From: Martin Nowak <[redacted]>
To: Jeffrey Epstein <jeevacation@gmail.com>
Subject: Fwd: [Fwd: in memory of George Williams]
Date: Sat, 11 Sep 2010 13:40:48 +0000

---

karl is finally drawn into action!!

Begin forwarded message:

From: Karl Sigmund [redacted]
Date: September 11, 2010 4:37:13 AM EDT
To: [redacted], Martin Nowak <[redacted]>, [redacted]
Subject: Re: [Fwd: in memory of George Williams]

Dear Bob,

Martin keeps me abreast of the letters he receives re eusociality. I must say that I greatly prefer this new one of you to that other letter ('127 experts against Nowak') where your voice was just one of the chorus. Whenever I read 'qvatch', I know who is speaking.

I do not harbor great illusions that your relationship with Martin could ever be repaired. Moreover, I emphatically will not be able to beat any sense into Martins Kopf. But I would like to explore how far you and I agree on what is sense.

What strikes me most about the Nature paper is the fact that it is a hybrid, even a chimera, consisting of two widely separate parts. There is the Nowak-Wilson part. It really deals with the evolution of eusocial behavior (I use the definition in your sense, and think it blurs the issue to include birds etc.) I do not underestimate the role of haplo-diploidity, but whoever believes that a construction of eusociality can be entirely based on this should beware of termites!

In any case, this is a hugely important question, but strangely, not the one which raises all that fuss.

This is the second part, Nowak-Tarnita. It deals with inclusive fitness. This is not the same thing as kin selection! In contrast to Martin, I think that indirect fitness is a useful concept, and that inclusive fitness can, in principle at least, be well defined even without all the assumptions about weak selection etc. Like with the Price equation, we are talking about a method of book-keeping. It is a matter of taste whether one finds it enlightening or not. But it is certainly possible to use alternative methods of accounting to track selection-mutation effects. It may make sense to compute the inclusive fitness of a worker ant, but it probably stops making sense to compute the inclusive fitness of a body cell in a multicellular organism. However, a school of thought (Alan Grafen, Stu West, Andy Gardner, Peter Taylor, …) has elevated the IF method into a mantra. Their actual results are quite limited, circling endlessly around the island model, assuming weak selection etc. They lead to some interesting results, but this does not imply that IF is THE true and only explanation of all forms of cooperation. This claim is implied in a lot of their work, however. It does not get better when Gardner and West, for instance, pontify that 'what should no longer be done' is using game theoretical models, as that infamous Nowak has done (they did not mention me, by the way, nor you, Bob: but certainly, reciprocal altruism is not included in their list of 'what should be done').

Quite generally, such trifling things as frequency dependent selection, between-species mutualism, multi-level selection etc. do not seem to rank high on their agenda. I should add that while I think that non-random assortment is all-important for the evolution of cooperation, to reduce it all to relatedness is to engage in semantic break-dance. I think that relatedness should be used as the 'man on the street' uses that term, and not be replaced by increasingly complicated statistical expressions. I am ready to acknowledge any rule 'b/c>something' as an extended version of Hamilton's rule, but do not think it is fruitful to baptize that 'something' as relatedness. I can conceive of some 'Hamilton's rule' for cooperation between species, based on preferential assortment. But hailing it as kin selection would be going too far.

I should have thought that it is enough to quietly wait till the current infatuation with that new, very technical, unnecessarily complex and, in my eyes, contrived version of IF fades down. But if there is one thing Martin cannot do, it is waiting. I see his paper as a reaction to a provocation. Of course a good strategist should never get provoked, but it may actually be beneficial to the health of the field that the furor erupts sooner rather than later.

Well, this is 'sense' in my sense. It is far enough from Martin's sense, but may nevertheless strike you as qvatch as qvatch can!

All the best

Karl

Am 10.9.2010 15:20, schrieb [redacted]:

perhaps you don't agree with me Karl but Martin's recent paper is one of the stupidest i have read in a long time--you are one of the two people referred to at the end whom i am sending this paper to in case you can beat some sense into Martin's Kopf but perhaps you have a different view of the paper

alles gute, immer noch

robert

---------- Original Message ----------
Subject: in memory of George Williams
From: [redacted]
Date: Fri, September 10, 2010 12:10 am
To: [redacted]
Cc: "Jeffrey Epstein" <jeevacation@gmail.com>
"Karl Sigmund" [redacted]

Dear Martin,

Your recent paper with Ed on eusociality is the worst piece of evolutionary theory to land on my desk since Joan Roughgarden last sashayed out to tell us about sexual selection and sex roles.

>From your very first sentence until the very last, you make unwise assertions and assumptions, completely misrepresent whole areas of knowledge (with which you appear to have at best glancing acquaintance) and worst of all misrepresent the foundations of all of evolutionary logic in a very fundamental way. And you extend your carelessness to every little detail.

Take your very first sentence. Eusociality has always been associated with sterile, non-reproductive castes, yet you unwisely loosen this definition (presumably to permit your last sentence) so that at least 2% of bird species now qualify as eusocial, as well as almost all mammals. You tell us later that the association of haplodiploidy with eusociality at some time "fell below statistical significance" but you offer no statistics because this is not, in fact, a scientific paper at all but a case of argumentation by assertion—neither logic nor data need apply.

And I can assure you, you are dead wrong—by almost any statistical test you assume, including biomass, long-term success, frequency of evolution, taxonomic diversity, complexity of societies, etc, the notion that haplodiploidy has played some kind of a role is statistically obvious. To give one example of your blindness—ant ratios of investment are—due to

haplodiploidy—1:3. This is predicted by theory and confirmed now by an immense body of evidence. This 50% extra investment in females, the sex doing the investing, every generation, may be another reason for the long-term success of ants.

No other system in nature (e.g. asexual or inbred clones) provides increased female PI without major genetic costs, but haplodiploid species provide all the advantages of outbred sex plus increased investment in the investing sex (the only genetic cost being that there are only ½ as many males in genetic competition, a trivial cost, under most theories). Can you produce in all your argumentation a force as strong as this?

Hard to see how such possibilities are even supposed to surface in the world you are creating. You are way, way behind the time, making false assertion after false assertion—and I have not even finished p1—attempting to prove that an inevitable logical extension of Darwin's RS to Hamilton's IF is all for naught, without mathematical foundation and of no significance. As we say in your home country, ganz qvatsch, start to finish.

You say that kinship theory works only under mild selection, but it is trivial to prove (and has been proven) that the same logic works under strong selection. You name a series of other arbitrary limitations, which have long ago been handled, but I do not need to see the details. As a mathematician I know that it is absurd for such an obvious extension of fundamental theory as kinship theory to come with a series of arbitrary new limitations, as if the organism arrives with a series of deformities. Your mathematical intuition may not be as deep as mine, but in that case, I suggest you adopt a more cautious tone in your argumentation Alternatively you have achieved a major mathematical results--that a trivial extension of theory deeply deforms

Martin, do you really think a quick and cheap paper like this is going to displace W.D. Hamilton and kinship theory? Do you think anybody actually expert in these matters is going to swallow this tripe? Do you think this is a worthy use of your time? I hope you do not.

To you as a fellow organism I say this: overweening ambition, combined with little or no common sense, can be fatal. Try not to join a long list of examples.

I will share this with a few of your friends who I know have your interests more at heart than I do. Maybe they can talk some sense into your Kopf.

All the best,

Robert Trivers