and Kroodsma 1999, 2006) and instead falsely assume that a chipping sparrow acquires a song with a trill rate that honestly conveys his performance ability and his overall quality (in line with Podos 1997).</p> <p>[5) Goodwin and Podos (2014, 2015) also omit all reference to how male chipping sparrows display competitively in lek-like arenas well off their own territories (Liu 2004, Kroodsma 2007), and instead invent cooperative alliances and coalitions to explain any gathering of singing males.]</p> <p>Conclusion: <i>There is no truth in Goodwin and Podos (2014, 2015). There are no precise assessments of mating signal features, no teams of rivals, no alliance formations, and no coalitions.</i></p> <p>The good story is created by ignoring the basic biology of the study animal (items 4 & 5). The good story is also created by discarding all analyses that don't produce a $p < 0.05$ on which the good story can be built, and even then the statistics that produce the 'good' p value are done wrong.</p> <p>As for Biology Letters addressing these issues, there is a long history there as well. They have in hand an "institutional investigation" that no one will share with me ("secret," per "university rules"), and the findings of that investigation are "independent, rigorous, thorough," submitted by Podos, signed by a university review panel. Publishing a critique of Goodwin and Podos there is not possible.</p> <p>I had simply asked AFO to address the science of this matter, and if the science is wanting, and there is no truth in it, I had asked AFO to consider retracting the award. To declare that it is just a matter of opinion as to whether the paper is true or not is ducking the issue, and I will challenge anyone who relies on that simple-minded solution to avoiding the issues.</p> <p>I will go away now, but probably not for as long as you or others would like.</p> <p>Kind regards. . . Don</p>
10/17/2015	<p>Dear Surayya, for Biology Letters (copies to University of Massachusetts, Amherst: Dean Steve Goodwin, Dean John McCarthy, Rolf Karlstrom as Chair of Biology Department, and Elizabeth Jakob as officer of Animal Behavior Society and faculty member in Organismic and Evolutionary Biology Graduate Program):</p> <p>When I describe to my friends, both scientists and nonscientists, the circumstances surrounding Goodwin and Podos (2014, 2015), they are dumbfounded. For everyone, it is inconceivable that 1) the authors refuse to communicate with me or others about their work; that 2) instead they use the university police to threaten me with criminal harassment charges for trying to talk to them about their research; and that 3) they had a special university review panel submit a secret report to Biology Letters in their support. Secret! I am not allowed to see it, according to "university rules." Everyone is incredulous, because everyone knows that's not the way science is supposed to be done.</p> <p>As for the University of Massachusetts special review panel that exonerated Goodwin and Podos, it does remain top secret. Dean Goodwin and Dean McCarthy at the University are unresponsive, and there's no way for me to know what has transpired. Let me briefly address</p>

that review and the panel here:

In addressing Goodwin and Podos (2014), my claims are quite simple:

Goodwin and Podos 1) knowingly 2) omitted reference to two, known biological facts about chipping sparrows 3) that would have fatally undermined 4) their story. (There are many other problems with that publication, but I will keep this simple.)

To exonerate Goodwin and Podos, the review panel would have to address the following issues:

1) *knowingly*: The university panel would have to claim that Goodwin and Podos did not know about the Ph. D. thesis and the research published in mainstream journals by another graduate student in their own Biology Department who worked on the same population of chipping sparrows that they did.

2) *omitted reference to two, known biological facts about chipping sparrows*: It is a fact, not easily refuted by any review panel, that there is no reference in Goodwin and Podos to the two known facts, on a) how chipping sparrows actually learn their songs from an adult tutor (trill rate cannot reflect male quality as assumed in Goodwin and Podos), and on b) how the birds use those songs competitively (not cooperatively as assumed by Goodwin and Podos) outside their daytime territories. References to these known facts are supplied in my attached document (“No team of rivals or coalitions in territorial sparrows”).

3) *would have fatally undermined*: All claims of Goodwin and Podos are false, because they are entirely at odds with what chipping sparrows actually do. For one of the known facts (on song learning), I explain fully in the attached document (“No team of rivals or coalitions in territorial sparrows”).

4) *their story*: Goodwin and Podos (2014) is truly a story, with no truth in it. By definition, it is “fiction” (definition from Merriam-Webster: “written stories . . . that are not real: literature that tells stories which are imagined by the writer”). How the story was generated is another matter, best left to another venue (addressed in part by Akcay and Beecher 2015). Goodwin and Podos (2014) was for me a “last-straw” story, as I had read many others over the years (excerpts from a longer review are in the attached document, “Podos and Performance Studies”), and I finally decided to commit the effort to debunk it; little did I know the resistance I would face in doing so.

5) Furthermore, the review panel would need to dismiss as irrelevant the extreme efforts the authors have taken to avoid discussing their research (see attached file entitled “Podos and Criminal Harassment”). Using police to threaten criminal charges does not promote the kind of open dialogue that most scientists expect when searching for truths about how the world works. In fact, many scientific societies, such as the Animal Behavior Society (where Podos is, ironically, president-elect), explicitly address these matters in their “Ethics in Publishing” statement to authors:

Animal Behaviour publishes papers by scientists conducting research at locations around the globe . . . Professional integrity in the conduct and reporting of research is an absolute requirement of publication in the journal, as is a willingness to share information with other members of the scientific community. Consequently, as a condition of publication in Animal Behaviour,

authors must agree both to honour any reasonable request for materials or methods needed to verify or replicate experiments reported in the journal and to make available, upon request, any data sets upon which published studies are based. Anyone who encounters a persistent refusal to comply with these guidelines, or has reason to suspect some other departure from acceptable standards of scientific conduct, should contact the appropriate Executive Editor (European or American) of the journal. The Executive Editors will act in accordance with the guidelines of the Committee for Publication Ethics (<http://www.publicationethics.org>) and may inform an author's institution of a purported infraction. Statements on scientific integrity by the Association for the Study of Animal Behaviour and Animal Behavior Society can be found at, respectively, <http://www.asab.org> and <http://.animalbehaviorsociety.org>.

I challenge Dean John McCarthy, Dean Steve Goodwin, or anyone on the secret review panel to address these matters directly and openly, and tell us all that this is the way science is done at the University of Massachusetts. I realize that my message is rather inconvenient, as I address the scientific credibility of the head of one of their graduate programs at the university, one of their stars, and the president-elect of the Animal Behavior Society. But science isn't about convenience; it is about seeking truths about the natural world. The credibility of the review panel, and the University itself, is at stake here, and the word "cover-up" comes to mind; anyone who objects to that characterization is welcome to communicate with me and have a brief conversation about what is at stake here (I'm at 413-247-3367). I have always been open about my concerns, allowing Biology Letters to send any of my communications directly to Podos himself (he is protected by police from my direct communication), with the understanding that Biology Letters would facilitate a dialogue; instead, however, "university rules" demand a one-way communication, that anything coming back from Podos is secret. The continuing effort to stifle all scientific discourse is extreme. The obvious question one must ask, of course, is "Why?"

Rather than try to answer that simple question, may I please ask you, Surayya, for Biology Letters, and in the interest of science alone, to rise above all that and reconsider your offer of 2 March this year? You wrote the following:

. . . you may wish to submit an online eletter. These are published alongside the online article and are more informal than comments. They are moderated in-house, sometimes with advice from the journal editorial board.

Using a single figure, and minimal text, I can show that the published articles by Goodwin and Podos (2014, 2015) are not true. I ask that you please consider publishing the attached as an online eletter ("No team of rivals or coalitions in territorial sparrows").

Thank you for your consideration.

And, as I have in the past been completely open about my concerns, you are welcome to send this document directly to Podos and the University of Massachusetts. I will in fact copy the two Deans on this letter, and two other university personnel whom I suspect may have been part of the review panel (chair of the Biology Department, and a fellow officer of the Animal Behavior Society and member of the Organismic and Evolutionary Biology Graduate Program of which Podos is head).

Kind regards . . . Donald Kroodsma

10/22/2015	<p>Hello Surayya:</p> <p>Some day we will stop meeting like this; nothing personal, but I hope it's sooner rather than later.</p> <p>Thank you for telling me about the continuing problem with eLetter. I can be patient. Regarding Goodwin and Podos (2014), I am determined that science will prevail (understatement), and I would very much like Biology Letters to be part of the solution.</p> <p>I should add a note here about the critique written by Akcay and Beecher (2015). I first alerted Beecher to the problems in Goodwin and Podos (2014), after which Beecher and his student submitted a critique to Biology Letters, without telling me (or acknowledging me). Hence my surprise in your and my correspondence earlier this year about who could possibly have submitted a critique before I did. Akcay and Beecher, unfortunately, devoted their critique to relatively unimportant matters (e.g., were the birds banded?), and didn't address the issues that unequivocally render Goodwin and Podos untrue. The weak critique allowed Goodwin and Podos to restate their false claims, in the guise of "this is just a small matter of difference in scientific opinion." That then is also the conclusion that you came to after receiving the secret report from the University of Massachusetts.</p> <p>Regarding the institutional review, it seems most suspicious that Dean McCarthy at the University knows nothing about this review exonerating Goodwin and Podos (2014), yet you were under the impression that I could get a copy of the report from him. In fact, I wager that Podos himself wrote the institutional review. And why any institutional review would be necessary at all is a puzzle. Why can't Podos just respond directly to my questions about his work?</p> <p>You and others at Biology Letters must also be more than a little puzzled by the use of university police and criminal harassment charges to protect Goodwin and Podos from inquiry about their work; the police in fact told me that I must tell the ~50 people I was corresponding with that none of them were allowed to contact Goodwin and Podos either. The blatant attempts at intimidation and secrecy are, to me, signs of desperation.</p> <p>Perhaps you could allow me two questions:</p> <ol style="list-style-type: none"> 1. Could you give me the name of another person from the University of Massachusetts who might be able to provide me a copy of the institutional investigation, or at least talk to me about it? 2. Would you, given all of the circumstances surrounding Goodwin and Podos, consider publishing my contribution as a regular critique instead of an eLetter? Then we could be done with all this, and I will go away! <p>Thank you.</p> <p>Kind regards . . . Don Kroodsmma</p>
10/23/2015	Dear Donald,

	<p>I'm afraid that I don't have another name from the university that could assist you with this. In addition, we have a policy of publishing only one comment on an article in Biology Letters and so cannot consider a critique from you. I do understand this is not ideal but I do hope you understand the journal's position in this matter.</p> <p>Many thanks and best wishes,</p> <p>Surayya</p>
10/23/2015	<p>Hello (again) Surayya:</p> <p>Thank you for your response. In spite of my persistence on this matter, your replies have always been courteous and considerate and professional, and I thank you for that.</p> <p>Yes, I know of and understand the policy of only one comment on an article, but given the (rather extraordinary) circumstances, I was hoping that maybe you'd accept a second comment.</p> <p>My claims are about as serious as they come in science: The authors have fabricated a story by inventing (two) biological traits for their subjects that are well known not to exist. Then, without telling readers, they discard data that don't tell the story, selectively reporting only a few statistically significant tests gleaned from all possible tests, and do the statistics wrong, both initially (2014) and repeatedly after their errors have been pointed out (2015). The consequences for the scientific literature are the same as if the authors had fabricated data from the outset, i.e., a literature of mistruths.</p> <p>As I understand your email, the only name on the institutional review is that of Dean McCarthy, who claims to know nothing about any review. And the review was sent to you by the authors, or by Podos alone. The intrigue about this "independent" review deepens.</p> <p>May I please ask one last question, and then I will go away for a while:</p> <p>When you write "I don't have another name from the university that could assist," I take that to mean that the only name on the institutional review is that of Dean McCarthy. Could you please confirm my interpretation?</p> <p>Thank you.</p> <p>Kind regards . . . Don Kroodsma</p>
10/29/2015	<p>Dear Dean McCarthy:</p> <p>For nearly a year and a half, I have been trying to achieve some resolution on a scientific matter with Jeff Podos in the Department of Biology. I have met many forms of resistance, from stonewalling, intimidating threats of criminal harassment from UMass police, and multiple charges against me behind my back (e.g., personal vendetta, attacking graduate</p>

	<p>students).</p> <p>I have been open and honest in all of my communications, allowing everything I write to be forwarded directly to Podos (I can't send anything to him directly because of his threats of criminal harassment) and repeatedly asking for a dialogue to settle these matters.</p> <p>In contrast, every response from Podos is kept secret, with nothing ever coming back to me. The latest top secret communication is an "institutional review" document that Podos has submitted to the journal Biology Letters with apparently only your name on it, but because of "university rules" I am not allowed to see this document.</p> <p>May I please have your help in understanding what is going on here? Your earlier email to me suggested that you knew nothing of this secret review (24 Sept: "I did not send anything to any journals"), even though your name is on the review. Who, then, did write this review? And why the great secrecy? What kind of university rules would these be that must keep such a document secret and prohibit open scientific discourse?</p> <p>It would be very helpful if I could have some kind of a response from you by Monday noon (November 2) before I meet with the Vice Chancellor for Research.</p> <p>Thank you.</p> <p>Kind regards . . . Donald Kroodsma donaldkroodsma@gmail.com; 413-247-3367)</p>
10/29/2015	<p>Dear Dean Goodwin:</p> <p>Other than acknowledging you received a letter from me ("I have received your letter and will look at it today"—July 10, 2015), you have been silent on the issues I am trying to address.</p> <p>Below I copy a letter that I just sent to Dean McCarthy. If you would like to offer any thoughts on this matter before I meet with the Vice Chancellor for Research, I would welcome them as well.</p> <p>Thank you.</p> <p>Kind regards . . . Don Kroodsma</p> <p>Response from Dean Goodwin Don, I am not in a position to do any more on this issue. Respectfully, Steve</p> <p>Response from Kroodsma Hi Steve: Thank you for your response. The "any more" suggests that you've done something, but I suspect that you can't tell me what that is either. I will continue to pursue this matter without your help. kind regards . . . Don</p>

11/2/2015

Kroodisma to Vice Chancellor for Research

To: Dr. Michael F. Malone, Vice Chancellor for Research & Engagement

From: Donald Kroodisma, Professor Emeritus, Biology

Re: Scientific and Ethical Misconduct; Professor Jeff Podos, Biology

For almost a year and a half, since May 2014, I have been trying to address what I feel is scientific and ethical misconduct by Dr. Jeff Podos, Biology Department and Organismal and Evolutionary Biology Graduate Program. My efforts have been thwarted by stonewalling, by intimidation (UMass police threatening me with criminal harassment charges), by an "institutional review" that I am not allowed to see (secret because of "university rules"), and whatever means Podos has had at his disposal to avoid dealing with me and his work directly.

I come to the Vice Chancellor for Research as a last resort, after all other avenues to settle this matter have been exhausted (details below). Most recently, the following attempts have failed:

2 November 2015. From Biology Letters: "We consider the matter now closed."

2 November 2015. Dean McCarthy. No response by the date I had asked for a response, if one was forthcoming.

29 October 2015. Dean Goodwin. "I am not in a position to do any more on this issue."

I do not take this matter lightly, as will be abundantly evident below, because I feel the methods used by Podos are an affront to scientists and science everywhere, undermining our collective integrity.

And I care first hand because Podos makes a mockery of a field of science that I value enormously, the study of birdsong. To make matters worse, Podos trains another generation of graduate students in the same techniques of what can best be called "pseudoscience." I realize that these are strong words, but I believe entirely warranted. I elaborate in this document and the attachments.

In studying the "Procedures for Dealing with Charges of Misconduct in Research and Scholarly Activities at the University of Massachusetts Amherst," I was struck by the following quotes, which are particularly relevant for the case I address (emphases mine):

Misconduct in research and scholarly activities is injurious to the University's teaching, research, and public service missions and cannot be tolerated.

Research and scholarly misconduct involves *misrepresentation of the procedures and outcomes of research to gain some advantage*. Misconduct may often be difficult to separate from error or poor judgment, from which it is *distinguished by the intentions* of the person(s) involved.

1. Falsification or fabrication: This includes falsification, modification, or fabrication of data or facts, or *selective inclusion or exclusion designed to mislead or to support false conclusions*.

5. Misrepresentations in publication: This form of misconduct involves the publishing or public circulation of material *intended to mislead the reader*.

I here provide a history of what has transpired since May 2014, when I first began trying to address this situation.

May 2014. At an ornithological conference in Rhode Island, Goodwin and Podos deliver an oral paper that I immediately realize is false. In a highly polished presentation, they tell a clever, exciting, and novel story about "Teams of Rivals" in chipping sparrows, how territorial males form alliances and coalitions based on precisely assessing the trill rates in each other's songs. Goodwin won a Best Student Paper Award (as did, not coincidentally, the other Podos student in the competition—the only two awards at the scientific meeting went to Podos' only two students, a "clean sweep," boasted Podos on his website).

Goodwin and Podos create their story in large part by omitting reference to two key biological facts about their study animals. These are not obscure facts (about how the sparrows learn their songs, and where they routinely use them), but were published in mainstream journals by another graduate student in the same UMass Amherst Biology Department a few years before, working on the same sparrow populations that Goodwin and Podos worked on. Those two biological facts fatally undermine the Goodwin and Podos story (see attached; for short version, see **No Teams of Rivals or Coalitions in Territorial Sparrows**; for longer version, see **No Support for Honest Signaling in Birdsong and the Motor Constraints Hypothesis**). I would learn later that most of the data and

statistical tests for the study were never mentioned in the publication (i.e., discarded), because they were not statistically significant. Instead, only a few tests that reached statistical significance were reported, those consistent with the story of song performance as promoted by Podos since 1997 (and those statistics were done wrong—Akçay and Beecher 2015).

I now read back to the VCRE document on misconduct: “*selective . . . exclusion designed to mislead or to support false conclusions.*” Yes, precisely. Or “*misrepresentation . . . to gain some advantage.*” Yes again, at the immediate expense of every graduate-student scientist attending the meetings, and the general expense of science everywhere, and scientists in all fields of endeavor; Goodwin won a Best Student Paper Award for a story fabricated by omitting reference to the basic biology of the subject animal, inventing two traits known not to exist, and selectively excluding inconvenient data and statistical tests. I have not studied the best-student paper presented by Dana Moseley at those meetings (she also won a major award at another scientific meeting the year before), but based on other evaluation of her work with Podos (see attached, **Podos and Performance studies**) I suspect it is equally contrived and false.

I also read in the VCRE document the following:

Misconduct may often be difficult to separate from error or poor judgment, from which it is distinguished by the intentions of the person(s) involved.

If the Goodwin and Podos oral paper and publication of that paper in *Biology Letters* were an isolated event and dealt with professionally by the authors, one might give the authors’ the benefit of the doubt and attribute the matter to “error or poor judgement,” even though they knew all about the biology of the chipping sparrow that refuted their paper before it was published. But I think that the intentions of the authors become clear when one considers what has transpired since May 2014, as well as before May 2014.

Failure to communicate—“Ethics in Publishing”

Repeatedly, from July 2014 to December 2014, I tried to establish a dialogue with Goodwin and Podos (see attached document, **Podos and Criminal Harassment**). Never did I receive a reply. At one point, in case emails weren’t working, I sent a U. S. mail letter. Here are excerpts from those eight emails (full content of emails in attached document):

- 1) 9 July 2014 “I wondered if you could help me understand why in the paper you focused only on trill rate.”
- 2) 1 October 2014 “ . . . if you’d like to talk some of these things through, let me know, and I’ll come over to UMass”
- 3) 8 October 2014 “I have no idea what is in your head . . . I’m still available to talk”
- 4) 15 October 2014 “JEFF, HERE’S ONE LAST ATTEMPT TO ENGAGE YOU”
- 5) 16 October 2014 “something well worth reading, from half a century ago” (Feynman 1985, *Cargo Cult Science*; on the “utter honesty and scientific integrity” needed to do science)
- 6) 9 November 2014 “I’d welcome any dialogue with you”
- 7) 1 December 2014 (early in day) “If you want to talk about any of this . . . , I can be reached at 413-247-3367, just across the river from you. Or email me”
- 8) 1 December 2014 (late night) “I thought it appropriate to seek any feedback that cited authors or others might like to provide. A dialogue would be welcomed” [At this point, exasperated at failing to evoke any response from Goodwin and Podos, I sent my larger review out to all those whom I critiqued, an ethical requirement from the Animal Behavior Society.]

This failure to communicate is troubling. Podos is president-elect of the Animal Behavior Society, which has a strong Ethics in Publishing statement on its website (see

<https://www.elsevier.com/journals/animal-behaviour/0003-3472/guide-for-authors#5001/>):

Animal Behaviour publishes papers by scientists conducting research at locations around the globe . . . Professional integrity in the conduct and reporting of research is an absolute requirement of publication in the journal, as is a willingness to share information with other members of the scientific community. Consequently, as a condition of publication in *Animal Behaviour*, authors must agree both to honour any

reasonable request for materials or methods needed to verify or replicate experiments reported in the journal and to make available, upon request, any data sets upon which published studies are based. Anyone who encounters a persistent refusal to comply with these guidelines, or has reason to suspect some other departure from acceptable standards of scientific conduct, should contact the appropriate Executive Editor (European or American) of the journal. The Executive Editors will act in accordance with the guidelines of the Committee for Publication Ethics (<http://www.publicationethics.org>) and may inform an author's institution of a purported infraction. Statements on scientific integrity by the Association for the Study of Animal Behaviour and Animal Behavior Society can be found at, respectively, <http://www.asab.org> and <http://.animalbehaviorsociety.org>.

The president-elect of the Society is in clear and extreme violation of its own Ethics in Publishing statement.

Intimidation by Threatening Criminal Harassment Charges

As if not responding weren't sufficient, Podos engaged the UMass police force to silence me (see transcript of 18 December 2014 phone call from Officer Liptak, in the attached **Podos and Criminal Harassment**): If I attempted any more communication with Podos and his students ("UMass Biology"), I would be charged with criminal harassment. In a further email with the police, I was told to inform the ~50 international correspondents on this topic that none of them were allowed to contact Goodwin and Podos either. At that point I told the officer that I was embarrassed for her, and for Podos, and I would not inflict this kind of international disgrace on anyone.

Continuing Defense of Goodwin and Podos (2014)

Even though both I and a University of Washington group told Goodwin and Podos that we knew of the serious flaws in their study, which clearly rendered it false, Goodwin and Podos presented the same paper later at another meeting during August 2014 (Animal Behavior Society). And, in spite of knowing all of the flaws, they vigorously defended the paper again in their 2015 rebuttal to Akcay and Beecher (2015), arguing cleverly enough that anyone not reading carefully will assume this is just another case of minor disagreements of opinion about whether, for example, birds could be "fingerprinted" based on their songs alone, without having the birds marked. I will state here in the strongest possible terms that this is not a matter of opinion. It is science vs. non-science, or pseudoscience, or junk science (choose your terms).

The Continuing Secrecy

In frequent exchanges with Biology Letters, in an attempt to have Goodwin and Podos (2014) simply retracted, matters have taken another strange twist. Biology Letters writes to me the following (18, 22 September 2015):

. . . the authors' institution have conducted a thorough independent investigation into this matter [Goodwin and Podos 2014] and deemed this to be a difference of scientific opinion, of the kind that is very common in this field and across all scientific research. We are satisfied with the rigour and findings of this institutional investigation and therefore will not be taking any further action on this occasion . . .
Dean McCarthy should be able to provide these findings accordingly

Interestingly, Dean McCarthy seems to know nothing about this investigation (24 September 2015):

Dear Dr. Kroodsma,

I did not send anything to any journals, nor could I have done so legally. My communications with students, when I act in my capacity as Dean of the Graduate School, are student records covered by the Family Educational Rights and Privacy Act (FERPA).

John McCarthy

Senior Vice Provost for Academic Affairs and Dean of the Graduate School

Inquiring further about this mystery, Surayya at Biology Letters tells me (30 September, 23 October, 2 November 2015) that

The report findings were sent . . . by the authors and this was signed off by the university review panel who investigated the case . . . I cannot send a copy of the findings on to you . . . [because of] . . . the rules as laid out by the university . . . I'm afraid that I don't have another name [besides Dean McCarthy] from the university that could assist you with this [getting a copy of the report] . . . the report was provided to us in confidence . . .

This "institutional investigation" remains a mystery to me. As I understand the facts, 1) there exists a UMass Amherst investigative report with Dean McCarthy's name on it, 2) but Dean McCarthy knows nothing of this report (in follow-up emails to attempt to verify this, Dean McCarthy has not responded); 3) the report is signed off by some review panel, 4) but the review panel seemingly has no names and is not communicating with Dean McCarthy; 5) the report is secret because of university rules laid out by the review panel with no names, and 6) the secret report exonerates Goodwin and Podos on all fronts; and 7) the secret report (deemed by Biology Letters to be independent, thorough, and rigorous) was submitted by Goodwin and Podos.

The continuing efforts to stifle all scientific discourse are extreme. Taking the facts at face value (e.g., Dean McCarthy has good memory and honestly tells me he knows nothing of the report, there is only one Dean McCarthy, no one is playing games with words, etc.), the possible explanations for what has transpired in this institutional review are limited, and not so good.

I think that the office of Vice Chancellor for Research will have a better chance of getting to the bottom of the above mystery than I will, so I am going to stop trying to obtain a copy of this secret report, or figure out who wrote it. But, I doubt very much that anyone, review panel or not, will publicly defend Goodwin and Podos (2014), because anyone who condones their work as science or their behavior as ethical will no doubt have their own work scrutinized.

A History

If all of the above were an isolated incident involving a single paper, even given the most bizarre series of events surrounding it, one might still cut the authors some slack, but . . . eleven years ago, on an in-house review, I pointed out to Podos the difference between science and what he was doing:

"Science is the search for truth, regardless of how good the story is; marketing and advertising is the search for a good story, regardless of the truth" (October 2004).

To me, Podos was clearly and cleverly marketing sexy ideas with no substance; that was not science, I pointed out. Since 2004, there has been no communication between me and Podos or any of his students, until 2014 (even though I'm an emeritus professor in the same Biology Department, UMass Amherst). By definition, we have a long-standing disagreement about what constitutes science (but that is not the same as a long-standing vendetta, as I have been accused of). Frankly, I do not believe Podos does science, but instead throughout his career has marketed himself and his ideas by "*misrepresentation of the procedures and outcomes of research to gain some advantage*" (VCRE document on scientific misconduct).

After studying Goodwin and Podos (2014), I reviewed some of the other papers Podos had published on his idea of song performance (see attached, **Podos and Performance Studies**). In spite of Podos' attempt to prove otherwise, there is not a single credible bit of scientific evidence to support the hypothesis that he continues to market.

The first paper among my reviews, that by Podos, Peters, and Nowicki, is highly informative. It shows how selective the authors are in finding support for their story, claiming a dozen times how certain selected data are *consistent with* their performance hypothesis, but never once

do the authors help the reader fully appreciate the contribution (or lack of it) that they are making to science. No alternative explanations are considered, and data inconsistent with their favored explanation are ignored. With the admission (inadvertent, in an email by Goodwin to Univ of Washington group before all communications were silenced) by Goodwin and Podos (2014) that most of their data and statistical tests were discarded, one sees how the stories develop from the outset.

Or consider my review of Lahti, Mosely, and Podos—it is, frankly, highly deceptive in how it manipulates readers to a point of view in the Introduction, and then does its best to conceal the real story in the data and instead promote Podos' false performance hypothesis (see review in **Podos and Performance Studies**).

Accusations against Me; My Motivations

I am not a welcome messenger, I realize, and all manner of accusations have been leveled against me by Podos (and probably his graduate advisor Nowicki), but never to my face, never where I can address them directly (e.g., in confidential statements to the editor of the journal *Animal Behavior*, where I was hoping to publish my findings). I have been accused of attacking graduate students (Goodwin, Moseley), for example, but I would simply ask about all of the graduate students who were denied a fair competition for a Best Student Paper Award at the ornithological meetings in May 2014. I do not believe cheaters should be rewarded, especially not in science (not in the Olympics either, where medals are retracted when cheating is discovered).

Why am I committing so much energy over the last year and a half to addressing this issue? I will copy here what I wrote to Dean Steve Goodwin on 17 June 2015:

One final thought. I am sure that the motivation for my efforts will be challenged. As scientists, let's list the possible explanations/motivations:

1. I loathe what is called "junk science," especially in my cherished field of birdsong. **True.** There's abundant evidence for this explanation. See previous papers I have published that challenge scientists in my field to do better work, such as the following:

Kroodsma, D. E. 1989. Suggested experimental designs for song playbacks. *Animal Behaviour*. 37:600-609.

Kroodsma, D. E. 1990. Using appropriate experimental designs for intended hypotheses in song playbacks, with examples for testing effects of song repertoire sizes. *Animal Behaviour*. 40:1138-1150.

Kroodsma, D. E. 1990. How the mismatch between the experimental design and the intended hypothesis limits confidence in knowledge, as illustrated by an example from bird-song dialects, p. 226-245. *In: Interpretation and explanation in the study of animal behavior*. M. Bekoff and D. Jamieson (eds.). Westview Press, Boulder, Colorado.

Byers, B. E., and D. E. Kroodsma. 2009. Female mate choice and songbird song repertoires. *Animal Behaviour*. 77:13-22.

2. I would like to undermine the career of anyone who publishes pseudo-science and who, with every publication, removes us farther and farther from a true understanding of what birds actually do. **True.** This is not personal. This is science, and a frontal attempt to stop pseudo-science.

3. I'd like to give Jeff Podos the opportunity to deal with these issues on his own terms. **True,** but I've given up (my naïve) hope that he'll come to terms with his ways

unless forced to. I've given him every opportunity to do so in the past (I could send you copies of my previous emails to him), only to be threatened with charges of criminal harassment. Meanwhile, Podos continues, defiantly: Goodwin and Podos repeated their same flawed paper at a second scientific meeting last year, even after all of the flaws were pointed out to them; and they have defiantly defended their original publication in *Biology Letters*. If you want to try to convince Jeff to come to terms with these issues on his own, be my guest. I believe it will be far better for him in the long run if he himself addresses these issues candidly, rather than my (and others) taking him to task in a public forum for his misconduct.

4. This is a heads-up for you, in a leadership position, as to what is just over the horizon, to deal with in whatever way you wish, professionally or personally. **True.** My review has considerable momentum, solid support from leaders in the field of avian bioacoustics, and is nearly finished (awaiting data collected during 2015 spring/summer), but if some way is proposed to avoid a publication of my review, and these matters can be addressed in some satisfactory way without my public thrashing of the style of 'research' published by Jeff Podos, I'd be willing to consider any proposal from you.

5. I want to stop Podos from destroying any more graduate students. **Absolutely true.** I have studied the papers of only Goodwin and Moseley, and both are highly flawed, but excellently marketed. Science will be far better off without someone like Jeff training graduate students to work as he has.

6. I fret over abuse and waste of tax-payer money. **Not so much.** But I think that UMass would fret about this matter considerably, given that Podos has used over a million dollars of federal monies in these pseudoscientific endeavors.

7. This is a long-standing personal vendetta against Jeff Podos. **NONSENSE. FALSE.** I could send you my 2004 intra-departmental letter to Jeff in which I encourage him to do "science" rather than "marketing and advertising." There's nothing personal about that, but our friendly relationship stopped with that review, as he's never communicated with me since. ***This is about science. Period.*** It's about people like Jeff systematically and relentlessly undermining a field of endeavor that I cherish, i.e., learning what birds actually do. It is my opinion that we would know more about what birds actually do if Jeff Podos never published a 'scientific' paper. And, if this were really a personal vendetta against Jeff, I would have followed the demand of the UMass police (and presumably Podos) and told all 50 people with whom I was communicating that all of them were also liable for criminal harassment charges if they attempted to communicate with Podos about his science; it was at that point that I told Officer Liptak of the UMass police that I would not comply with her demand, because following her orders would have made Podos appear as a complete fool to a large international audience.

A Solution, an Accounting

Here is what I wrote to Dean Goodwin on 17 June 2015. It may sound harsh, but I still believe now what I wrote then.

Each of us as scientists has our own standards by which we measure the ethical and scientific behavior of others. My accounting for Goodwin and Podos, and especially for Podos alone based on his other publications as well, would be swift and severe, as they undermine not only science as a way of knowing but also the public trust in scientists of all fields, from scientists who study birdsong to scientists who study climate change. In my accounting, I'd ask for the following:

1. A full retraction of Goodwin and Podos (2014, 2015) in Biology Letters.
2. The Association of Field Ornithologists retracts its best student paper award to Goodwin, for her oral presentation of Goodwin and Podos (2014) at their 2014 meetings in Rhode Island.
3. All papers published by Podos are now questionable, not only for their obvious flaws, but also for practices in data manipulation or selection and analysis that cannot be detected; until Podos himself convinces scientists that some of his papers qualify for the test of “utter honesty” and “scientific integrity” that Feynman advocates (see http://neurotheory.columbia.edu/~ken/cargo_cult.html), there would be a moratorium by all responsible scientists for accepting and citing these papers at face value.
4. The Animal Behavior Society, if it is a serious society that wishes to promote the scientific study of animal behavior, requests (or demands) that president-elect Jeff Podos resign that position.
5. The million or so dollars of tax-payer money that Podos has already spent on this “research” and graduate student training has done enough damage, and the Grants and Contracts Office at the University of Massachusetts, Amherst, should restrict all future grant applications from Podos until he has been rehabilitated as a scientist.
6. Graduate students in the Organismal and Evolutionary Biology program at the University of Massachusetts, and especially those under Podos’ training, should use these documents . . . as a case study in how not to conduct oneself professionally. And no students would be allowed to train with Podos unless they are co-advised by another faculty member.

How to do science

In a wide-ranging critique of another scientist who claimed to have found a weakness in Podos’ performance hypothesis, Zollinger, Podos et al. (2014; pdf attached) lecture that scientist on a number of issues:

- 1) faulty measurements and errors in methodology,
- 2) how data are interpreted,
- 3) validity of results,
- 4) experimental rigour,
- 5) alternative explanations and hypotheses for data,
- 6) the ability to reject hypotheses,
- 7) appropriate use of skepticism, and
- 8) problems in published papers that “undermine the validity of the results reported and the conclusions reached.
- 9) More generally, Podos and his coauthors fret that those who fail on the “basic principles” of science
- 10) will “have a profound adverse effect on the way the research field is viewed by the rest of the scientific community.”

That critique by itself is well-taken, and any scientist would agree with all of the generalities dictated there, but in the context of all that I have written here about the quality of Podos’ own research, that critique is an extraordinary document . . . about which I need say no more.

Summary

As I wrote to Biology Letters on 23 October 2015:

My claims are about as serious as they come in science: Goodwin and Podos (2014) have fabricated a story by inventing (two) biological traits for their subjects that are well known not to exist. Then, also without telling readers, they discard data that don't tell the story, selectively reporting only a few statistically significant tests gleaned from all possible tests. Furthermore, they do the statistics wrong, both initially (2014) and repeatedly after their errors have been pointed out (2015), and then do their utmost to hide from public inquiry. The consequences for the scientific literature are the same as if the authors had fabricated data from the outset, i.e., a literature of mistruths.

And here are some unwelcomed supplementary facts, because I think they are not irrelevant. Podos trained with Steve Nowicki (now at Duke University), and the techniques used by Podos are also used by Nowicki (see first reviewed paper, by Podos, Peters, and Nowicki, in the attached document **Podos and Performance Studies**). Nowicki was a postdoc in the same program as Marc Hauser, a former professor at Harvard University, who is now disgraced in academic exile for scientific misconduct (<http://www.thenation.com/article/disgrace-marc-hauser/>). The literature that Podos produces is as false, if not even more so, than that produced by Hauser.

This entire process has been no fun since last May, but I have been unable to sit idly by as a field of science that I cherish is so abused. Science is "self-correcting," I've been told, and I am compelled to be part of the correction process. I know there are others who share my passion, and I am confident that, in the end, science will win. Science must win.

What next?

That is largely up to you, I understand, Dr. Malone. I look forward to hearing from you and settling these issues. And, as I have written numerous times before, anything I write may be shared directly with Jeff Podos (because of his legal threats, I cannot do that directly, or I would), but I would ask for the courtesy of a dialogue with Podos rather than a repeatedly one-way communication, with every response from Podos being kept secret.

Also, I can supply you with more documents, such as all of my emails with Biology Letters, with the Association of Field Ornithologists, with Deans Goodwin and McCarthy, the journal *Animal Behavior*—whatever you want. Ever since being threatened with criminal harassment, I have kept very thorough records of everything that has transpired.

Sincerely . . . Donald Kroodsma, emeritus, Biology Department, UMass Amherst
52 School St., Hatfield MA 01038; 413-247-3367; donaldkroodsma@gmail.com

List of attached documents:

1. VCRE--Scientific and ethical misconduct--Podos, Biology.docx (this letter)
2. No Teams of Rivals or Coalitions in Territorial Sparrows
3. Podos and Criminal Harassment
4. Zollinger, Podos et al. On doing science (2012)
5. Podos and Performance Studies
6. Honest Signaling and the Motor Constraints Hypothesis

Relevant references

Akçay, C., and M. D. Beecher. 2015. Team of rivals in chipping sparrows? A comment on Goodwin & Podos. *Biology Letters*. 11:20141043.

Feynman, R. P. 1985. *Surely You're Joking, Mr. Feynman!* W. W. Norton & Company, New York City.

Gitzen, R. A. 2007. The dangers of advocacy in science. *Science* 317:748-748.

Goodwin, S. E., and J. Podos. 2014. Team of rivals: alliance formation in territorial songbirds is predicted by vocal signal structure. *Biology Letters* 10:Article Number: 20131083.

Goodwin, S. E., and J. Podos. 2015. Reply to Akçay & Beecher: yes, team of rivals in chipping sparrows. *Biology Letters*. 11:20150319.

Simmons, J. P., L. D. Nelson, and U. Simonsohn. 2011. False-positive psychology: undisclosed flexibility in data collection and analysis allows presenting anything as significant. *Psychological Science*. 22:1359-1366.

Zollinger, S. A., J. Podos, E. Nemeth, F. Goller, and H. Brumm. 2012. On the relationship between, and measurement of, amplitude and frequency in birdsong. *Animal Behaviour* 84:E1-E9.

VCRE to Kroodsma, 3 November

Dear Professor Kroodsma,

I acknowledge I have received the attachments and our office will review and get back to you.

Regards,

Mike

11/2/2015

Kroodsma to journal *Animal Behavior*

1 November 2015

To: Susan Foster, US Editor of *Animal Behavior*

From: Donald Kroodsma (Prof Emeritus, UMass, Amherst)

Re: Submission of a Forum article

Dear Dr. Foster:

I would like to submit a Forum article. As I understand from the web, a Forum article is designed to generate constructive exchanges, and that is my goal. I have attempted to establish a dialogue with a number of the "target" authors, but none will engage (more details below). A past president of ABS (Beecher) has advised me that the only way to generate a dialogue is to force one in a public forum, so that is where I am.

I have contacted all authors that I critique in my Forum article. Eight times over several months during late 2014, for example, I requested a dialogue with Jeff Podos at UMass

Amherst, and the only response I received was after the eighth attempt, and that response was from the University of Massachusetts police, threatening me with criminal harassment charges if I tried one more time to communicate with Podos (or his student Goodwin, or, for that matter, anyone in UMass Biology, where I am emeritus). I understand that Podos is president-elect of the Society, yet this behavior is in direct violation of the ethics promoted by the ABS, as copied below from the AB Author Information Pack:

Professional integrity in the conduct and reporting of research is an absolute requirement of publication in the journal, as is a willingness to share information with other members of the scientific community. Consequently, as a condition of publication in *Animal Behaviour*, authors must agree both to honour any reasonable request for materials or methods needed to verify or replicate experiments reported in the journal and to make available, upon request, any data sets upon which published studies are based. Anyone who encounters a persistent refusal to comply with these guidelines, or has reason to suspect some other departure from acceptable standards of scientific conduct, should contact the appropriate Executive Editor (European or American) of the journal. The Executive Editors will act in accordance with the guidelines of the Committee for Publication Ethics (<http://www.publicationethics.org>) and may inform an author's institution of a purported infraction

Apparently this is an issue that I am supposed to bring to your attention as Executive Editor, but contacting the author's institution will do little good, I fear, as the university (a "review panel") has filed a secret report with Biology Letters that has thwarted my attempt to address scientific issues there. (I can provide more details on all this if you want; in brief, it is the most bizarre set of circumstances I've ever encountered professionally.) Two other target authors (Searcy, Vehrencamp) acknowledged receiving a draft of my Forum article, but offered no comments; Nowicki did not respond. I did have a constructive exchange with Becky Cramer and David Lahti.

It will be a significant challenge to obtain fair reviews of what I have written, and the effort to suppress it will continue. The sphere of influence of Podos/Nowicki/Searcy is considerable; they are highly respected in the Society, as past, current, and future presidents. My Forum article is, to say the least, very inconvenient in many ways, not only for the Society, but for UMass as well. I welcome any and all scientific reviews of what I have written (I've waived the double-blind feature of reviews), by any of the target authors, as I welcome the strongest challenge possible to my thinking. But, I also ask any editor to see through the tangled, political web and address the science, and the science alone. I am not confident that two of your 'birdsong' editors, namely, MacDougall-Shackleton and Yasukawa, would be able to address the science in an objective manner, given past exchanges with them. I think your best bet would be to engage someone you trust as a special editor for this Forum article, someone with no immediate affiliation to those addressed in the paper.

I have enormous files of correspondence with many parties on this Forum article, beginning during 2014, and I am happy to make available any of it, though I am not sure of what use it will be. It will show that after eight attempts to communicate with Goodwin and Podos, asking them to help me proceed in a fair way, I gave up and, ethically following AB guidelines, sent my draft document to all of the authors that I critiqued. The US editor of AB immediately cried foul, and angrily rejected any possibility of accepting a Forum article, sending her rejection letter to everyone I was ethically bound to communicate with. Much discussion ensued (you have access to all of this in the AB archives, I'm sure; I could supply it as well), and I finally let it all rest, knowing that during 2015 I needed to collect the necessary original recordings for chipping sparrows and swamp sparrows in order to finish my proposed Forum article.

With that information now in hand, I begin anew the process of submitting this Forum article.

According to the web guidelines, there is no word limit. I don't take that literally, as I know that less is often more; I count about 16,000 words—that's a lot, but I see no other way to satisfactorily critique a large body of work. I must be thorough, or I will be accused of being selective. And I want to demonstrate that nowhere, leaving no paper unturned, can I find credible scientific support for a hypothesis that has been repeatedly confirmed over the last decade.

I am in "full disclosure mode," but I can't think of anything else to say at this point. I am happy to respond to any questions about process or personnel that you inquire about.

Sincerely . . . Donald Kroodsma

(52 School St., Hatfield MA 01038; DonaldKroodsma@gmail.com; 413-247-3367)