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Friday, April 1, 2011

Important Harvard Scientists Attack Kin Selection: Context

So, a couple of days ago, I made a video dramatizing the scientific kerfuffle surrounding a paper published in Nature by Martin Nowak, Carina Tarnita, and E. O. Wilson of Harvard. My original goal had been to create something that would be entertaining to the people involved in the argument.

The original post, which contains the video, is [here](#).

Over the past day or so, it has become clear that a lot of people are seeing the video who are maybe not familiar with the context in which the kerfuffle arose. If you're one of those people, here's an attempt to provide a little background.

Nowak and Wilson, two of the authors of the article, are two of the most prolific and high-profile evolutionary biologists working today. If you're in the field, you probably own at least one of Wilson's books. Tarnita is a postdoc working with Nowak who already has an impressive set of credentials. Last August, the three of them published a paper in the scientific journal Nature, which, for biologists, is one of the the two super-high-profile places where your papers can be published. It is incredibly difficult to get a paper into Nature, and, if you are a young scientist, a publication in Nature will go a long way towards getting you an academic job.

Modeling and eusociality

Their paper was about the evolution of eusociality, which is the thing that you sometimes find in species like bees and ants, where one individual – the queen – makes all the babies, while everyone else builds the nest or the hive, and does not reproduce themselves. These are interesting evolutionary systems, because, if you think about it naively, why should the worker ants or worker bees give up their own reproduction so that the queen can have babies? If natural selection is all about who passes on the greatest number of copies of their genes, how can you possibly get this worker behavior, where a huge number of individuals don't reproduce, and are, in fact, willing to sacrifice their lives so that someone else (the queen) can reproduce?

[Note: this is a cartoon description. The real biology is, as always, enormously more complicated, and there is a huge amount of variation in the way in which eusociality works, in insects and elsewhere.]

Here's the way that I like to think about it. Think about a cell in your brain. There is absolutely no chance for that cell to pass on copies of its genes to the next generation. That brain cell is an evolutionary dead end. In fact, no genes in any cell in anyone's brain have ever been passed on.

Nevertheless, natural selection has created genes that lead to enormously complex functions in the brain. The reason is that for every gene that is present in your brain, there is an identical (probably) copy of that gene in your germ line (in your testes or ovaries) that can be passed on. So, genes that lead to brain functions that help you to survive and reproduce can be favored by selection, even if the gene copies that are physically present in the brain are not passed on themselves.

That is the basic idea behind the evolution of eusociality. Workers that don't reproduce have evolved because they help the queen to reproduce, and, in particular, they help her to make more

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About the Blauthor



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Jon is a Professor at the Santa Fe Institute, where he studies

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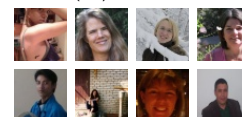
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queens, who go off and start their own colonies. So, in a sense, the colony as a whole reproduces, and genes that facilitate that non-reproductive worker behavior are passed on, even though they are not passed on by the workers themselves.

At this verbal, qualitative level of description, everyone agrees about what is going on. But, in evolutionary biology, we are interested in developing mathematical, formal, quantitative descriptions of the process. This is where the divisions start.

There are different ways that these ideas can be formalized. Traditionally, the two major formalisms have been "kin selection" or "inclusive fitness" models on the one hand, and "group selection" models on the other. I won't go into detail here about the differences, because I don't personally find them interesting. The fact is, if you do your math correctly, you can accurately describe any system using either of these frameworks, as well as others. They're not *totally* identical, in the sense that certain systems can be described more simply using one framework than another, or in that some questions can be more natural to ask in one framework than another, or in that the framing entailed by your choice of model can influence how you tend to interpret the results of the model. That being said, there is a deep way in which all of the different modeling frameworks are mathematically interchangeable, and this interchangeability has been demonstrated repeatedly over the past few decades.

The problem with the paper

The thing about this particular paper that roused the ire of so many evolutionary biologists was that much of the text was devoted to discrediting the kin selection approach. The problem with the paper is that it does not actually go after any of the core ideas that underlie the kin selection approach. Nor does it criticize models of kin selection in the way that people actually use them.

Instead, the paper sets up a straw man, and then tears it down. The "kin-selection approach," as it is described by them, would certainly be a limited, flawed modeling framework. But the limitations that they describe in the paper fall into three categories:

1. Limitations that are not a part of kin-selection models as they are actually used by anyone.
2. Limitations that may apply to particular models applied to particular systems, but are not limitations that are inherent in the approach.
3. Limitations that apply to all evolutionary models, including the alternative that they are championing.

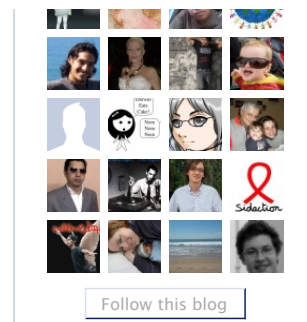
I won't go on more here. If you're interested, I recommend reading the original criticisms, which I have cited and linked to in my [original post](#).

Now, one interesting thing about this paper is that many of the papers that have extended kin-selection models beyond the limitations that the paper accuses them of are actually cited in the supplementary materials. And yet, the main text of the paper (which is the only thing that most people will read) seems to be written as if none of those papers exist.

The personalities

Back in the 1970s and 1980s, E. O. Wilson was a polarizing figure in evolutionary biology, due to his role in championing the application of evolutionary reasoning to the study of behavior, particularly human behavior. (In a [previous post](#), I recommended [this article](#), which provides an entertaining overview of the sociobiology wars, in which Wilson was a central figure.) However, over the past couple of decades, Wilson has become one of the Grand Old Men of evolution, and is nearly universally respected.

Martin Nowak, by contrast, is a controversial and polarizing figure in evolutionary biology today. However, whereas Wilson became controversial for his ideas, Nowak is controversial for the way that he presents his ideas. In particular, many people within the evolutionary biology community feel that Nowak has a tendency to oversell the importance and originality of his own work. More specifically, many people feel that he systematically fails to give enough credit to previous work by other scientists.



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So, while I believe that the criticisms leveled against this particular paper – specifically those in the published responses in Nature – are all legitimate, I can see how it might seem like a lot of controversy over a little problem. I would like to suggest what I think might be an explanation for the volume (both number of words and loudness of those words) of the response that the paper seems to have elicited. Although I know some of the letter writers personally, and know many of them professionally, I claim no privileged insight as to their motivations. So, what I am presenting here is pure speculation, and should be taken with large quantities of salt, but here it is:

My suspicion is that the response was as broad and strident as it was specifically because it was a response to Nowak. The shortcomings that they have pointed out the current paper are certainly all there. But, I think that those shortcomings perhaps seem all the more galling because they represent an extreme case of a style of argument and presentation that Nowak has used repeatedly over the years, and which has long been infuriating to many evolutionary biologists, including, I suspect, many of the authors of the letters.

The politics

I mentioned at the beginning of this post that the paper was published in Nature, and publications in Nature are worth their weight in gold in terms of a biologist's career. But the reputation of Nature within evolutionary biology is a complicated one. Many people will routinely dismiss Nature as a "science tabloid" that is very interested in publishing flashy results, but interested enough in whether or not those results are true. At the same time, most of these same biologists would gladly trade their right gonad for a Nature publication themselves, as Nature publications open the door to future success, like getting academic jobs, getting grant money from funding agencies, and getting, well, more Nature publications. As one colleague of mine put it, it's like how everyone wants to have their picture taken with the dictator.

So, one thing that is going on here is that there are a lot of people who have published a lot of very good work in a lot of very good journals. Then, along comes this paper, which basically dismisses that whole body of work. You could say (as a different colleague of mine did), "Well, if the arguments in the paper are wrong, why not just let it go. No one will believe it in the long run anyway." The problem is that the impact of this one article in Nature may outweigh the impact of all of those very good articles in all of those very good (non-Nature) journals, at least in the eyes of anyone who is not, themselves, an evolutionary biologist. So, while this paper will have little to no effect on the way that evolutionary biology is done, it may have a big impact on the way that evolutionary biology is perceived by people outside the field.

So, some of this is probably a combination of righteous indignation and sour grapes, similar to what you might feel upon seeing some celebrity interviewed on CNN as an "expert" on some topic that you feel they don't really understand, and that you feel that you, in fact, understand much better.

Then, there is the funding issue. There are two funding sources that Nowak has that are viewed with some suspicion by many evolutionary biologists (and probably most academics, more generally): the [Templeton Foundation](#) and [Jeffrey Epstein](#), both of which/whom are thanked in the acknowledgements of the original paper.

The Templeton Foundation funds a lot of science, but has a particular interest in science that relates to issues of religion and spirituality. This interest is, in itself, enough to make many evolutionary biologists feel that any research supported by Templeton is inherently suspect. I have no horse in that race, and my view is that as long as they are not dictating the *outcome* of your research, there is no problem. Then, of course, there is the fact that Nowak himself is a devout Catholic, which, I suspect, makes his relationship with Templeton seem even more problematic to your average evolutionary biologist.

Jeffrey Epstein is, of course, the hedge-fund mogul who pled guilty a couple of years ago to a charge of soliciting an under-age girl for prostitution. There is an [argument to be made](#) that his extreme wealth allowed him escape much more severe charges, such as sex trafficking. More recently he has [been in the news following accusations](#) that he "trained up" a girl who lived with him from age 14 to 18, and loaned her out to his rich friends.

The progress

I also pointed out that that was the tiniest bull penis I'd ever seen and Victor implied that I was "just spoiled". I'm not entirely sure how to take that one.

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Now, one can take a range of positions on this issue. One viewpoint, probably espoused by many academics (including me), is that any money from someone like Epstein is inherently dirty, and that the choice to take money from him casts doubt on one's ability to make valid moral – and, by extension, scientific – judgments.

An alternative viewpoint would be that money is money, there's not enough of it out there to support all the interesting research that could be done, and you've got to take money where you can get it. An even more extreme viewpoint would be that every dollar that you take from Epstein for science is one dollar that he won't be spending to pay some underage girl to give him a "massage."

As I say, I side with the first viewpoint, that Epstein's moral violations are severe enough that there is no excuse for interacting with or taking money from him. However, I suspect that some people may feel differently without necessarily being bad people.

The point is that Nowak's associations probably color how he is perceived by the academic community. That does not mean that those associations have affected his science. And, in fact, I believe that the scientific points of the argument can be completely understood without any reference to these other issues.

However, I think the intensity of the response to this paper was enhanced by things that form a part of the sociology of science, rather than a part of the science itself. It is in this vein that I mention Nowak's associations, which are fairly well known to most evolutionary biologists (who, like all academics, are a gossipy bunch).

Wrap-up

So, what you have in Martin Nowak is a guy who has been enormously well funded and enormously prolific, publishing a huge number of papers in high profile journals. As a result, Nowak has become one of the best known evolutionary biologists, particularly outside the field. However, many other evolutionary biologists are suspicious (and probably resentful) of his high profile. This suspicion comes in part from a feeling that he has not really earned his reputation, that his reputation exceeds his actual accomplishments, and that he associates with unsavory characters. It is not surprising, then, that he is something of a lightning rod in the field.

I doubt that I have written anything here that will be surprising or new to anyone who actually works in evolutionary theory, or follows it closely. But, I wanted to lay this out because I know that this sort of academic dust-up always looks really bizarre and petty when viewed from the outside. And, it is clear in this case that the debate is emotionally charged. So, if you've stumbled upon this, and were confused, but interested enough to slog through this whole post, I hope that maybe this provided some degree of context.

Nowak, M., Tarnita, C., & Wilson, E. (2010). The evolution of eusociality *Nature*, 466 (7310), 1057-1062 DOI: [10.1038/nature09205](https://doi.org/10.1038/nature09205)

Update: PS If you came here through finding the video posted on Richard Dawkins's [site](#), it is *shoebucket* productions, not *shoebox* productions.

Posted by Jon Wilkins at 11:19 AM 

Labels: [biology](#), [evolution](#), [Politics](#), [science](#)

2 comments:

Anonymous said...

Nice summary. It was worth the read. -Matt R.

[April 1, 2011 1:12 PM](#)



Jeremy B. Yoder said...

Wow. That's a depth of politics of which I was only dimly aware, at best.

On first reading Nowak *et al.*, I remember getting the distinct feeling that someone was

playing fast and loose with the argument against kin selection in the first half—they dropped most of their argument into the Supplement, for crying out loud! Then in the second half, they built up a model for the origin of eusociality that made a lot of sense to me, but also seemed to [make eusociality a bad model for human cooperation](#), which I'd always thought kin selection was originally proposed to explain.

All in all, it's a very strange piece of scientific writing.

April 1, 2011 2:39 PM

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