



The descent of Edward Wilson

by Richard Dawkins / MAY 24, 2012 / 151 COMMENTS

A new book on evolution by a great biologist makes a slew of mistakes



The Social Conquest of Earth

By Edward O Wilson
(WW Norton, £18.99, May)

When he received the manuscript of *The Origin of Species*, John Murray, the publisher, sent it to a referee who suggested that Darwin should jettison all that evolution stuff and concentrate on pigeons. It's funny in the same way as the spoof review of *Lady Chatterley's Lover*, which praised its interesting "passages on pheasant raising, the apprehending of poachers, ways of controlling vermin, and other chores and duties of the professional gamekeeper" but added:

"Unfortunately one is obliged to wade through many pages of extraneous material in order to discover and savour these sidelights on the management of a Midland shooting estate, and in this reviewer's opinion this book can not take the place of JR Miller's *Practical Gamekeeping*."

I am not being funny when I say of Edward Wilson's latest book that there are interesting and informative chapters on human evolution, and on the ways of social insects (which he knows better than any man alive), and it was a good idea to write a book comparing these two pinnacles of social evolution, but unfortunately one is obliged to wade through many pages of erroneous and downright perverse misunderstandings of evolutionary theory. In particular, Wilson now rejects "kin selection" (I shall explain this below) and replaces it with a revival of "group selection"—the poorly defined and incoherent view that evolution is driven by the differential survival of whole groups of organisms.

Nobody doubts that some groups survive better than others. What is controversial is the idea that differential group survival drives evolution, as differential individual survival does. The American grey squirrel is driving our native red squirrel to extinction, no doubt because it happens to have certain advantages. That's differential group survival. But you'd never say of any part of a squirrel that it evolved to promote the welfare of the grey squirrel over the red. Wilson wouldn't say anything so silly about squirrels. He doesn't realise that what he does say, if you examine it carefully, is as implausible and as unsupported by evidence.

I would not venture such strong criticism of a great scientist were I not in good company. The Wilson thesis is based on a 2010 paper that he published jointly with two mathematicians, Martin Nowak and Corina Tarnita.

When this paper appeared in *Nature* it provoked very strong criticism from more than 140 evolutionary biologists, including a majority of the most distinguished workers in the field. They include Alan Grafen, David Queller, Jerry Coyne, Richard Michod, Eric Charnov, Nick Barton, Alex Kacelnik, Leda Cosmides, John Tooby, Geoffrey Parker, Steven Pinker, Paul Sherman, Tim Clutton-Brock, Paul Harvey, Mary Jane West-Eberhard, Stephen Emlen, Malte Andersson, Stuart West, Richard Wrangham, Bernard Crespi, Robert Trivers and many others. These may not all be household names but let me assure you they know what they are talking about in the relevant fields.

I'm reminded of the old *Punch* cartoon where a mother beams down on a military parade and proudly exclaims, "There's my boy, he's the only one in step." Is Wilson the only evolutionary biologist in step? Scientists dislike arguing from authority, so perhaps I shouldn't have mentioned the 140 dissenting authorities. But one can make a good case that the 2010 paper would never have been published in *Nature* had it been submitted anonymously and subjected to ordinary peer-review, bereft of the massively authoritative name of Edward O Wilson. If it was authority that got the paper published, there is poetic justice in deploying authority in reply.

Then there's the patrician hauteur with which Wilson ignores the very serious drubbing his *Nature* paper received. He doesn't even mention those many critics: not a single, solitary sentence. Does he think his authority justifies going over the heads of experts and appealing directly to a popular audience, as if the professional controversy didn't exist—as if acceptance of his (tiny) minority view were a done deal? "The beautiful theory [kin selection, see below] never worked well anyway, and now it has collapsed." Yes it did and does work, and no it hasn't collapsed. For Wilson not to acknowledge that he speaks for himself against the great majority of his professional colleagues is—it pains me to say this of a lifelong hero—an act of wanton arrogance.

The argument from authority, then, cuts both ways, so let me now set it aside and talk about evolution itself. At stake is the level at which Darwinian selection acts: "survival of the fittest" but, to quote Wilson's fellow entomologist-turned-anthropologist RD Alexander, the fittest what? The fittest gene, individual, group, species, ecosystem? Just as a child may enjoy addressing an envelope: Oxford, England, Europe, Earth, Solar System, Milky Way Galaxy, Local Group, Universe, so biologists with non-analytical minds warm to multi-level selection: a bland, unfocussed ecumenicalism of the sort promoted by (the association may not delight Wilson) the late Stephen Jay Gould. Let a thousand flowers bloom and let Darwinian selection choose among all levels in the hierarchy of life. But it doesn't stand up to serious scrutiny. Darwinian selection is a very particular process, which demands rigorous understanding.

The essential point to grasp is that the gene doesn't belong in the hierarchy I listed. It is on its own as a "replicator," with its own unique status as a unit of Darwinian selection. Genes, but no other units in life's hierarchy, make exact copies of themselves in a pool of such copies. It therefore makes a long-term difference which genes are good at surviving and which ones bad. You cannot say the same of individual organisms (they die after passing on their genes and never make copies of themselves). Nor does it apply to groups or species or ecosystems. None make copies of themselves. None are replicators. Genes have that unique status.

Evolution, then, results from the differential survival of genes in gene pools. "Good" genes become numerous at the expense of "bad." But what is a gene "good" at? Here's where the organism enters the stage. Genes flourish or fail in gene pools, but they don't float freely in the pool like molecules of water. They are locked up in the bodies of individual organisms. The pool is stirred by the process of sexual reproduction, which changes a gene's partners in every generation. A gene's success depends on the survival and reproduction of the bodies in which it sits, and which it influences via "phenotypic" effects. This is why I have called the organism a "survival machine" or "vehicle" for the genes that ride inside it. Genes that happen to cause slight improvements in squirrel eyes or tails or behaviour patterns are passed on because individual squirrels bearing those improving genes survive at the expense of individuals lacking them. To say that genes improve the survival of groups of squirrels is a mighty stretch.

With the exception of one anomalous passage in *The Descent of Man*, Darwin consistently saw natural selection as choosing between individual organisms. When he adopted Herbert Spencer's phrase "survival of the fittest" at the urging of AR Wallace, "fittest" meant something close to its everyday meaning, and Darwin applied it strictly to organisms: the strongest, swiftest, sharpest of tooth and claw, keenest of ear and eye. Darwin well understood that survival was only a means to the end of reproduction, so the "fittest" should include the most sexually attractive, and the most diligent and devoted parents.

Later, when 20th-century leaders of what Julian Huxley called the "Modern Synthesis" deployed mathematics to unite Darwinism with Mendelian genetics, they co-opted "fitness" to serve as a variable in their equations. "Fitness" became "that which is maximised in natural selection." "Survival of the fittest" thus became a tautology, but it didn't matter for the equations. The "fitness" of an individual lion, say, or cassowary, became a mathematical expression of its capacity to leave surviving children, or grandchildren, or descendants into the indefinite future. Parental care and grandparental care contribute to individual fitness because an individual's descendants are vehicles in which ride copies of the genes that engineer the caring.

But lineal descendants are not the only such vehicles. In the early 1960s, WD Hamilton, arguably the most distinguished Darwinian since RA Fisher, formalised an idea that had been knocking around since Fisher and Haldane. If a gene happens to arise which works for the benefit of a sibling, say, or a niece, that gene can survive in the same kind of way as a gene that works for the benefit of offspring or grandchildren. A gene for sibling care, under the right conditions, has the same chance of surviving in the gene pool as a gene for parental

care. A copy is a copy is a copy, whether it sits in a lineal or a collateral relative.

But the conditions have to be right, and in practice they often aren't. Full siblings are usually harder to identify than offspring, and usually less obviously dependent. For practical reasons, therefore, sibling care is rarer in nature than parental care. But as far as Darwinian principle is concerned, sibling care and parental care are favoured for the same reason: the cared-for individual contains copies of the genes that programme the caring behaviour.

Half siblings, nephews, nieces and grandchildren are half as likely as full siblings or offspring to share a caring gene. First cousins are half as likely again, and are harder to identify. Hamilton summarised all this in the form of a simple equation, which has become known as Hamilton's Rule. A gene for altruism towards kin will be favoured if the cost to the altruist C is outweighed by the benefit to the recipient B devalued by r , which is a subtle but computable index of probability of sharing genes. For example, r for full siblings and parents and offspring is $1/2$; r for grandchildren, half-siblings, nephews and nieces is $1/4$; r for first cousins is $1/8$, and so on. A gene for altruistic care will spread through the population if $rB > C$. It is extremely important not to forget B and C and conclude that only r matters in evaluating the success of the theory in particular cases. I am sorry to say that Wilson, in his allegation that Hamilton's ideas don't apply to particular cases, comes perilously close to doing just that. It is as though r is so interesting and novel that B and C are overshadowed.

Hamilton replaced "classical fitness" (which took account only of lineal descendants) by "inclusive fitness," which is a carefully weighted sum embracing collateral as well as lineal kin. I have informally (and a touch facetiously but with Hamilton's blessing) defined inclusive fitness as "that quantity which an individual will appear to maximise, when what is really being maximised is gene survival." In his previous books, Wilson was a supporter of Hamilton's ideas, but he has now turned against them in a way that suggests to me that he never really understood them in the first place.

"Inclusive fitness" was coined as a mathematical device to allow us to keep treating the individual organism ("vehicle") as the level of agency, when we could equivalently have switched to the gene ("replicator"). You can say that natural selection maximises individual inclusive fitness, or that it maximises gene survival. The two are equivalent, by definition. On the face of it, gene survival is simpler to deal with, so why bother with individual inclusive fitness? Because the organism has the appearance of a purpose-driven agent in a way that the gene does not. Genes lack legs to pursue goals, sense organs to perceive the world, hands to manipulate it. Gene survival is what ultimately counts in natural selection, and the world becomes full of genes that are good at surviving. But they do it vicariously, by embryologically programming "phenotypes": programming the development of individual bodies, their brains, limbs and sense organs, in such a way as to maximise their own survival. Genes programme the embryonic development of their vehicles, then ride inside them to share their fate and, if successful, get passed on to future generations.

So, "replicators" and "vehicles" constitute two meanings of "unit of natural selection." Replicators are the units that survive (or fail to survive) through the generations. Vehicles are the agents that replicators programme as devices to help them survive. Genes are the primary replicators, organisms the obvious vehicles. But what about groups? As with organisms, they are certainly not replicators, but are they vehicles? If so, might we make a plausible case for "group selection"?

It is important not to confuse this question—as Wilson regrettably does—with the question of whether individuals benefit from living in groups. Of course they do. Penguins huddle for warmth. That's not group selection: every individual benefits. Lionesses hunting in groups catch more and larger prey than a lone hunter could: enough to make it worthwhile for everyone. Again, every individual benefits: group welfare is strictly incidental. Birds in flocks and fish in schools achieve safety in numbers, and may also conserve energy by riding each other's slipstreams—the same effect as racing cyclists sometimes exploit.

Such individual advantages in group living are important but they have nothing to do with group selection. Group selection would imply that a group does something equivalent to surviving or dying, something equivalent to reproducing itself, and that it has something you could call a group phenotype, such that genes might influence its development, and hence their own survival.

Do groups have phenotypes, which might qualify them to count as gene vehicles? Convincing examples are vanishingly hard to find. The classic promoter of group selection, the Scottish ecologist VC Wynne-Edwards, suggested that territoriality and dominance hierarchies ("peck orders") might be group phenotypes. Territorial species are more spaced out, and species with peck orders show less overt aggression. But both phenomena are more parsimoniously treated as emergent manifestations of individual phenotypes, and it is individual phenotypes that are directly influenced by genes. You may choose to treat a dominance hierarchy as a group phenotype if you insist, but it is better seen as emerging from each hen, say, being genetically programmed to learn which other hens she can beat in a fight and which normally beat her.

But what about the social insects, Wilson's area of expertise? Hamilton's, too, and indeed the social insects were an early, stunningly successful showcase for his theory.

Female bees, ants and wasps are genetically capable of developing into fertile queens or sterile workers. Each individual is switched into either the queen pathway or the worker pathway (one of several worker pathways in ants) by an environmental switch, and the point is utterly crucial. No gene for outright sterility could survive. But a gene for sterility under some environmental conditions but not others could easily be favoured, and it was. A

female bee larva fed on royal jelly and housed in a large queen cell will develop into a fertile queen. Otherwise she will develop into a sterile worker. Genes that find themselves in sterile bodies programme them to work for copies of the same genes in fertile bodies—either the old queen (their mother), or young queens (their sisters) or young males. The result is that queens evolve to become more efficient, full-time specialist egg-layers, with all their needs taken care of by their sterile daughters or sisters.

Because of how the B , C and r values in Hamilton's Rule turn out for bees, genes for sterility are favoured under some conditions, hyper-fertility under others. The same is true for ants and wasps; and termites but with differences of detail (for example termites have male as well as female workers—alas I have no space to expound Hamilton's elegant explanation of this difference and many other intriguing facts). With more differences of detail, the same is true for some non-insect species such as naked mole rats and a few crustaceans.

It truly is a beautiful theory. Everything fits, exactly as it should. Darwin himself, with characteristic prescience but using the pre-genetic language of his time, got the point. As so often, he drew inspiration from domestication:

"Thus, a well-flavoured vegetable is cooked, and the individual is destroyed; but the horticulturist sows seeds of the same stock, and confidently expects to get nearly the same variety; breeders of cattle wish the flesh and fat to be well marbled together; the animal has been slaughtered, but the breeder goes with confidence to the same family. I have such faith in the powers of selection, that I do not doubt that a breed of cattle, always yielding [sterile] oxen with extraordinarily long horns, could be slowly formed by carefully watching which individual bulls and cows, when matched, produced oxen with the longest horns; and yet no one ox could ever have propagated its kind."

In modern, Hamiltonian terms we would interpret Darwin's "seeds of the same stock" as sharing genes with the vegetable that has been cooked. The sterile ox with the long horns shares genes with the same stock from which we breed. Darwin, lacking the concept of the discrete, Mendelian gene, spoke of going with confidence to the "same family" rather than the same genes. Wilson now interprets this as a form of "group selection," the "group" in this case being the family. But what a staggeringly unpenetrating—even perverse—use of language. Kin share genes, that is the point, and Darwin would have loved it. The fact that a family can also be seen as a "group" is entirely beside the point and an unhelpful distraction from it.

When Hamilton's twin papers on inclusive fitness were first published in 1964, John Maynard Smith, who was the referee chiefly responsible for recommending them, published a short paper in *Nature* in which he called attention to Hamilton's brilliant innovation. Maynard Smith coined the phrase "kin selection" specifically in order to distinguish it from group selection, then in the process of being discredited by him and others such as the ecologist David Lack. Soon after this, Wilson, in *The Insect Societies* (1971), enthusiastically adopted Hamilton's ideas. He continued to press them in *Sociobiology* (1975), but in an oddly misleading way which indicates that he was already flirting with a watered down version of his current folly. He treated kin selection as a special case of group selection, an error which I was later to highlight in my paper on "Twelve Misunderstandings of Kin Selection" as Misunderstanding Number Two. Kin may or may not cling together in a group. Kin selection works whether they do or not.

Misunderstanding Number One, which is also perpetrated by Wilson, is the fallacy that "Kin selection is a special, complex kind of natural selection, to be invoked only when the allegedly more parsimonious 'standard Darwinian theory' proves inadequate." I hope I have made it clear that kin selection is logically entailed by standard Darwinian theory, even if the B and C terms work out in such a way that collateral kin are not cared for in practice. Natural selection without kin selection would be like Euclid without Pythagoras. Wilson is, in effect, striding around with a ruler, measuring triangles to see whether Pythagoras got it right. Kin selection was always logically implied by the neo-Darwinian synthesis. It just needed somebody to point it out—Hamilton did it.

Edward Wilson has made important discoveries of his own. His place in history is assured, and so is Hamilton's. Please do read Wilson's earlier books, including the monumental *The Ants*, written jointly with Bert Hölldobler (yet another world expert who will have no truck with group selection). As for the book under review, the theoretical errors I have explained are important, pervasive, and integral to its thesis in a way that renders it impossible to recommend. To borrow from Dorothy Parker, this is not a book to be tossed lightly aside. It should be thrown with great force. And sincere regret.

Edward Wilson replies:

Richard Dawkins's review of *The Social Conquest of Earth* (*Prospect*, Issue 195, 24th May 2012) makes little connection to the part he criticizes. The central issue in the book, which he urges others not to read, is the replacement of inclusive fitness theory (kin selection theory) by multilevel selection theory (ie, individual and group selection combined), with a new and major role assigned to group selection in the origin of advanced social behavior. The original formulation was made by Martin Nowak, Corina Tarnita, and myself in 2010 (*Nature* 466: 1057–1062). We demonstrated that while inclusive fitness theory sometimes works, its mathematical basis is unsound, and inclusive fitness itself is an unattainable phantom measure. Multilevel selection in contrast is mathematically sound, analytically clear, and works well for real cases—including human social behavior.

The science in our argument has, after 18 months, never been refuted or even seriously challenged—and certainly not by the archaic version of inclusive fitness from the 1970s recited in *Prospect* by Professor Dawkins. While many have protested (incidentally, not including Steven Pinker and Robert Trivers, as Professor Dawkins claims), many others of equal competence are in favor of the replacement proposed. In any case, making such

lists is futile. It should be born in mind that if science depended on rhetoric and polls, we would still be burning objects with phlogiston and navigating with geocentric maps.

Edward O. Wilson, Harvard University

Google +



NBeale

May 24, 2012

Hamilton's rule is fine as a rule of thumb. But if you look into the maths rigorously it doesn't work properly. Dawkins is no mathematician and doesn't understand this but [this seminal paper](#) went through a completely rigorous peer review process (befitting the world's top scientific journal) and although many biologists didn't like the results no-one has ever refuted them.

Trotting out old dogma that you learned as a grad student is no answer to 21st century mathematics – as Dawkins should know.

[REPLY](#)



NBeale

May 24, 2012

PS: an obvious non-mathematical counterexample to Dawkins' assertion that "Group selection would imply that a group does something equivalent to surviving or dying" is the fact that groups can be defined by cultural ties. (Dawkins' "meme" idea although lacking any real rigour ought to give him a clue in this area, were he not so dogmatic) At it's simplest, if members of a group share a common language which allows them to communicate and hence fight/forage more effectively they will survive better than a group that doesn't (other things being equal). Although the ability to learn a language certainly does have a genetic basis, the particular language that you learn certainly doesn't.

Dawkins would learn much from reading Nowak's masterly book [Evolutionary Dynamics](#) and also from [Evolution in 4 Dimensions](#) and [Questions of Truth](#).

[REPLY](#)



Thomas Earle Moore

May 24, 2012

I can hardly believe I am reading the author of the concept of the meme. Are not memes real replicators of group characteristics that are carried forward by, for example, religious sects?

[REPLY](#)



Mark Cowan

May 24, 2012

Dawkins and Wilson have something in common, they are both authors in 2 of the 10+ schools of thought that have tried to generate a theory of culture from evolutionary theory over the last 153 years since 'Origin'. The reason for that is simple, evolutionary theory cannot.

Dawkins refers to Darwin's and artificial selection, "As so often, he drew inspiration from domestication" and it is worth nothing that artificial selection was the entire focus of 'Origin' from which Darwin platformed his argument from an analogous process in nature, the metaphorical process of natural selection. Natural selection is one rhythm through nature and for Darwin it was the main mechanism but one among others. For neo-Darwinians like Dawkins & Co they would misguide you into believing that natural selection is synonymous with evolution. This is quote misguided, dogmatic and vulgar science.

Dawkins and (Daniel) Dennett both concede in interview and lecture that humans are the first/only intelligent designers on the tree of life, and artificial selection is an extension of human foresight and imagination. A city (the most common setting for humankind) is/was not built by nature, but by culture. Darwin wrote in September

1860 that if he started again he would have called the mechanism 'natural preservation' because nature preserves (from random variations) and it is more accurate to think of genes as 'units of preservation'. To call them 'units of selection' is to misread the evolutionary process in such a way that process becomes purpose. Stephen Jay Gould highlighted this when he said that Dawkins confused bookkeeping (which is what genes do) with their causality. Genes are part of the process of evolution, and an important part but for Dawkins & Co they have become the 'purpose'.

1953 – the structure of DNA was discovered.

1960s – Hamilton's work on DNA and social behaviour

1975 – Wilson 'Sociobiology'

1976 – Dawkin's 'The Selfish Gene' and his neo-Darwinian conjecture of 'memetics' as a theory of culture from his gene-centrist view.

Here we can see the historical bandwagon effect that led these thinkers to believe (human level knowing) that DNA was in some way destiny. It is not. Bandwagon effects are emergent within a set of given conditions, not in our biology. Human design can now manipulate genetic structure, and artificial cells have been created and they have been imprinted with a watermark by the designers. We now know that genes can be affected by the environment and their molecular structure can be affected by diet, pollution and other environmental factors. This means that neo-Lamarckism needs to be rekindled as we realise that not all changes to the genome are 'random' and some are altered structurally by the environment, or "the inheritance of acquired characteristics" goes the view attributed to Lamarck, whose evolutionary theory predated Darwin.

Richard's career is rooted on gene-centrism, but as the years go by this view drifts ever more towards historical footnote. The process of evolution (and Darwin would back me up on this) is 'natural preservation' not 'natural selection'. To assert NS is very anthropomorphic. Darwin's view is not adaptation it is 'assimilation' of new and emerging body parts into bodies. Lamarck's view is more accurately thought of as 'adaptation' to account for the feedback that does occur as our genome alters in the face of environmental conditions. Culture, the domain of design and art involves both 'adjustment' and 'adoption' by humans that decide, not nature.

Darwin – assimilation

Lamarck – adaptation

Culture – adjustment/adoption

This is the process we need to understand and by failing to grasp the difference between 'selection' and 'preservation' and nature's loading capacity to accommodate a lot of different species of life (100,000s different kinds of beetle) then we don't need 'natural selection' to account for a diversity of species not division, struggle and winnowing.

Wilson's book is perhaps the last throw of the dice from this scholar and perhaps a more accurate title would have been 'The Cultural Conquest of the Earth' and from 1969 when humans stepped on the moon, that conquest went that little bit further afield. Coen's new book 'Cells to Civilisation' is an attempt to link evolution, development, learning and human culture and while this is encouraging we need to understand that:

1. there is a theory that connects the social sciences, arts and humanities and the laws of motion, even equation and measurement are beyond the explanatory capacity of evolutionary theory.
2. this doesn't undermine science, it underlines it. It may well highlight the dogmatic attempts from Wilson, Dawkins and Co over the last few decades to explain culture by (at times) by cavalier evolutionary metaphor, but their position(s) are the lingering bandwagon from those who thought destiny was in DNA, and more generally natural selection is akin to evolution.

They are wrong. Richard is wrong. He took his narrow gene-centrism of evolution and cut and pasted it onto culture as memetics and this has went nowhere. Seriously, nowhere. Ideas move around, there is no insight there. You can scatter as much evolutionary language around the notion of ideas but that is not a theory, much less real science. Richard and Co would love to generate an evolutionary theory of culture but that is an impossibility. So whatever criticism Richard levies at Wilson and this book and however far you/he may want to throw Wilson's book away, it will still fall short of how far Richard's books on culture should be thrown. Actually, that is not true. Richard's books on culture and how it works should be held tight, read and re-read as a way not to do critical thinking on the most telling question of our, or any other time. We don't understand the central issue of the human condition by weak, even lazy metaphor from evolutionary theory. That might be an inconvenient truth for Richard

and Co, tough, this is science and cutting edge knowledge here. It's demanding.

Wilson tried Sociobiology and was short. He wrote 'On Human Nature' and was still short. In 1981 he came back with 'gene-culture co-evolution' and in the 2000s he lectured that the social sciences should generate a theory of culture. That is a mind open to critical thinking on this issue. We can contrast this with Richard's 1976 'memetics' which has lingered around not because it has merit but because some neo-Darwinians would like it to be true.

That is belief, pure and simple. And more simple.

[REPLY](#)



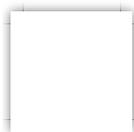
Jason Mills

May 25, 2012

Comparisons with memes are inappropriate: the meme is not tied to a biological host like the gene is. Its reproduction is horizontal. If it is part of a larger "organism", that unit is not a group of human beings but a 'memeplex' of ideas.

A language or a religion ceases when it runs out of 'users'. This may coincide with the extinction of individual humans or a group of humans, but it need not: people integrating into a new society may cease to use their old language, and people exposed to different ideas may drop their religion, perhaps in favour of another. In neither case have either the individual humans or their nebulous 'group' disappeared. And indeed, both phenomena could theoretically revive centuries later without any biological carriers in between, just as hieroglyphics and (less convincingly) druidism have returned to active status in human culture. Memes neither define nor depend upon individual human beings nor groups of them.

[REPLY](#)



Peter

May 25, 2012

Yes memes propagate, but don't miss Richard Dawkin's point, there has to be a replicator to span generations, to carry gradual change forward. Genes are the replicator for biology, while written/oral communication is the replicator for memes. Without a replicator there is no descent with modification from one generation to the next.

Species -> Genus -> Family -> Order -> Class -> Phylum, etc, these are all labels of convenience for the variety into which life has branched as it has evolved over billions of years, they are designations of convenience for us, not active participants in evolution. The value of groups will only contribute to evolution if that involvement replicates genetically, as in the example of infertile ants supporting fertile queens, caring parents & siblings, etc. Each of these contributes to the success of genes and so is replicated from generation to generation.

[REPLY](#)



Noah Smith

May 25, 2012

I'm not a biologist, but several things that Dawkins asserts here sound obviously wrong to me.

1. Genes are the unique replicators? What about the genomes of organisms that reproduce by asexual reproduction?
2. As for group selection, if a gene can recognize itself in another organism (as is necessary for kin selection), couldn't a pair of genes exist with the ability to "recognize" each other and help each other out in times of need? Wouldn't that constitute "group selection"?

[REPLY](#)



Christopher Klerkx

May 25, 2012

In response to NBeale:

It does no good to cite the very paper that Dawkins criticizes. Don't you think it's a bit premature to call a paper published in 2010 "seminal"?

You mention language and the resulting cultural transmission as a counterexample to Dawkins, but you'll have a hard time explaining how the mutations that enabled language became prominent in the human gene pool unless you think those mutations also carry benefits for individuals. In that case, resorting to group selection is unnecessary.

[REPLY](#)



Jason Mills

May 25, 2012

Mark Cowan above says: "We can contrast this with Richard's 1976 'memetics' which has lingered around not because it has merit but because some neo-Darwinians would like it to be true."

The central tenet of memetics is that ideas propagate because they are good at propagating, not necessarily because they are useful. Mark's comment shows that he accepts this even while he denies it. If the idea of the meme is not useful, why is it still around? It could only be that it propagates despite its lack of merit – which, with enjoyable paradox, demonstrates its merit!

Memes are a distraction in this discussion, however, as Wilson's claim is that group selection operates in biology, not only in the special case of human culture.

[REPLY](#)



NBeale

May 25, 2012

Most readers of Prospect won't know the paper and many might be taken in by his absurd suggestion that the paper wasn't properly peer-reviewed. Dawkins hasn't made any significant contributions to science (his last research paper published in a top journal was in the 1980s and was hilariously wrong) and doesn't understand the mathematics of evolution.

The fact is that Hamilton's rule is a rule of thumb which is often useful but doesn't actually add anything to standard natural selection and is sometimes completely wrong. This completely undermines Dawkins' dogmatism, and readers should be aware of this fact.

[REPLY](#)



Jason Mills

May 25, 2012

NBeale, ad homs against Dawkins aren't relevant. Rather than just repeating (with what looks like the kind of dogmatism you accuse him of) that kin selection can't work and that this somehow undermines opposition to group selection, why don't you give a brief account of why you think so? Your bare assertions are unpersuasive (and your Nature link appears inaccessible – perhaps to non-subscribers).

[REPLY](#)



Stuart Munro

May 25, 2012

It's a shame to be reading a review of a working scientist of some merit by a rhetorician who makes his living by pretending to be a scientist.

Read "Genetic Calvinism, or Demons and Dawkins" by David Stove to see the wings pulled off The Selfish Gene argument. Very entertaining, and gives a proper understanding of Dawkin's scientific weight.

[REPLY](#)

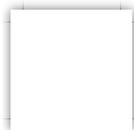


Jason Mills

May 25, 2012

Stuart Munro, your comment is yet another that simply disses Dawkins without addressing what he has said. Why don't you say what you think is wrong with his argument against group selection?

[REPLY](#)

**NBeale**

May 25, 2012

Stuart: Thanks, I hadn't come across this book or writer. Some telling points and most entertaining.

[REPLY](#)**Felipe Glez**

June 24, 2012

You are going to love David Stove: he not only dishes The Selfish Gene, check out his other magna opera "the intellectual capacity of women" or "Racial and other antagonisms"

[REPLY](#)**csrster**

May 25, 2012

Why no link to the Nature article?

[REPLY](#)**Peter Clarke**

May 25, 2012

This is a complex issue, and I don't know who will turn out to be correct, but I appreciate the comments of NBeale Marik Cowan and others above. The Nowak-Tarnita-Wilson paper in Nature was based on rigorous and detailed mathematics. If the mathematics was wrong, why don't the critics point out the mistake(s)?

[REPLY](#)**Chris Lawson**

May 25, 2012

David Stove is a very entertaining writer, but unfortunately for a scholar of science he had no understanding of how science works — as late as 1983, Stove had this to say of the great scientific figures of the 20th century:

"Who is the most important thinker of the present century? Einstein, many would say. I am too ignorant, unfortunately, to judge whether that is true. Change the question a little: which thinker is the most important for the light he has thrown on human and terrestrial affairs? Freud? Wittgenstein? Konrad Lorenz? These answers I can judge, and I do not agree with any of them. My answer is, Immanuel Velikovsky."

[REPLY](#)**Jesse Ellis**

May 25, 2012

NBeale and others – One thing that it appears you may not know... peer review is by no means a perfect process. Two reviewers usually review a paper. And they may not be the best reviewers. For journals like Nature and Science, there is some level of prestige associated with reviewing such an article, which means that even if one is not competent to review a paper, one might try anyway. On top of that, both of those journals exist to promote themselves. They do so not only by publishing important groundbreaking work, but by being cited. And one good way to get cited is to publish something that everyone else will hate, or that the popular press will eat up (one memorable example was a short note on a South American duck with a penis longer than its body, from the early 2000s – theoretically uninteresting, but quite titillating). It's entirely possible that one or both reviewers hated the paper and yet the editor thought it would be provocative. Dawkins does not imply the paper was not peer reviewed. He implies that over 140 scientists responded to it (which generates more traffic for the journal – tricky, see???).

[REPLY](#)**Terry A. Davis**

May 25, 2012

God is proven. It is now time to work-out the implications. A new avenue is to ask God



for information. To some extent, the way this is done is through exchange — you figure-out what God likes, he tells you stuff, and you decide if you want to share or enjoy off-the-record things He might say.

Monkey mothers holding babies for nursing was the hardest thing in evolution.

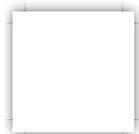
Fish shoulders.

The day fruit was made was God's happiest.

"Are stegasuruses lame like turtles?" "Not pet rocks" ROFLMAO God gets mad when you don't praise His work.

Brontosaurus had pain when stepping.

[REPLY](#)



Michael Bentley

May 25, 2012

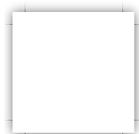
Reply to NBeale

Nature often publishes articles not because they are correct or because they represent the best science has to offer, but because they will stimulate debate and further research. Dawkins does comment on the Nowak paper you mentioned in his essay:

"When this paper appeared in Nature it provoked very strong criticism from more than 140 evolutionary biologists."

Richard Dawkins may be no mathematician, but I suspect his mathematics is better than you think. Alan Grafen and Stuart West (just to give two examples I know of) certainly do understand "21st century mathematics" (as you put it) and have also strongly criticised Nowak's research. I don't think you are helping anyone by casting this debate as people who understand mathematics and those who don't.

[REPLY](#)



Andrew Lyttle

May 25, 2012

Chris Lawson,

That fact does not detract from Stove's argument, however, since it is a logical dismantling of Dawkins's language. As such, it is quite devastatingly effective.

Look, RD has made a long career out of popularizing and simplifying the work of other scientists, making bad philosophical arguments that get attention because they fit in with the reigning ideology in his field, and generally making a hash of any complicated idea he tries to address. Wilson may or may not be off the mark, but Dawkins simply does not possess the credibility necessary to be taken serious here.

[REPLY](#)



Charles Goodnight

May 25, 2012

I was asked to comment on this article by a friend, and decided to offer this comment to everybody.

Dawkins book the selfish gene is a an absurdest redux of Fisher's 1930 book without any of the nuance. Fisher, like Dawkins, essentially develops a model in which selection acts directly on genes. Unlike Dawkins, Fisher develops a formal model, and along the way states the assumptions that are necessary for his model to hold. Also unlike Dawkins, Fishers model was a reasonable starting point in 1930. The Shellfish Gene came out in 1976, and he has been singing that tune ever since. As I said, Fishers model was a brilliant first step back in the days when we didn't know what a gene was, less than 20 years after the rediscovery of Mendel, and 30 years before the first electronic computer. Some 80 years after that one would hope that our models had advanced. The only excuse that Dawkins has is that he is not a biologist, so he can't be expected to understand biology.

In general I emphatically dislike the Dawkinsian approach, and in particular, I think the language he has introduced is destructive, and deserving of nothing more or less the

derision. (I wonder if I can expand on the Destructive Dawkinsian Dialectic Deserving of Derision?). Phenotypes are not vehicles, unless they are horses, in which they are indeed vehicles for humans. Genes are not replicators, xerox copiers are replicators. Phenotypes make new phenotypes. They use genes, and culture, and symbionts and the environment to make the new phenotype what it is. Phenotypes are no more vehicles for genes than they are vehicles for language or anything else.

Where this failing on Dawkins' part becomes most clear is looking at memes. Back in the Darwin's day the concept of particulate inheritance was so ridiculous that nobody gave it a second thought. Along comes the rediscovery of Mendel, and the new synthesis and particulate inheritance becomes the standard. Using the *reductio ad absurdum* approach of Dawkins we reduce selection down to the gene, which is a particle. Now when Dawkins wants to look at cultural inheritance he similarly wants to reduce this obviously continuous process to "particles", which he calls a meme. I would argue that the "meme" is better termed a "figment" of Dawkins' imagination.

Where does Dawkins fall short? Well Fisher was well aware of gene interaction and interactions among individuals. Thus, he explicitly assumes that populations are very large, and that genes are effectively well mixed. Under this situation every gene exists in all possible genetic and environmental combinations. In such a situation it is mathematically legitimate to model selection acting on single genes. The reality is that organisms do not live in large randomly mating and randomly interacting populations. In this case reducing selection to selection on individual genes is a mathematical charade that is doomed to failure. The problem is that while Fisher was well aware and explicitly stated his assumptions, Dawkins had blindly accepted his results while blissfully ignoring his assumptions.

To critique this particular paper, I would point out that E. O. Wilson is in good company. In particular, Darwin, Hamilton, and Price all recognized that "kin selection" is a form of multilevel selection. If I remember correctly Hamilton did not like the term kin selection, and only grudgingly accepted it when it became popular. Wilson is a bit wrong, actually, however. It turns out that Hamilton's original formulation of inclusive fitness was stunningly unsuccessful as a research paradigm, and over time has been replaced by the "direct fitness approach". This approach turns out to be identical to contextual analysis, which is a well established approach in multilevel selection. Where the two approaches differ is that the direct fitness kin selectionists take the equations and solve them for the fitness maximum, that is where $dw/dx = 0$, whereas the multilevel selectionists solve the same problem for the rate of change. Thus, kin selectionists and group selectionists use EXACTLY the same equations, they just solve them differently. Thus, Wilson is wrong to reject kin selection over multilevel selection because they really are different perspectives on the same problem.

I also find it stunning that people like Dawkins have no respect for biology as an experimental science. Nowhere in there do I see mention of the extensive experimental work on multilevel selection. Group selection has been shown to work in plants, insects, chickens, and hogs. The theory is well developed, and has been experimentally tested in some detail, with experiments being published as early as 1976. We know WHY group selection works, and why the old models incorrectly predict that it doesn't work. None of this is in there. Instead he makes a false argument by referring to authority. Importantly, that authority includes a large number of people who have made their name based on additive theory. None of them are scientists that have made a name doing experimental research on structured populations. Why aren't Wade, Goodnight, Muir, Bijma, Weing, Okasha, Eldakar mentioned. These are the people who actually have data.

Dawkins states " 'The beautiful theory [kin selection, see below] never worked well anyway, and now it has collapsed.' Yes it did and does work, and no it hasn't collapsed. For Wilson not to acknowledge that he speaks for himself against the great majority of his professional colleagues is—it pains me to say this of a lifelong hero — an act of wanton arrogance."

Actually, no, it doesn't work. I recently did a literature search to find somebody, anybody, who actually reported the strength of kin selection. That even measured kin selection and said it was stronger or weaker than some other force. I found three references. Two were by Wade (one sole authored one with Felix Breden), and one by Joan Silk, who acknowledges Wade. This is important for two reasons. One is that the strength of individual selection and group selection is routinely measured. When group selection is measured it is also routine to compare it to the strength of group selection. The three measures of the strength of kin selection I found were all done using a multilevel selection approach (pioneered in part by Wade).

I would argue that it is impossible to measure the strength of kin selection using kin selection theory since it is an optimality theory, and as such in principle you CANNOT

measure the strength of kin selection. If we want “kin selection” to move beyond the “does it occur” and on to becoming a quantitative science we would do well to follow Wilson’s lead and abandon kin selection theory in favor of multilevel selection theory.

[REPLY](#)



Mark Van Cleve

May 25, 2012

Hm. Mr. Dawkins accusing someone else of arrogance. And, of having a ‘patrician tone’. Hello, pot? This is kettle. You’re black.

But beyond that, all I’m reading is that Wilson has settled on group selection, that Dawkins disagrees, and that many other people do too. Of course Dawkins refers to group selection as being simply ‘erroneous’, as $2+2=5$ is erroneous. But that’s simply, er, arrogance. Wilson has a different idea, there will be much debate, and all will learn from the debate.

[REPLY](#)



Anthony K

May 25, 2012

I’m not a biologist either, but Dawkins makes an inference here that I’ve never quite found convincing. I agree with him that “Evolution, then, results from the differential survival of genes in gene pools.” I’m less clear on why he insists that we equate the gene pool with the individual organism.

Even taking for granted that most selection happens at the level of the individual organisms, it’s easy to imagine scenarios where the survival of genes might depend on their frequency in the organism’s background population. (e.g. a gene conferring a predisposition to non-kin altruistic behavior would be more or less successful at replicating depending on how altruistic the rest of the population is).

For that reason (without even getting into differential survival of certain groups) it’s entirely plausible that testable hypotheses could be made that view the organism’s population, rather than the individual organism, as the relevant “gene pool” in which gene frequencies are measured.

It doesn’t strike me as inherently unreasonable then to ask whether the frequency of certain genes in a population-level gene pool could affect the overall number of organisms in the population. Nor would it be unreasonable to ask whether the presence of certain genes affects one group’s differential survival relative to other groups of the same species.

The willingness of non-related individuals to band together to repel a common threat is not valuable in an individual unless other members of its group also carry the trait. I’m sure an individual-level account can also be given, but it’s certainly not ridiculous to ask whether a banding together gene could propagate through the differential survival of populations exhibiting higher frequencies of a trait that is individually useless.

I’m personally still unconvinced/agnostic that science is restricted to an atomized, organism-centric view of evolution. That said, I’ve read Wilson’s book, and it was extremely disappointing to find him such a poor advocate for his own ideas. His rejection of kin selection, rather than integrating into the larger point he wants to make, is unfortunate.

[REPLY](#)



Jason Mills

May 25, 2012

Mark Cowan writes: “We can contrast this with Richard’s 1976 ‘memetics’ which has lingered around not because it has merit but because some neo-Darwinians would like it to be true.”

If an idea can linger around, and indeed spread widely, despite lacking merit, then by happy paradox the central claim of memetics is demonstrated even as Mark seeks to demolish it.

Noah Smith says: “1. Genes are the unique replicators? What about the genomes of

organisms that reproduce by asexual reproduction?

2. As for group selection, if a gene can recognize itself in another organism (as is necessary for kin selection), couldn't a pair of genes exist with the ability to "recognize" each other and help each other out in times of need? Wouldn't that constitute "group selection"?"

In answer to 1, the genomes of asexual organisms are composed of genes that mutate, just as are those of sexual organisms. When such an organism reproduces, the resulting organisms will not always be identical, leading to differential survival in the usual way.

In answer to 2, genes working in concert are a commonplace, but this is not what is meant by group selection (where 'group' refers to organisms). If a species of bird has varied colours, and a gene that becomes associated with red birds enjoys mutual benefit with a gene that becomes associated with blue birds, then the blues and the reds might do better than the yellows, but the selection is still for the beneficial genes. Red and blue birds lacking the genes would not benefit, and if we define the group as those birds which POSSESS the helpful genes, then we reduce the selection back to genetic.

Charles Goodnight says: "Phenotypes are no more vehicles for genes than they are vehicles for language or anything else."

Which came first, the genes or the phenotype? If the phenotype, how did it reproduce?

With honourable exceptions, many of the comments above merely diss Dawkins rather than addressing what he says in the article. Ad hominem are no more persuasive than arguments from authority (on either side).

[REPLY](#)



Charles Goodnight

May 25, 2012

"With honourable exceptions, many of the comments above merely diss Dawkins rather than addressing what he says in the article."

Yes, I don't like Dawkins' ideas. My post started as a slightly tongue in cheek response to a friend, then I decided to post it here in case it was entertaining to others. That said, I stand by what I said, and I believe that what I said about the Selfish Gene in general and this article in particular will stand up to careful scrutiny, even if it perhaps it should have been said more politely.

As to which came first the genes or the phenotypes: Its pretty obvious that phenotypes came first. There are several good conceptual models that provide a natural framework by which a "phenotype" could incorporate other elements (i.e., genes) to promote the development of heritability. Also, the current understanding is that we had an RNA world before we had a DNA world. This suggests that at some point there was an RNA organism that converted over to using DNA (a much more stable molecule). So, if genes came first, those genes were probably not DNA genes.

[REPLY](#)



Mark Cowan

May 25, 2012

Jason, your comments on 'memetics' embody the charm, indeed infatuation of an undergraduate exposed to the 'face value' of a problem whose head is turned.

For centuries social science has been aware that ideas (non corporeal 'matter(s)') influence behaviour and thought. They move around. Big deal, there is no insight there. If I renamed ideas 'shit' and wrote a paper littered with Darwinian and evolutionary metaphor it may look plausible narrative at face value but at the deeper levels of laws of motion and ultimately equation I would be talking 'shit'.

Read p.112 of The Extended Phenotype and Dawkins lists the 4 reasons why memes (dogmatic rebadging of the term 'idea') can never be thought of as like genes. Dig that book out and read the top half of the page. It's compelling.

If you (or anyone here) think that highlighting Dawkin's fatal and consistent failure to grasp cultural hypercomplexity is just 'dissing' him then do some more reading. I've given you a start, ponder on fidelity and the Weissman barrier for starters.

Memetics was, and remains a vulgar cut & paste of gene-centrism onto the realm of culture. As Dawkins concedes half way down p.112 of The Extended Phenotype he 'hasn't done the reading' on culture. He's in good company here then.

[REPLY](#)



Elmer Rich

May 25, 2012

The commentators for the most part, and predictably, make a updated version of the pathetic fallacy. They ascribe to the trivial dayily "mind" chit chat and solipsistic daily stuff that each brain (uniquely) perceives as something even close to the biological laws and forces of genetic physic and chemistry.

Memes are a silly idea and just ideology – wishful thinking.

Like all arguments for magic, the underlying claim (plea) is for mind over matter. No, the internal chit chat, and even physical cultural stuff we feel comes from it (it may or may not), don't do anything like reproduce in the Darwinian sense.

[REPLY](#)



Jason Mills

May 25, 2012

Charles, yours was a post I was classing under the honourable exceptions.

Not seeing the relevance of whether the technology of genes is RNA or DNA. More pertinently, I struggle to see how a phenotype can reproduce (or even be described as a phenotype, really!) without a genetic underpinning. (I did read Woolfson's "Life Without Genes", but it struck me as simply prolonged special pleading.) Still, if you can point me towards a not-too-technical account of models for this, I'd be interested, if doubtful.

Mark, I find the notion of the meme useful, albeit it has failed to develop into concrete applications (although arguably those already existed, in forms such as advertising and propaganda). But Dawkins acknowledged its limitations from the outset and only introduced it as a demonstration that there could be kinds of replicators other than biological. Nonetheless, by the time I got around to reading that chapter 30 years later, the CD I happened to be playing at the time offered up the song "Virus of the Mind" (Heather Nova) – a pleasing indication of the persistence of the meme idea itself.

But this is all by the by: Wilson's claims are about group selection operating at a BIOLOGICAL level, not merely in the special case of human culture. The merits of memetics are a sideshow here.

[REPLY](#)



Stanley K. Sessions

May 25, 2012

Wonderful! Science at its best. Enjoy the fight!

[REPLY](#)



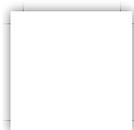
Guy Hoelzer

May 25, 2012

In my view, Dawkins' gene-centered view is more non-Darwinian than the multilevel selection view advocated now by Wilson. Dawkins entire argument is founded on the notion that the level at which you find the highest fidelity in replication is the only level at which natural selection acts and influences evolution at all correlated levels. In my opinion, this claim simply does not hold water. Consider, for example, the literature on the evolution of mutation rate. Clearly natural selection cannot work at all on a population of perfect replicators without mutation, because there would be no variation to work with. There must be enough mutation to create variation, which inherently diminishes the perfection of replication at that (the gene) level. So natural selection requires imperfect replication, and different levels of biological organization

exhibit a wide variety of replicator perfection (mutation rates, heritabilities...). I think this leads naturally to the notion of multilevel selection theory, and it seems inconsistent with centering so strongly on the gene level. In fact, I would personally argue that the gene level is extremely unavailable as a level of selection due to Mendelian mechanisms of genetic inheritance. These mechanisms virtually ensure that every gene copy has an equal chance of making it into a gamete, which means that at the gene level there are no fitness differences (except for situations where the gene replicates independently of the cell or individual, such as with meiotic drive). I advocate the hypothesis that Mendelian segregation evolved as a consequence of selection at the whole organism level that disfavors selection at the gene level. Intragenomic conflict is harmful to individual fitnesses, so it is adaptive at the individual level to minimize fitness differences among genes. I could go on and on, but I will leave my argument at this point. Dawkins is a brilliant writer, but in my opinion he is not good at seeing the errors in his own reasoning or appreciate valid alternative points of view.

[REPLY](#)



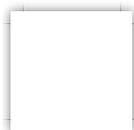
NBeale

May 25, 2012

With the best will in the world neither West nor Grafen are Mathematicians (degrees in Zoology, Experimental Psychology, and Economics). Nowak is a Professor of Mathematics (and of Biology) at Harvard and Tarnita won the prize for best PhD thesis from the Harvard Math Dept and was elected to the Society of Fellows.

West tries to state a rigorous version of kin selection post N-T-W in [this paper](#) and I haven't had time to go through it all, except to say that basing your entire work on linear regressions shows that you are an economist/zoologist and not a mathematician! The world is usually much more complex than this. No-one denies that Hamilton's Rule is a good rule of thumb, but that's not what the Dawk believes.

[REPLY](#)



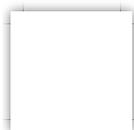
NBeale

May 25, 2012

Jason: do you think AI-based life is logically impossible?

It is at best a contingent fact about biological life on earth that at present it (seems to) use genes – although even the concept of a gene is a bit slippery and in reality genes don't exist – it's just the molecules. Genes are a (highly) convenient mathematical abstraction but as with almost everything in biology what I'm tempted to call "[Beale's law of biological systems](#)" applies

[REPLY](#)

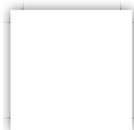


Aylwin

May 25, 2012

Comment threads on articles are like parasitic slime entrails.

[REPLY](#)



Bill R

May 25, 2012

I'm just going to note that it's self evident to soldiers that personal survival depends on group survival and group survival depends on individuals accepting fair shares of risk.

The words "coward" and "hero" have emotional impact because people recognize in their bones that reciprocal risk-taking is in everyone's vital interest. These emotions come, somehow, from our genes. Everyone wants to be a hero, everyone wants to survive. The emotions are in conflict but they are both there.

The reciprocity is important. A soldier will expose himself to provide covering fire for a soldier who is advancing because he knows that the other soldier will do the same when it is his own turn to advance. Military discipline has something to do with it but the idea – or ideal if you like – of mutual risk taking is part of human nature.

And it's not just combat. Every day, we do large and small kindnesses for strangers. We expect them to reciprocate and have a whole dictionary of abuse for people who won't reciprocate (the first word in that dictionary is "asshole"). The desire to succor

and rescue others is part of our emotional suite. It stronger for our children and weaker for assholes but it's always there. It conflicts with other emotions but it's still part of human nature.

I think Edward Wilson himself made the point that when we act on our emotions to help, rescue, and defend others we generally say we are being "good". When we give way to our conflicting emotions that tell us to just worry about ourselves, we are being "bad".

How this works out depends on circumstances and culture, as well as genes but to me at least, the instinct for altruism is part of human nature. So is the competing instinct for selfishness.

Finally I think that for Dawkins and some others, classic Darwinism has become a sort of substitute religion. They're full of contempt for non-believers and when challenged with any new idea they immediately cite the church elders and the sacred texts.

[REPLY](#)



Michael Bentley

May 25, 2012

With the best will in the world it doesn't take a Professor of Mathematics (and of Biology) at Harvard to be able to follow a set of linear differential equations, which is what Novak et al's model of eusociality is based upon. This debate doesn't exist because one set of people can do their sums and the other set can't (which appears to be how you are characterising this).

By the way, I'm not taking a side here since I haven't made my mind up yet – I have plenty of reading to do. I just think your comments about the Nowak et al paper are misleading/unhelpful. Their work will be judged in time on its merit, not on where it was published or who by.

[REPLY](#)



rd

May 25, 2012

Nowak is a hack and his book, *Evolutionary Dynamics*, only appears impressive. He may be brilliant as a mathematician, but he is useless as a biologist. Fittingly, his book may be a fun way for mathematicians to think they are doing biology, but it is unhelpful for anyone wanting to do real, serious evolutionary biology.

Almost all of Nowak's funding comes from Jeffrey Epstein, a Wall Street investor who knows nothing about science and was later convicted as a sex offender. If he had been convicted a few years earlier, Harvard would never have accepted his money, and Nowak would be a virtual nobody. Sadly money has allowed Nowak to promote positions that lack substance. Why Wilson chose to collaborate with him is beyond me.

Read all five of the responses to the Nowak et al. paper that were published in *Nature* in March 2011 and you'll see why real biologists and mathematical biologists (including myself) aren't impressed by it. Nowak keeps trying to make a name for himself, but he will ultimately be forgotten in the history of biology.

On a side note to NBeale: Michael Bentley said that Grafen and West 'certainly do understand "21st century mathematics"' NOT that they were mathematicians. Either way, your siding with mathematicians over biologists in a discussion over evolution baffles me. As someone who moved from math to biology, I tend to trust biologists when they talk about the biology even if I trust the mathematicians when they talk about the models. As you can probably guess, this is why I have little time for Nowak's novel views on evolution.

[REPLY](#)



David Evanoff

May 25, 2012

Curiously Professor Dawkins does not address in his book review the crux of Professor Wilson's argument for eusocial evolution. Professor Wilson summarizes five stages of eusocial evolution on pages 186-7 of his book *The Social Conquest of Earth*, which are paraphrased here for brevity. 1) group formation 2) nest dependency 3) knockout of dispersal behavior 4) emergent group interactions 5) changes in

lifecycle and social structure.

Stage 2 marks a Crossing of the Rubicon wherein the niche is amalgamated into the phenotype. Of course there are plenty of examples of group nesting dependency across species. Establishing the relatedness of individuals within nesting groups should decide whether kin or group selection prevails. Group selection would seem to be the only available explanation for wild parrots in New York City.

[REPLY](#)



Tony Afanasiew

May 25, 2012

A slating by Dawkins is recommendation enough.

[REPLY](#)



Jason Mills

May 25, 2012

NBeale says: "do you think AI-based life is logically impossible?"

It is at best a contingent fact about biological life on earth that at present it (seems to) use genes"

I don't think AI is impossible, though it may be intractable. But achieving AI doesn't necessarily mean that we should label the outcome 'life'. We might want to reserve that for biological systems.

I take your point that AI 'life' might not reproduce with genes, or at all. But that's only because it would be designed wholesale. To get to organisms nature had to build up from scratch, and that must start with genes (ie. for these purposes a thing that can reproduce itself) and grow a more complex shell slowly over epochs. It may be 'contingent' that life on Earth uses DNA (or RNA), but I am persuaded that the use of mutable, heritable, digital particles of information is the only way to do it, regardless of the particular chemical substrate.

To return to the article, it would be nice if a Wilson-fan/Dawkins-basher would explain how they think group selection works, in such a way that it isn't equivalent to a more parsimonious explanation in terms of genetic selection.

[REPLY](#)



Bill Eaton

May 26, 2012

No argument is any better than its assumptions and the addition of mathematics does not change that. See economics for many examples. Evolutionary biology is still in a similar state.

The real difficulty of separating genotype from phenotype is obvious at the dawn of life. Nature has "done some work" on that since, but Nature leaks. Darwinian selection is just proof-of-concept for that.

My analogy from IT is the difficulty of understanding any sufficiently-complex computer code. This gets worse every time the code is altered. Beale's Law may be optimistic.

[REPLY](#)



Goffredo Smeets

May 26, 2012

Nice food for thought, especially the comments, thanx. So here's my song for supper. I get the impression that the issue is metaphysics, the basic 'assumptions.' Evolution as natural selection, or as natural preservation, or as natural drift. Each of those leading to a different partition of the cake, implying it's own research design and set-up, offering it's specific perspectives, causing debate with it's specific absurdities and paradoxes. And each of them torturing the usual suspect. Poor maths, and especially statistics. It cannot prove a thing. It simply shows some logical implications of the interrogator's assumptions.

The concept of the gene illustrates the point, as some of you pointed out. Dawkins' genie is the gene as nature's purpose. That's teleological bias from assumption. I'm not enough of a historian of biology to be able to assess Wilson's book. What's more,

I haven't read it. From what I read here, I presume he made a switch somewhere on the line between basic assumption and logical consequence. So here's a double question: does anybody of you out there have an idea about the position of Wilson's switch on that line? And does Wilson himself comment on the reason (other than the usual suspect) of his switch?

[REPLY](#)



Owen Gilbert

May 26, 2012

I think a couple of quotes from Karl Popper are relevant here:

"I dislike the attempt, made in fields outside the physical sciences, to ape the physical sciences by practicing their alleged 'methods'—measurements and 'induction from observation.' The doctrine that there is as much in a subject as there is mathematics in it, or as much as there is measurement or 'precision' in it, results upon a complete misunderstanding. On the contrary, the following maxim holds for all sciences: Never aim at more precision than is required by the problem at hand."

There are two challenging question that mathematicians should answer. The first is why Hamilton's simple model can yield such useful predictions. Nowak et al. clearly did not address this problem: they only sought to expose the lack of precision in the original formulation (which, by the way, has long been known). The second question, which is more important in my opinion, is what phenomena cannot be explained due to the limitations of Hamilton's model, even in the newer more generalized versions. Nowak et al. argued that the primary limitation was in the assumption of additivity of the effects of helping behavior. However, they did not emphasize what new phenomena was explained by their model (certainly not in the main paper; one has to search through the supplement, and it is not clear there either). Instead, they emphasize the imprecision of Hamilton's original model; however, this was already known.

Another quote from Popper is apropos:

"... there is only one way to science—or to philosophy, for that matter: to meet a problem, to see its beauty and fall in love with it; to get married to it, and to live with it happily, till death do yea part—unless you should meet another and even more fascinating problem, or unless, indeed, you should meet another and even more fascinating problem, or unless, indeed, you should obtain a solution. But even if you do obtain a solution, you may then discover, to your delight, the existence of a whole family of enchanting though perhaps difficult problem children for whose welfare you may work, with a purpose, to the end of your days."

Hamilton did not provide for the "welfare" of his first brood of children alone. Trivers was the first great tutor. Price taught them how to live in groups. Dawkins and Wilson helped make them famous. Innumerable empiricists exposed them to the real world. But most the children survived and flourished. A few dropped by the wayside (one named 'haplodiploidy hypothesis'). Hamilton fathered a number of other broods as well.

S. J. Gould once wrote, "The arrow of history specifies a sequence of changing contexts within which the same old questions are endlessly debated – "The thing that hath been, it is that which shall be, and that which is done is that which shall be done." (Ecclesiastes 1:9)."

Indeed.

[REPLY](#)



Scotty

May 26, 2012

There is an inbuilt fallacy of objectivity in science, both a fallacy and a paradox. Quantitative data is objective provided it remains within the realm of the physical realm. Nobody can disagree with the results of a blood-cell count, a tissue sample, an MRI scan or any other test. There may be a difference of opinion on interpretation of course, but that's a quite separate argument. There is little room for argument over the presence or absence of a physical entity ie a bacterial/viral sample. Objectivity breaks down when subjective interpretation and meaning has to be considered. There is inherent complexity around every corner when you get to this level. The trouble with quantitative data is that it can be utterly devoid of meaning—entirely redundant and empty.

Quantitative data is objective only if it can be verified by groups, who all can agree exactly with the data. This is indeed rare. Reductionist-style orthodox medicine prefers quantitative data precisely because reductionism favors physical, material aspects of reality.

Now where is the paradox? The paradox enters when you interpret this so called 'objective' quantitative data. Interpretation involves assigning meaning and meaning inevitably requires the observer to exert some degree of subjectivity towards the object.

The objectification of nature through technological innovation is a critical step in the history of science, and also interestingly the development of the evolution of consciousness.

Beneath all this wonderful 'progress' is the background silence of chaos in the universe. The poets are better at giving this reality a voice than the scientists:

'When one looks at it, one cannot see it;
When one listens to it, one cannot hear it;
But when one uses it, it is inexhaustible'.
Tao Te Ching (Chp. 35)

[REPLY](#)

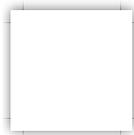


Jonathan Dore

May 26, 2012

NBeale writes as if maths were sufficient to demonstrate a theory in biology. Maths is a useful, indeed indispensable, tool in all branches of sciences, but unlike physics, in which the equations can sometimes lead, suggesting new realities that experimentalists then need to test for, biology is about complexity — and even the simplest single-celled organism is in many ways far more complex than, say, a star. Maths is only useful in understanding biology to the extent that it accurately models the reality of biological systems, and Dawkins's point in this article, it seems to me, is that however "irrefutable" the mathematics in Wilson et al's Nature paper may be in the abstract, they do not accurately model reality.

[REPLY](#)



Jonathan Dore

May 26, 2012

Tony Afanasiew: "A slating by Dawkins is recommendation enough."

Do you outsource all your thinking along similar lines?

Prospect is meant to be an intelligent periodical, but some of the ad hom. cattiness of the comments here — originating it seems from quite other agendas than the actual contents of this article, which breathtakingly few people have actually commented on in detail — beggar belief. It's a large part of the reason I stopped subscribing a couple of years ago.

[REPLY](#)



NBeale

May 26, 2012

RD: Why don't you have the courage to use your real name in making these absurd allegations. Or are you the Dawk in disguise?

Nowak has over 60 peer-reviewed papers published in Nature, Science and PNAS, let alone lesser publications. This is far more than all the celebrity atheist scientists put together. As you would know if you were a research scientist, this is nothing to do with where your money comes from and entirely a question of the calibre of your work. Wilson collaborated with him because he is one of the greatest mathematical biologists of our time. Many other truly outstanding scientists, including Bob May and Bert Vogelstein have collaborated and co-authored with him (I declare an interest, so have I).

Jason: see [here for a response](#), more detailed than can be posted in this comment section.

[REPLY](#)



Darren Thompson

May 26, 2012

Owen, I love the Popper quote. Could you please post the source? Thanks if you can.

[REPLY](#)

Owen Gilbert

June 2, 2012

Popper: Realism and the Aim of Science, p.7

Gould: in Patterns of Evolution, as Revealed by the Fossil Record, p.1; A. Hallam Ed.

[REPLY](#)

Jason Mills

May 26, 2012

NBeale, your blog link offers as examples of 'group selection' Christianity v Paganism 2-5C, and N Korea v S Korea. But these groups haven't been 'selected' – they all still exist. To assert that one is more "successful" than another requires choosing a measure for "success" that is more nebulous and arbitrary than any biological one. They aren't well defined – a person could move between all four of those and conceivably be members of all of them at once. They aren't obviously in conflict over the same resources (is S Korea trying to steal N Koreans?). They aren't biological – their prevalence or disappearance does not depend on the births and deaths of members. They aren't relevant to the non-human world – there's precious little 'culture' among aardvarks and ants, let alone aspens and algae. They are groups defined by ideas – so any claim for 'selection' among them must surely be a claim for selection among ideas, aka memetics. I could go on!

No one seems to be offering concrete examples of how THIS group of giraffes (say) is inevitably 'selected' over THAT group of giraffes because THIS group is intrinsically superior due to some trait that is not explained genetically. (If it were, they would not be groups in a genetically 'identical' population, but early speciators.) If that sounds like formulating a question in such a way that it can't be answered, well, therein lies the problem.

David Evanoff posted (though I can't see his post above at time of writing): "Group selection would seem to be the only available explanation for wild parrots in New York City." If you're there, David, can you say what problem this claim supposedly solves? Living in groups is not selection between groups, so I expect you mean something more than that.

[REPLY](#)

NBeale

May 26, 2012

jason. You miss the point. Christians were (and are) more successful in evolutionary terms ie (roughly) have more surviving grand children. This was nothing to do With genetic differences but group behaviour.

[REPLY](#)

David Evanoff

May 26, 2012

Jason Mills, selection between groups is indeed a defining characteristic of group selection. A characteristic I neglected to consider when offering my suggestion of group selection among wild parrots in New York City (oops). New York parrots now exhibit eusocial evolution stage 2 in that they construct large group nests to survive harsh winters. Whether these groups are stable, suggestive of selection between groups, remains to be seen.

While group selection is likely a valid hypothesis, like all such hypotheses in biology it does not explain nuances of the dynamics. In this case social dynamics. Biology must be the most intractable among the sciences in this regard, since effects are dependent across so many dimensions. Eusocial evolution could provide a new foothold for studying social dynamics. If half of all species are parasites, and parasites tend to become symbiotes, then perhaps eusocial evolution is the universal destiny. Perhaps Teilhard de Chardin shall have the last laugh.

[REPLY](#)

**Joseph Polimeni**

May 26, 2012

I have often wondered whether resistance to group selection is related to scientists not having played enough team sports. Every observant athlete knows that games can be won or lost over the subtlest disposition of a few teammates. In hockey, for example, a defenseman who unwittingly lines up closer to his defensive counterpart (the equivalent of a neutral mutation) can sometimes, by serendipitously stumbling upon a better defensive formation, determine the outcome of a game. Small positional differences that are neither advantageous nor disadvantageous to the individual (i.e. it won't change how much the individual player scores) can reap dividends for the group. Using evolutionary parlance – neutral traits can eventually become long-standing vestigial traits if there is no better phenotypic alternative. This, for example, explains the existence of the human coccyx bone and many other vestigial traits (male nipples, appendix). Now, imagine that the vestigial trait helps the group – that's group selection.

Dawkins claims that such actions are "incidental" (implying an action that is both accidental and inconsequential). Group selectionists agree with the accidental part (i.e. mutation) but not the inconsequential part. In fact, Dawkins uses the cyclists' slipstream metaphor as an example of group welfare being incidental, but, in fact, it is just the opposite. Such tactics are not accidental nor inconsequential.

[REPLY](#)**Kevin Foster**

May 26, 2012

Like Charles Goodnight, I find both Wilson and Dawkins to have an unnecessarily polarized view on these issues. With a deliberate bias (as I was defending kin selection), I attempted to illustrate the unity in social evolution thinking here (includes quite a few of Charles' papers incidentally):

<http://www.zoo.ox.ac.uk/group/foster/Foster%202009%20CSHL.pdf>

In the end, the utility of the different theoretical approaches depends upon the question, the biology and personal preference.

[REPLY](#)**Frances Morey**

May 26, 2012

My favorite imagery was: "... however far you/he may want to throw Wilson's book away, it will still fall short of how far Richard's books on culture should be thrown." That's a great line, among many. I enjoyed the erudite commentary as much as Wilson's scientific nuances. The fight between he and Richards was better than a Super Bowl. I peered at this article and I give it a good review.

[REPLY](#)**Duncan Scrymgeour**

May 26, 2012

I am by no means qualified to judge the coherence of either E. O. Wilson's theory or Richard Dawkins' response. But Dawkins, the creator of a theoretical construct with little real evidence to support it, the meme, is on shaky ground when he compares a colleague's theory to a variety of anecdotal silliness because he feels that there is no evidence to support that theory. Also, Dawkins, perhaps a wee jealous of his spotlight as one of the few celebrity biologists, employs a class "mean girl" swipe when he provides a litany of biologists who bash Wilson's thesis. One would think a simple reference to the Nature response would be enough. But like the most popular girl in class who is bullying her gentler classmate, she names all the other girls in his year who hate the classmate she is attacking. "They may not be household names," but they are cheerleaders who hate you to and that must account for something.

This is because, presumably, new ideas are not to be exposed as untenable, but important ideas to be falsified. (E.O. Wilson was far more charitable to postmodernist critics of science.) No, new ideas are, apparently, to be denigrated, because everyone knows that knowledge is something that only moves forward by committee.

While this particular game, at least, is in Dawkins court—he is far more embarrassing when he attempts to disabuse people of their religious beliefs—I can't help but wonder at his love of type of weak totalitarian meme. The best thing to do with wrong ideas is to quash them utterly. If you can do it while sneering at their makers and attempting to

provoke snickers from your supporters all the better.

Scientists have the right to defend their theories and no one wants to be wrong, but in his attempt to be clever, Dawkins' eye-rolling does little to recommend his science, although it does go along way in confirming my personal belief that his incontrovertible eminence as a biologist aside, he has something of the proverbial "little bitch" about him.

[REPLY](#)



Jeffrey Foster

May 26, 2012

Aside from the merits of either argument, I can vouch for what Richard Dawkins says about the arrogance. Wilson was on National Public Radio recently and presented his theory as if everyone had accepted it, contrary to "Oh yes, that selfish gene idea" that he claimed "didn't really work out".

He gave the distinct impression that everyone had discarded the genetic view and saw things the way he did, without a hint that there might be any dissenters. Somewhat amazed, I looked around enough to assure myself that this wasn't the case, not even remotely was it the case, quite the contrary.

To entirely ignore the widespread objections to your own ideas, indeed not just ignore, but pretend to others that they don't exist, and by contrast give the false impression that the contrasting theories are the ones that everyone is objecting to — It was a very strange performance, and sheer, unbridled arrogance the only conclusion I could come to also about where the whole thing could have come from.

[REPLY](#)



Bill Sweet

May 26, 2012

Is the article discussing both religious sects and religious insects?

On the news last week was a mother and uncle who drove a car away with a relative who just murdered someone that was in the trunk. That has to be the Kinship gene in action. Who would do such a risky action otherwise knowing the consequences?

A couple posters have played with the idea of proof of God. Mankind and science isn't there yet. Where are we then? Perhaps modest consciousness experiments that involve such things as prayer and ESP data at least suggests something exists beyond the projections of the Matrix aka the physical senses. You might like to see the very controversial experiments of prayer and human consciousness at <http://www.SpindriftResearch.org> Even believers have a hard time with what Spindrift has attempted to do; to uniquely combine religion with science.

[REPLY](#)



James V. Kohl

May 27, 2012

Last month (see below) I responded to a request for information about The Social Conquest of Earth. I think Dawkins' review and Wilson's book are like cries for help in the wilderness of evolutionary theory.

When these prominent theorists bicker, it can only mean good things for those with a better understanding of adaptive evolution/natural selection/sexual selection, via the basic principles of biology and levels of biological organization that link sensory cause directly to hormones and their affects on mammals, like us.

Wilson says we evolved via group selection. Dawkins, as always, posits the primary role of genes as replicators, as if epigenetic effects of the sensory environment is not responsible for their stochastic expression (e.g., in cells).

On 4/11/2012 8:05 PM, JVKohl wrote:

His [Wilson's] focus is somewhat on eusociality in insects and humans. He drops kin selection and inclusive fitness for humans, but acknowledges the Westermarck effect; drops effects of mammalian pheromones on hormones / neurotransmitters while accepting concealed ovulation and the ever-present but altogether unsubstantiated claim that humans have reduced olfactory acuity and specificity compared to other animals. (If true = no Westermarck effect, for example). Auditory stimuli and faces are causal for the mother-infant bond et al.

Suddenly, “epigenetic rules” offer explanatory power about half-way through the book, which allows him to link everything except olfactory/pheromonal input to group selection and an attack in the last chapters on religious beliefs.

Overall, I think he's been somewhat forced to acknowledge epigenetic/regulatory influences on the genome, since the sequencing of the human genome made it clear that our environment directly (e.g., epigenetically) alters the proteome involved in development of the body and brain. I am, however, confounded that his invertebrate expertise seems to have left him relatively clueless about the cause and effect of eusocial behavior. He seems not to realize that it is the diet of the queen bee, for example, that epigenetically determines her pheromone production and everything about the interaction of the colony including the epigenetically-determined neuroanatomy of the worker bees brains, and that this olfactory/pheromonal concept is the binding concept across evolution, not just in eusocial insects and eusocial humans. (In other mammals, for example, we have the epigenetic effects of nutrient chemicals and pheromones on luteinizing hormone (LH): the link between sex and the sense of smell — as also seen in brain imaging studies)

To him, epigenetic rules are rules of gene-culture co-evolution and group selection, I think, but maybe I missed something since I'm not a good listener. I only had time for the audiobook, not a more thorough read. I encourage those less biased by molecular biology to see if what he's saying makes more sense to them than it does to me. Overall, I think many sex researchers will be happy to see his careless disregard of the concept of human pheromones, for example, and be better able to link what he's saying about 'epigenetic rules' to some other model for the development of sexual preferences.

[REPLY](#)



Jason Mills

May 27, 2012

NBeale says: “Christians were (and are) more successful in evolutionary terms ie (roughly) have more surviving grand children. This was nothing to do With genetic differences but group behaviour.”

Isn't every christian a descendant of a pagan? Don't proto-pagans have the most grandchildren of all?

But suppose one religion 'wins out' over another (and certainly some do cease to be current). How is this “evolution”? The organisms are biologically unchanged. If this is evolution at all, it is cultural (whether or not you think this is well captured by the notion of the meme). I reiterate: how is this relevant to the non-human world?

Take my giraffes. Suppose there are two genetically 'identical' groups of about the same size who inhabit the same feeding range but rarely interact. The first Giraffe Group eats only Acacia – call them GGA. The second group, without genetic change, learn a new trick of eating Bushes, so we'll call them GGB. (One of the GGB giraffes learns the trick and the others see and copy it.) Since the groups don't meet much, GGA don't learn the bush trick.

Is GGB now at an advantage? They have more food available, but on the other hand they'll eat less Acacia, leaving more for GGB. But suppose the populations rise to the point where all the Acacia is eaten. Now GGB can survive, while GGA might die off. Ostensibly, GGB has been 'selected' for a non-genetic trait (though of course the capacity to learn new tricks is genetic).

Well and good. Over the next decades, the Acacias repopulate and the Bushes are struck by a blight. Since GGB know how to eat both, they switch to Acacia. A decade later the Bushes come back – but now there are no giraffes in GGB who know the Bush trick, as there has been no opportunity to practice it, much less copy it, and the ones who knew it are dead. So what is this group now? It's GGA. Where's the evolution?

For the new behaviour to persist demands cultural continuity. Humans are alone in having created means of perpetuating ideas independent of continuous use. (Genes do this for physical innovations, of course.) Animals can preserve ideas for long periods, but always contingent on the circumstances that favour that trait remaining in place. Elephant culling in Africa used to target the old 'worn out' elephants, yet it was they who knew the routes to the distant watering holes, so the still 'untrained' younger elephants suffered. Animals bred in captivity often cannot be released into the wild because they haven't learned the skills they need from their parents and peers. To argue that these learned behaviours are part of what constitutes the organism, and

that they lead to group selection, is to concede that the 'evolution' under discussion is cultural and not biological. At a stroke this wipes out any possibility of its application outside the animal kingdom for a start, and greatly limits its application within it – to those animals able to learn and copy new tricks, and to geologically tiny timescales. I'm not even persuaded that it's a necessary concept within that narrow range. Can somebody say why I should regard this dubious and limited notion as a significant force in evolution?

[REPLY](#)



John Flux

May 27, 2012

As a graduate student of Wynne-Edwards, I have been following these comments with interest. I suggest those of you looking for examples of group selection should read his two books (if any copies remain unburnt). I'm not joking; when I submitted a manuscript to *Science and Nature* in turn, referees said it was acceptable provided I remove all reference to W-E. The paper gives evidence to support W-E's contention that vertebrate populations remain well below their food limits, and eventually appeared in *Oikos* 2001. How they do it is obviously open to debate, but I have no difficulty accepting that selection works at many levels concurrently.

[REPLY](#)



Mark Cowan

May 27, 2012

Jason writes:

"Ostensibly, GGB has been 'selected' for a non-genetic trait (though of course the capacity to learn new tricks is genetic)."

There are a number of technical errors you have made in your comments Jason. I'll address 'the capacity to learn new tricks is genetic' point another time, citing the power of biography over biology, particularly with humankind. In part they go right to the heart of the historical misreading of nature over the last 153 years of development (not evolution) of evolutionary theory. First of all, Darwin's entire first chapter of 'Origin' was about the very real process of 'artificial selection'. Humans 'design' ways of breeding and cross fertilisation of plants and animals for a host of reasons. Darwin worked with pigeon fanciers a lot who were motivated not by reasons we can attribute to classical evolutionary motivations but by "man's fancy" as Darwin wrote on a number of occasions. He was very clear on this, "nature for the good of the organism", culture "for man's fancy" (also pleasure he wrote). Dawkins and Dennett, in lecture and interview both acknowledge that humans are the only 'intelligent designers' on the tree of life so when we discuss the social world of humankind, with most humans now living in the culturally created techno-cities around the world, these artificial settings designed and built "for man's fancy" represent a different challenge for social science (and evolutionary theory) than understanding botanical and biological change over geological change in the nature setting.

The central problem of nature before and during Darwin's time was the question of 'design'. Did God make all species who were immutable and had remained the same since the days of Noah and the Ark? Once the human world began travelling all over the world, expeditions finding fossils, improving technology, scientific development (again, not evolution) and improved understanding of the geological world sophisticated our understanding of time and the knowledge of the time and since changed to view that species had been changing and as we know the human story of life on earth dates back around 3.7 billion years. As Gould reminds (through his work) Darwin set out to two different things, to (1) prove the fact of evolution, and (2) show that natural selection was the main mechanism by which change took place. In demonstrating the fact of evolution Darwin (and others) have been proved correct and that species do change over geological time but there remain deep issues with the assertion that 'natural selection' is the dominant rhythm running through the evolutionary process, and at what levels 'natural selection' can be said to work. First of all, natural selection is a metaphor. Like all metaphors, they are 'carried over' from one domain from another and have explanatory power, but as Lewontin is careful to caution (quoting cybernetics theorist Norbert Weiner) "the price of metaphor is one of eternal vigilance". All systems (and this includes ecosystems) have a preserving quality and an ebb and flow of equilibrium. At all times we have to constantly remind ourselves that interpretations of the evolutionary process are part of scientific knowledge generated in the here and now (and there and now) and preserved for good reasons and for bad. The only true selection in the living world comes from humankind and from human mind. Only true 'selection' comes from

'intelligent designers' with control and responsibility (the ability to manage one's own response) and this is why in human courts around the world we don't put plants and animals on trial. That is a short statement open to discussion, nevertheless it remains a fact. Nature doesn't design, although there is a creative outcome from the evolutionary process but how we express and explain that process is always contingent of the level of human knowing of that time. Dawkins generated the term 'designoid' to highlight that the apparent design in nature was an illusion 'the illusion of design' and that is what evolutionary theory tries to explain, the apparent design of life in the nature setting. If we take vast expanses of geological time and we try and understand that random mutations can create differences that lead to phenotypic changes, it is not necessarily the case that all different varieties of the same species out-compete one another, and if you take a strictly Darwinian viewpoint, that is required. Nature has a 'loading capacity' that can accommodate a lot of different forms of similar species and it's even possible to understand change in the nature setting with the process of variation alone and geological time itself without having to refer to 'natural selection' as a rhythms in/through nature.

Darwin's 'eureka' moment came while reading Malthus' work on population. This was also an influential read for Wallace as well. With 100,000s of people pouring into the new industrialised cities (again, an artificial phenomenon) Malthus pondered on the implications of this. While some people did die humans 'design' ways to overcome problems through welfare systems, systems of sanitation, housing, employment opportunity but Malthus' book offered Darwin the 'mechanism' he had been searching for, and understanding that goes to the heart of 'selection' and 'the illusion of selection'.

I think 'natural selection' (or what is generally understood as 'natural selection') is a rhythm in/through nature but it is an action and not an agency and for some (and I'll refer to Dawkins & Co here) it is the entire basis of not only understanding nature, but all other systems, cosmos, culture and mind. This position is called 'Universal Darwinism' and remains the scientific UD, to religion's ID (Intelligent Design) in terms of dogma. Failing to appreciate the 'loading capacity' of nature to 'hold' vast amounts of life, and changing life over geological time which allow the process of variation (both Lamarckian and Darwinian) leads to a simplistic, even vulgar understanding of constancy and change in the nature setting. I totally reject the idea that some advance that natural selection is synonymous with evolution and vice versa. That is not something that evidence-based science points toward.

Christians say that 'Jesus Saves'. Whether or not that is the case we can say for sure 'Nature Preserves', she doesn't select. That is an epistemological artefact of human thought. The diversity of life on earth (the issue of design) was, and remains the central problem of the biological and botanical sciences, but for the students and scholars of the social world of humankind, the central problem is not one of design, it is one of drive, and that is a hypercomplex problem involving an understanding of the physical, natural and social levels and how they interact. We have to balance cradling the knowledge we have gleaned over human history, yet challenge it as well. That is difficult, often controversial and even exhaustive. For those who think that criticism of Richard Dawkins is knee-jerk, even blind 'Dawkins' dissenting' I would argue that Richard's entire evolutionary world view is rooted in his 'faith' in natural selection as more than an action in/through nature, to the point where it is a guiding agency. He completely misreads genes (which are the units of preservation, with an important role) as 'the units of selection' and this takes him off on the wrong causal pathway. In 'The Greatest Show on Earth' (only humans seem evolution as 'a show') he refers to four memories across the living world, genes, immune system, culture, nervous system. Placing his argument of 'replicators' to one side for the moment, all systems require some form of memory that enables the system to platform on subsequent change, if indeed that does take place. A better way to understand the four memories that drive the physical, natural and social worlds are 'time' (cosmos), 'genes' (nature), 'social organisation' which is the sum total of trillions of human designed artefacts (culture) and the 'unconscious mind' (mind). These different 'memories' provide the required level of structure across their level of operation. Richard wrote on the first page of the 30th anniversary of 'The Selfish Gene' (his human interpretation of gene function) that he could also have called the book 'The Immortal Gene' because the information in a gene is immortal. That simply is not the case with human-level cultural and mental information. Human level information is 'meaning' and it is constantly subject to change through conscious and unconscious processes, from the individual and/or the collective. To understand meaning is to try and grasp 'mutual information' (as it is referred to in quantum information theory), 'contingently true information' (as it is has been referred to in evolutionary theory) or 'continuously regraded entities' (as it is has been referred to in the social sciences). Animals and plants do not engage in meaning, it comes from the human level engagement of the world around us/them, which we call 'reality'. Reality is not everything; it is a specific (although at times 'rangey') human-level something. It is specific to each of us individually and collectively and is constantly being refreshed placing supreme challenges on social scientists in locating the laws of motion that underpin cultural

and mental action.

We can take the time to 'drill down' and find those causal laws of motion of mind and culture or we can use as metaphor theories from the neighbouring fields and domains, as long as we understand that they have a limited capacity to explain the phenomena we seek to understand. The world of meaning, of art, of technological adjustment and adoption is not one that evolutionary theory can explain to the level of causal laws of motion. That is fanciful, and yet in the past Richard Dawkins and other have been advocates of 'Universal Darwinism' (UD) which represents a dogma running counter to critical thinking. Again, we have to cherish knowledge at the same time as challenging it. Nature doesn't select, that is a human epistemological artefact, even a statistical artefact. Nature 'preserves' and understanding that demands that we reach out and understand the 'interaction' of the various levels of the complex and hypercomplex worlds around us. It is easy to hark back to more simple notions of 'intra-action' like we can see in gene-centrism but that bandwagon is slowing by the day.

I'll return to your giraffe example finally. The case in nature is that different varieties of species can co-exist and do exist without competition a necessary outcome driving out one species for another. Again, we can refer to 100,000s of different forms of beetle as a quick way of acknowledging nature's 'loading capacity' for life, and abundant life. What we refer to as 'natural selection' is a rhythm in/through nature but there are possibly 100s of different kinds of niche pressures and relationships, if not more and to frame all ecologies in Malthusian terms is a very human way of setting up the parameters for understanding. Different kinds of elephant do exist, and clearly different kinds of beetle exist. The most common 'state' of nature is no change at all, and yet if design is how we see the problem we focus on 'change' as the state of interest. Just by this act of focussing on change we set ourselves up to misread the real story of nature and that is a complex weave of different ecologies interacting across many levels, which demands a lot of time to understand this process. That is time a lot of people simply do not have and there is an allure in gene-centrism (intra-action) that makes the story of life seem more accessible. That is not the way to truth however and have to be more critical of neo-Darwinian attempts to explain culture, and even some of the elements of evolutionary theory that they think are givens. If you frame your giraffe 'thought experiment' in terms of competition then it will lead the discussion in a particular direction. If however the zoologists, ecologists and field researchers go out and see what is really happening in nature, while there is undoubted competition, there is also cooperation and a 'loading capacity' in /through nature which can enable diversity without always invoking the metaphor 'natural selection'.

I'll leave you with one example that Gould used to use to make a similar point. Sixty five million years ago a meteor hit the earth and wiped out between 65-90% of life on earth. That event was one of many contingent events in the story of life and the surviving species weren't there because they were 'fitter'. In some cases it might just have been they were on the opposite side of the globe from the meteor hit. We don't need to refer to 'cosmic selection' or 'natural selection' to understand that constancy and change over the geological time of botanical and biological species (including elephants, giraffes, and all manner of animals and plants) is not one size fits all (evolution = natural selection) and from this anyone seeking to mathematically represent what is going on in nature will struggle in part, if not wholesale, be that Hamilton, Nowak, anyone.

You mentioned giraffe-culture (this is my final point) but I would say this is proto-culture. Culture is a very particular (but not particulate) process which humans engage in. Everything short of this (and that includes early hominids) is proto-culture, indeed that is how it is referred to in palaeoanthropology. I think we can use terms and phrases like 'giraffe culture' as long as we know that we are really talking about 'giraffe proto culture' ("the price of metaphor is one of eternal vigilance") and the difference can be seen by understanding the term natural and artificial. Artificial is both 'human-made' and 'unnatural' and while evolutionary theory has tended to focus on micro (although important) issues like altruism, it cannot come to grips with human creativity, and certainly not the notion of artificiality, and culture. It is one level of understanding to appreciate that humans have created trillions of artefacts that woven together are our primary source of experience, and yet another level of understanding to realise they that they cannot stop creating, designing and artificially manipulating and managing (and mismanaging) the world around them.

REPLY



NBeale

May 27, 2012

I saw Martin Nowak yesterday and we discussed this whole thread. Various comments [here](#). And FWIW Dawkins implies that Pinker and Trivers signed one of the objecting letters to Nature but this is false. And although there are 1 or 2 pretty good

biologists in the rest of the list that Dawkins cites, none is really of the 1st rank. With approximately 4M biologists in the world the fact that 154 signed some letters to Nature objecting to a paper that turns a dogma into a rule of thumb is not such a big deal.

Jason: We're all "descendants" of Homo Erectus – that doesn't mean that H. Erectus is a more successful species! Your concepts of evolution and biology are far too limited and simplistic. Read eg The Music of Life or Evolution in 4 Dimensions.

[REPLY](#)



Mark Cowan

May 27, 2012

Goffredo asked "And does Wilson himself comment on the reason (other than the usual suspect) of his switch?"

I've nailed my colours to the mast on this and I am keen to be clear (or as clear as we can be) on the difference between 'selection' and 'preservation'. There are multiple levels of preservation and this is something that Dawkins does not deny. In the only debate I have listened to between Gould and Dawkins, Richard acknowledged that selection could take place at different levels but that his 'interest' was the level that interested him most, and that was at the gene-level and his support for kin selection. Gould (in this debate) was more a pluralist restating that 'selection' (or preservation as I see it, and Darwin for that matter) worked at various levels but it worked on bodies. The bookkeeping of bodies is in DNA/genes but that is different from saying that selection works at the gene-level.

For me, Wilson and Dawkins are both guilty of the same error and that is trying to explain culture in evolutionary terms rather than drilling down on culture on the terms and conditions of the phenomena we need to study, i.e. culture itself/herself. For me, Wilson is less guilty because over the last near 40 years there has been some movement in his position.

1975 – Sociobiology, short on humankind and culture.

1976 – On Human Nature, still short on humankind and culture.

1981 – Gene-culture co-evolution, still short on humankind and culture.

2000s – lectured that the social sciences needed to generate a general theory of culture.

Wilson's book is called 'The Social Conquest of Earth' and culture is a very specific (although rangey) kind of social. Wilson has humans alone on the fourth pillar of social evolution. The more you 'drill down' on culture you realise that biography (individual and collective) is as telling, if not more so than biology in accounting for human drives, and motion. I don't think that Wilson has drilled down far enough, and how could he. He is not a Professor of any Social Science, although he is an unquestioned authority of a number of fields. That is not a slur; it is a statement of fact.

Richard has not drilled down really at all on this. He took his 'gene-centrism' which is by no means a majority view amongst evolutionary theorists and cut & pasted it on to culture rebadging 'ideas' 'memes'. He even wrote in The Selfish Gene that he wanted a term that sounded like genes. This is not the painstaking rigour that we associate with Darwin. This was a misguided 11 page muse as 'memetics' that a stronger editor should have hived off into a discussion paper elsewhere. Daniel Dennett would later write in 'Darwin's Dangerous Idea' that memetic and genetic evolution matched exactly. This is a totally ludicrous claim without a shred of empirical credibility.

Dawkins and Wilson are short, but Wilson (for me) less so. Today in the UK there is a book released 'Cells to Civilizations: The Principles of Change That Shape Life' by Enrico Coen. The book sets out to do what Wilson tried to in his 2001 'Consilience: The Unity of Knowledge' by looking at four areas: evolution, development, learning and human culture. It is important to note that 'human culture' is defined as a question but no compelling answer or laws of motion are offered. It is noteworthy that in our time this idea of the unity of knowledge is becoming more accepted. Physical laws can be found from the level of physicality around us. Once conditions are right we have the chemical, bio-chemical and then the biological (and botanical). It is important to note that we can study the physical world as much as we want but this will not give us the answers to the natural world. Understanding particles and how they work doesn't tell us how 'life' works, or how evolution works. Likewise, from the biological emerges (not 'evolves') the social and then the cultural and mental. Just as physicists didn't generate the theory of evolution in the nature setting it runs counter to the specialisation that embodies scientific rigour and discipline that the natural sciences can explain culture to the level of laws of motion. Dawkins' faith in Universal

Darwinism (UD) although waning is indicative of his seduction to 'natural selection' as more than a rhythm in nature, and something much wider, even universal. Wilson understands that we need something more, although he (indeed everyone) remains unskilled in knowing what that more really is. We can study ants all day long but that won't however give us the required insight into art, creativity and the consequences of the global technosphere.

We know that evolution is the process of change in the organic setting of nature but 153 years after 'Origin', being clear about selection and preservation and the multiple levels of organisation, we can see this through continues to challenge us. Good.

[REPLY](#)



David Evanoff

May 27, 2012

Jason Mills, let me modify your giraffe model to perhaps be more compatible with Professor Wilson's description of group select: Both GGAs and GGBs are eating just acacias. GGBs are more temperamental than GGAs so that when elephants or other giraffes come to ravage or denude the acacias the GGBs become enraged, striving disparately, even suicidally, to kick the offender where it hurts most, thus driving them off. All the giraffes are at stage 1 in that they form a group. The GGBs are at stage 2 in that the acacias have become for them a valuable and defensible nest. Since the GGBs are so successful at preserving their acacias, having run off all the GGAs, the acacias thrive and the GGBs have no need to migrate. They have now crossed into stage 3, they no longer disperse. Now the GGBs begin to get a bit over populated and the acacias come to the edge of being denuded. Since this is a hot button issue for these temperamental GGBs they strike out and kill whoever is vulnerable, which happens to be mostly the younger giraffes, and thereby their over population is checked. They have thereby crossed over into stage 4 exhibiting a new interaction within the group. Since the GGBs are so successful at hording the acacias for themselves, and being that they are mostly between 3 and 5 meters tall, the 2 meters of understory are not being harvested as intensively as it might be and it becomes advantageous to be a shorter GGB. Hence, over the generations the range of sizes of GGBs becomes wider, and the GGBs have crossed over into stage 5 and there are new differentiations within the group.

Now this an absurd example, because giraffes are not very well predisposed to becoming eusocial. But if you substitute termite for giraffe in this example it doesn't look quite so absurd. Professor Wilson stresses how infrequently eusociety has evolved over the eons; how astounding it is that it has occurred in a hominine. Yet once it is established, eusociety is astonishingly successful, transforming whole ecosystems. Professor Wilson once championed kin selection, but has since become disillusioned with it because kin selection models do not fit the evidence in the field. His is now championing a new model of group selection to counter kin selection that seems to better fit the field data. Seems to me that biology is suffering now from a bad case of physics envy. They yearn for the expression of biological dynamics in an elegant and precise prescription, just like physics. Trouble is biology is more like economics which has been likened to be as a "science" with 50 simultaneous equations of 100 variables. In order to find solutions you have to arbitrarily fix half the variables. Therefore you have no science at all, just someone guessing.

The prevailing distinction between phenotype and niche is arbitrary. To a very large extent organisms create their own niches. Why else would half of all species be parasites? Parasites must manage their hosts or they become extinct. I'm no biologist, but I don't think prevailing notions of natural selection account for this.

[REPLY](#)



Ficking Chucken

May 27, 2012

I haven't read Wilson's book yet (I received it back in April); I have been taking time instead reading the reviews. So far it appears as if the scholarly reaction is mostly negative. Most of the objection states that a bunch of academia has published a rebuttal of Wilson's et al, paper that was published in Nature. I was wondering if there is any response to the math which Wilson, et al, uses as the foundation of their newish view. I would like to see some more of that and less of the unqualified opinions. Tell me why the math is wrong.

[REPLY](#)

**Goffredo Smeets**

May 27, 2012

Mark, thanx for answering my question. BTW, the point about selection/preservation I'd already taken in the nineteneeighties, reading Maturana & Varela, The tree of knowledge.

[REPLY](#)**Mark Cowan**

May 27, 2012

Goffredo (indeed, anyone else)

Letter 2931 – Darwin Correspondence Project
Darwin to Charles Lyell, September 28th, 1860.

Two points of lasting note: "I quite agree with what Hooker says that whatever variation is possible under culture is possible under nature;— not that the same form would ever be accumulated & arrived at by selection for man's pleasure, & by natural selection for the organism's own good."

Nature does not make computers, cars, satellites, oil rigs, or any other of the trillions of artefacts that human foresighted design has made. The difference between "the organism's own good" and "for man's pleasure" is a difference of dimension. Culture remains tellingly different but not detached from nature.

On the second part of the letter Darwin writes: "Talking of "Natural Selection", if I had to commence de novo, I would have used ;"

Darwin was a scribbler and Lyell thought he had wrote 'natural persecution' because he was under pressure from religious figures at the time. Any change from 'natural selection' at the time could/would have been seen as a potentially fatal retreat from the theory in its infancy. Science and knowledge have to reflect constantly and we need to understand that culture 'preserves' and this links evolutionary theory into a deeper level of import with physical laws of conservation. The misreading of nature 'selecting' has led to the promotion of action as agency, and process as purpose, which is most clearly seen in gene centrism. Pondering on the gene as the unit of preservation, rather than the unit of selection changes your view of nature almost immediately.

This letter (for me) remains the most important in the history of evolutionary theory apart from the letter that Wallace wrote to Darwin explaining that he too had reached a similar conclusion .

[REPLY](#)**Mark Cowan**

May 27, 2012

On that last quote Darwin said he would have used 'natural selection' if he had to start over ""Talking of "Natural Selection", if I had to commence de novo, I would have used ;"

It demands an entire change in outlook.

[REPLY](#)**Stuart Mathieson**

May 27, 2012

Richard's reaction to Wilson's latest book reinforces my impression that a youthful epiphany by the preternaturally clever becomes increasingly dogmatic with age. Too much invested you see. I have always admired EOW's tenacity and courage in the face of quite ferocious neo-Lyshenkoism. I can't wait for his book to arrive in my mailbox. Incidentally, group selection like continental drift has always been obvious to babes and sucklings.

[REPLY](#)**Michael Bentley**

May 27, 2012

NBeale:

Thanks for pointing out the Nowak lecture in Cambridge. I assumed you meant Harvard, Cambridge but I was pleased to see it's a little closer to home.

General:

There seems to be a lot of confusion here about what the Nowak paper does and does not say about group selection. Their mathematics detail a model of eusociality evolving in which inclusive fitness theory need not be invoked as an explanation. The model is not complicated, just a set of ordinary differential equations. It is also not a model of the type of group selection many of you here seem to be advocating.

So where does the group selection bit come in? They discuss 5 phases of the evolution of eusociality. The first 4 phases are more or less included in their mathematical model. But what about phase 5? This is given its own short paragraph in the discussion:

"In the fifth and final phase, between-colony selection shapes the life cycle and caste systems of the more advanced eusocial species. As a result, many of the clades have evolved very specialized and elaborate social systems."

I believe that, whatever the merits of their mathematical model and views on inclusive fitness theory, this last statement is unfounded. They present no evidence, empirically or theoretically, to show that 'clades have evolved very specialized and elaborate social systems' as a result of selection between groups.

[REPLY](#)

Feedback

Michael Bentley

May 27, 2012

Mark:

I have to disagree with you. These semantics are largely uninteresting and haven't changed my view of nature one jot. To describe something as 'preserved', makes that something sound like it is unchanged/unchanging. That doesn't really capture the essence of evolution to me.

[REPLY](#)

Mark Cowan

May 27, 2012

Michael Bentley

Your comments are telling. Humans are driven by interests (my remarks apparently didn't interest you) which are often more powerful than instincts. Darwin wrote in 'Origin' that habits (which interests mostly are) are almost indistinguishable from instincts.

Nature, over the expanse of 3.7 billion years is largely unchanging Michael, so if you want to capture the true essence of nature you'll have to be more open to that. Darwin was not a trained social scientist yet in 'Descent' he wrote that there was an apparent uniformity in nature. To preserve is to 'keep' and if you take on board nature's 'loading capacity' if we want to accurately comment and capture nature's essence then it's more truthful to say that nature wants to save ever species. If you want to adopt the view that nature 'selects' that is an entirely different world/nature view. To rubbish that as semantics is to fail to grasp the deeper import of what I am saying, and what Darwin himself was commenting on. To write that if he started all over again he would go with 'natural preservation' rather than 'natural selection' is a pretty big difference. It is part of the charm of human expression that we can dismiss things we don't find interesting as 'mere semantics' but often it is the case that this (and I'm not saying this in your case) that this shows a failure to understand how meaning works. Granted, it is tricky.

If you adopt the view that nature 'selects' (which is to confuse agency with action, purpose from process) then economic systems like capitalism become normal, even natural. There is no analogue of capitalism in nature. Nature emerges and changes over geological time, whereas capitalism is an economic system rooted in growth for growth's sake. The only empirical justification for capitalism as a system of growth on growth is capitalism itself. Schumpeter famously referred to it as "creative destruction" and this has been adopted into the language of evolutionary theory to a degree.

If you want then you have to work hard at it. Taking on board the story of life is 3.7 billion years old, that for most of that time constancy is the norm (or essence) as you

put it means holding a lot of variables through an array of disciplines. If however, the human lens wants to concertina nature's geologically timed complexity and diversity into a book solely about change then we can have a better understanding of change, but that is not the same as having a better grasp of the evolutionary process. Understanding that evolution involves contingency, the power of variation, neutral processes of change over geological time and a loading capacity to accommodate immense diversity may well be inconvenient for you (or uninteresting) but that is what is and has been going on out there in nature.

Humans design. We see a designed world all around us and I've already noted that the human world of design is the primary source of experience for most humans in their daily lives. It is all too easy to read design into nature but as Dawkins himself has written, that is an illusion. There are a number of key variables involved in understanding nature's 'preservation' while it's far easier to run with the 'natural selection' idea. That is more about convenience rather than truth however.

There is a difference between 'the essence of nature', and the 'essence of evolution' as the process of biological and botanical change over geological time. There is a profound difference there and if that difference is conveyed using words that is not merely semantics, it gets to the heart of human knowledge as epistemological artifact. Views aren't changed by single internet posts, but the difference between a more neutral, preserving nature over geological time and a more 'selective' nature 'designing' species is a deep one. All the work on drosophila in the labs provides a concertina'd view of evolution, but that remains a process of artificial selection, or design and purpose. Nature remains a deeper, geologically timed system than culture and we should avoid of 'reading' cultural processes into nature that don't belong there, no matter how convenient.

[REPLY](#)



Michael Bentley

May 27, 2012

I'm sorry Mark, but whether or not we choose the metaphor of nature as 'selector' or nature as 'preserver', it is still anthropomorphism. We still confuse 'agency with action, purpose from process' as you suggest.

I can see how both metaphors could be useful, but whichever we decide to use, the underlying biological process remains the same. And I wouldn't agree at all that constancy is the norm in nature. That massively depends on the spatial or temporal scale one chooses to observe. As a systems biologist studying social evolution, I can tell you that I would love it if nature would just behave and sit still for a minute or two.

[REPLY](#)



Mark Cowan

May 27, 2012

Michael Bentley

All human expression is subject to anthropomorphism. It is a lasting social and cultural fact that humans communicate through non-equation, and words and all manner of expressions have a fluidity built in. Such is the challenge in understanding how meaning 'works' for social science.

The underlying biological processes do not remain the same as you assert. These are two very different emphases of the what is going on in nature. One view is that nature is almost inert, has a loading capacity to accommodate abundant life and is captured by 'natural preservation' which I will remind you was Charles Darwin's favoured term. The other framing of what is going on in nature is that nature is 'like' culture (but this is a metaphor) and 'like' natural selection and more active shaping and crafting species constantly making them fit their environment more efficiently all the time.

I put it to you that 'natural selection' is perhaps the most misleading 'anthropomorphic' expression of what nature does, and one you seem to adhere to. As a systems biologist you deal in dynamism and for all the motioning at the molecular level, with each species (according to E.O.Wilson) sticking around on average for between 1-10 million years the idea that nature lacks apparent uniformity lacks credibility. Evolution is a process framed over geological time, only cosmic time offers an even more near-static view of the world.

There are levels of rhythm here and it may well be the case at the molecular level there exists dynamism that is just not the case when it comes to understanding what

organisms do in their ecological niche. Likewise, a subatomic particle physicist at a level of analysis can express wonder at the universe-like motioning at one level, but the physical world is the most stable and near-static between the physical, natural and social worlds.

When it comes to culture the difference could not be more stark. Stephen Jay Gould called culture "the Lamarckian juggernaut" and we can compare this with "the slow, gradual degrees" of evolution, which you seem to reject, the human engagement with temporal sequence is a very different one than experienced with plants and animals.

You study social evolution so you are between worlds. There is a settled theory of biological and botanical change over geological time in the nature setting (evolutionary theory) yet we remain without an equally strong theory for the social world, and lack even further behind in the quest for a general theory of culture. I don't see how you can deny biologically and botanically stable animals and plants in stable localised environments over periods of time difficult for the mind to fathom doesn't represent constancy, and a degree of stability quite detached from the ever quickening pace of change in the designed human world.

I accept that the dimension of 'social' is more fluid than the biological and more complex but when we are dealing with the animal kingdom it is far more local than the globalising species that is humankind enabled by culture 'proper' as opposed to primordial and proto-culture.

All biological processes have to be expressed and ultimately read. Two different people can read the same research paper differently, and that is an inherent wonder and frustration in having a human mind, a human 'mine' in terms of the way you, I and others all individually seem the worlds around us. At the scale of seeing nature, landscapes, species, ecologies, at that level nature is truly stable and constancy within and through geological ages is the norm. I'm at a loss as to how you can deny such an apparent given.

[REPLY](#)



Will

May 27, 2012

I don't fully understand why we require the genes be exact copies when talking about relatedness. For example, an allele for green eyes is (on the nucleotide by nucleotide level) 99% identical with a blue-eye allele, with only a couple of substitutions.

It's the same way with all extant animal relations. I share many mitochondrial genetic elements with chimpanzees and cats, yet our r is defined as ~ 0 . I guess I understand that the math is easier if r becomes $1/2$ with each branching of heritage, but doesn't that neglect a huge degree of similarity in genotype and phenotype between all earthlings?

[REPLY](#)



Michael Bentley

May 27, 2012

Mark you appear to have misunderstood me. I said that whether or not one observes constancy is dependent on the spatial and temporal scale being investigated, not that constancy doesn't ever occur.

I agree that a species may exist (or appear 'constant') over a geological time scale. A particular allele may last only one generation though. Or it could be strongly selected for (or against) and rapidly reach fixation (or disappear) in a population. More often than not, the allele will rise and fall in frequency remaining in a state of flux for long periods until its fate is determined. This is just an example of a level at which constancy often doesn't exist, don't read too much into it.

I also didn't say, in any way, that metaphor/anthropomorphism isn't common/useful in scientific discourse. I was merely saying that natural 'preservation' is as much a metaphor as natural 'selection'. I said this because you suggested that the use of selection confuses 'agency with action, purpose from process'. I agree, to some extent, but exactly the same is true if 'selection' is replaced by 'preservation'.

[REPLY](#)



NBeale

May 27, 2012



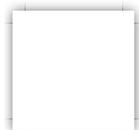
The older comments seems to have become hidden so for the benefit of any newcomers let me see if I can summarise:

The paper Dawkins refers to is [here](#). It was the front cover paper of Nature. Contrary to Dawkins' ridiculous assertion it was thoroughly peer reviewed.

137 biologists signed a letter to Nature objecting to the paper, (and 17 signed 3 other such letters). However [Robert Trivers](#) and Stephen Pinker were *not* among these signatories.

The paper shows conclusively that Hamilton's Rule does not always apply. This is a scientific fact. That Hamilton's Rule is a useful rule of thumb, that can provide insights into many real biological situations, is not denied. But it adds nothing at all to the standard theory of natural selection.

REPLY



Jerry Coyne

May 27, 2012

Note: I left this comment yesterday when it was "awaiting moderation." Since it hasn't appeared, and has nothing in it that would seem to preclude publication (including length, since it's not as long as other posted comments), I will try again.

I'd like to make four points. First, the fact that Nowak et al.'s paper was published in Nature after peer review does not mean that it's right or even makes a substantive contribution to the field. Plenty of papers—even those in Nature and Science—turn out to be either wrong or trivial. My own guess is that having Wilson's name on the paper helped its publication, but that's just a guess.

At any rate (point 2), the paper is misguided in several ways: it argues that "kin selection" (i.e., the concept of inclusive fitness) is not part of "regular" natural selection, and claims that group selection is a better explanation for eusociality than is kin selection. In fact, as everyone knows (except, apparently, Nowak et al.), inclusive fitness is simply a natural extension of "regular" natural selection.

Further, the authors claim that inclusive fitness makes no difference in their model, but that's flatly wrong, for they did not make a model that varies the level of relatedness, and start with a related group to begin with. They cannot in fact say anything about whether their model would work better with more relatedness. They also ignore the substantive evidence that eusociality in fact is promoted by higher relatedness, including the general 3:1 ratios of reproductive females to males predicted by worker control of the brood (a prediction that's pretty much been verified) and the paper by Hughes et al. (in Science—another prestigious journal!) showing that eusociality evolved only in insect lineages whose ancestors mated singly. That is a statistically significant relationship, and has no explanation other than a connection between relatedness and eusociality. See the paper here: <http://wsbs-msu.ru/res/DOC187/EusocialityKin.pdf>

And as long as "NBeale" is throwing out c.v.s and prestige in defense of the Nowak et al. paper, weigh that against the 125+ scientists, many of them highly accomplished biologists and/or mathematicians, who find severe fault with that paper, and whose criticisms were also peer reviewed and published in Nature. Indeed, one must take those opinions seriously, since they include people who are quite conversant with the issue at hand. Their criticisms can't be ignored, though Nowak et al. did a pretty good job of ignoring them in their response, which was lame. They basically responded by saying "we did the math." Well, so did some other evolutionists who know their equations, and found the model inadequate to buttress the conclusions.

But let's ignore authority and just look at the facts. There are many phenomena in nature, beginning with parental care and running through sex ratios, that have come within the ambit of scientific understanding through considering inclusive fitness. The 120+ authored paper (which includes my name) gives a list of contributions that kin selection has made to our understanding of evolutionary phenomena.

In contrast, I can't think of a single phenomenon, with the POSSIBLE exception of sexual reproduction, which is explained better by group selection than by considerations of inclusive fitness. We see no cases of pure biological (non-reciprocal) altruism in nature, which is a prediction of inclusive-fitness theory but should be fairly common if group selection is an important force.

The simple fact is that people go on and on about group selection and multilevel selection, but the evidence for those processes is slim at best. Even some of the lab experiments purporting to demonstrate it really don't, but of course you can always manipulate group dynamics in the lab to produce group-level effects.

The real question is whether group selection has been an important evolutionary force IN NATURE (not in tubes of beetles or flocks of domesticated chickens), and the weight of scientific evidence (and authority, if you want to count famous scientists as NBeale seems to do) is that group selection ONCE seemed a good idea but has fallen on hard times due to lack of evidence. Further theory, as elucidated by Richard in his piece, tells us why that form of selection is likely to be ineffectual. I'm talking here not about cultural evolution, or "memes" (a concept I've never found very useful), but about real genetic evolution in the wild.

I will say about group selection what Laplace said about the idea that, as suggested by Napoleon, God had a hand in celestial mechanics: "We have no need of that hypothesis."

[REPLY](#)



Hugo de Toronja

May 27, 2012

It is exceptionally repulsive that Mr. Dawkins should exploit Prospect as a convenient platform for advancing an insidious and baldly hate-mongering agenda.

Mr. Dawkins writes, "The American grey squirrel is driving our native red squirrel to extinction, no doubt because it happens to have certain advantages. ..."

As Mr. Dawkins is no doubt aware, the American grey squirrel did not arrive in Britain on tiny rafts deployed by the Spanish Armada, nor was he smuggled into the country by 18th century French "engineers."

The American grey squirrel was wholeheartedly *invited* into Britain by novelty-starved Victorians who were quite understandably seduced by the American grey squirrel's delightful personality and magnificent physical charms. And it's obvious that the American grey squirrel has done so very well in Britain precisely because he unreservedly adores his adopted homeland and *not* because he fiendishly desires to undermine its ecological integrity.

Mr. Dawkin's sick-making bigotry ought be trundled off to the same abyss into which Reason and Science have jettisoned countless other moral malignancies.

[REPLY](#)

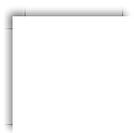


MarinaS

June 24, 2012

You unambiguously win the thread. Maybe even the internet.

[REPLY](#)



Jason Scott

May 28, 2012

I think it should be pointed out that NBeale (i.e., Nicholas Beale, whom I presume most of you have never heard of) is a religious apologist who has coauthored a book called "Questions of Truth" which among other things promotes the view that the "universe exists for the express purpose of making the existence of humans possible." <http://richarddawkins.net/articles/3671-book-review-questions-of-truth-god-science-and-belief-by-john-polkinghorne-and-nicholas-beale>

If this is true, he has absolutely no credibility in a scientific discussion. Also, for someone that has repeatedly criticized Dawkins "dogmatism", he is oddly submissive in the way he speaks of Nowak's authority.

I recently attended a talk by Wilson where he mentioned that Nature flew an editor out to discuss the 2010 paper with Nowak et al. before it was published. I spoke to other people at the meeting who have also published in Nature and this is most definitely not standard practice. I'll let you draw your own conclusions, but if that's not special treatment then I don't know what is.

[REPLY](#)



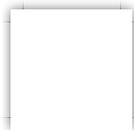
Brian Josephson

June 25, 2012

Jason, I take it you have a scientific *disproof* of the Beale's view that "the universe exists for the express purpose of making the existence of humans

possible."? If not, then you have no business attacking someone who suggests that.

[REPLY](#)



Andy Lock

May 28, 2012

Dawkin's writes above that

With the exception of one anomalous passage in The Descent of Man, Darwin consistently saw natural selection as choosing between individual organisms.

What Darwin actually consistently stated was:

Can it ... be thought improbable, seeing that variations useful to man have undoubtedly occurred, that other variations useful in some way to each being in the great and complex battle of life, should sometimes occur in the course of thousands of generations? If such do occur, can we doubt (remembering that many more individuals are born than can possibly survive) that individuals having any advantage, however slight, over others, would have the best chance of surviving and of procreating their kind? On the other hand, we may feel sure that any variation in the least degree injurious would be rigidly destroyed. This preservation of favourable variations and the rejection of injurious variations, I call Natural Selection. Origin, 1st edition, 1859, pp.80-1, and subsequent editions

What Darwin is stating here is that natural selection is a consequence of variation. Natural selection is Darwin's descriptive term for this process. For Darwin, then, evolution is a logical consequence of variations between individuals competing for finite resources. This is very different from saying natural selection 'causes evolution' or as Dawkin's puts it 'chooses between individuals'.

My conclusion is that if Dawkin's can be so wrong in his interpretation of Darwin, then one should be wary of trusting his understanding, or critique, of Wilson.

[REPLY](#)



Robert

May 28, 2012

Here, this is why Dawkins is wrong: <https://www.youtube.com/watch?v=dEaMtSLwkDs>

[REPLY](#)



BDL

May 28, 2012

And then there is that other pesky charlatan, Samir Okasha, whose book defending multi-level selection (Evolution and the Levels of Selection) won the Lakatos Award for philosophy of science a few years ago.

[REPLY](#)



NBeale

May 28, 2012

Jerry: Thanks. I remember that "monogamy" paper and it's neat.

I think your comments highlight the difference in outlook between typical biologists and typical mathematicians, but that on substance you don't differ much (here at least) from Nowak et. al.

Everyone agrees that Hamilton's Rule is a useful rule of thumb that has helped bring various phenomena "within the ambit of scientific understanding" and that relatedness is very important in biology. But Nowak & al's main point seems to me to be that when you look at the maths carefully in detail you find that Hamilton's Rule is often an approximation and sometimes plain wrong, and that (as you agree) it never provides a (correct) explanation that differs from standard Natural Selection.

Most biologists (at least 20th C ones) tend to react to this with "well OK, the mathematicians may not like this model but it's pretty useful in our field, we all know that models are imperfect that that biological systems are very complex" whereas most mathematicians will think "OK, refuted, next..."

You do also need to engage with some of the biology in NTW. For example "among the 70,000 or so known parasitoid and other apocritan Hymenoptera, all of which are

haplodiploid, no eusocial species has been found. Nor has a single example come to light from among the 4,000 known hymenopteran sawflies and hornets, It has further turned out that selection forces exist in groups that diminish the advantage of close collateral kinship... Most of these countervailing forces act through group selection or, for eusocial insects in particular, through between-colony selection."

REPLY



Mark Cowan

May 28, 2012

In their 2010 paper 'The evolution of eusociality' Nowak, Tarnita & Wilson state that eusociality is "the most advanced forms of social organization and the ecologically dominant role of social insects and humans."

There may be weight in that assertion in terms of insects but in terms of humans, it is false. We are 153 years after 'Origin' and despite 10+ schools of thought from the evolutionary perspective there has been no credible theory of culture from evolutionary theory. It's important to note that there has been no credible general/grand theory of culture from the social sciences either.

'Social' and 'culture' appear to mean something different for the biological realm than it does for social science and it's always important to get your foundation right otherwise the variables and relationships you end up dealing with will be poorer as a result. 'Social' is communicability, and the effects of that communication. Human 'social networks' are all too often (this online forum one example) technological and that is not a biological-level phenomena, that is a cultural-level phenomena. Roy Baumeister 'The Cultural Animal' writes that culture "is a new kind of causation" and (from my near 20,000 hours on this) we have to understand meaning and time interwoven as the fabric of society as the social level that humans engage in/through. Once the social sciences generate a full, rounded theory of culture with laws of motion and even equation (which I think will happen in our lifetime) this 'new kind of causation' has tremendous implications for the current evolutionary attempts to come to terms with social, eusocial and culture.

A general theory of culture will benefit all critically thinking people interested in evolution, which is not all change but a process of biological and botanical change in the nature setting. Right now, while not a free for all, evolutionary theorists simply do not know how much 'social' behaviour their perspective (individual or collective) should try and account for. If your variables are not correct or cloudy that can only yield maths that are subject to scrutiny and even criticism. We can see from this thread (expressive fabric) that there is discussion, debate and even accusation of 'dissing'.

I've stated that the bandwagon on gene-centrism is slowing. Genes are bookkeepers for preserved random mutations over geological time. If the 'loading capacity' of an emerging ecology can accommodate a wide range of species there is no need to invoke 'natural selection' (a human reading on preservation) to account for apparent design in nature. When we take the vast story of nature and concertina it down into a book of change is it important to realise that we are now discussing 'change' in nature, and not nature herself. It is possible (but not the full story) to understand change in nature by really taking on board the expanse of geological time, random mutation, buffer rather than efficiency in nature and 'loading capacity' of an ecology. In more Malthusian relations the idea of 'natural selection' as a rhythm is valid but the trend to towards promoting evolution as synonymous with natural selection and vice versa is a vulgar portrayal of what is really going on nature.

In 2012 we know more and more that systems-level understanding demands that we seek to understand the variables, how they interact and that there is an ebb and flow 'through' the system, rather than trying to argue, and over-state the important of the 'in' of an individual variable. Genes are important, super-important even as part of the evolutionary process, but if you are rooted to the idea that genes are the 'purpose' of evolution then as we learn more about social emergence and the extension of culture from nature it's only going to be a bumpier and more uncomfortable intellectual ride for you in the future.

I've stated previously that Wilson has at least tried to reflect and revise his position on understanding the social world, which he referred to in his 2001 book *Consilience: The Unity of Knowledge* as "hypercomplex". I agree. If, as I think theory of culture will show, culture is a new kind of causation, with laws of motion and more, then there will be a bridging, emerging realm of the 'biosocial' to account for. Hopefully at that point the variables and their interactions will become clearer and the maths will become cleaner, and even more elegant if we can say that.

Until that day (just as in nature with many species) culture can accommodate alternative views just fine.

[REPLY](#)



NBeale

May 28, 2012

Jason: Thanks for the plug for [Questions of Truth](#). You might not like what it says but it was endorsed by 2 Nobel Laureates [Hewish](#) and [Phillips](#). FWIW I'm also 1st author of a [PNAS paper](#). The days when "thought crime" disbarred you from science are long gone, except it seems in the sad sub-culture of Dawkins and some of his acolytes.

[REPLY](#)



NBeale

May 28, 2012

Jason: PS FWIW I've just had an unsolicited email from another Nobel Laureate supporting my stance here hand saying he's filed a comment which is apparently awaiting moderation. You might want to be a bit more careful in future before making *ad hominem* attacks 😊

[REPLY](#)



Jerry Coyne

May 28, 2012

NBeale, I find your arguments curious: you never address the biological arguments against group selection I gave, but continue to cite authority figures, c.v.s, etc. I gave some evidence, and neither you nor anyone else has bothered to refute it, much less address it. All you say is that one paper that SUPPORTS the importance of kin selection in the evolution of eusociality (Hughes et al.) is "neat." That's hardly engaging in the argument.

And you keep citing Nobel Laureates, but the two you cite are physicists who support some book you wrote with John Polkinghorne on the harmony between science and faith. Why is this at all relevant to the scientific argument. Do you have some animus against Dawkins because of his stance on religion. Nobel Laureates who are accommodationists are completely irrelevant to this discussion.

Your comments are almost devoid of scientific substance, Please address, for example, the paper of Hughes et al., and tell me why you think it's wrong:

Hughes, W. O. H., B. P. Oldroyd, M. Beekman, and F. L. W. Ratnieks. 2008. Ancestral monogamy shows kin selection is key to the evolution of eusociality. *Science* 320:1213-1216.

And then there is this critique of Nowak et al.:

Rousset, F., and S. Lion. 2011. Much ado about nothing: Nowak et al.'s charge against inclusive fitness theory. *Journal of Evolutionary Biology* 24:1386-1392.

and this discussion of the generality of Hamilton's rule:

Gardner, A., S. A. West, and G. Wild. 2011. The genetical theory of kin selection. *J. Biogeography J. Evol. Biol.* 24:1020-1043

As for engaging in the biology, nobody has said that haploidploidy is an automatic route to eusociality. We realized that a long time ago, and several of the people who criticized the Nowak et al. paper have said that in other publications.

The model of Nowak et al is neither a model for the full evolution of eusociality nor does it show that inclusive fitness is not important in that evolution. I'd appreciate it if you'd stop plugging your science-and-religion book and say something substantive to rebut the critics of the Nowak et al. paper.

[REPLY](#)



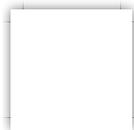
Robert

May 28, 2012

"He's confused. Richard Dawkins is a good man, but he does not publish in peer reviewed journals and has not really examined the basic theory." –EO Wilson

<http://www.youtube.com/watch?v=dEaMtSLwDs#t=3043s>

REPLY



NBeale

May 28, 2012

Jerry: Thanks. I'll try to respond at greater length later if I get time, but

- Hughes et al. No-one says relatedness is not important. But you also have to explain the biological counter-points from NTW I mention [here](#).
- Gardner et al. I'm told that the main problem is that they redefine B, C and R so that clones and (normal) brothers don't have $R=1$ and $R = 1/2$ and that B and C become functions of R and indeed of the detailed evolutionary dynamics.
- Rousset et al. I hadn't seen this rather obscure paper (3 citations so far) and I'll try to read it in detail and comment.

Can you respond perhaps on two points:

- Do you agree that "biological systems are more complex than you think, even when you allow for them being more complex than you think"?
- What have you to say about Nowak's [latest overview](#) of the question?

REPLY



Brian Josephson

May 28, 2012

As others have noted here, what matters is whether or not the maths is correct (and correctly applied), rather the number of people, who haven't gone into the maths, who disagree with Wilson. While I haven't studied the detail of this particular case, I do know from experience that (i) where the latter is applicable, intuition/gut feeling is inferior in its utility to mathematical analysis; (ii) as a result of the groupthink phenomenon, what all the experts consider to be the case can on occasion be a poor guide to the truth.

REPLY



Charles Goodnight

May 28, 2012

I can't seem to find it in the comments, but according to my email Anthony K said: "I'm not a biologist either, but Dawkins makes an inference here that I've never quite found convincing. I agree with him that "Evolution, then, results from the differential survival of genes in gene pools." I'm less clear on why he insists that we equate the gene pool with the individual organism."

My response: The big issue is additivity. Additivity is the world of "bean bag genetics". That is where each gene has its own incremental effect on the phenotype, and one that can be assigned to a gene independent of what ever else may be happening in that organism. Fisher was able to get away with this assumption because he assumed that populations were infinitely large and randomly interacting. (For those that know this means that the average effect = the average excess. If they don't, in Falconer's words, efforts to estimate genetic parameters are "doomed to failure"). Because of Fisher's success many, including Dawkins, and apparently several who contributed to this discussion, continue to assume, usually implicitly, that interactions can be ignored.

If interactions either don't exist or can be ignored then from a genetic perspective it makes little difference at what level selection is acting. It frankly does not matter if it is groups, organisms or genes that are living or dying the outcome is all the same. This is Dawkins world (Does that make him an additivistic individual?).

If the world is NOT additive, that is if genes interact, or more importantly if individuals interact, then the phenotype of individual X will be influenced by the genes in individual Y, and if there is nonrandom mixing and mating, those influences will be heritable. This opens a whole new can of worms. Now there is a qualitative difference between selection at the different levels. Genic selection gives an entirely different result than does individual selection or group selection.

A good example: In my thesis I selected for increased leaf area in interacting pots of plants. Interestingly when I selected for large leaf area the leaf area declined! Theory (if you care Griffing) says that this is exactly what you expect if the genes that increase leaf area of an individual do so at the expense of their neighbor. In other words by selecting for large leaf area I was getting mean plants. The plants put more

energy into competition, and the net result was a lowering of leaf area.

SO, yes, Dawkins can make the assertion that all selection is selection on genes, but he does so by ignoring reality. The reality he is ignoring is that there are genetically based interactions among individuals that qualitatively influence the response to selection. As a result there are qualitative differences between the response to selection at different levels.

Two aside points: (1) my plants responded very nicely to group selection. (2) what I was really seeing was what Harper calls the "constant yield law" This is almost ubiquitous in plants, studies have shown that it is a mix of individual and group selection, and it has almost certainly influenced evolution of many plants. One could easily argue that "group selection" or more correctly, multilevel selection is nearly ubiquitous.

[REPLY](#)



NBeale

May 28, 2012

I've now read [Rousset & Lion](#) They agree that, if Hamilton's Rule is to work, R is *not* relatedness and B and C are *not* the Benefit and Cost in the payoff matrix. But they claim that this is well understood in Inclusive Fitness literature (and they certainly can cite some papers which make some parts of this point). They then go off into a diatribe about "misrepresentation...straw man...calls into question the efficacy of the editorial process" (possibly related to the fact that they have never had a paper of theirs accepted by Nature?)

Now it's clear that if you redefine B,C and R suitably you can make Hamilton's Rule always true. But it then becomes nothing much to do with kin selection (indeed as far as I can see you can have a population of clones where $R=0$) and indeed as they basically admit the only way you can work out B, C and R is to do the whole evolutionary dynamics calculation in which case, as NTW point out, this "rule" becomes an accounting method.

But the wider question is how come their "true" version of Hamilton's Rule was not known to the Referees of Nature, let alone to NT&W? They admit that "the distinction between pay-off and fitness is often left implicit in expositions of inclusive fitness theory" and clearly NTW and the Referees believed that the "naive" version was widely held enough to be debunked.

So what I think everyone can agree on is:

1. Hamilton's Rule doesn't work reliably if R,B and C are the "normal" definitions of Relatedness, Benefit and Cost.
2. With modified R, B and C Hamilton's Rule can be made to work, but you have to do the Evolutionary Dynamics properly to find out what these values should be. Once you have done this, the problem is solved and the Rule adds nothing.
3. Nevertheless it can be a useful heuristic in many cases, provided it is recognised for what it is.
4. biological systems are more complex than you think, even when you allow for them being more complex than you think.

Is that fair?

[REPLY](#)



Goffredo Smeets

May 29, 2012

Doing biology is about what it means to be human. There are a lot of ways around it but no scientific shortcuts to avoid that epistemological fact. If I remember correctly, it was Warren McCulloch who said that one's claim to have no epistemology is bad epistemology.

Dawkins is very clear about his stance: humans serve a purpose, i.e. to give genes a ride. That's plain teleology (obsession with purpose and 'why') and I guess his fascination with the main competitor in the Purpose League, theism, is no coincidence. For various reasons I do not sympathise with the teleological project. As for Wilson, he's less clear about his stance but equally biased by a 'natural selection' reading & interpretation of data.

Michael Bentley in his reaction to Mark Cowan states "I was merely saying that natural 'preservation' is as much a metaphor as natural 'selection'. I said this because you suggested that the use of selection confuses 'agency with action, purpose from

process'. I agree, to some extent, but exactly the same is true if 'selection' is replaced by 'preservation'."

Well, no, they are not metaphors, they are epistemological 'first steps,' injunctions for how the collecting & reading of data is done. Replacing 'selection' by 'preservation' apparently forces researchers to describe evolution in terms of action & process (and not agency & purpose) of adaption. True, it opens a new can of worms but teleology will not be one of them – which is no little feat. And let's face it, it's time biologist hurry to get a better understanding of how nature work instead of why.

[REPLY](#)



NBeale

May 29, 2012

Jerry: re [Hughes & al.](#) Their paper shows that monogamy was (very probably) the ancestral state for all the 8 eusocial lineages they investigated. BUT AFAIK the way biologists infer whether extinct precursor species were monogamous is to look at their existing "descendants" and if the majority of the descendants are monogamous then the precursor is inferred to be monogamous. This therefore in fact tells us much less than the authors claim about whether monogamy drives eusociality or vice versa. Evidently none of the reviewers assigned by *Science* was a professional statistician.

[REPLY](#)



Mark Cowan

May 29, 2012

Goffredo, you are correct in the difference between 'preservation' and 'selection' being axiomatic. Science without the correct philosophical and theoretical foundations remains inadequate, even poor science.

I'll repeat the point because it will never go away. The entire focus of Darwin's first chapter of 'Origin' was about human 'artificial selection'. This requires foresight, creativity, design and conscious choice. From this process and opening chapter Darwin (by no means the first, but certainly the most influential) metaphorised an analogous process in nature 'natural selection'. The closest we can find to a eureka moment for Darwin was reading Malthus and its not too simplistic to state that what is referred to as 'natural selection' is 'Malthusian selection' in that there has to be a struggle in a finite setting with an unfolding process of winnowing.

Now, if through random mutation different species emerge over geological time and nature's loading capacity can accommodate this emerging abundant diversity of life, then that is not 'natural selection' but a consequence of a variety of processes. Variation, loading, the inertia of emerging life, buffer rather than efficiency, etc. Clearly what is referred to as 'natural selection' (Malthusian selection) does exist but this is one of several rhythms in/through nature and the trend from some evolutionary biologists to synonymise evolution AS natural selection, and vice versa, is a vulgar reading of what is going on in/through nature.

Culture is however a process of conscious choice and selection, although at times this involves unconscious selection, something Darwin referred to in his opening chapter of 'Origin'. Cultural Expression (not cultural evolution) is an extension from nature, different but not detached. Humans guide that direction and we can accurately talk of 'advance' and 'progress' in the social world of humankind in a way that we cannot in nature.

Darwin illuminated our understanding of A rhythm of nature and its mechanism, but its not the only rhythm or variable in understanding the diversity of life on earth. If we are to move towards a general theory of nature it would have to hold evolutionary theory, a Gaia-like systems understanding and I think that through Richard Lewontin's triple helix approach (which has differential equations as part of that approach) would be encouraging way to go. Part of that understanding might well involve a degree of kin-selection type thinking, group selection and other rhythms and patterns of action interacting rather than trying to find 'the holy grail' in intra-action, gene-centrism for example.

If we fail to move towards a more integrated understanding of what is really going on in nature then you can bet that we'll still be here 10, 20, even 30 years from now viewing broadly similar views being expressed by often polarised sides.

Selection is purposeful and quickens temporal sequence. Culture has become near exponential in pace and we need only reflect how much the world has changed in our own modest lifetimes. The difference between the world of selection (culture) and the

world of preservation (nature) "slow gradual degrees" is very different, and Michael Bentley doesn't seem to grasp that profound difference, which is one of deep causal engagement.

[REPLY](#)



Goffredo Smeets

May 29, 2012

Two remarks for Mark:

- human ways of knowing don't differ much from other animal's ways of knowing. The one thing that makes us stand out is that we know we know. More precisely: we know (to some extent) HOW we know. Therefore, advance and progress in culture, as you suggest, no. Expansion of domains of knowledge, yes. Beware of the teleological wolf.

- I do not intend to engage, on this forum, in a discussion about your suggestion that human culture (note that other species have their own culture) is a process of un/conscious choice and selection. Again, beware of the wolf. I'm prepared and willing to discuss the matter with you somewhere else. Here, I prefer to concentrate on what people have to add to Dawkins' assessment of Wilson's book.

[REPLY](#)



Mark Cowan

May 29, 2012

Goffredo,

Fair enough, another place. However, I have to correct a pretty major error in your thinking. The human engagement with the external world is to a telling degree of our own terms and conditions. Nature doesn't build cities, humans do. Even Richard Dawkins concedes that humans are the first/only intelligent designers on the tree of life. That is only possible because human level knowing is in the flexible state we call 'being'. To be a human being is to be a 'human believing'.

Beliefs are not in our biology. As humans extend from nature they create and design their own world. This new dimension of behaviour is culture and is quite different from everything that went before, including primordial and proto-culture. You say beware of the teleological wolf and I would more than caution you against 'clumping' vast diverse areas of behaviour under single terms: like 'culture' or the equally abused term 'tool-making'. We can say that animals make 'tools' through narrative alone, but they don't make compound tools. It is one level of analysis to realise that humans have created trillions of artifacts which woven together is the primary source of their, and our experience, and another to say that they can't stop making and creating. We can compare that to all animals and plants that don't engage in that level of foresight and creativity.

We can locate specific examples of rudimentary primordial and proto-culture, but culture (proper) is a definable process with laws of motion and is not understood from within our biology, but extending away from it. You can come back with opinion on this but the legal system is quite precise on this. We can hold humans to account for their actions, but we don't of animals and plants. I'm not making a value judgement there, I think humans have to take control of the control they have over the planet if we are to save a vast number of species endangered not by forces of nature, but by forces of culture.

You could dwell on the term artificial a little more. Art, articulation (7,000 languages), artisan, artist, artifact. Artificial means 'human-made' and 'non-natural' at the same time. If we take stable, biological forms in stable localised environments over geological time then we compare that with a single species that have created their own dimension of experience, can send humans into space and to moon. We can create planes and fly in the sky, and build technology that takes us to the depths of the ocean. That is only possible through the world of meaning and that is a level that no plant or animal engages in.

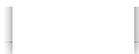
As you said, maybe this is for another forum, but the difference is so stark it bemuses me how it can be denied. Culture is different, but not detached from nature however. I'll end on that important point.

[REPLY](#)



Goffredo Smeets

May 29, 2012



Mgark, of your point. It makes me realise I didn't put my point clear enough: I understand cultural behavior to be the transgenerational stability of behavior that is ontogenetically acquired in any social environment. Obviously, humans stand out in this respect but are not 'hors categorie.' Cordiali saluti goffredofabbro@hotmail.it

[REPLY](#)



Guy Hoelzer

May 29, 2012

It periodically strikes me that the debate between advocates of kin selection and advocates of multilevel selection (myself included) is a dumb waste of time. Jerry Coyne hit the nail on the head when he wrote:

"In fact, as everyone knows (except, apparently, Nowak et al.), inclusive fitness is simply a natural extension of "regular" natural selection."

Indeed. Although Hamilton did not recognize it when he first published about kin selection, the notion of inclusive fitness implicitly evoked multilevel selection. Hamilton pointed out that selection acting on the individual level can reduce altruistic behavior because it reduces individual fitness, while at the same time an allele causing the altruistic behavior can selectively increase in frequency under a social structure that causes the altruism to be aimed at other individuals carrying the same allele. Kin selection IS a subset of multilevel selection, so ANY kind of support for kin selection is also support for multilevel selection. If somebody is uninterested in the world of multilevel selection outside of kin selection, then that is fine. There is no sense IMO for that individual to be defensive by attacking multilevel selection theory. If another person is only interested in aspects of multilevel selection that does not involve kin selection, then I don't see any reason for that person to attack the idea of kin selection. Kin selection is firmly embedded within multilevel selection and the two are entirely consistent.

[REPLY](#)

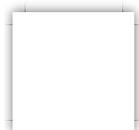


David Sloan Wilson

May 30, 2012

I have been following the conversation with interest and appreciate the input of several distinguished colleagues. I have just written an article based on Dawkins' review titled "Richard Dawkins, Edward O. Wilson, and the Consensus of the Many", which has been published on [EVOLUTION: THIS VIEW OF LIFE](#) (use link above). Dawkins and Wilson are both outliers who fail to realize that the days trying to pit group selection against kin selection are over. Almost everyone else knowledgeable on the subject appreciates the concept of equivalence and the ability to address the important issues using either framework, however much they might disagree on other points.

[REPLY](#)



Thomas Moore

May 30, 2012

Thanks for the spirited discussion. Too bad the principals did not join in, but they may have been put off by the insults and ad hominem attacks.

[REPLY](#)



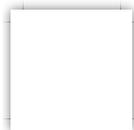
NBeale

May 30, 2012

Guy: The point is that "kin selection" implies that R (relatedness) is based ONLY on the genetic relatedness of the individuals. As everyone now acknowledges (and some "inclusive fitness" people had been making the point in papers published before NTW) this is not the case. Hamilton's Rule only works in general if R, B and C are NOT genetic relatedness and the Benefit and Cost in the standard Payoff Matrix.

What people like NTW are objecting to is the silly fundamentalist attitude of people like Dawkins who push the idea that kin selection and "selfish genes" are the ONLY things that drive evolution. This seemed plausible to some people in the 70s and 80s but it is now completely clear to almost all serious scientists that there is a lot more going on than this.

[REPLY](#)

**Mark Cowan**

May 30, 2012

Thomas Moore (good name)

It is not an insult to hold Wilson and Dawkins for their assertions on culture, especially if you take on board that culture is the dimension of behaviour after 'eusociality', the primary source of human of human experience and one that has troubled 10+ schools of evolutionary thought since Darwin.

I've contrasted Wilson's attempts at revising his approach with Dawkins' cut & paste 'memetics' from gene-centrism to explain culture. By any measure this was an ill-conceived move and lazy pseudo-science. Jerry Coyne doesn't rate memes and if you read p.112 of The Extended Phenotype where Richard highlights 4 reasons why memes can't be like genes you won't rate them either. Half way down that page Richard (almost writing in the third person) announces that he doesn't want to discuss them further because he hadn't done the reading on culture. Clearly.

To tell the reader to throw Wilson's book away is quite insulting, even arduous censorship. I am insulted that Richard's gene-centrism, a narrow reading of what is happening in nature still lingers on. I am insulted that biologists think that culture is a process that can be explained biologically when the evidence says something quite different. Trying to redefine culture as some kind of advanced eusociality so that you can then describe it in more comfortable evolutionary terms may be comforting and convenient, but that's not how you get to the truth.

[REPLY](#)**jrand**

May 30, 2012

"Genes programme the embryonic development of their vehicles... ." But only with a lot of help and hindrance from genes belonging to the owner of that vehicle manufacturing facility (the womb) and to all those around her, which in the human species would include kith and kin as well as friends and neighbors and hospital staff. The resulting organism is a product of all those influences, some of which may have a profound effect on its survival, and thus a product of the expressed genes of the entire group.

[REPLY](#)**Mark Cowan**

May 30, 2012

jrand

What about the genes of the 10,000s of patent holders who have contributed to the technology, medicines and surgical equipment available in hospitals?

Gene-centrism is a narrow, although (for some) a comforting view of nature. The evidence is pouring in now that Lamarckian processes (the inheritance of acquired characteristics) occurs through the social world of humankind, so to talk of genes 'causing' unfolding behaviour is less than half the story, and ignores:

1. that genes are the bookkeepers for these acquired characteristics, and
2. that structure and system can be stored in the external social world of humankind in/through trillions of artifacts all networked together. They don't dance to the tune of nature, but culture.

It's stunning in 2012 that such narrow gene-centrist views persist. I shouldn't really blame you as a person though, it's just your genes I guess.

[REPLY](#)**Jonathan Dore**

May 30, 2012

Mark Cowan: "The evidence is pouring in now that Lamarckian processes (the inheritance of acquired characteristics) occurs through the social world of humankind ...[such that]... genes are the bookkeepers for these acquired characteristics".

Thanks Mark, that's fascinating. Can you give us an example?

[REPLY](#)

James V. Kohl

May 31, 2012

Kohl, J.V. (2012) Human pheromones and food odors: epigenetic influences on the socioaffective nature of evolved behaviors. *Socioaffective Neuroscience & Psychology*, 2: 17338. <http://dx.doi.org/10.3402/snp.v2i0.17338>.

The article exemplifies with the honeybee model organism how ecological niches determine the social niches responsible for adaptive evolution. Simply put, nutrient chemicals determine the ecological niche and their metabolism to pheromones determines the social niche.

Nutrient chemicals and pheromones epigenetically calibrate intracellular signaling responsible for stochastic gene expression, which enable species diversity. The transgenerational epigenetic inheritance of effects of nutrient chemicals and pheromones control reproduction through the germ line. This is the model of the apparent design in biology that most of us can see.

For example, the visual appeal of food and conspecifics is epigenetically altered by experience with olfactory/pheromonal input that conditions the hormone response to what we see, hear, etc.

From the perspective of epigenetic effects and their transgenerational epigenetic inheritance there is no other model that incorporates the fact that both sufficient nutrition and a sufficient number of conspecifics are required for adaptive evolution. When Wilson or anyone else plays down the role of human pheromones, he might just as well play down the role of food odors, but that would be silly wouldn't it? Dispensing with human pheromones means dispensing with the Westermarck effect and many other aspects of self / non-self recognition, kin selection, and mate selection that are essential to survival of all species.

REPLY

Mark Cowan

May 31, 2012

Jonathan,

<http://cancerres.aacrjournals.org/content/72/9/2304.full.pdf+html>

This research article provides an insight into the field of epigenetics focussing on the environmental factors affecting DNA methylation, including smoking, alcohol and pollution.

When Stephen Jay Gould called culture "the Lamarckian juggernaut", he wasn't wrong. The trillions of artifacts that make up the primary source of human experience have been called 'exogenetic' and this is a comment on their influencing structure from the external. It's one thing culture influencing and shaping our brains through/as plasticity, and yet another when it affects gene function through the epigenome.

There is a complexity, even hypercomplexity that has to be understood here and we don't get there by overstating the causal weight of one variable (genes), no matter how important. When we do that we ignore the other levels of structure/system that need explanation.

REPLY

James V. Kohl

May 31, 2012

The cancer research merely addresses the adverse effects of epigenetic influences. What do you think about the more typical epigenetic effects I detailed in my article? "Nutrient chemicals and pheromones epigenetically calibrate intracellular signaling responsible for stochastic gene expression, which enable species diversity. The transgenerational epigenetic inheritance of effects of nutrient chemicals and pheromones control reproduction through the germ line. This is the model of the apparent design in biology that most of us can see."

Whatever adverse epigenetic effects pervert the design, endocrine disruptors for example, do so by altering the 'normal' epigenetic effects on intracellular signaling and gene activation of cells in the mammalian brain that secrete the hormone that links the epigenetic effects of food odors and pheromones to brain development, learning and memory, and behavior. That hormone is gonadotropin releasing

hormone, the biological core of mammalian reproduction. It regulates the estrogen linked to breast cancer, but also links many interactive neuronal systems to differences in endocrine up/down regulation of receptor content in sexually dimorphic tissues like breast tissues, and those in the brain that are responsible for sex differences in behavior.

Moving so quickly to cancer research leaves much of the current understanding of molecular biology out of the evolutionary construct.

[REPLY](#)



Mark Cowan

May 31, 2012

I feel like I have overstayed my presence here, but one last comment to Jason Mills. First of all thank you for acknowledging that culture is a special case. Despite this you seemed seduced by the cut & paste pseudo-theory of memetics as a theory of that 'special case'.

Daniel Dennett was also seduced because he wrote in his 1996 'Darwin's Dangerous Idea' that memetic evolution matched genetic evolution "quite exactly", which is absurd. I've already directed you to p.112 of The Extended Phenotype where Richard himself highlights 4 reasons why memes can never be thought as like genes. He goes on to say that he doesn't wish to stay on the matter of culture because he hadn't done the reading. It is no ad hominem to say you have that in common with Richard.

Culture is important, in fact it's more important than. We are the only 'terms and conditions' species on the tree of life that to a telling degree shapes our own lives, laws and direction. We can take Darwin as an example as someone who drilled down, did the work and sophisticated our understanding of nature or you can be inspired by Richard who cut & paste his gene-centric view of nature onto cultural phenomenon. At the end of the day that remains a lazy approach, and one we shouldn't revere.

Ideas shape human thought and collectively across generations so does ideology and human foresight (ideals). We can try and understand the mechanisms, laws of motion and even equation of those terms and conditions that shape human thought and behaviour (and I think that is possible) or you can advocate (as you do) some 'survival of the fittest' unknowledge that memetics is, if you are claiming that it is the basis for a theory of culture that would pull the social sciences, arts and humanities into a connecting, unified theory.

History says something quite different. Memetics is one of 10+ schools of thought that have tried to account for 'the special case' of culture and they have all failed. That is not conclusive on its own but the idea that culture and nature are seamless in their explanation through gene-centric and/or meme-centric approaches is a fiction. You're clearly not going to go off and read p.112 of The Extended Phenotype, I'm an educator so I'll help you:

1. "The copying process is probably much less precise than in the case of genes"
2. "Memes may partially blend with each other in a way that genes do not"
3. "New 'mutations' may be 'directed' rather than random with respect to evolutionary trends"
4. "The equivalent of Weismannism is less rigid for memes than for genes: there may be 'Lamarckian' causal arrows leading from phenotype to replicator, as well as the other way around"

So, memes are really like genes at all then. The only thing that 'holds' memetics as a theory together is narrative and evolutionary metaphor, the goal of a theory of culture is metanarrative, a far more demanding pursuit.

Richard finishes this section with a quite stunning admission:

"These differences may prove sufficient to render the analogy with genetic natural selection worthless or even positively misleading. My own feeling is that its main value may lie not so much in helping us understand human culture as in sharpening our perception of genetic natural selection. This is the only reason I am presumptuous enough to discuss it, for I do not know [a real spelling mistake] enough about the existing literature on human culture to make an authoritative contribution to it."

- a. worthless, or positively misleading
- b. it doesn't help us understand human culture

c. he hadn't done the reading

Yet for you Jason, it's all good. I don't wish you well in promoting such a view. Adiós.

[REPLY](#)



Dylan Kiewel

May 31, 2012

Are not genes to individuals as individuals are to their respective groups: replicators conveying the adaptive blueprint for the larger entity's parameters and quality of development into some given environmental space? Is not the environment itself in large part the dynamic aggregate of groups vying with groups, individuals with individuals, genes with genes, etc. and the throbbing recursive feedbacks among all of these hierarchies? Life is really so strange.

[REPLY](#)



Shaun Johnston

June 2, 2012

Richard says, "Kin selection is logically entailed by standard Darwinian theory." Does that mean failure of kin selection logically entails the failure of standard Darwinian theory? Take motherhood. Most mortality occurs prior to developing reproductive capability. Through the additional progeny a mother is likely to have she is more likely than her first born alone to pass on her genes. One would expect a mother to start risking her life to save her progeny only as they augment and her prospects of giving birth diminish. Yet mothers guard firstborns with as little concern for their own lives as they do later-borns. Here, if anywhere, kin selection has had time and opportunity enough to govern behavior, but it doesn't seem to.

[REPLY](#)



Peter Nonacs

June 3, 2012

I keep seeing the relationship of apparent ancestral monogamy for the eusocial Hymenoptera being touted as strong evidence for the importance of kin selection. Two points. First, if the majority of solitary Hymenoptera are monogamous (which may be advantageous for reasons that have nothing to do with being social), then this relationship carries no more weight than the observation that all eusocial Hymenoptera are haplodiploid. It may be important, but it also may just be a spurious coincidence. Second, 3 recent papers have modeled the effects of the number of matings for the evolution of cooperation, and have found that monogamy often is not intrinsically advantageous. Indeed, I found that polygamy often spreads cooperation faster through populations. See:

Nonacs, P. 2011. Monogamy and high relatedness do not preferentially favor the evolution of cooperation. *BMC EVOLUTIONARY BIOLOGY* 11: 58.
 Fromhage L. & Kokko H. 2011. Monogamy and haplodiploidy act in synergy to promote the evolution of eusociality. *NATURE COMMUNICATIONS* 2: 397.
 Leggett H. C. et al. 2012. Promiscuity and the evolution of cooperative breeding. *PROCEEDINGS OF THE ROYAL SOCIETY B* 279: 1405-1411.

[REPLY](#)



James V. Kohl

June 3, 2012

Eusocial Hymenoptera exemplify the respective roles of ecological niche and social niche construction in invertebrates. That's what allows for the adaptive evolution of a neurogenic niche in vertebrates. It's merely a continuation of the epigenetic effects of nutrient chemicals and pheromones on the neuroanatomy of the brain during its development, and on the plasticity required for adaptations to changes in the sensory environment.

Adult brain-based behavior is different than juvenile behavior due to changes in neuroanatomy driven by the availability of nutrients in an environment of conspecifics, as are changes in behavior of unicellular organisms with no eyes, ears, or brain. How can it not be clear that nutrient chemicals and the pheromones of conspecifics drive the adaptive evolution of eusociality across the species that exhibit it whether or not they are unicellular or multicellular? The molecular biology is the same for the required species-specific behaviors.

It's great to see you cite your own work, as I like to do.

Kohl, J.V. (2012) Human pheromones and food odors: epigenetic influences on the socioaffective nature of evolved behaviors. *Socioaffective Neuroscience & Psychology*, 2: 17338 open access at <http://dx.doi.org/10.3402/snp.v2i0.17338>

I also think it is long past time to discuss the basic principles of biology and levels of biological organization required to link sensory cause directly to behavioral affects in the context of model organisms and their common molecular biology. By doing so, we could avoid the leap of faith required in EO Wilson's jump from eusociality in honeybees to eusociality in humans. At the same time, we could drop Dawkin's leap from no faith in the apparent design of biology that allows him to make genes responsible for everything sans the effects of sensory input from the environment that cause changes in intracellular signaling and stochastic gene expression.

What does a gene do in a cell that lacks sufficient nutrient chemicals? That's a rhetorical question. No matter how selfish the gene is, the cell adapts or dies just as any organism that lacks sufficient nutrition does. What does a species do that lacks sufficient nutrient chemicals to support its conspecifics in an ecologically dependent social niche? That's another rhetorical question, for impact.

The species adapts or dies. Given these biological facts about the life and death of organisms and their species survival it should be easier for others to conclude, as I have, that olfaction and odor receptors provide a clear evolutionary trail that can be followed from unicellular organisms to insects to humans whether or not a particular species is judged to be eusocial via any theoretical approach. When are the biologists who have stayed current on advances in their disciplines going to start conveying their biological facts to evolutionary theorists? Or shall we accept the fact that old dogs cannot learn anything new, whether or not it's a trick?

REPLY



James V. Kohl

June 3, 2012

I was happy to find Nonacs 2011 is open access at <http://www.biomedcentral.com/1471-2148/11/58>, and recommend that others here at least scan it. He makes a clear statement of the importance of kin selection throughout evolutionary history.

We can't all just take a flying leap to any approach that downplays its importance. I, for one, can agree with certainty that models of adaptive evolution must aim for a better understanding of the evolutionary correlates of kin selection and selection for genetic diversity. But I don't necessarily agree that these are mere correlates.

The adaptive evolution of self / non-self recognition enables every organism's ability to respond to the difference between nutrient chemicals and the pheromones of conspecifics. It's adaptive to eat the genes of a heterospecific; it's not adaptive to eat a conspecific.

What's worse than eating a conspecific is to eat close kin, and even microbes are unlikely to do that. Thus, instead of correlates we have two sides of the same biological approach to adaptive evolution. Learn to recognize the difference between a nutrient and a conspecific (via their chemical properties and receptor-mediated epigenetic effects on stochastic gene expression) and share the nutrients with kin, but don't choose them for mates. Their "chemistry" makes them unappealing because it's too similar to your own. And pheromone-dependent mating is as important to adaptive evolution as is the ingestion of nutrient chemicals to genetic similarity and genetic diversity.

We can eat the same foods as other genetically similar animals, which we may also eat. But we're not going to mate with them because species-specific differences in pheromones standardize and control nutrient-dependent (i.e., calibrated) reproduction.

REPLY



Mitsu Hadeishi

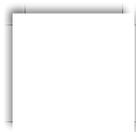
June 4, 2012

I find it odd that this articles focuses almost entirely on attempts to think about individual vs group selection at the level of concepts primarily related to trying to think

of mechanisms for group selection building from the bottom-up, so to speak. But phenotype, as we've come to learn, has an extremely complex and distant relationship with individual genes for most things (look at the results of genome-wide association studies for diseases — they're finding that most diseases and conditions can only be understood in terms of very small contributions from individual genes). Behavior and phenotype are so many levels above the level of genes, it's rather odd to focus nearly entirely on bottom-up descriptions of natural selection.

In the case of humans, consider the simple case of two tribes: the more organized, cooperating tribe is obviously going to have a selection advantage over the more disorganized one. Militarily, agriculturally, and so on. More territory, more access to resources, etc.

[REPLY](#)

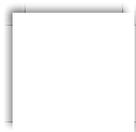


EvoTheoryHasHoles

June 10, 2012

Evolution is impossible. And this is proof. Scientists can't decide on anything. All of you are wasting time. Time for you to turn to God. Neither group selection or nor kin selection is true, nor do we need to know if they are true or not. What matters is God. Evolutionary theory and the study of it has no practical application in real world. Why not stop wasting time and instead focus on what's important: serving mankind, fixing their problems and obeying God's will?

[REPLY](#)



NBeale

June 10, 2012

Incidentally, Wilson's book got a [very positive review](#) in Nature, though the reviewer did suggest that Wilson was a bit over-dismissive about inclusive fitness ("Albert Einstein, after all, didn't disparage the numerous physics experiments showing that Isaac Newton's simple formulae work remarkably well under specific conditions")

[REPLY](#)



Jerry Coyne

June 10, 2012

Incidentally, Wilson's book was ripped to pieces by Steve Mithen in the June 5th New York Review of Books, which faulted not only his neglect of the many criticisms of his views on group selection, but also for elementary errors about human cultural evolution. Apparently Wilson didn't even cite in his book the 130+ scientists who disagreed with his dismissal on kin selection, though there book was published late enough to have done so.

A quote from Mithen's review:

"So how marvelous it felt a few months ago to have received an email from [Wilson] asking if he could reproduce a diagram relating to human evolution from one of my own publications in his forthcoming book, The Social Conquest of Earth. What an honor to then be invited to review the book for The New York Review. And how awfully disappointed I have been."

[REPLY](#)



Brian Josephson

June 10, 2012

130+ geologists thought Wegener was wrong about continental drift. And

130+ French scientists dismissed the idea that meteorites fell from the sky. Dismissing something requires no effort at all!

[REPLY](#)



NBeale

June 10, 2012

Basically: people with a limited mathematical background who write books

rather than publish peer-reviewed scientific papers don't think much of NTW's ideas. Ah well.

As Jerry will know it's amazing how far in advance a book text has to be "frozen" and I don't know whether EO W could have cited the 130+ objectors. But as Brian points out they are rather beside the point. Besides, have you looked at the "predictions of kin selection" that the cite. They are incredibly feeble compared with the predictions made by proper rigorous work on Evolutionary Dynamics (I'm just doing a blog post on this, give me a few hours).

REPLY

Jerry Coyne

June 10, 2012

What a fatuous remark! Did you even READ the comments? It did require effort, both mathematical and biological, to "dismiss" Nowak et al. Those comments were reviewed, you know.

And yes, sometimes the consensus of scientific opinion is wrong, but more often than not the lone person who stands against that consensus is not ultimately vindicated, but remains a crank. You didn't mention all the people who criticized cold fusion, the idea that AIDS isn't caused by a virus, etc. etc. etc.

REPLY

Brian Josephson

June 10, 2012

Ha! Your bringing up cold fusion exactly proves my point — take a look at Edmund Storms' lecture at <http://youtu.be/3oHXy1knGrM> to educate yourself on the subject. What happened in that case, once again, is that people thought they had conclusive arguments to disprove the phenomenon but they all, rather transparently, contained hidden assumptions. The experimental evidence in favour of cold fusion being real is pretty well conclusive — look in the Library at <http://lenr.org> to find hundreds of papers observing the effect.

REPLY

NBeale

June 10, 2012

The great [John Bardeen](#) "dismissed" Brian's work but Brian still won a Nobel Prize for it. When you make a contribution to science at least 10% as significant and Brian's you might be in a better position to pontificate about his remarks. The review process for "Correspondence Arising" is quite different from that for a primary paper anyway.

If [this](#) is one of the best examples of a quantitative prediction of inclusive fitness, then I think EO Wilson's comments about little value have a lot of merit.

REPLY

Brian Josephson

June 10, 2012

Your bringing up cold fusion exactly proves my point. If you go YouTube and search for 'Edmund Storms cold fusion lecture' you'll find the actual facts, as opposed to the fictionalised science generated by the 1300+ people whose thinking is dominated by inapplicable models, with invalid assumptions built in. The reality of cold fusion has been demonstrated by the numerous careful experiments, blocked by journals such as Nature and Science but published in other creditable journals, many of which papers you can read in the library you get to by searching on 'LENR library'.

(PS: earlier I posted a comment including links, but it got intercepted by the system because of the links. It might get approved by the moderator tomorrow)

REPLY

Brian Josephson

June 11, 2012

The links are:

<http://youtu.be/3oHXy1knGrM> (Edmund Storms' lecture on cold fusion),
<http://lenr-canr.org> (Site including a library with 100s of papers on cold fusion)

[note to moderator: no need to approve yesterday's posting held for moderation as a link-free version of the same is already there]

[REPLY](#)



NBeale

June 10, 2012

Jerry: the great John Bardeen "dismissed" Brian's work, but Brian still won a Nobel Prize for it.

Can you find examples of a paper published in Nature or Science by two scientists of stature comparable to Wilson and Nowak that has subsequently been found to be completely wrong? None spring to my mind...

[REPLY](#)



Jerry Coyne

June 10, 2012

Don't bother with the post, Mr. Beale, at least not for my benefit. I stopped reading your website (judging by the lack of comments, apparently nobody else does, either) when you made the ridiculous claim that Nowak has more Science, Nature, and PNAS papers than those 130 authors combined. That's simply wrong and you know it. I have at least seven to my own name. And to dismiss dozens of biologists as all having "limited mathematical background," as you have dismissed Stuart West and Alan Grafen, is simply churlish and unfair. So what if they don't have a PhD in math. Do you? If not, why do you consider yourself qualified to judge the math.

[REPLY](#)



NBeale

June 10, 2012

Jerry: Try to stick to the scientific issues. FWIW I read Maths at Trinity Cambridge and have an Erdos Number of 4 (What's yours?)

I know you have about 10% of Nowak's number of publications in Nature/Science/PNAS but you are one of the more published of the 130. I said "Nowak probably has more papers in Nature, Science and PNAS than all of them put together and he's much younger than most of them." Feel free to count the other 129 and share the results – it's a minor point.

If we try to stick to the science, can you find any examples that I requested? Or indeed (more to the point) do you agree with the 4 points I posted earlier, because I honestly don't think they should be controversial:

1. Hamilton's Rule doesn't work reliably if R,B and C are the "normal" definitions of Relatedness, Benefit and Cost.
2. With modified R, B and C Hamilton's Rule can be made to work, but you have to the Evolutionary Dynamics properly to find out what these values should be. Once you have done this, the problem is solved and the Rule adds nothing.
3. Nevertheless it can be a useful heuristic in many cases, provided it is recognised for what it is.
4. biological systems are more complex than you think, even when you allow for them being more complex than you think.

[REPLY](#)



Brian Josephson

June 11, 2012

The weak point in the original article, it seems to me, is the suggestion (reminiscent of a famous assertion by one Margaret Thatcher) that, normally, "there's no such thing as group phenotype". It seems to me, speaking naively as a physicist, that interactions between members of a group should be sufficient for collective properties (such as, in the case of physical systems, magnetism, crystal structure, superconductivity) to emerge. Perhaps biological groups are essentially different from physical ones. If so, it would be interesting to know why.

[REPLY](#)



**Brian Josephson**

June 12, 2012

Phenotypes such as 'disbelief in cold fusion' and 'disbelief in telepathy' provide excellent illustrations of the group selection process. These are collective group properties, transmitted by a range of methods. Individuals with different beliefs, conflicting phenotypes, are liable to be excluded from the group by various methods, e.g. loss of job (see the video 'expelled-no intelligence allowed' for examples), or being denied funding — that is group selection at work.

[REPLY](#)**JHannunen**

June 18, 2012

David Sloan Wilson wrote a piece about Dawkins vs. Eo Wilson in Huff Post (or one of their blogs) that may be of interest for people reading this article.

http://www.huffingtonpost.com/david-sloan-wilson/richard-dawkins-edward-o-b_1588510.html

[REPLY](#)**Red Raleigh**

June 24, 2012

That's precisely how I got here, through a process of natural selection. 😊

[REPLY](#)**Richard Evans**

June 24, 2012

"Scientists dislike arguing from authority, so perhaps I shouldn't have mentioned the 140 dissenting authorities"

Yet he did, and you also stated you wouldn't criticise a scientist unless others have done so, which I find totally perverse and anti-scientific.

[REPLY](#)**Richard Evans**

June 24, 2012

"It therefore makes a long-term difference which genes are good at surviving and which ones bad. You cannot say the same of individual organisms (they die after passing on their genes and never make copies of themselves). Nor does it apply to groups or species or ecosystems. None make copies of themselves. None are replicators. Genes have that unique status."

Surely genes do not "work" alone? It is the collection of genes in an organism that makes it successful. And without the organism the collective makes, genes are nothing.

[REPLY](#)**Brenn Campbell**

June 26, 2012

Stephen Pinker clearly does not agree with group selection and is firmly in the Dawkins camp.

<http://edge.org/conversation/the-false-allure-of-group-selection>

Criticizing Dawkins for his rather weak theory of mimetics is unreasonable; Dawkins clearly points out that his theory is little more than an analogy to genetic evolution. Different populations/groups of a species will adapt to their respective different surroundings. If this adaptation is behavioural, it could be referred to as cultural evolution. If the change becomes genetically coded for, it is simply a stage in speciation.

Feral parrots in New York City (as mentioned in a previous post) may now nest in groups to keep warm, which they didn't do in their homeland. This is indeed a behavioural change (presumably like some species of weaver birds who nest in large colonies for protection from predators). And this may become a permanent change if that particular parrot population continues to live in such a cold climate.

It is a behavioural adaptation and could be described as cultural evolution, in that the parrots' 'culture' has changed and that it persists over generations. But does that

change have a genetic basis? It may well do over time. Birds who remain steadfastly independent at breeding and don't join the group at nesting time will die of cold. If their persistent independence is because of genetically based timidity/aggression/independence/, the genes for these traits will consequently disappear from the population.

Genes that lead to cooperative group nesting will be selected for and it is through genes that the change in group behaviour change will take place. (If after a period of time this group of parrots was rounded up and released back into their original habitat, the communal nesting behavior may well persist, but could conceivably drift back to individual nesting, as communal nesting would confer no particular advantage; a move back to individual nesting could occur regardless of whether the behaviour of group nesting had become genetically coded for or not).

Should another group of pet parrots escape to the wilds of New York, they wouldn't have this cooperative nesting behavior, and the first group would have an advantage over the second, and be more successful than the second. This could be interpreted as group selection, but really the selection is still taking place at the level of the genes.

[REPLY](#)



Tom Dobrzeński

June 30, 2012

Enjoy dissenting with Dr. Dawkins on THIS website.

Try it on HIS website and he will likely censor you.

[REPLY](#)



nicolaas stempels

July 8, 2012

This is a big thing when 2 of one's 'heros' clash. Dawkins changed the way I view the world, and Ed was always the kind of guy I wanted to be.

First I'd like to point out that mathematics in biology is as good as the model it works upon. Mathematics is an instrument which can help us understand and sometimes discover things we don't yet know. As are models. But never they can replace that what is observed, models make assumptions that are rarely precisely replicating reality. That is their exact reason for being and at the same time the reason to not take them as "Gospel Truth" 😊. So whether eg. Grafen or Nowak are good at maths (certainly better than me) is not really relevant (well it would be if they were bad at it, which they clearly are not).

Group selection, or multilevel selection MLS, is still a contentious issue. Dawkins chip with group selection is with the kind forwarded by the likes of Lorenz or WE: the good of the species etc. One of Dawkins' achievements is to show that that cannot possibly work. Cheaters will always chip in. I think there is no serious doubt about that anymore.

Does that mean that Dawkins does not accept group selection? Of course not. He may not acknowledge that it is group selection (but I doubt he wouldn't), but genes in an eukaryotic cell -and even prokaryotic cell- work in groups. Dawkins even wrote he could rename his 'Selfish Gene' the 'Cooperative Gene' without changing one iota of the text. And cells in multicellulars work as groups: individuals, vehicles, to which selection (or preservation- i think the distinction is a useless semantic exercise) applies. And eusocial insect societies may even be considered a kind of super-organism. All groups, and selection.

Hamilton's 'inclusive fitness' was of course the first step taken (note that the haplo-diploidy of some social insects is unnecessary, in fact a diversion, for his theory). After he pointed it out, it appears so self evident that it does not really needs any further mathematical models to prove it. Nearly a tautology. Moreover, it is shown to work eg. Alexander's prediction of the naked mole rats.

Of course we later got reciprocal altruism (eg Trivers) and 'reputation' (eg Alexander), but I'd like to point out that within a species -or even outside- we share a lot of genes. Within a species close to 99%. Can inclusive fitness not be extrapolated? If -big if- that could be, group selection (in the narrower sense) need not be contradictory to the gene level. Am I just trying to reconcile my heroes?

David Sloane Wilson (another Wilson, another 'hero') made quite a strong case for group selection in his highly recommended "Darwin's Cathedral", where he goes deeply into Calvin's Geneva religious community. However, I think he fails on 2

points: 1 he fails to show that it cannot be explained more parsimoniously by direct genetic interests, and 2 more importantly, fails to get into memetics. Nevertheless, strict religious rules may possibly defeat 'cheaters'.

Back to brass tacks. The greatest differences within species is the ability to withstand parasites (disease). Not something generally clearly advertised, gleaming peacock feathers notwithstanding.

The most common gene in our genome is for reverse transcriptase, a viral parasite, and most of our genome (LINE's and SINE's) are of retroviral origin. The initiator gene for forming the placenta (trophoblast) is retroviral and probably the ones for forming the ocular lens too. So the question remains (Dawkins would love this) 'whose genes'? Parasites!

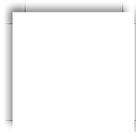
And most of the other genes effects is to switch on or off other genes, initially generally influenced by environment, and environment often is other genes. A complexity of which we are only scratching the surface.

All this leads us far from group selection, but it illustrates that our models are not really reflecting reality, and should be taken with a pinch of salt.

I'd like particularly to exonerate Dawkins with his Memes. He proposed it as a comparison with replicators that might not be vertically Darwinian (and acknowledged Darwin's priority with words in a language). However, his memetics was quite fruitful. It even gave us a good (immo the best we have) hypothesis of our hypertrophic human brains (Cf Susan Blackmore's "The Meme Machine")

There is a lot we do know (such as inclusive fitness, natural selection, symbiosis) and a lot we do not yet know (eg circumstances where group selection might work, memetics, the real work of parasites, evodevo, symbiosis). Not Dawkins and not the Wilsons, as all of them would undoubtedly admit. But I'd still want to hear what they have to say.

[REPLY](#)



sean lydon

July 10, 2012

You've lost me

[REPLY](#)



Robert Gibbs

August 21, 2012

'Group selection would imply that a group does something equivalent to surviving or dying, something equivalent to reproducing itself, and that it has something you could call a group phenotype, such that genes might influence its development, and hence their own survival.'

Not bring a biologist or a mathematician,, most of this is over my head. But it seems to me that the problem with honeybees is that a lot of them are dying out as groups?

And I read that Japanese honeybees have evolved a group behaviour against one of their main threats by smothering hornets. Is that cultural or Lamarckian?

[REPLY](#)



Mark Van Cleve

August 28, 2012

Huh. A strong group is going to make better decisions, manage its affairs better, be more resilient to threats. Means more reproduction and better chance of infant survival in that group. So, if your group communicates well and includes some warriors, some hunters, and some gatherers, etc., you're more likely to reproduce, even if you possess none of those skills yourself. If you accept that, then it makes sense for a species to produce disparate individuals in a social species, even if they wouldn't do so well as individuals. Right? A species with only one kind of individual will be out-competed in terms of reproduction.

[REPLY](#)



James V. Kohl

August 28, 2012

Mark,

Your grasp of group dynamics seems to exceed that of most others. It is also exemplified in eusocial organisms like the honeybee where what the queen eats determines her pheromone production and everything about the interactions among colony members including the neuroanatomy of the worker bee's brains. The ability of the queen bee's nutrient chemical-dependent pheromones to epigenetically alter brain development in different castes can also be traced to similarities among ant species and their ability to recognize the difference between a conspecific and a heterospecific that elicits attack behavior. That ability and more is what I detailed using the honeybee model organism in a recent published paper in *Socioaffective Neuroscience & Psychology* — open access at <http://dx.doi.org/10.3402/snp.v2i0.17338>.

I have also elaborated on that model in a series of blog posts at my domain. As I believe I mentioned earlier in this thread, once Wilson decided to not have human pheromones be involved in anything, he disconnected adaptive evolution via ecological, social, neurogenic, and socio-cognitive niche construction from its origins. He missed the fact that "Olfaction and odor receptors provide a clear evolutionary trail that can be followed from unicellular organisms to insects to humans." — as detailed in Kohl (2012).

[REPLY](#)



Tim Beardsley

November 26, 2012

Herbert Gintis provides a strong defence of the essence of Wilson's book—and a generalization of Hamilton's famous rule about the conditions for the evolution of altruism—here:

<http://www.jstor.org/stable/full/10.1525/bio.2012.62.11.8>

[REPLY](#)



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Author

Richard Dawkins



Richard Dawkins is an evolutionary biologist and author of 11 books, including "The God Delusion"

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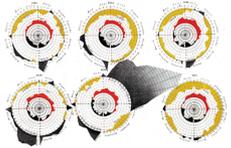
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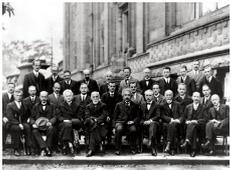
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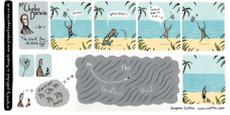
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