

Correspondence, for the following:

Honest Signaling in Birdsong and the Motor Constraints Hypothesis of Podos (1997):

A Contrary View

Donald Kroodsma, Hatfield MA 01038

Introduction

Threatened with Criminal Harassment charges, I am documenting here "for the record" what has transpired in the chain of events since I

- 1) heard Goodwin and Podos (2014) presented orally at scientific meetings during 2014,
- 2) then read the published document in Biology Letters,
- 3) tried to engage Podos and Goodwin directly on this research, to no avail,
- 4) in exasperation wrote a review of that paper, plus other papers on "performance,"
- 5) tried to establish a dialogue with about 50 of the published authors that I cite in the document (following what I think of as routine professional courtesy and ethics), and
- 6) was then chastised publicly for so doing by the editor of Animal Behaviour, who publicly and angrily rejected my review before it was even submitted.
- 7) And now, I am threatened with charges of Criminal Harassment against me, with a threatening phone call from the University of Massachusetts Police.

What follows below is the trail of email communications that lead up to my 31 December letter to Michelle Scott, editor of Animal Behavior, capping off a most bizarre 2014. See the Index below for a summary of that letter. And then the saga continues during January of 2015.

In the Appendix is the full documentation of my "Criminal Harassment" charges from Podos and his two students in the Department of Biology, University of Massachusetts, Amherst, the very department in which I am an emeritus professor. Never once in this entire process, I might point out here as well as elsewhere in this document, has Podos ever replied to any of my inquiries about his research, and his first communication is indirect, via the Police, threatening me as a criminal.

Contents

Introduction..... 1

Threatened with Criminal Harassment charges, I am documenting here "for the record" what has transpired in the chain of events 1

(for July thru October: see APPENDIX) 5

4 November 2014: Kroodsma to Podos, Nowicki, Searcy, Vehrencamp.	5
“I’d welcome any dialogue with you”	5
1 December 2014: Kroodsma to ~50 cited authors.....	5
“I thought it appropriate to seek any feedback that cited authors or others might like to provide.”.....	5
8 December: Michelle Scott to me, plus Podos, Goodwin, Moseley, Searcy, Nowicki, Vehrencamp.....	6
“what tipped me . . . to angry was your e-mail distribution to so many colleagues”	6
8 December: Michelle Scott to ~50 cited authors.....	7
A public rejection of unseen, unsubmitted manuscript.....	7
10 December: Pavel Linhart to Scott.....	8
“I am confused after your reaction to professor Kroodsma's email . . . it might be worth explaining”	8
10 December: Scott to Linhart.....	9
A partial explanation.....	9
10 December: Kroodsma to Scott.....	10
“I am dumbfounded” by your public, angry reaction	10
10 December: Scott to Kroodsma.....	11
More explanation: “I chose to say as little as possible to spare your reputation . . .”	11
10 December: Eugene Morton to Scott.....	12
“Why would you not consider the finished ms? Don is right on with his criticism”	12
13 December: Scott to Morton (forwarded from Morton to me 6 January).....	12
A new charge against Kroodsma—“attacking graduate students”; and a response.....	13
And an implied charge, that I have made “this fight so public”	13
31 December: Kroodsma to Scott, capping off a most bizarre 2014.....	14
Why I am writing this letter “to” you, but not “for” you.....	14
I am also asking president of ABS and ethics editor to weigh in.	14
1) My reputation—you feel you need to protect me from myself?.....	15
I have taken calculated risks during my career	15
The bogus charge that I am against young women in science.	16
The more general charge that I am “attacking graduate students.”	17
Criminal harassment (for details, see APPENDIX).	17
Other diversionary smears.	17
Challenge my reputation as a scientist? I invite critical appraisals.....	18
Consider my last paper.....	18

Or the paper before that	18
Oliver Twist asks the Animal Behavior Society for MORE SCIENCE.....	19
2) I follow AB protocol and am publicly chastised for it?.....	20
3) You have no conflict of interest?.....	20
4) Your prejudging my nonsubmitted manuscript	21
5) Other difficulties.....	21
6) Just what should be “stopped”?	21
7) Not one iota of science has been addressed.....	21
7) My unprofessional tone—let’s think about that.	21
Summary of Goodwin and Podos (2014).....	22
Summary of Zollinger, Podos, et al. (2012).....	23
Let’s search for “objective professional language” to address the above two papers.....	23
Ethics issue #1.....	23
Ethics issue #2.....	24
Scientific/ethics issue #3.....	25
Scientific/ethics issue #4.....	25
On “Science” vs. “Make-believe.”.....	25
And more	25
The Ultimate in Hypocrisy—Tell me it’s not	25
8) Some obvious other questions arise:.....	26
1) Personal attack? Please give me one example.	26
2) Use of tax dollars for what?.....	26
3) Compared to Marc Hauser situation?	26
4) In all of the papers that I have critiqued, what has been learned? Next to nothing!	26
5) Graduate student education, the next generation of researchers?	27
6) Michelle, why not use your role as editor to facilitate rather than stifle scientific discourse?	27
7) Why not take a leadership role in promoting AB as a credible scientific journal?.....	28
8) Why not try to help fix this broken system?.....	28
9) Why not help science trump nonsense?.....	29
In Parting.....	29
THE SAGA CONTINUES	29
1 January 2015: Editor Scott to Kroodsma and others, with Kroodsma response	29

Correspondence with University of Massachusetts Police.....	31
1 January: UMass Police to Kroodsmas; Tell all ~50 people they should stop communicating with UMass Biology (Podos) on this matter.....	32
3 January: Kroodsmas to UMass Police. This is beyond absurd.	32
14 January: UMass Police to Kroodsmas.....	33
6 January. Pavel Linhart to Editor Michelle Scott, Ana Sendova-Franks, and me	34
6 January. Kroodsmas response to Linhart, copy to Scott, Sendova-Franks.....	35
7 January. Kroodsmas to Scott, Sendova-Franks—on the charge of “attacking graduate students”	37
If you want to accuse me of something, do so to my face.	37
An adviser’s role in mentoring and publishing with graduate students.....	37
Let’s ask graduate students if I am unfair	38
Fallen hero malarkey.....	39
7 January: Scott to Kroodsmas	39
7 January: Kroodsmas to Scott	39
8 January. Michelle Scott to Pavel Linhart.....	39
APPENDIX. The Charges of Criminal Harassment	40
Threatening phone call from UMass Amherst police on “Criminal Harassment” emails that I have sent to “UMass Biology”	41
Transcript of phone conversation, 18 December 2014	41
The evidence: My emails to UMass Biology (Podos or Goodwin):	42
1) 9 July 2014 “I wondered if you could help me understand why in the paper you focused only on trill rate.”	43
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”	43
3) 8 October 2014 “I have no idea what is in your head . . . I’m still available to talk”	44
4) 15 October 2014 “JEFF, HERE’S ONE LAST ATTEMPT TO ENGAGE YOU”	44
5) 16 October 2014 “something well worth reading, from half a century ago”	46
6) 9 November 2014 “I’d welcome any dialogue with you”	47
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”	47
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”	49

(for July thru October: see APPENDIX)

4 November 2014: Kroodsma to Podos, Nowicki, Searcy, Vehrencamp.

“I’d welcome any dialogue with you”

Hello Jeff Podos, Steve Nowicki, Bill Searcy, and Sandy Vehrencamp:

Jeff has known for some time that this is coming, though he’s never acknowledged receiving any messages from me (neither email nor US mail). This review was precipitated by listening to the oral presentation of Goodwin and Podos (2014) at the ornithological meetings during late May, as told in the Prologue of the attached document.

If you choose to read any of what I have written, you will see that I am rather critical of the motor constraints hypothesis of Podos (1997) and all of the “tests” that seemingly attempt to confirm its significance. I believe there’s nothing about male quality and honest signaling in the trill rate-bandwidth graph.

I am sending this document first to the four of you, as you have all been prominent in promoting this hypothesis. I’d welcome any dialogue with you, and if you choose to reply, I’d appreciate hearing from you by December 1. At that time, I will decide what to do next with this document. Most likely at that time I will send it to others who have been cited in the document, seeking their feedback as well, and then January 1 decide what to do next.

Regards . . . Don Kroodsma

1 December 2014: Kroodsma to ~50 cited authors

“I thought it appropriate to seek any feedback that cited authors or others might like to provide.”

dmennill@uwindsor.ca MortonE@si.edu dmoseley@bio.umass.edu m.naguib@nioo.knaw.nl kumstatova@post.cz and 28 more...

Hello birdsong enthusiasts (cited authors and a few “interested parties”—Group 2):

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.

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.

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.

Feel free to forward this document to anyone you wish, especially any coauthors, with the same invitation for contributed commentary.

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.

Regards . . . Don Kroodsma

8 December: Michelle Scott to me, plus Podos, Goodwin, Moseley, Searcy, Nowicki, Vehrencamp

"what tipped me . . . to angry was your e-mail distribution to so many colleagues"

me

CC

Jeff Podos Sarah Goodwin Moseley, Dana Bill Searcy snowicki@duke.edu slv8@cornell.edu

Ana Sendova-Franks

Dec 8 at 1:23 PM

Dear Don,

I have been considering your proposed article for *Animal Behaviour* very carefully for quite a while. As you know, I have no dog in the fight (bird song); my job is to do the best thing for the journal. I have suggested how you might change your approach to make a real contribution to the journal and to the field but this version is substantially unchanged from the previous ones.

Therefore you are on notice that *Animal Behaviour* will not send out for review any manuscript from you on this topic. This project, at least where *Animal Behaviour* is concerned, is dead.

Frankly, what tipped me from being sympathetic to angry was your e-mail distribution to so many colleagues. It was not the helpful comments from so many that you sought but you used the journal (by mentioning that you were preparing a Forum article) to promote your continued attack. As you might imagine, several people on your lists have contacted me. One even said that you were a hero to them in your earlier days of publishing but now they are greatly saddened by what they consider, your unprofessional attack.

I am sorry that things are ending this way.
Sincerely,
Michelle

Michelle Pellissier Scott
Executive Editor, Animal Behaviour
Michelle.Scott@unh.edu

8 December: Michelle Scott to ~50 cited authors

A public rejection of unseen, unsubmitted manuscript

To

jwatwell@indiana.edu beballentine@wcu.edu mdbb@martinbeebee.com brumm@orn.mpg.de
jbyers@uidaho.edu gcardoso@cibio.up.pt becky.cramer@nhm.uio.no selvino@selvino.nl
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michael.s.reichert@hu-berlin.de rek@amu.edu.pl leigh.simmons@uwa.edu.au kws@unc.edu
drwilson76@gmail.com zollinger@orn.mpg.de akcay.caglar@gmail.com
beeher@u.washington.edu Beecher bbyers@bio.umass.edu gahr@orn.mpg.de
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CC

Ana Sendova-Franks me

Dec 8 at 2:19 PM

Hello All,

You are receiving this e-mail because you are a recipient of Don Kroodsma's manuscript on **Honest Signaling in Birdsong and the Motor Constraints Hypothesis of Podos (1997): A Contrary View**. I am saddened that he has broadcast his message so widely suggesting that he is preparing it for publication in Animal Behaviour as a Forum article. This journal will not consider it for publication. I have now made that clear to Don. I have suggested several legitimate alternatives for him to present his views with a more balanced perspective but I have not seen them put into action.

Sincerely,
Michelle Pellissier Scott
Executive Editor, Animal Behaviour

Michelle.Scott@unh.edu

10 December: Pavel Linhart to Scott

"I am confused after your reaction to professor Kroodsma's email . . . it might be worth explaining"

On Wednesday, December 10, 2014 3:33 AM, Pavel Linhart <pavel.linhart83@gmail.com> wrote:

Dear professor Scott,

I am confused after your reaction to professor Kroodsma's email that was introducing his prepared manuscript to involved audience. I do not know what the other involved people think but it might be worth explaining to the audience why you decided explicitly state that you will not publish the manuscript that is in a stage of preparation and have not been submitted yet to Animal Behaviour?

Professor Kroodsma stated in his email:

'I don't know the eventual outlet of my document, but I am aiming for a Forum article in Animal Behavior.'

He clearly expressed his intentions and gave an opportunity to clarify the issues before the submission which is requested for AB Forum article:

'In the case of Forum critiques of published papers, the author(s) of the target article must be contacted and trivial points of difference or misunderstanding resolved; this correspondence must be submitted in a cover letter accompanying the Forum article with the knowledge of the author(s) of the target article.' (AB Guide for authors)

Also, the topic of prof. Kroodsma's article seems to fit well the format of the Animal Behaviour Forum:

'The section accepts critiques of published papers relevant to the areas of interest of the Journal, and provides an opportunity for constructive exchanges on issues surrounding particular fields of study.' (AB Guide for authors)

I think the current version of prof. Kroodsma's article and his email was maybe too much offensive (and I can understand that, if it is true that criticized authors never answered his objections), nevertheless he raises important questions. I also believe that the final version of the manuscript will be free from any personal objections and will focus on the debate and will provide constructive critique. If not, then I think it would be the time to reject the manuscript.

So, I would like to ask again why you decided explicitly state that you will not publish the manuscript that is in a stage of preparation and have not been submitted yet to Animal Behaviour? Your explanation did not tell me much: 'I have suggested several legitimate

alternatives for him to present his views with a more balanced perspective but I have not seen them put into action.' Did you discussed the way how the manuscript should look like before dissemination and prof. Kroodsma did not follow your suggestions or what? I think more detailed explanation might be appropriate to make the situation more transparent.

Thank you for your answer.

Sincerely, Pavel Linhart

10 December: Scott to Linhart

A partial explanation

CC

me Ana Sendova-Franks

Today at 1:56 PM

Dear Dr. Linhart,

I appreciate this criticism. Your point is well taken as I purposely explained very little of my history with this proposed manuscript. Don and I had been corresponding for about two months. I had seen, I think, two previous versions. We had discussed the potential forms it could take. I suggested that he redirect his good ideas and write a proper Review that would set out the weaknesses and strengths of previous work on bird song and hopefully propose alternative hypotheses. This is what I would most like to have seen. Alternatively, he could write a Forum article. As you say this section of the journal does accept critiques. Although we do not have a page limit for these Forum articles, usually they are quite short (~3000 words).

Don decided on the Forum format but he never removed the aspects of personal attacks, which I had repeatedly told him we could not publish. In his last e-mail to me, he did say that he would tone it down when he submitted his manuscript but I saw no sign that he was willing to change. It needed more than being "toned down". This version was as long and as angry as the previous ones. When Don disseminated this manuscript to 54 people, I felt that he did not do this in good faith. He had not removed the tone of personal attack. I felt that he sent it out to further his crusade. I also felt that by mentioning the journal, it might seem that we were sanctioning this attack.

You are right that Don does raise important questions, there are good ideas imbedded in his manuscript and it is important to further scientific discourse with honest criticism. However, I saw no sign that Don would be able to change his tone or the structure to achieve this.

Sincerely

Michelle Scott

Executive Editor

10 December: Kroodsma to Scott

"I am dumbfounded" by your public, angry reaction

Hello Michelle:

I want to state here explicitly and honestly that I have not prompted the email from Pavel Linhart, other than sending to him (and others) the original document. He and I are not "friends or allies" in anything. His letter comes "out of the blue."

He does, however, express well the sentiments I've had when receiving your emails. I am dumbfounded that I felt I was following the protocol established by Animal Behavior for a Forum article, only to be bashed publicly by you for so doing; in a second email that Linhart did not see, you conveyed not only to me but to multiple others (Nowicki, Podos, Searcy, Goodwin, Moseley, Vehrencamp--probably those who wrote to you) how angry you were at me for "using" the journal (huh?). I think my crime, in retrospect, is that I have the audacity to critique a very large bandwagon, and you don't realize the magnitude of the problem (or the seriousness of the implications for a broad range of issues, including the very future of ABS).

It doesn't help, for example, that I am critiquing 1) a past president of ABS, 2) a president-elect of ABS who is also the 2014 winner of the Exemplar Award, and 3) someone who is running for president-elect of ABS (and is a collaborator with the current president and is a close colleague at UMass of an ABS member-at-large officer), all three of whom you have said you greatly admire. Also at the center of my critique are 4) someone who came within a whisker of being cited for ethical misconduct for publishing practices and 5) the very unfortunate students they all continue to train in this culture of advocacy. None of them will communicate about the substance of these scientific issues (one of them deletes emails without reading, she says), but I can well believe that you have received an earful from all of them expressing their horror at my unprofessional attacks. But let me ask you: Have they addressed one iota of science in their emails to you? Has Podos addressed any of the serious charges of scientific and ethical misconduct in his 2014 paper with student Goodwin? Just one of them? If so, could you please forward to me just one scientific issue on which any one of them is willing to publicly disagree?

Here, for some transparency, I'll forward to you comments that I've received in an email:

"The message from Pavel Linhart is really interesting and informative, because it provides validation from a pretty obviously neutral reader . . . it's encouraging to see that someone who doesn't have friends on either side understands that you never implied prior endorsement from AB, that it is highly appropriate to circulate the document, that any comments deemed offensive in an early draft can be altered prior to publication, and that no editor has any business "pre-rejecting" a manuscript that hasn't even been submitted yet."

Perhaps the transparency that Linhart requests might involve forwarding a summary of the emails that you have received that influenced your decision. What was said? Frankly, I think it is somewhat challenging to accept that my following AB protocol for a Forum article would shift you from sympathetic to angry.

I'm grateful to and highly respect anyone in this field (thank you, Pavel Linhart) who is willing to address matters of science in a straightforward, candid, transparent manner. That is the only way we will learn anything about the world around us, including animal behavior. In contrast, at every turn, as authors have refused to communicate with me and other scientists about their work, the problems that I address escalate, and continue unabated. And if the journal Animal Behavior (through its editors and elected officers, the conflict of interest immense, I should point out explicitly) refuses to address these serious issues of scientific and ethical conduct, ABS will cease to be a credible scientific society.

(The above is a brief prelude to a future communication, i.e., yet another escalation, I suppose. If you'd like to talk about any of this at any time, Michelle, I am at 413-247-3367. Please accept that I am not angry at you; you have a tough job to do, and, as I see it, you can neither afford to consider my manuscript nor afford not to. You can't win here. I have no desire to make your editorship unpleasant or difficult, but I do have a strong desire to see science at the core of animal behavior research.)

regards . . . Don

10 December: Scott to Kroodsma

More explanation: "I chose to say as little as possible to spare your reputation . . ."

CC

Ana Sendova-Franks

Today at 6:11 PM

Dear Don,

This is a very sorry state of affairs. Pavel Linhart is not the only person I have heard from who was not pleased with the brevity of my e-mail to your mass recipients. I chose to say as little as possible to spare your reputation as much as possible but enough to explain the situation (apparently not enough). As for the e-mail to you (that Linhart did not see) I used "reply to all" since you had involved these multiple others. (Only about half of them had corresponded with me and that was only in the past couple of weeks.)

As you know, I encouraged you - you have very useful things to say - but I was clear that you had to abandon the tone of personal attack. Until you did your blast e-mail, I thought we were having an honest discourse. However I was getting frustrated as you did not follow my advice to use the voice of an objective professional. I understand (now) that you thought that you were following the protocol for a Forum article. True it has no page limits (but they are usually shorter

than other types of papers). True you were asked to communicate first with the authors of the paper(s) you critique to iron out minor misunderstandings and I suppose that the 54 people on your blast e-mail were authors and coauthors of papers that you criticize. This was not how I interpreted your actions.

I would like to think that the reputations and ABS affiliations of the major recipients of your ire have nothing to do with my decision to terminate even the informal consideration of your article. If you had formally submitted what I saw, I would have had to reject it without review. What impartial reviewer could I have found? No one would be willing to review such a long manuscript - it is hard enough getting reviewers for regular ones. And even as a Forum article (on line only) I don't know what Elsevier would have to say about the length. None of that is really the issue though. It was your tone and I did not believe that you would change it sufficiently for us to consider it. My goal was to put a stop to this. Although things could have been different, this discourse is not doing any of us any good.

Sincerely,
Michelle

10 December: Eugene Morton to Scott

Morton, Eugene

To

Scott, Michelle jwatwell@indiana.edu beballentine@wcu.edu mdbb@martinbeebee.com
brumm@orn.mpg.de and 49 more...

CC

Ana Sendova-Franks me

Dec 12

Dear Michele,

"Why would you not consider the finished ms? Don is right on with his criticism" although obviously not ready to publish his ms at this draft stage. Isn't he an ABS Fellow? Doesn't that mean something to you?

I am not a fellow so I would not consider Animal Behavior for my critiques.

Gene Morton

13 December: Scott to Morton (forwarded from Morton to me 6 January)

Dear Dr. Morton,

Don and I had been corresponding for about two months. I had seen two or three previous versions. We had discussed the potential forms it could take. I suggested that he redirect his good ideas and write a proper Review that would set out the weaknesses and strengths of previous

work on bird song and hopefully propose alternative hypotheses. This is what I would most like to have seen. Alternatively, he could write a Forum article. The journal does accept critiques but they usually quite short (~3000 words).

Don submitted a proposal (as the journal requires) for a review. I conferred Ana Sendova-Franks (UK editor) and we rejected it as inappropriate as a review and inappropriate in tone. He decided on the Forum format but he never removed the aspects of personal attacks, which I had repeatedly told him we could not publish. In his last e-mail to me, he did say that he would tone it down when he submitted his manuscript but I saw no sign that he was willing to change. It needed more than being "toned down". This version was as long and as angry as the previous ones. When Don disseminated this manuscript to 54 people, I felt that he did not do this in good faith. He had not removed the tone of personal attack. I felt that he sent it out to further his crusade. I also felt that by mentioning the journal, it might seem that we were sanctioning this attack.

You are right that Don does raise important questions, there are good ideas imbedded in his manuscript and it is important to further scientific discourse with honest criticism. However, I saw no sign that Don would be able to change his tone or the structure to achieve this. That he is an ABS fellow does not figure into accepting or rejecting a manuscript. (This would not have been the first ABS fellow's manuscript that I have had to reject.) You say this one was "obviously not ready to publish". Why didn't he produce one that was ready for review? (Can you imagine getting a request from an editor to review an 87 page single spaced manuscript!) **Why make this fight so public?**

If you are his friend and supporter, help him produce something that is publishable - without **attacking graduate students**.

Sincerely
Michelle Scott

A new charge against Kroodsma—"attacking graduate students"; and a response

To Michelle Scott:

I have a request: *If you want to accuse me of something, do so to my face, not behind my back.*

At my request, Gene Morton forwarded this accusation of "attacking graduate students" to me more than three weeks after you wrote this to him. He had assumed I had been copied on the original.

This accusation is sufficiently infuriating that I am going to write a separate letter about this topic to you, 10 January (see elsewhere in this document). Not only is the charge troublesome, but so is the unfiltered accusation that almost certainly has merely been passed on from your friends in ABS, those you admitted from the outset that you greatly admired.

And an implied charge, that I have made "this fight so public"

There's a second implied charge, that *I* have made "this fight so public." In every step of the way, I have invited Jeff Podos (or his students) to a dialogue, and I made it clear what the logical

trajectory of this manuscript would be, unless they were willing to interact: Get comments from those I critiqued, and then submit a revision for publication. Repeatedly along the way I offered Podos any solution to this situation that he wanted to propose. Early on 1 December I practically begged him and his coauthors to speak up, so that we could deal with this in a direct fashion. I heard NOTHING from them, NEVER, as if they were eager to have me share the document with a wider audience, because that was the preferred method to deal with it. So late on 1 December I sent the ms to all critiqued authors.

Here is the question you should be asking: WHERE IS JEFF PODOS? Why does he refuse to respond to any inquiries about his research?

So, go ahead, accuse me of taking this public. But had I blindsided all of the authors I critiqued, submitting a critical review without trying to settle our differences first, I'd also be faulted, and rightly so according to the very ethics guidelines promoted by ABS. My crime is that I am taking on a substantial number of people who believe in make-believe.

31 December: Kroodsma to Scott, capping off a most bizarre 2014

Hello Michelle:

I know that your instructions to me are to "go away," but that's not going to be so easy. I don't think that's in the best interest of anyone except those most heavily critiqued in my review.

And, yes, I agree with you that this is a very sorry state of affairs, and I am very very very very tired of it. But it gets more bizarre with every email, your last one of 10 December (see especially point #1 below).

Why I am writing this letter "to" you, but not "for" you. I am going to elaborate on several issues here, but before I do that I want to make something clear. I am not writing this letter to try to convince you of anything; I am not writing this letter "for you." I am writing this letter/email and sending it to you (and perhaps a few others) so that I have a well-documented trail explaining exactly what has transpired and when, and I ask that you archive this correspondence in the AB archives. I want there to be no doubt about my motivation and intent, no doubt about the issues involved, no doubt about the choices that I have laid out for AB and ABS, and I want a clear record of it.

I am also asking president of ABS and ethics editor to weigh in.

It's not that I don't respect your position as editor, Michelle, but I want to receive a clear message from the Animal Behavior Society that my message is dead at ABS. I realize that the president and ethics editor also have close ties to those whom I critique, and what you write about getting an impartial hearing for my message is a challenge. Nevertheless, I ask for them to have a say. I do this for a couple of reasons, one of which, you will no doubt be surprised at, is to protect you. I don't want to leave you hanging by solely making the decision here.

Please realize that I'm not desperate to publish my review in Animal Behavior; I have, in fact, been encouraged by several people to go elsewhere, to a more important venue, and that's probably what I

should do now. But it feels only proper, in my mind, to give ABS another chance to rise to an occasion that has more than a little significance for it.

1) My reputation—you feel you need to protect me from myself? Now I learn from your 10 December email that one of your major considerations is to spare my reputation. That's thoughtful of you, but I think you have far bigger things to worry about than my reputation.

I have taken calculated risks during my career. A quarter century ago I took the field of bioacoustics to task for inappropriate experimental designs (i.e., pseudoreplication, among other issues) and sloppy science. My "attack" drew the ire of many (including my postdoctoral adviser Peter Marler), and my immediate adversary was William Searcy, who published a rebuttal, to which I replied. References below:

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

Searcy, W. A. 1989. Pseudoreplication, external validity and the design of playback experiments. *Animal Behaviour* 38:715-717.

Kroodsma, D. E. 1989. Inappropriate experimental designs impede progress in bioacoustic research: A reply. *Animal Behaviour* 38:717-719.

On this same issue, Steve Nowicki informed me a few years ago (July, 2011), at meetings in Millbrook, New York, that pseudoreplication didn't matter, because no papers that had pseudoreplicated had ever been refuted. Go figure.

Just five years ago, Bruce Byers and I submitted to *Animal Behavior* another article that took a broad swipe at the field of avian bioacoustics. In spite of untold numbers of articles that confirmed how important songbird song repertoires were for females, our critical appraisal revealed no solid, scientific evidence. Zero. Our article deflated much of the legacy of our immediate adversary, who, then serving as an editor at AB, ignored vast conflict of interest and handily rejected our article. Standing in the way of good science was, once again, Bill Searcy. (When these issues were pointed out to the executive editor, our paper was quickly accepted and published.)

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

Now, I am willing to risk taking the field to task once again. This time it's more difficult, because of more blatant issues of scientific and ethical misconduct, and because advocacy for favored ideas and the need to "sell" a good story to journal editors and reviewers has escalated over the years. It is the selling (marketing) of good stories that has led to the trouble, and the sorry fact that the rewards of the selling far outweigh the risks. I would hope that my review would help to increase the risks, and therefore help return the study of animal behavior in general, and birdsong in particular, squarely back to the realm of science.

I have not made any friends in the above endeavors. Maybe some respect. I'll settle for the satisfaction that perhaps I've nudged some people closer to being scientists rather than story-tellers.

The bogus charge that I am against young women in science.

I've heard that from one of the Podos students. It is a good diversionary tactic, one that shifts the focus from matters of science to matters of prejudice and injustice on my part. Moseley played this "gender card" with me, as I called it, and I in turn played the "science card" with her. I'm willing to listen to arguments that I am wrong, willing to hear anyone out who wants to argue that we should in fact have lower standards for women than for men when it comes to doing science.

Failing to act now, failing to address these critical issues of scientific and ethical misconduct sooner rather than later, will only put more young female and young male scientists in harm's way. I deeply regret how some young investigators may be harmed in this correction process, but if not now, when? What satisfactory alternative is there to addressing these issues head on, now?

Goodwin has never responded to any of my many emails, and here is my last email to her, on 1 December, in which I explain how we got to this current predicament, and in which I invite her to respond one last time before I send the document out to a broader audience. I express regret for any pain she might be experiencing, but I have no clue as to whether she is in deep distress or dismisses me as an angry old fruitcake. Again, I received no response (my first clue was to come 18 days later, when the police threatened me with criminal harassment).

To Sarah Goodwin
CC Jeff Podos Dana Moseley david.lahti@qc.cuny.edu
9:20 AM

Hello Sarah (copy to Jeff and Dana and David):

It's 1 December, colder than it was back in May, literally too. I'm sorry where all of "this" has headed, especially for a young graduate student just starting out in science. But, sadly, what you published with Jeff is not "science," and therein lies the problem. It's even more of a problem when authors refuse to communicate about their published work (see "Ethics in Publishing," copied below from the Animal Behaviour Society website). How a graduate advisor would allow his student to publish what you did, and then stonewall communication about it, is beyond anything I can imagine. Perhaps others might have dropped the matter, but your and Jeff's (and Dana's; but not David's) refusing to communicate led me to read further and further, uncovering a culture of advocacy and non-science in which you (and Dana) represent the third generation.

In an attempt to curb this advocacy and return birdsong to the realm of science, and to try to prevent other young graduate students from being recruited into this culture, I've pressed on. Later today, barring any last-minute stays, I'll send my accumulated reviews out to a larger audience (Podos, Nowicki, Searcy, and Vehrencamp have already had it for a month), and eventually seek publication for the document.

I wish you the best in science, learning about the behavior of nature, but the key word is "science." If instead you plan to publish more of the same, pursue a career much as Jeff has, and then recruit graduate students of your own into this culture, I respectfully suggest that another career might be more appropriate for you.

(This all seems so harsh, and I've sat staring at these words for what seems an eternity. There's such a non-human element to it all, and I struggle with it. But, in the end, I reconsider your paper with Jeff that pushed me over the edge, and I have to say "enough," and I have to follow through. And then I try to click on the "send" button, and falter yet again . . .)

If you want to talk about any of this before I continue with the above plan, I can be reached at 413-247-3367, just across the river from you. Or email me. I'm very sorry for the pain this must be causing.

Regards . . . Don Kroodsmma

I can well imagine how the above letter would be distorted. The argument would go something like this: "Kroodsmma's personal attack on Goodwin and Podos is reprehensible, made even more so because he is telling a young female scientist that she should find another career." Anyone who wants to believe this will. In contrast, anyone who objectively reads my critique of Goodwin and Podos (2014) and then reads the above letter will, I am confident, come to a different conclusion.

I suppose these charges could be continued. Further evidence against me is the critique that I wrote to the paper that Dan Mennill and two young female scientists published in *The Auk*. Again, among the authors is an older (male) scientist training young (female) scientists how not to do science. If we are to learn about what birds (or other animals) actually do, we do not need more papers like theirs.

Kroodsmma, D. 2011. Neither individually distinctive songs nor "lek signatures" are demonstrated in suboscine Screaming Pihas. *Auk*. 128:789-790.

The more general charge that I am "attacking graduate students."

I deal with that elsewhere in this document. If what I am doing is attacking, it is an attack on a culture of advocacy that is transmitted from graduate adviser to graduate student. Let the graduate adviser step forward and defend what is being published.

I am going to consolidate some of this information on "attacking graduate students in a 10 January email to Michelle Scott, as this accusation is especially troubling and infuriating.

Criminal harassment (for details, see APPENDIX).

Is this the reputation that I am being protected from, that of a Criminal Harasser? In a word, "pathetic." My advisers had other words (some of which are censored here): ridiculous, stupid, paranoid, horrible, a desperate attempt to intimidate and silence you, an explanation of why Michelle Scott suddenly behaved so uneditor-like.

Never once does Podos even acknowledge receiving an email from me, let alone ask me to stop sending them. At any time he could have asked not to receive emails from me and I could have honored his request. Rather than contact me directly, he asks the police to intervene?

All this, and never, *NEVER* is one iota of science addressed.

Other diversionary smears.

I expect them, as they are the only defense against what I critique in my manuscript. If I can be discredited, then my critique can be discredited.

I am quite sure that the campaign has already started, and that some of the emails you have received are good evidence of that. In fact, you'd have a tough time convincing me that those emails from your friends

whom you highly respect in the ABS were not instrumental in turning you from sympathy to anger on this topic. I respectfully ask that the emails that you have received on this topic also be archived with AB materials. At some time in the future, when some historian is contemplating the history of AB and ABS, all of these materials will be very helpful.

Challenge my reputation as a scientist? I invite critical appraisals

I welcome a dialogue from anyone and seek out critical appraisals about papers that I publish.

Consider my last paper. For my latest paper on song learning by bellbirds, for example, I requested that the journal ask for a review from Dan Mennill, because I was confident he'd challenge almost anything I would write (based on an exchange we had in *The Auk*—see above). That research has been evaluated thoroughly (or not) twice, to my knowledge:

1) At NSF, the panel for Animal Behavior declared, in a rather insulting and condescending tone, that there were "obvious" other explanations besides vocal learning for my data, and the panel summarily dismissed the proposal in just a couple of sentences. When I called John Byers, then director of the program, and asked for just one other explanation for my data, he had none, because there were none. (The birdsong expert on the panel at the time was simultaneously publishing papers the likes of Podos, Peters, and Nowicki (2004), as reviewed in my document.)

2) From the *Wilson Journal of Ornithology*, which published this paper, I received the Edwards Prize, for best journal paper of the year, and simultaneously received their Margaret Morse Nice award, for lifetime achievements in ornithology. I welcome a serious, scientific challenge to any of the statements or conclusions in this paper (just don't tell me, as others have, that the results can be explained by global warming, or by dying batteries in tape recorders over the decades, or by sick birds, or by hybridization with an unknown species just over the hill, and the like—I want serious challenges with some biological basis):

Kroodsma, D., D. Hamilton, J. E. Sánchez, B. E. Byers, H. Fandiño-Mariño, D. W. Stemple, J. M. Trainer, and G. V. N. Powell. 2013. Behavioral evidence for song learning in the suboscine bellbirds (*Procnias* spp.; Cotingidae). *Wilson Journal of Ornithology* 125:1-14.

Or the paper before that, in *Animal Behavior*, with Bruce Byers:

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

It is a critical appraisal of avian bioacoustics research, much of it by Searcy and Nowicki, so I requested Steve Nowicki as a reviewer, because I knew he could mount the strongest possible criticism for this paper (Searcy, it turns out, unbeknownst to us, would be the journal editor for this paper; for whatever reason, Nowicki did not review it). I want to know the weak points of my thinking in any research that I publish. I know of no serious rebuttal or challenge to our paper, but I'd welcome one from anyone who wants to take it on, ***JUST AS I WELCOME A SERIOUS SCIENTIFIC CHALLENGE TO THE DOCUMENT THAT I HAVE PROPOSED AS A FORUM ARTICLE FOR ANIMAL BEHAVIOR. SO FAR I HAVE NOT SEEN A SINGLE IOTA OF SCIENCE CHALLENGED.***

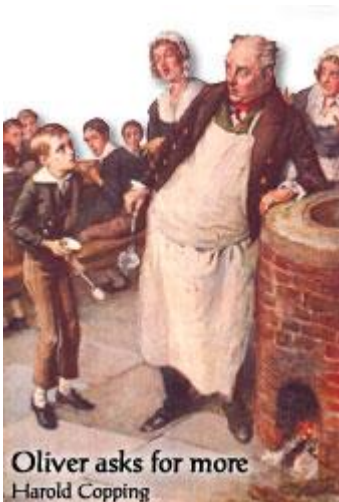
Inviting critical appraisals rather than confirmation of one's research would go a long way toward reducing the nonsense that gets published. Ten years ago, for example, I told Podos that if he wanted his papers published, he had better tell journal editors and NSF panels to avoid me as a reviewer. I've not seen a single item to review since.

I cherish the following compliment from Andrew Horn, written one day before I attended the ornithological meetings in Rhode Island that set this entire saga in motion. I had complimented him on a 1996 chapter that he had co-authored with two former graduate students of mine, Cynthia Staicer (young female *scientist*) and David Spector. In my strong opinion, all of the literature I have critiqued has little to do with what birds actually do.

Thanks for the kind words about the chapter, though I must admit its merits are largely thanks to Cindy and David's field skills and exacting attention to what birds actually do — timeless qualities I've always associated with the folks that came out of your lab.

I have asked for more true science from those who study animal behavior, with the kind of scientific integrity that Feynman speaks of. In turn, I feel I have been treated by AB in much the same way that Dickens' Oliver Twist was treated when he asked for more food. For just a bit of levity, but a serious message:

Oliver Twist asks the Animal Behavior Society for MORE SCIENCE



Oliver asks for more
Harold Copping

Oliver Twist and his companions suffered the tortures of slow starvation . . . at last they got so voracious and wild with hunger, that one boy, who was tall for his age, and hadn't been used to that sort of thing (for his father had kept a small cook-shop), hinted darkly to his companions, that unless he had another basin of gruel per diem, he was afraid he might some night happen to eat the boy who slept next him, who happened to be a weakly youth of tender age . . .

Child as he was, Oliver was desperate with hunger, and reckless with misery. He rose from the table; and advancing to the master, basin and spoon in hand, said: somewhat alarmed at his own temerity:

'Please, sir, I want some more [SCIENCE].'

The master was a fat, healthy man; but he turned very pale. He gazed in stupified astonishment on the small rebel for some seconds, and then clung for support to the copper. The assistants were paralysed with wonder; the boys with fear.

'What!' said the master at length, in a faint voice.

'Please, sir,' replied Oliver, 'I want some more.'

The master aimed a blow at Oliver's head with the ladle; pinioned him in his arm; and shrieked aloud for the beadle.

The board were sitting in solemn conclave, when Mr. Bumble rushed into the room in great excitement, and addressing the gentleman in the high chair, said,

'Mr. Limbkins, I beg your pardon, sir! Oliver Twist has asked for more!'

There was a general start. Horror was depicted on every countenance.

'For MORE!' said Mr. Limbkins. 'Compose yourself, Bumble, and answer me distinctly. Do I understand that he asked for more, after he had eaten the supper allotted by the dietary?'

'He did, sir,' replied Bumble.

'That boy will be hung,' said the gentleman in the white waistcoat. 'I know that boy will be hung.'

Nobody controverted the prophetic gentleman's opinion. An animated discussion took place. Oliver was ordered into instant confinement; and a bill was next morning pasted on the outside of the gate, offering a reward of five pounds to anybody who would take Oliver Twist off the hands of the parish. In other words, five pounds and Oliver Twist were offered to any man or woman who wanted an apprentice to any trade, business, or calling.

'I never was more convinced of anything in my life,' said the gentleman in the white waistcoat, as he knocked at the gate and read the bill next morning: 'I never was more convinced of anything in my life, than I am that that boy will come to be hung.'

As I purpose to show in the sequel whether the white waistcoated gentleman was right or not, I should perhaps mar the interest of this narrative (supposing it to possess any at all), if I ventured to hint just yet, whether the life of Oliver Twist had this violent termination or no.

In summary, I'd appreciate it if you left my reputation for me to worry about. If you want to worry about reputations, look more closely at the scientific merits of my document, and who is doing what, and how the Animal Behavior Society is represented. Your energies would be better spent worrying about those individuals, some of whom you greatly admire, and about the reputation of the Animal Behavior Society itself.

2) I follow AB protocol and am publicly chastised for it? You misinterpreted my actions, which led to your belief that I was dishonest, and then you morphed from sympathetic to angry. I was simply following AB protocol for a Forum article, and you publicly rebuked me for doing so. Maybe a public apology would also be appropriate.

And, in your 10 December "sorry state of affairs" letter, I resent your repeated pejorative use of the term "blast email," which is a term used for mass marketing.

What more can I say? A disaster on several fronts.

3) You have no conflict of interest? You feel conflict of interest does not affect your decisions.

I'm sure that Bill Searcy (see above) felt the same way when he handily rejected our manuscript that debunked a good bit of his legacy.

It matters not that my manuscript soundly criticizes some whom you greatly admire in the ABS, and that these are the very presidential leaders and faces of the Society? It is possible that you could remain totally impartial in your decision, of course, but who would believe that? Avoiding the very appearance of conflict of interest is as important, I believe, as avoiding the conflict itself. I would suggest that you consult with your ethics editor, but then I need to remind you of her close relationship with Sandy Vehrencamp, who is featured in my document.

4) Your prejudging my nonsubmitted manuscript. If I had formally submitted what I disseminated, of course you would have rejected it. But I didn't formally submit it, and never would have. That long, somewhat informally written draft was designed to be thorough (taking papers one by one), to get any feedback that anyone would want to offer, and to open a dialogue to address issues of science. I think you've effectively scuttled a thorough attempt at dialogue. (Quote from an email I've received: "I am not surprised the ABS person . . . [unkind adverb deleted] . . . chose to butt in.")

5) Other difficulties. Reject a proposal based a) on anticipated difficulty of finding impartial reviewers, b) on anticipated length of unseen manuscript and burden on a reviewer, c) on the unknown reaction of Elsevier to a manuscript of unknown length, or d) on the tone that you didn't think I'd be able to achieve?

I have nothing more to say on this topic.

6) Just what should be "stopped"? You wrote "My goal was to put a stop to this." What is the antecedent for "this"? What actually should be "stopped"? We have a serious difference of opinion about what should be stopped.

7) Not one iota of science has been addressed. I had requested that you send to me just one iota of scientific discourse on which someone has disagreed. Given that you haven't sent anything, I take it that no science has been discussed with you.

Here's a quote from Gene Morton's email, sent to you and everyone else, summarizing the scientific issues: "Don is right on with his criticism . . ."

7) My unprofessional tone—let's think about that. My tone continues to be that of "personal attack," not the "voice of an objective professional," you say.

I want to spend some effort thinking about this tone. I told you that I'd work to your specifications on the tone in the submitted manuscript, but you chose not to believe that. OK. That's your choice. You are the gatekeeper for AB, and I accept that. What you say goes, but I think this issue is far bigger than you realize, and in the end I think that you will want reinforcements from ABS officers (e.g., president, ethics editor) so that you don't stand alone on these issues. As this all moves beyond consideration by AB and ABS, you will not want to stand alone (sorry, now I am looking out for your reputation!). None of that is a threat, just fact.

What I'm going to do here is summarize my critique of two papers, and then I'm going to ask the following question: "What words can be used to describe what has transpired here so that the words sound like a 'professional objective voice' and not like a 'personal attack'?"

Summary of Goodwin and Podos (2014)

(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.)

Here I briefly summarize my longer critique of Goodwin and Podos (2014):

1) Because of items 2-5 below, there is simply no truth in the title (above), no truth in the strong claims made in the abstract (see excerpt just below), and no truth in any claims made throughout the paper:

Our results provide the first evidence that animals like chipping sparrows rely on precise assessments of mating signal features, as well as relative comparisons of signal properties among multiple animals in communication networks, when deciding when and with whom to form temporary alliances against a backdrop of competition and rivalry.

2) Most importantly, the authors *knowingly* omit reference to two biological facts that fatally undermine the premises of their paper. These two features of the natural history for chipping sparrows (concerning how a chipping sparrow acquires his song and how he uses space) were revealed in the same study population that was used by Goodwin and Podos, and published by a student in their own department, but the biological facts are entirely at odds with the assumptions and results of this paper. (Details provided elsewhere.)

3) The authors did three major analyses (focusing on trill rate, on frequency bandwidth, and on a combination of the two), but discarded and never mentioned the two that did not give a statistically significant result and therefore did not support their preconceived ideas of "performance"

4) The one retained data set (on trill rate) is "statistically significant" only through misuse of statistics, and when correct probabilities are used for the binomial test, even this data set is statistically nonsignificant. (But even if statistical significance would have been achieved, the results would still be biologically meaningless.)

5) Many other uncertainties render the paper problematic (unmarked birds, how playback stimuli were prepared and how often used, whether observer was blind, etc.)

Secondary issues:

6) The authors have refused to communicate with birdsong specialists who have inquired about this paper, in violation of the "Ethics in Publishing" code promoted by the Animal Behavior Society. (Instead, the authors have used the University Police to threaten me with criminal harassment charges if I attempt to communicate with them in any way.)

7) This paper attracted attention with a best student paper award at a scientific meeting during May 2014 (Association of Field Ornithologists), and even though multiple correspondents then pointed out the serious flaws in the study, the authors gave the same flawed talk at the ABS meeting during August 2014. (The authors might claim that a different paper was given; those listening carefully in the audience might

have detected a hint of a difference in the second talk, but the message of the paper was the same, and equally false.)

Summary of Zollinger, Podos, et al. (2012)

Podos and coauthors chastise Cardoso and Atwell (2011), who are claimed to have failed on the following qualities of good science (quoted from my longer document):

- 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,
- 8) problems in published papers that "undermine the validity of the results reported and the conclusions reached,
- 9) using "basic principles" of science, and
- 10) and, more broadly, how papers failing on these measures will "have a profound adverse effect on the way the research field is viewed by the rest of the scientific community."

(My take on Podos' authorship here: The real "crime" of Cardoso, Atwell, and coauthors, from the perspective of Podos, had to be that they had begun to debunk in other publications the entire performance line of research championed by Podos. Hence, Podos' authorship is a not-so-subtle attempt to discredit the opposition, in much the same way that I expect to be discredited.)

Let's search for "objective professional language" to address the above two papers

Let's search for the "objective professional" voice, one that does not involve "personal attacks."

- 1) Let's start with ethics.

Ethics issue #1. Here is an excerpt from the Animal Behavior website:

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>.

Here is the first of many emails and US mailings that I have sent to Podos, Goodwin, or Moseley:

Email to Sarah Goodwin, 9 July 2014

Hello Sarah:

Chipping sparrow season is almost over, I assume. It can be a long field season.

In talking with Mike Beecher at Univ of Washington, I learned that for your 2014 Biology Letters paper you did analyses not only on trill rate but also on frequency bandwidth and the combination of the two measures. I wondered if you could help me understand why in the paper you focused only on trill rate.

Thanks.

Regards . . . Don Kroodsmma

I never heard back from Goodwin, but at least the other Podos student had the courtesy to write back and inform me of the following:

Oct 10, 2014

I am writing to let you know that I have come to the decision to delete personal/professional emails from you without reading them.

Sincerely,
Dana Moseley

Podos has never responded to any inquiries.

I am not alone in this matter. Someone at another institution has also inquired about the Goodwin and Podos (2014) publication, and after some initial communication in which some rather serious shortcomings were revealed, all communication stopped, leading that person to write to me:

I agree that it is important to try to communicate with other people about our science and you've certainly tried to make them do that. It seems however that the Podos group is in a perpetual circle-the-wagons mode for reasons I don't fully understand but also find unethical . . . I will be happy to see your review in print and I think it will be a valuable document especially for people just starting out in this field.

Animal Behavior says unequivocally that refusing to communicate about one's publications is unethical. But if I say that Goodwin and Podos have been unethical, I am sure that I will be accused of a personal attack, for using language not fit for professional discourse.

Ethics issue #2. When authors knowingly communicate a highly flawed study to an audience, as was done at the ABS meetings during August 2014, what do you call that? To me, this is even more unethical

than refusing to communicate about something that has been published. Presenting a knowingly flawed paper is a lie, a deception; dishonesty at its worst; a disgrace to what we value as science. But I know I am not allowed to say anything like that. What words can be used to convey a sense of outrage over this issue? Probably none, because some deeds are so heinous that they are beyond reproach, outside the realm of what we as scientists think even possible or capable of or comfortably addressing.

Scientific/ethics issue #3. As I write above, "The authors knowingly omit reference to two biological facts that fatally undermine the premises of this paper." How can I write anything about this topic with words that sound professionally objective, when what they have done is simply deceptive and reprehensible (though generates a far better story)? Might the authors claim that a) they didn't know about these facts, even though they were published by a student in their own department, working on the same sparrow population? Or b) that they knew about these facts but didn't realize the relevance of them? Or c) that they knew the facts but they were omitted because they just didn't fit with the storyline. I'm not allowed to even write the third possibility, which I think might be closest to the truth. Is this the worst in scientific conduct, ethical conduct, or both? What words can one use to satisfactorily address these issues? It would help immensely if the authors were willing to discuss their work, and then we wouldn't be in this bind.

Scientific/ethics issue #4. Discarding data just isn't done. Is it a personal attack to say that? May I cite authorities who say this is simply unethical and not the way science is done?

On "Science" vs. "Make-believe." Those words aren't professional either. How far may I go in saying that this Goodwin and Podos (2014) is just make-believe, with not a shred of truth in it? I can't say it's a "pack of lies," because that implies intent, that the authors knew they were publishing non-truth. Anyone can weigh the evidence that I lay out and come to his/her own conclusion, I suppose.

It is, of course, a personal attack to call someone's work make-believe. But tell me it isn't make-believe, and if it is make-believe, then tell me what words that an objective professional may use to say what needs to be said. Don't just shoot the messenger and worry about the messenger's reputation.

And more. You get the point. I could continue, listing the other issues, but the tactics used in this Goodwin and Podos (2014) paper are so far beyond comprehension that any messenger who tries to reveal these matters is likely going to be considered the culprit. It is therefore my tattered reputation, or what is left of it, that you feel you must protect, and you are not worried in the least about anyone else, because how could the things of which I write possibly be true?

The Ultimate in Hypocrisy—Tell me it's not, in reference to the Zollinger, Podos, et al. article and their list of items that characterize good science. I have no idea how to address this issue. My critics censored everything I tried to write about this topic, and in the end I simply said the following:

Perhaps I could simply say (without consulting my advisers) something to the effect that there seems to be somewhat of a mismatch between this list of laudable qualities for good science provided here and the quality of research papers on which the second author has his name, many of them reviewed not so positively in this document.

I will be more blunt (and honest) here than in the document I sent to cited authors: Every item in that Zollinger-Podos-et al. list about "good science" is violated to the extreme in Goodwin and Podos (2014), and in his other papers as well. *Every item*, I want to scream. How do I say that using words that don't sound like a personal attack? How do I say that the Podos who is marketed in Zollinger-Podos et al. is very different from the real Podos who is revealed in his own research papers and those of his students? I think this whole topic is probably off limits for any discourse that uses a "professional objective voice." It is just too unseemly, as it is an evaluation of character, too far beyond what we can imagine we have to deal with as scientists. It is the kind of evidence, once revealed, that the judge in the courtroom instructs the jury to ignore, because it all needs to be stricken from the record.

8) Some obvious other questions arise:

1) Personal attack? Please give me one example. In view of all of the above, I would ask you to go to the document that I circulated and point to one example where, instead of addressing matters of science, I have personally attacked the researcher. I think it would help me understand better the charges that you are leveling against me.

2) Use of tax dollars for what? Podos, one might ask incredulously, has received over a million public tax dollars from NSF in the last decade to pursue this kind of work, and to teach the next generation of researchers, as in Goodwin and Moseley and others? Anti-science Republicans in charge of the House and Senate just drool over this kind of non-science and abuse of tax-payer money, not to mention attempts to cover it up. The damage to scientists everywhere, in all fields, is immense. It is in the best, long-term interest of every true scientist, especially those who study animal behavior in general and birdsong in particular, to stop this kind of pseudoscience and the use of tax dollars to support it. (Depending on how matters develop in the near future, excerpts from this correspondence will or will not go directly to NSF.)

3) Compared to Marc Hauser situation? Just how does this publication by Goodwin and Podos differ in its merits from those published by Marc Hauser? What is the difference between 1) adding a few numbers to get the story one wants and 2) discarding numbers until the remaining numbers tell the story one wants? Hauser is accused of outright fabrication of data, but how is that different from Goodwin and Podos knowingly conducting themselves and publishing as they have? I would like to hear anyone give me an explicit answer to this question. In my opinion, the consequences for the literature on animal behavior are identical, and devastating.

4) In all of the papers that I have critiqued, what has been learned? Next to nothing!

What has been learned about the behavior of nature? What has been learned about what birds actually do? Go ahead. With the informed, critical eye that I have provided, look at paper after paper and make a list of the things you have learned about what birds actually do.

I found very exciting what I learned about song development in Lahti et al. (2011), but then the authors did their (or one of the authors did his) damndest to cover it all up, so as to make the results conform to Podos' performance dogma (a word used by one of Podos' own students).

I challenge anyone to look at all of those papers and come up with a list of what has been learned about what birds actually do. I find this embarrassing, and beyond sad. For all of the person-years and millions upon millions of dollars spent doing the work and publishing these papers, I find it tragic that almost nothing has been learned. What a waste!

5) Graduate student education, the next generation of researchers? Graduate student Moseley won an Allee award at ABS last year (2013), I believe. Podos on his website boasts of a "clean sweep" at the ornithological meetings in Rhode Island this year (2014), where Goodwin and Moseley received the two best student paper awards. Here's the message to graduate students: "*Gloss trumps substance*" any day, and the rewards of advocacy and pseudoscience and good stories far outweigh the risks. It is very clear how to get ahead. The way my critique of the field is being treated by AB is even more evidence of how to publish and get ahead, as no one will be held accountable for even the worst of misdeeds, and the worse the misdeeds the better off you are, because no one will dare to address them.

Here, taken from Podos' website, written by a current member-at-large officer of ABS and close colleague of Podos, is one of the reasons he got tenure (sorry, Beth). If the grad students in the audience only knew . . .

Jeff's research is highly regarded by the Animal Behavior Society, a lighthearted group of people always up for a humorous talk. Jeff's talks never fail to entertain. Graduate student attendees, in particular, seem to love to deconstruct Jeff's talks: he seems to have taken the unique approach of teaching by negative example, so students enjoy trying to find all the flaws in the presentations, ranging from experimental design, faulty analyses, and poor presentation style. While he is speaking, one can hear muffled cries of "That's seven!" "I've got nine!" from the back of the room. What a wonderful way to teach!

6) Michelle, why not use your role as editor to facilitate rather than stifle scientific discourse? In just a few minutes time, you could write to Podos, Searcy, Nowicki, and Vehrencamp, all senior people (include Goodwin, Moseley, and Lahti if you wish), and tell them, for whatever reason, that you'd like their opinion about the scientific merits of my review. Assign them just one paper to address, point by point, not in general arm-waving form. Better yet, tell them that you will share their response with me, to begin a (forced) discourse on these issues; only you can do that, and that carries just a bit of responsibility, I'd think. After weighing the outcome of our responses, you might even choose to publish a (much-revised, shortened, toned-down, etc.) Forum version of my critique, or at least keep options open.

Suggested reviews:

Podos: Goodwin and Podos (2014)

Nowicki: Ballentine et al. 2004 or Podos et al. 2004

Searcy: Dubois et al. 2009

Vehrencamp: Illes et al. 2006 or deKort et al. 2009

Lahti: Lahti et al. 2011 (David Lahti is a young investigator who had a postdoc with Podos, but he has been open about these issues of science and communicated readily with me and others. I applaud his behavior in all this. I would encourage you to give him the opportunity to respond, as it is just possible he would eagerly do so. I can say no more, and it is possible he can't either.)

Please note that I am not asking you to backtrack on any earlier decisions. I am giving you an opportunity, if you want to consider it, before I move on, because I think it is not only in your best interest but also in the best long-term (though not short-term) interest of the ABS. I'm serious.

7) Why not take a leadership role in promoting AB as a credible scientific journal?

Why not try to create another positive from this "sorry state of affairs"? In the Instructions to Authors, insert something to the following effect:

"Every author must declare that he or she has read Richard Feynman's 'Cargo Cult Science' commencement address to Caltech and Robert Gitzen's Science article on 'Advocacy'. And every author must declare that he or she has 'bent over backwards' to do science, with integrity, and that there is no marketing and advocacy of the kind that Gitzen describes. On a scale of 1 to 10, I rate the scientific integrity of my paper as XX, my only hesitation being that I don't know about such and such . . ."

8) Why not try to help fix this broken system?

In my attempt to establish a dialogue with researchers, I received a number of thoughtful replies. Here is one response from an interested party:

. . . you reference the need for more natural history, and more descriptive information on song use. I think the biggest issue here is the publication industry: it is only possible to put so much information into a single paper, and it is more difficult to publish purely descriptive papers. Unlike scientists who are beginning their careers and still have to make their reputations, you are in a position to try to change this, by influencing how journals work. It would be a lot of fun to do more acoustic natural history, if I thought I could get it published. In many ways, I feel like our system is broken: publication does select for interesting, well-sold stories, and it selects against replication of previous experiments, and we have to publish if we want to be academic scientists. Why don't we try to change the system?

Hers is a superb summary of the problem for the performance literature that I review. Early on, you'd think that for the trill rate/frequency bandwidth graph someone would have published a simple *description* of how variation in "performance" is distributed among males and song types. It is highly likely that all of the literature on performance never would have happened, because it would have been realized early on that there's no consistent information on relative male quality available in the songs. A male can be "high performance" on one of his songs, and low performance on another, *as if this measure of performance were of no significance and didn't really matter to the birds*. (Cynical me, of course, believes that the obvious description has been done, but the results were contrary to the performance story and were therefore not published.)

You, as editor and gatekeeper, Michelle Scott, are in a position to help fix this broken system. Embrace this little comment that Bruce Byers and I published 20+ years ago: "To experiment first is human, to

describe first divine." It is the good descriptions that are sure to have lasting value; the experimental work I review in my paper is next to worthless, now and forever.

Why not have at it, in whatever way you choose, to fix this broken system??

9) Why not help science trump nonsense? You write that this is a sorry state of affairs. Yes, it really is, but we have very different perspectives on what is sorry about it. I still believe that, in the end, science will trump nonsense (though maybe not at AB and ABS). Those of us who care cannot let it be any other way.

In Parting

Be assured that I write all of the above in a calm voice. Somehow, somewhere, the issues that I raise will be publicly and professionally addressed, and they will be addressed in a way that, I hope, will influence how future graduate students will be trained to do science, Feynman style. It pains me enormously to see Goodwin and Moseley caught in this debacle, for example, and I hope that my persistence in this matter will save more than a few future students from being caught in this web of advocacy and pseudoscience.

Increasingly, given the conflicts of interest within ABS, and given your responses, I am confident that AB will not be the place for these issues to be aired. That is unfortunate, because it will not reflect well on ABS to have stifled this discourse only to have it emerge elsewhere.

THE SAGA CONTINUES

1 January 2015: Editor Scott to Kroodsma and others, with Kroodsma response

CC :ana.sendova-franks@uwe.ac.uk rhfmacedo@unb.br ophir@okstate.edu

GPatricelli@ucdavis.edu

Dear Don,

I am truly sorry that it is coming to this. I did not know that you have been charged with criminal harassment although I did know that something that you said to Jeff Podos did frighten him. From the correspondence from the scientific community that I have seen, I do think that you are hurting your reputation. As I said earlier, you were once a hero to the bird song world. Actually after my brief e-mail to the recipients of your mass dissemination of your manuscript, I heard from 3 people asking me for an explanation. Of course I answered all of them (including Gene Morton) with the outline of what had passed - including that I formally rejected your submitted proposal for a review article following the protocol of the journal. (I thought it was a small step to say that I would reject it as a Forum article as well.) I was purposely brief in my mass e-mail to spare your reputation as much as possible. I have tried to be tactful to you and to everyone involved but as I said before, your manuscript is much too personal. Several people have said to me that this goes way beyond scientific discourse. However several people have also expressed

to me that many of your ideas are sound and valuable but that they need to be expressed in a different format.

Kroodsmas:

- 1) I said something to Podos that "frightened him"? Everything I have communicated to Jeff is in the attached emails. He should be frightened. It is his career that is at stake. And he has dug himself a foxhole so deep that even the UMass police are telling me I need to tell all 50 correspondents they're not supposed to communicate with him. Beyond bizarre!
- 2) My reputation is being hurt? Two points:
 - a) When is the last time you heard defendants say something kind about the prosecutor? All of your correspondents are defendants.
 - b) The hero stuff. I can well imagine one of Podos' young grad students saying I'm a "fallen hero" (especially with your accusation of 13 December that I am "attacking graduate students"). Given the context, I'll live with that just fine. How about surveying a cross section of graduate students and getting a wider opinion? I suggest grad students who gave talks at the 2013 ABS (where a Podos student won the Allee Award) and the May 2014 ornithological meetings in Rhode Island (where the two Podos students took top honors, a "clean sweep" the boast on Podos' website). Give all of those grad students my documents, letting them realize how they were out-glossed by nonscience, how they were all cheated out of an opportunity to win top *scientific* honors. How do you think they feel? I sat in the ornithological audience when the two Podos awards were announced; I myself felt cheapened, and that the entire scientific endeavor of the meetings had been undercut and devalued. I don't think you'll get any of the "fallen hero" nonsense from those grad students.

You have misunderstood the role of "ethics editor". Alex is concerned with the wellbeing of the animals used in experience. Gail is one of the scientific editors who sees mostly the bird manuscripts. Ana and I and the publisher deal with the problems of scientific ethics. I don't know quite what you are asking me to do with regard to the Animal Behavior Society and the Executive Committee. They do not usually get involved with which manuscripts the Executive Editor accepts or rejects. The journal has a policy that has been in place for at least 6 years that the Executive Editor can reject manuscript without review if they are judged to have no chance of a favorable review. I have made that judgement in your case. Please go back to your personal advisors and ask for their help to prepare a critique of less than 3,000 words. If you do, you and I can talk again. It is definitely not my desire to suppress valid scientific discourse.

Kroodsmas: Yes, I guess I have misunderstood the role of an "ethics editor"—they're about ethical treatment of animals, not ethical behavior by scientists. And sure, I fully accept that you have the power to accept and reject. I am simply saying that I don't think you want to stand alone in making an advance decision to deny a submitted manuscript from me on this topic. Do I need to elaborate? Perhaps. Given that the top "defendants" are people you "admire," and given that they are past and future presidents of ABS, it will not look good for you or ABS to deny a potential airing of

these serious issues in the ABS journal, only to have the issues aired elsewhere. I'll leave it at that. That's not a threat. It's just a fact.

One more thing. In your 13 December letter to Morton I am accused of "attacking graduate students." I'll reprint here my unsent response to that email:

I rarely coauthored papers with my graduate students, because I wanted them to have full credit for the work they did. But I have been told that one reason advisers publish with their graduate students is to give the paper credibility, to show that the adviser stands behind the paper, and for the adviser to take the ultimate responsibility for what is published in case something goes wrong. So I have a simple question: Where is the adviser's response to my repeated inquiries?? I am "attacking" a culture of advocacy that is passed from adviser to graduate student, and I would like to see the adviser defend what is being published.

I will be preparing a manuscript to address these issues, in the most concise, matter-of-fact way I can muster. If it is more than 3000 words and you choose to reject it based on length alone, that is your prerogative. Until then, as you suggest, we have nothing to say to each other.

Kind regards . . . Don Kroodsma

Sincerely,
Michelle

Correspondence with University of Massachusetts Police

19 December: Kroodsma to UMass Police

Hello Officer Liptak:

Thank you for the conversation a few days ago about my criminally harassing members of the Biology Department at UMass. I will not in the future intentionally send any emails or attempt to communicate in any way with either Podos or Goodwin in the UMass Biology Department; I'll extend that to former student Moseley, who is on a postdoc elsewhere.

I should tell you that I had initiated a dialogue with about 50 other people involved in research on this topic, and UMass Biology will undoubtedly receive emails from them. Would you like me to request that they stop sending emails to UMass Biology?

regards . . . Don Kroodsma

1 January 2015: UMass Police to Kroodsma

Mr. Kroodsma,

My apologies in the delayed response as I have been away from work since December 18. To prevent any issues I would advise you to avoid contact with anyone at UMass Biology UNLESS you have a legitimate reason for contact. If you have any further questions please feel free to contact me.

Officer Liptak

1 January: Kroodsma to UMass Police

Hello Officer Liptak:

I think you miss my point. I personally will avoid communicating with your people in Biology, but I had initiated a dialogue among about 50 people, some or many of whom might be writing to your people in UMass Biology as a result. Do you want me to tell them not to correspond with UMass Biology on this matter.

regards . . . Don Kroodsma

1 January: UMass Police to Kroodsma; Tell all ~50 people they should stop communicating with UMass Biology (Podos) on this matter

Mr. Kroodsma,

Thank you for the clarification. Yes please advise them to stop.

Ofc. Liptak

3 January: Kroodsma to UMass Police. This is beyond absurd.

Hello Officer Liptak:

I don't think you realize how absurd this all seems to those of us who try to fathom what is going on here. I don't believe you care about any of what I am going to write here, as I don't think you can see beyond the criminal harassment charge, but I'm going to elaborate just a little anyway before telling you why I am not going to comply with your latest request:

Are you aware that never once has Podos (or anyone in "UMass Biology") communicated with me and asked me to stop sending emails to them? They have my email address and my US mailing address. I've sent them my phone number. I've repeatedly invited a dialogue. And, to top it all off, I'm a member of the same department at UMass (though emeritus).

How could my emails possibly have been honestly interpreted as harassment? Here is a summary of my unambiguous intent in the 8 emails I can find:

I repeatedly attempt to engage the authors [Podos and his students] on the substance of the science (emails 1, 2, 3, 4, 6, 7, 8)

I offer to come to UMass to talk face-to-face (2) or talk on the phone (3; giving my phone number, 7), or communicate in any way possible (4, 6)

I offer a last minute appeal to talk these things over before my review document is sent out to a larger audience (7)

Those are the professionally courteous and ethical ways to proceed when there is a disagreement about science, and it is professionally courteous and ethical to respond to such inquiries.

So, after all this, Podos goes to the UMass police to threaten me with criminal harassment, because Podos can't communicate with me directly? Doesn't it puzzle you just a little that they need you to communicate with me? Sure, they may be distressed, as their careers are on the line with what they have done, but it seems to me that they are simply using you to try to intimidate and silence me. I can tell you that won't be successful (though I certainly will comply with the request not to communicate with them anymore).

You might say I "baited" you in asking whether you wanted me to write to all 50 people to tell them not to write to Podos et al. I needed to know if you took Podos seriously and at face value (you did, based on your request to email everyone), or whether you were merely part of an in-house, local coordinated plan to intimidate me, in which case you'd never ask me to take this situation to the larger audience (based on your email request, I'm confident you were not involved in so unprofessional an approach to this situation).

You may view me as villainous, and that's fine with me, but I will not stoop to intentionally doing (unnecessary) emotional and professional harm to Podos et al. Sending out an email to all 50 people, explaining this situation that is beyond bizarre and telling them not to write to Podos, would be embarrassing beyond measure for Podos. It's even possible that true harassment would then begin.

I admit that I am far from understanding what is in the head of Podos; he won't reveal anything to me. If, however, you write back to me and tell me that you have consulted with Podos, and my instructions are still to explain this to all 50 people, I will do so. I suppose not even that request would surprise me, given all that has (or has not) already transpired in this ridiculous sequence of events (most of which you know nothing about).

Respectfully . . . Donald Kroodsma, emeritus Biology

14 January: UMass Police to Kroodsma

Mr. Kroodsma,

Thank you for writing. My primary responsibility is to the University employees (Podos, Goodwin and Moseley) to assure they are able to complete their work without concern of intimidation or harassment. You have agreed to not communicate with them in the future, thank you. Your business with others in the field of Biology is yours to conduct as you wish.

If I can be of further assistance please let me know, otherwise I consider the matter resolved with your agreement not to contact these three people.

Thank you,

Officer Liptak

6 January. Pavel Linhart to Editor Michelle Scott, Ana Sendova-Franks, and me

Dear Dr. Scott, Dear Dr. Kroodsma,

Thank you for your answers in a matter of rejecting Don Kroodsma's manuscript. I wanted to react immediately to your emails but I had a lot of other duties to be done prior Christmas.

Dr. Scott, I can accept your explanation. I assume that you have the authority to do such decisions and also I can understand that you would like to see different tone of the letter and the manuscript by Dr. Kroodsma.

However, I also think that it is a pity that you chose responding this way.

Unfortunately, you left an open space to think that there were other, 'behind the curtain' influences on your decision. It would be so much better to see that your decision is backed with the opinions of other editors or some independent jury.

You also closed one legitimate door for dr. Kroodsma how he could express his doubts to public, probably further deepening his feeling that the issues he raised were deliberately ignored.

To protect the AB journal, it would be possible to send email in which you would state that the current version would be unacceptable for AB due aspects of personal attacks, etc.

I think the only proper way to answer the insults and critique is to politely respond the issues that were raised. I also believe that for the credit of AB and ABS it would be very advisable mediate the discussion and to publish the critique in AB. The ABS fellows are criticized and AB published many of the criticized papers. Thus, AB and ABS should have an imminent interest in clearing out any doubts...

The current situation is very likely difficult for all interested parties. I do not want to further escalate the problem. However, I would like to know what the other interested people think about the manuscript and its rejection. I thought about making a brief online questionnaire (few anonymous questions like for example: is the manuscript a critique or attack? is it too much offensive? is the rejecting manuscript prior review appropriate response?) but I think I will first ask dr. Kroodsma what were the answers he got and whether people commented on some of these points.

I hope you got in touch and discussed the situation once again. I hope that the situation will come to a good ending.

Best, Pavel

RNDR. Pavel Linhart, PhD

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Institute of Animal Science

Ethology Department
Pratelstvi 815
104 00 Praha Uhrineves

6 January. Kroodsma response to Linhart, copy to Scott, Sendova-Franks

Dear Pavel Linhart:

Thank you for your letter of inquiry. A lot has happened since your original letter.

You raise many of the concerns that I have expressed, about ABS fellows, about ABS past and future presidents, about conflicts of interest in protecting ABS, about suppressing scientific discourse, about pre-rejecting a nonsubmitted manuscript, and so on. I will not elaborate here, in this letter, but I will attach a document in which I am carefully keeping track of all that has transpired in these interactions. I do so partly out of a desire just to be thorough (perhaps to a fault), but also because I am threatened with a charge of criminal harassment by the University of Massachusetts police, and I want a clear record of all that has happened.

In short, as you can see in the 1 January email to me, after a number of exchanges editor Scott is now willing to consider a manuscript from me. Given all that has happened, and given the parties involved, I have doubts that my manuscript will receive a fair evaluation at AB, but I believe (as you express) that it is in the best long-term interest for ABS to address these issues head on, in its own journal, rather than having them be forced elsewhere.

You will see in the attached document how this whole endeavor has become stranger than fiction. No one could make this up. It's all there, should you care to read it, so I will not elaborate or highlight anything here.

For reasons of privacy, and honoring one request, I have not included the responses I have received directly, although I excerpt a few in my growing document. I can say unequivocally that not one of my scientific criticisms has been dismissed or even so much as challenged. Nor has a single one of the ethical matters that I raise. I can also say that relatively few people have responded, as if the defendants do not want to further jeopardize themselves by defending what they've published. Several people have suggested that I write two manuscripts, one addressing only matters of science, the other addressing matters of professional philosophy and ethics.

My document was too offensive to Podos? Yes, it no doubt shows my great exasperation at his unwillingness to communicate, even though we live just a few miles from each other, and I repeatedly offered to visit so we could talk science. But in the attached document I also discuss what a challenge it is, in nonoffensive language, to address the seriousness of the issues that I raise. (I have not sent anything to NSF.)

One thing is worth pointing out: My review and none of this dialogue would have happened if Jeff Podos would have been willing to communicate with others (not just me) about his research, and about the research of the students he is training and with whom he coauthors. In my opinion, legitimate scientists welcome scientific discourse, to learn from each other, to advance knowledge. Other interpretations come to mind when Podos digs a foxhole so deep so that, refusing to communicate directly with me, he uses the police to threaten me as a criminal.

I suggest that we let Editor Scott off the hook, telling her that there's no need for her to reply to you (unless you really want to hear from her). I admire someone like Michelle who takes on the task of editor, and I have no doubt that she is doing the best, most honest job that she can; I feel bad for her that I've introduced this little complication into her life. As I say in my letter to her (not actually sent directly to her, but under the 1 January entry in the document), I will prepare a manuscript. Until then, we have nothing more to say to each other. Let those who are now speechless rise to respond in the appropriate public forum when my paper is published, in AB or elsewhere.

Thank you very much for your interest in these matters. I welcome any response from you.

Kind regards . . . Don Kroodsma

7 January. Kroodsma to Scott, Sendova-Franks—on the charge of “attacking graduate students”

Hello Michelle:

I thought I wouldn't have anything to say until I produced a manuscript, but I have a request, arising from your email that Gene Morton forwarded to me just yesterday:

If you want to accuse me of something, do so to my face.

Not in an email to someone else (Gene Morton), who (willingly) forwards your email to me more than three weeks later, at my request (see 13 December entry), after I learn from you that you had responded to him.

Your charge is that I am “attacking graduate students.” And, almost as troubling, presumably this is a charge that is passed on, unfiltered, from your friends in ABS who refuse to communicate with me. I want to deal with these issues *now*.

An adviser's role in mentoring and publishing with graduate students

I rarely coauthored papers with my graduate students, because I wanted them to receive full credit for the work they did. My name went on a paper only when the student had trouble getting something published. It never occurred to me that I was being negligent in my advisor role, that I was not sufficiently shielding the student from criticism or blame if something improper were published.

But I have been told that (established) advisers publish with their graduate students to give a paper credibility, to show that the adviser stands behind the paper, and for the adviser to take the ultimate responsibility for what is published in case something does go wrong. Well, something very serious has gone wrong with papers published by Podos and his students.

So I have a simple question: Where is the adviser's response to my repeated inquires?? WHERE IS JEFF PODOS (other than at the university police threatening me with criminal harassment charges)? In my 15 October email to Podos (see appendix), I suggested that he step forward and address these issues, protecting his students in the process. NO RESPONSE. In a reprehensible reversal of roles, instead of the adviser protecting his students, he's hiding behind them, using them as a human shield, and I am being accused of attacking his graduate students.

The implication of the charge is that it is fair to address the ills of a paper only when a graduate student's name is not on the paper. In what seems to be a publication-greedy world, how many papers can be found that don't include both adviser and graduate student, or an early professional? By these standards, everyone is safe and happy.

What I am "attacking," if you want to call it that, is a culture of advocacy that is being passed from adviser to graduate student, and I would like to see the adviser defend what is being published.

Let's ask graduate students if I am unfair. As far as being unfair to graduate students, how about surveying a cross section of graduate students and getting a wider opinion? I suggest grad students who gave talks at the 2013 ABS (where a Podos student won the Allee Award) and the May 2014 ornithological meetings in Rhode Island (where the two Podos students took top honors, a "clean sweep" the boast on Podos' website). Let's explain to all of those grad students how they were out-glossed by nonscience, how every one of them was cheated out of an opportunity to win top *scientific* honors. What kind of behavior was rewarded, and whose careers were advanced by those awards? Whose careers were not advanced?

How do you think the cheated students feel? I sat in the ornithological audience in Rhode Island when the two Podos awards were announced. Only I knew what had just happened; I felt cheapened, and that the entire scientific endeavor of the meetings had been undercut and cheapened. It was a very sickening feeling.

OK, go ahead, do your own one-student survey. Tell the student this:

"You have just been to a scientific meeting and given the best scientific talk you possibly could. The best student paper went to someone else, and you were runner-up, but it was discovered later that there was not a shred of truth or real science in that winning paper (e.g., Goodwin and Podos). How do you feel?"

I bet you'll hear something like this:

. . . demoralizing . . . mad as hell . . . if that's what I have to do to be successful, I want no part of science . . . or maybe I cheat, too . . . in the Olympics, they take medals away from cheaters. I think they should do the same here, and award the medal to the next in line . . . a scientist . . .

I'm tempted to do just that, write to the ornithological awards committee that feted Goodwin and Podos and ask them to rescind the award and give it to the runner-up. It would be the only fair and proper thing to do.

The more I think about this, I just become angry. If there is anything criminal in all of these matters, this tops everything: Gloss trumps substance, nonscience wins over science, advocacy over scientific integrity. Those are the messages to the next generation of researchers in animal behavior. I am not happy in a world that works like this.

Fallen hero malarkey. I don't think you'll get any of the "fallen hero" nonsense from those grad students. Period.

Enough said. But I needed to say it, needed to have it be part of this permanent record that documents the process that I am going through to right what I think are terrible wrongs in the small corner of the world where I think about science.

Sincerely . . . Donald Kroodsma

7 January: Scott to Kroodsma

Can we just stop this until I get a forum manuscript from you <3,000 words?

7 January: Kroodsma to Scott

That is my clear preference, but if matters arise, as they did yesterday in your email to Morton and Linhart's email to us, I will address them.

8 January. Michelle Scott to Pavel Linhart

Dear Pavel,

I too hope that the situation can come to a good end.

I am really not an autocrat. My job is to look out for the interests of the journal, the authors, the editors and the reviewers (in that order - I am not trying to protect any ABS fellows). That is what I have been trying to do. As the journal requires, Don did submit a proposal for a review article. (As you will remember from my earlier e-mail, I have encouraged him to either submit a review or a forum article). I consulted with the UK editor and I rejected the proposal. I also judged this manuscript as it was, to be unsuitable as a forum article. Its tone was inappropriate. In addition it was 10x longer than our usual forum articles. There is no official limit but ~35,000 words is too long - I could not get reviewers for such a job. I have told him twice in recent weeks that I would send out for review a "sanitized" manuscript of <3,000 words. I hope that he does this. I have not closed any legitimate doors.

I have made a point to use the "reply all" button for these exchanges. This is why I chose "responding this way". I agree that it is unfortunate that it has all been so public. Because Don cc'ed a few editors and some members of the ABS EC I have had feedback from some of these parties. All have been supportive of my actions. Many people have also mentioned that Don has good ideas that should be aired - just not this way. I can't tell you who has said what to me, if these people wanted Don to know they would have cc'ed him. And I ask you please to consider carefully if you want to make a questionnaire asking for opinions. What I would really like you to do is to help Don get his ideas into a shape that I can send for review. He has mentioned that he has a group of supporters who are advising him. Perhaps you can join.

Sincerely,
Michelle

APPENDIX. The Charges of Criminal Harassment

UMass Amherst police: "Criminal Harassment" emails that I have sent to "UMass Biology" 41

 Transcript of phone conversation, 18 December 2014 41

The evidence: My emails to UMass Biology (Podos or Goodwin): 42

 9 July 2014..... 43

 1 October 2014..... 43

 8 October 2014..... 44

 15 October 2014..... 44

 16 October 2014..... 46

 9 November 2014..... 47

 1 December 2014 (early in day)..... 47

 1 December 2014 (late night)..... 49

Threatening phone call from UMass Amherst police on "Criminal Harassment" emails that I have sent to "UMass Biology"

A parking ticket, going back ten years? No, that couldn't be the reason the UMass police are calling, could it? With a little advance warning, I had the recorder running when I answered (transcript below).

Here, from the UMass Police Department, is the first hint that I have received that Podos and Goodwin (i.e., "UMass Biology," of which I am an emeritus member) have even received the emails from me, let alone were upset at receiving them. I may live in another dimension, but I'd think that the first course of action for someone receiving unwanted emails would be to ask the source to stop sending them. We all "unsubscribe" to unwanted emails without the aid of the police.

If there were extenuating circumstances (e.g., I was on an extended lunar excursion and I had left my contact information only with the UMass police), I could almost begin to understand this police action. Or if there could possibly be some ulterior motives (e.g., avoid addressing the scientific and ethical issues), then I could understand . . .

Transcript of phone conversation, 18 December 2014

Kroodsma: Hello, Don Kroodsma here. This line is being recorded.

UMass police: Hi, Mr. Kroodsma, this is Officer Liptak, also on a recorded line. How are you?

Kroodsma: Good, thank you, I'm fine.

UMass police: Thank you for calling me back. I just wanted to give you a call and touch base because I had some members from the Biology Department here on campus get in touch with the department because there has been some issues I guess between you and them and some publications that you want to publish or some manuscripts that you want to publish . . . ?

Kroodsma: Yeah

UMass police: So, basically, I can't stop you from attempting to do that. That's your freedom of speech. However, the reason I am calling is that the emails that are being sent to them, I believe there's been at least 8 that I can count, that I have copies of. Those need to stop. OK. Right now, as far as I'm concerned, there is no charges being pressed against you, as far as criminal charges, but if the emails continue it's bordering on criminal harassment, and we don't want to have to go that route. So, right now, you know, as long as the emails stop and there's no contact between you and the other members of the Biology Department, then I have no issues. But what I am asking is that you do stop those emails.

Kroodsma: Very interesting.

UMass police. OK . . . There's no reason for you to have contact with them as far as I can see, so the easiest way is for contact to just cease and desist, and then there will be no issues.

Kroodsma: I would like to tell you why there is every ethical reason in the world why I should be contacting them and why they should be responding. These are matters of professional ethics . . .

UMass police: OK. Professional ethics have nothing to do with me. I have everything to do with criminal law and upholding the criminal law, so if they continue, and if the emails continue to be harassing them, then you're bordering on criminal harassment and I don't think that's a route that you want to go.

Kroodsma: May I ask you who contacted you?

UMass police: That doesn't matter right now . . . OK

Kroodsma: OK.

UMass police: So, I think you know who you've been sending emails to. They've been advised that if they get any more emails beyond this point to inform me and then we'll take it from there if need be.

Kroodsma: OK

UMass police: ok.

Kroodsma: Thank you . . .

UMass police: Alright. Thank you very much. Have a very good night . . .

Kroodsma: bye . . .

UMass police: goodbye . . .

The evidence: My emails to UMass Biology (Podos or Goodwin):

Below are the eight emails that I can find, probably the eight in the files of the UMass police. Perhaps there are others. I do not intentionally omit any, and those that are here express the nature of the attempted communications.

Not to prejudice the reader on any of these emails, but after each email I comment briefly on the content and intent of the email.

Here is a summary of those comments:

I repeatedly attempt to engage the authors on the substance of the science (emails 1, 2, 3, 4, 6, 7, 8)

I offer to come to UMass to talk face-to-face (2) or talk on the phone (3; giving my phone number, 7), or communicate in any way possible (4, 6)

I offer a last minute appeal to talk these things over before my review document is sent out to a larger audience (7)

In exasperation, and in hopes of moving science forward, I send the review document to all cited authors, asking for a dialogue, before submitting a revised review for publication (8)

1) 9 July 2014 "I wondered if you could help me understand why in the paper you focused only on trill rate."

Subject: chipping sparrows

To Sarah Goodwin, first author of Goodwin and Podos (2014)

Hello Sarah:

Chipping sparrow season is almost over, I assume. It can be a long field season.

In talking with Mike Beecher at Univ of Washington, I learned that for your 2014 Biology Letters paper you did analyses not only on trill rate but also on frequency bandwidth and the combination of the two measures. I wondered if you could help me understand why in the paper you focused only on trill rate.

Thanks.

Regards . . . Don Kroodsma

KROODSMA: Professional ethics dictate that we engage each other about published papers and disputes that arise, so here was my first attempt at what seemed a reasonable approach. I received no response, and it was also about this time that all communication ceased from UMass Biology to Mike Beecher and a student at the Univ of Washington; they had raised some serious issues, even suggesting that the Goodwin and Podos (2014) paper be retracted.

I received no response.

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"

Subject: Motor constraints . . . talking it over . . .

Hello Jeff:

It's been ten years since we last communicated (October 2004). I had sent you an in-house review of one of your papers, and I think I never heard back from you.

Now we meet again, so to speak, as I attended Sarah's and your talk at AFO/WOS. I squirmed as I heard it, and more so when I read the Biology Letters paper. Then I read some more of your papers on the motor constraints hypothesis, squirming even more. Then I read other related papers, ten in all, having difficult credible evidence for this idea that the birds pay any attention to how difficult it might be to sing a song with fast trill rate and broad band frequency. Yet the idea seems to have taken the literature by storm (much as did the idea about evolution of large

songbird repertoires by female choice, for which Byers and I could also find no credible evidence).

I have a hunch you don't want to talk to me about any of these matters, as you seem to have squelched communication with those who are concerned about your Biology Letters paper. But if you'd like to talk some of these things through, let me know, and I'll come over to UMass—I'm free most days. If I don't hear back from you within a week, I'll assume you'd rather not talk.

Regards . . . Don

KROODSMA: A simple invitation to talk about his research, face to face, to resolve whatever could be resolved in a professional manner.

I received no response.

3) 8 October 2014 "I have no idea what is in your head . . . I'm still available to talk"

Subject: Combating Advocacy

Hello Jeff:

I have no idea what is in your head. Only two possibilities come to mind:

- 1) You truly believe you are doing fine research and learning about bird song. Given your graduate training, I suppose this possibility wouldn't surprise me too much, but I find it difficult to believe.
- 2) Research and publishing are a game not to be taken too seriously, and it's no big deal if what you write has no semblance of truth, no big deal that you dupe the vast majority of readers into believing things you know not to be true. I find this possibility difficult to believe as well, more difficult to believe than option #1.

So I'm left in total disbelief when I read your publications. I have no idea what you are thinking. To me and the way I approach science, you live in a land of make-believe, where the research you write about and the conclusions you come to just can't be true.

I'm still available to talk, but you will no doubt conclude that we have nothing to talk about. I will wait to hear from you, but if I don't hear from you within a week, I'll decide on my own what to do next.

Regards . . . Don

KROODSMA: A candid expression of my disbelief, still offering to talk things over.

I received no response.

4) 15 October 2014 "JEFF, HERE'S ONE LAST ATTEMPT TO ENGAGE YOU"

Subject: Combating a Culture of Advocacy

To: Michelle Scott, Editor of Animal Behavior

CC: Jeff Podos and coauthors Goodwin, Moseley, and Lahti

BCC: Six Advisers

From: Don Kroodsma

Hello Michelle:

Thank you for your reply. I realized when I sent my "Combating Advocacy" essay to you, of course, that you could not consider anything in that form for publication, but I chose (at least initially) to express my outrage bluntly and candidly anyway. Among my advisers, some have expressed shock ("you're right, but you can't write that; someone will be hurt!"), but the one I most trust (the best scientist among them) encouraged me to convey the outrage so that it was abundantly clear. So that's what you saw.

Thank you also for offering to consider a review article, addressing matters of science in a matter-of-fact tone. I would like to do that. I have your instructions on how to proceed:

"The process for a Review is for you to submit a 1-2 page proposal to the Elsevier system. I ask the UK office if there is anything like it in their pipeline and I give you the go-ahead. (This only takes a day or two.) Then you submit your manuscript as a US review and it is assigned to me."

I will find no pleasure in writing this review; I'd far rather be doing other things. But I'll write it for two primary reasons: 1) to try to return the study of birdsong squarely to the realm of science, and 2) to try to spare more graduate students from falling into this culture of advocacy.

I might find collaborators, but I might just go it alone, too. I appreciate the "uphill battle" you predict that I might face in getting my review published, but I will take my chances. I do believe that, when science and advocacy go head to head, science will win.

Jeff Podos (together with students Goodwin and Moseley) have chosen not to communicate with me; nor, as I understand it, will Podos or Goodwin communicate with mild-mannered, good-natured Mike Beecher, who together with his student actually suggested that Goodwin and Podos (2014) be retracted. As I wrote to Jeff, I have no idea what is in his head, whether 1) he honestly feels he is doing good science or 2) he knows he is deceptively marketing nonsense. Frankly, I can't believe either possibility, but it seems to me that one of them has to be true.

(David Lahti readily communicates, but acknowledges that there are some issues that he can't talk about freely.)

JEFF, HERE'S ONE LAST ATTEMPT TO ENGAGE YOU

Jeff, I take no pleasure in what I have been reviewing in papers by you and others over the past few months; in fact, it all leaves me in a really distracted, irritable mood. I especially dislike critiquing papers by young scientists (Goodwin and Moseley), because their careers are at stake, when it is you who are orchestrating it all, when you bear the ultimate responsibility for what your students are publishing, yet they will take the blame.

If you remain convinced that your science is solid (explanation #1 above), then you probably have no options; I will write the review on the "motor constraints" hypothesis, and whether you

defend yourself or not, the careers of Podos, Goodwin, and Moseley (and perhaps others) will inevitably suffer. But if you accept that you are "deceptively marketing nonscience" (explanation 2 above), then I believe you have the (honorable) option of saving your students by publicly taking the blame yourself. In essence, I think it boils down to this: Defend yourself to the end and everyone will suffer, or fess up and give your students a fighting chance for a career in science.

As I wrote to Dana Moseley, but who said she'd delete without reading any emails from me, I have nothing personal against you. You're a likeable guy. But I disagree strongly with what you do in matters of science, especially because you directly undermine something I truly cherish, and that is understanding birds and their songs.

As before, I'd like to hear back something from you within a week (November 22, by noon), or I'll again take the next steps on my own. [error: meant Oct 22, of course, within a week]

Michelle: Sorry to send all of this dialogue to you as well, but somehow I feel you ought to be kept informed as to what is or is not transpiring on a possible review paper.

Regards . . . Don Kroodsma

PS—The essay "Combating a Culture of Advocacy in Birdsong Research" is attached (once again), for it contains the continuing email dialogue in the COMMENTS AND RESPONSES section, including this letter. It is also my hope that correspondence with the editor of *Animal Behavior* would be deposited in an archive so that future historians and birdsong biologists would be able to look back on this period and get some answers to the question "What were they thinking?"

KROODSMA: Frustration with Podos et al. continues, with refusal to discuss matters, leading to this letter primarily to Michelle Scott, editor of *Animal Behavior*, outlining an approach for possible publication of my critique of the literature. Letter contains a LAST ATTEMPT to engage Podos on these research matters.

I received no response from Podos.

5) 16 October 2014 "something well worth reading, from half a century ago"

Subject: An extraordinary article on Science, by Richard Feynman

Hello Jeff et al.:

I have tried to limit my emails to the Wednesday weekly, but here is something well worth reading, from half a century ago. Perhaps you want to consider these 1974 thoughts on Science as you think about the future, both near and distant.

I've taken the liberty to hi-lite the sections that I thought most appropriate to the situation we are involved in.

best . . . Don

attached: Richard Feynman's 1974 commencement at Cal Tech, entitled "Cargo Cult Science"

http://neurotheory.columbia.edu/~ken/cargo_cult.html

KROODSMA: In my search for common ground with other scientists, I stumbled upon a most remarkable piece, one that I think every scientist should read multiple times, especially as a paper is about to be submitted for publication; I thought it would be especially appropriate reading for Podos at this time, so I shared it with him.

I received no response.

6) 9 November 2014 "I'd welcome any dialogue with you"

Subject: Honest Signaling in Birdsong and the Motor Constraints Hypothesis of Podos (1997): A Contrary View

Hello Jeff Podos, Steve Nowicki, Bill Searcy, and Sandy Vehrencamp:

Jeff has known for some time that this is coming, though he's never acknowledged receiving any messages from me (neither email nor US mail). This review was precipitated by listening to the oral presentation of Goodwin and Podos (2014) at the ornithological meetings during late May, as told in the Prologue of the attached document. And then everything else just seemed to follow.

If you choose to read any of what I have written, you will see that I am not sold on the motor constraints hypothesis of Podos (1997) and all of the work that seemingly attempts to confirm its significance. I believe there's nothing about male quality and honest signaling in the trill rate-bandwidth graph.

I am sending this document first to the four of you, as you have all been prominent in promoting this hypothesis and are therefore prominent in the critique. I'd welcome any dialogue with you, and if you choose to reply, I'd appreciate hearing from you by December 1. Any reaction you wish to register will be available for others to read in the last section of the document.

I'll be away until late November, out of contact. I'll check in late November.

Regards . . . Don Kroodsma

KROODSMA: Frustration continues in all attempts to engage "UMass Biology," so I share the accumulating document with Podos, with his mentor Nowicki and his frequent coauthor Searcy, and with Vehrencamp.

I receive brief acknowledgment letters from Searcy and Vehrencamp, but nothing from Podos or Nowicki.

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"

Subject: 6 months have passed since the May meetings . . .

Hello Sarah (copy to Jeff and Dana and David):

It's 1 December, colder than it was back in May, literally too. I'm sorry where all of "this" has headed, especially for a young graduate student just starting out in science. But, sadly, what you

published with Jeff is not "science," and therein lies the problem. It's even more of a problem when authors refuse to communicate about their published work (see "Ethics in Publishing," copied below from the Animal Behaviour Society website). How a graduate advisor would allow his student to publish what you did, and then stonewall communication about it, is beyond anything I can imagine. Perhaps others might have dropped the matter, but your and Jeff's (and Dana's; but not David's) refusing to communicate led me to read further and further, uncovering a culture of advocacy and non-science in which you (and Dana) represent the third generation.

In an attempt to curb this advocacy and return birdsong to the realm of science, and to try to prevent other young graduate students from being recruited into this culture, I've pressed on. Later today, barring any last-minute stays, I'll send my accumulated reviews out to a larger audience (Podos, Nowicki, Searcy, and Vehrencamp have already had it for a month), and eventually seek publication for the document.

I wish you the best in science, learning about the behavior of nature, but the key word is "science." If instead you plan to publish more of the same, pursue a career much as Jeff has, and then recruit graduate students of your own into this culture, I respectfully suggest that another career might be more appropriate for you.

(This all seems so harsh, and I've sat staring at these words for what seems an eternity. There's such a non-human element to it all, and I struggle with it. But, in the end, I reconsider your paper with Jeff that pushed me over the edge, and I have to say "enough," and I have to follow through. And then I try to click on the "send" button, and falter yet again . . .)

If you want to talk about any of this before I continue with the above plan, I can be reached at 413-247-3367, just across the river from you. Or email me. I'm very sorry for the pain this must be causing.

Regards . . . Don Kroodsma

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

Ethics in publishing

Animal Behaviour publishes papers by scientists conducting research at locations around the globe. Publication is, therefore, based upon mutual trust between publisher and authors. 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>.

KROODSMA: A summary of how we got to this “sorry state of affairs,” as Michelle Scott would characterize it all. A summary of the situation for first author Goodwin of Goodwin and Podos (2014), the paper which sent me into this entire endeavor, with regrets for “the pain this must be causing,” and with a last minute invitation to say something, anything, that would stop the following email to all cited authors in the document.

I received no response.

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”

Subject: Honest Signaling in Birdsong and the Motor Constraints Hypothesis of Podos (1997): A Contrary View

Hello birdsong enthusiasts (cited authors and a few “interested parties”—about 50 correspondents):

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.

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.

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.

Feel free to forward this document to anyone you wish, especially any coauthors, with the same invitation for contributed commentary.

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.

Regards . . . Don Kroodsma

KROODSMA: An attempt to collect feedback and initiate a constructive dialogue with the roughly 50 people cited in my document. Again, this approach seemed the only logical and ethical approach: Give those with whom I disagree a chance to rebut my arguments before, if I am wrong, I waste a public expression of the thoughts. And it just seems common courtesy. I was especially eager to establish a dialogue about birdsong and "scientific integrity," as defined by Feynman.

I received no response from Podos (other than that from the University Police), but a good dialogue from a number of other researchers in the field.