Re: Haldane's Dilemma

Source: http://sci.tech−archive.net/Archive/sci.bio.evolution/2005−02/0203.html

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From: John Edser (*edser_at_tpg.com.au*)
Date: 02/06/05

Date: Sun, 6 Feb 2005 01:12:25 −0500 (EST)

"Perplexed in Peoria" <jimmenegay@sbcglobal.net>

> > JE:−
> > Hoelzer has stated that all models
> > are testable but Felsenstein has stated that
> > none of them are. Hoelzer has junked Popper
> > but I have no idea if Felsenstein has.
> > O'Hara has agreed that the critical diagnostic
> > sign of c does remain arbitrary within Hamilton's
> > Rule but Hoelzer has not answered and neither
> > has Felsenstein. Hoelzer has commented that b/c
> > is a constant but nobody else here except
> > myself will even comment.

> JM:−
> I believe that you are misinterpreting O'Hara.
> He didn't use the word "arbitrary". That is your
> word, for your interpretation of what he said.

JE:−
I said I would not keep repeating Google requotes because it appears no matter how often you demonstrate what somebody previously wrote they simply refuse to acknowledge it. I have already posted the quotes O'Hara requested me to repost for him re: what O'Hara had previously agreed. I took time and trouble to compile them but O'Hara never even responded after they were reposted (for what I consider to be very obvious reasons).

Here is the compilation that I sent in March 2004 again:−

_____Quote March 2004_____
Newsgroups: sci.bio.evolution
Date: 2004−03−18 08:53:11 PST

Below is the key exchange sent Thu 22/01/2004
that set up all subsequent discussion:

−−−−−−−−−−−−−−quote −−−−−−−−−−−−−−−−−−−−−−−−

Thu 22/01/2004:

JE:−
What is the difference between
a reduced positive c and a negative c?
If c was an abolute measure of fitness
then yes, a real difference exists. However
c is only a relative fitness cost and not
an absolute fitness cost, so what is the
difference?

BOH:−
As far as the rule is concerned, none.

−−−−−−−−−−−−−−end quote −−−−−−−−−−−−−−−−−−−−−−

Here is a summery of what BOH has agreed to
within subsequent discussion:

−−−−−−−−−−−−−− start summery −−−−−−−−−−−−−−−−−−
sent: Thu 12/02/2004 11:42 AM

JE:−
If −c is mutualism and +c is altruism
but c is arbitrary within the rule then
the rule cannot discriminate between them.

BOH:−
Yes, I totally agree.

sent: Sat 14/02/2004

JE:−
Note that OFM is *NOT* outside of the rule
because Hamilton included OFM within the
rule as any condition of the rule where c is
negative. Because the sign if c remains arbitrary
within Hamilton’s rule it cannot discriminate
between OFA and OFM, period.

BOH:−
I totally agree.
My point is that Hamilton's rule was not intended as a rule to
discriminate between altruism and mutualism.

Sent: Thu 19/02/2004:
JE:−
Do you also agree that Hamilton's rule
was employed to suggest that OFA could
exist within nature,

BOH:−
Yes.

JE:−
It was the rule that was being proffered
to support OFA when group selection
failed to do so!

BOH:−
Indeed.

Sent: Tue 24/02/2004:
JE:−
Do you agree that Hamilton's rule only
measures differences in relative fitnesses?

BOH:−
Yes.

JE:−
If −c is mutualism and +c is altruism
but c is arbitrary within the rule then
the rule cannot discriminate between them.

BOH:−
Yes, I totally agree.

JE:−
Do you agree that the
sign of c remains arbitrary
within the rule?

BOH:−
Yes.

JE:−
..you agree that it is possible for the altruistic
gene to relatively spread as the absolute fitness of both
genes, i.e. Hamilton's hypothetical altruistic genes and the
wildtype non altruistic gene it is contesting becomes
absolutely reduced?

BOH:−
Yes.

JE:−
All sterile forms come from non sterile
forms and not vice versa. The effect sterile
forms can have is only selectable at the fertile
level of selection and not at a sterile
level of selection. Do you agree or disagree?

BOH:−
Yes, I agree.

Sun 29/02/2004:
BOH:−
..
Hamilton's rule can't be used to
separate out mutualism and altruism
at all.
_____end Quote March 2004_____

I strongly suggest you take up these
points of agreement with O'Hara himself.

> JM:−
> And I believe that you are misquoting Hoelzer on
> whether b/c is a constant.

JE:−
Here is what Hoelzer wrote in answer to you:

−−−−−−−−−−−−−−−−−− quote−−−−−−−−−−−−−−−−−−
From: Guy Hoelzer (hoelzer@unr.edu)
Subject: Re: Perpetually Perplexed
View: Complete Thread (70 articles)
Original Format
Newsgroups: sci.bio.evolution
Date: 2005−01−27 13:57:04 PST

>snip<
My position is that Hamilton assumed b/c is constant,
and that his Rule still holds for a varying b/c ration
as long as the behavior is perfectly graded in its
association with r.
>snip<

−−−−−−−−−−−−−−−−−−−−−−−−end quote−−−−−−−−−−

In future please do you own Google research.

> JM:−
> Your other points of disagreement are disagreements
> over epistemology, not disagreements over
> Hamilton.

JE:−
Incorrect. I do have epistemological
disagreements with many others here
which I consider must be settled before
CRITICAL disagreements of scientific validity
(not just mathematical validity) can
be settled.

> JM:−
> And I have to admire your persistence
> in getting people to even talk about epistemology.
> It is not too surprising that you can find disagreements
> on this subject if you dig. It is, as you would be
> the first to point out, not a subject that these
> people spend a lot of time thinking about.

JE:−
If a scientist cannot tell you what science
is it proves he is a poor scientist.

> > JE:−
> > One of the reasons so much disagreement exists
> > is that IBD relatedness used as a fitness criterion
> > by Hamilton was a mean probability. NAS stated that
> > a relatedness probability represented a _misuse_.

> JM:−
> Somehow, I suspect another misinterpretation. But
> I didn't witness this one, so I should probably keep
> my suspicions to myself.

JE:_
I suggest you ask NAS.

> > JE:−
> > To this end he eliminated it as probability by
> > just substituting a comparison to a mean population
> > relatedness. This eliminated the probability but
> > not the mean. You cannot employ means as valid
> > _actual_ fitnesses for obvious reasons.

> JM:−
> Not obvious to me.

JE:−
One selective event requires
a comparison of at least two
actual individual fitnesses
and not a comparison of
any two mean fitness.

> > JE:−
> > So the issue of relatedness
> > employed a valid fitness criterion remains
> > unresolved. Also, all empirical
> > (fitnesses documented within nature) genomic
> > gene fitness remain _non_ lineal (epistatic)
> > but all epistasis remains deleted from the
> > rule as a simplification.

> JM:−
> There is nothing special about the rule, in this
> regard. Epistasis remains deleted (if that is
> the right way to put it) throughout most of
> population genetics. And, as long as we are
> talking about the kin−selection part of Hamilton,
> I don't see that Hamilton is particularly
> sensitive to this deletion. If the "deletion of
> epistasis" is a mistake, then a lot more collapses
> besides the rule.

JE:−
Exactly. This is why Felsenstein et al
(O'Hara, Moran, NAS etc) remain evasive. There
is a lot more at stake than Hamilton's Rule.
I singled out the rule because it provided the
best example.

> > This means no
> > gene fitness can vere be real within Hamilton's
> > Rule but the cost c is real.

> JM:−
> John, there are no gene fitnesses in rb>c. There are
> only organism fitnesses there. Some derivations of
> the rule use gene fitnesses, but not all of them.

JE:−
Incorrect. "Allele" and "organism" are equal
terms within Hamilton's oversimplified model.

When Hamilton mathematically deleted e
from r^e by just fixing e=1 he only created
just the ONE massively oversimplified model;

Hamilton's Single Biological Model:
One entire organism that is comprised of
just one locus with two alleles.

Since sex is a shared
situation one allele now exactly equals one

entire parental organism genome because just
one allele inherited from each parent is
defined to be one entire genome. Hamilton's
massively oversimplified model allows one
allele to exactly equal one Darwinian organism.
Because the terms "allele" and "organism"
remain entirely interchangeable within Hamilton's
model he can mathematically delete _either_
of them at will and then switch back again,
when required. This works
fine, if and only if e=1 in NATURE.
It DOESN'T. Felsenstein will again suggest
the mathematics still works fine and that is all
that matters. The mechanic is still checking
the engine when the tail shaft lies on the
road in two pieces....

> > JE:−
> > The only agreement that appears to exist is
> > that rb>c represents valid mathematics. I
> > have agreed it is valid mathematics. I have
> > also agreed that one allele can relatively
> > (but not absolutely) be measured to increase
> > compared to another within a 100% heuristic
> > model genome comprised of just one locus with
> > two alleles via the rule. I also agree that
> > Felsenstein can mathematically derive Hamilton's
> > Rule from Classical Group selection assumptions in
> > which one group is stated to be selected over
> > another if it is larger than another
> > because it is expanding at a faster rate.
> > My point is that classical group selection
> > can be proven to be Darwinian selection
> > operating at just the ONE fertile organism
> > level of selection so that Felsenstein's
> > mathematical derivation only proves one
> > thing: Hamilton's Rule is a simplification
> > of Classical Darwinian selection.
> >
> > What I "have been talking about for the past
> > four years" concerns the _scientific_ validity
> > of Hamilton's Rule. To be a scientific
> > proposition the rule must provide a cause
> > and effect argument. Hamilton et al DO provide
> > one. At an _independent_ genomic gene level
> > where all genes now have a lineal fitness
> > so all of them compete against each other
> > and do not anymore only cooperate to maximise
> > one parental total Darwinian Fitness at just
> > the one fertile organism level of selection.
> > Only this rational can allow organism fitness

> > altruism to now be forced at the one Darwinian
> > level via selfish geneism. The mathematics can only
> > represent and not replace, this cause and effect
> > argument provided by Hamilton et al. However, the
> > reverse is the case. The mathematics has been allowed
> > to replace its scientific rational where such an
> > event remains scientifically invalid.

> JM:−
> Now, the above comes close to making sense. It does make
> sense, but it contains a mistake. The mistake is the
> implicit claim that no other argument for Hamilton
> (besides the gene−level one) is a cause and effect
> argument.

JE:−
WHY did you fail to include your
alleged but _entirely missing_ rational
that was only alleged to provide fitness
altruism at the Darwinian level of selection
without invoking selfish geneism? What gives
you the right to dam Edser with faint praise
by accusing him of being correct while making
some entirely unsubstantiated error?

> > JE:−
> > I question the scientific validity of the argument of
> > Hamilton et al and not the mathematical validity
> > of Hamilton's Rule. Felsenstein et al refuse to address
> > any problem concerning the _scientific_ validity of the
> > rule. When questioned they only reply that the rule
> > remains _mathematically_ valid. This only constitutes
> > an evasion of the question.
> >
> > Haldane's Dilemma suffered from the same
> > problem: an inability to examine the scientific
> > validity of the argument that only provided
> > a totally false dilemma. Only the empirical
> > evidence forced the dilemma to be "solved"
> > i.e. hidden away as if it never happened. Even
> > today the question of the scientific validity of
> > the argument that provided this false dilemma
> > remains ignored, i.e. lessons for science as
> > to why a false dilemma was produced in the
> > first place (which is the only scientific
> > value the dilemma has) remains ignored.

> JM:−
> If the assumption is that Haldane analyzed the situation
> wrong in 1957, and if the question is "why did he get it
> wrong?", then I think the answer is obvious. He got it

> wrong because it is a fiendishly difficult problem to
> come to terms with, and people rarely get such problems
> right on the first attempt. I'll bet that even ReMine
> would agree with this.

JE:−
The above is nonsense. Can I take it that
we agree that Haldane got it wrong? Would
it not be utterly imprudent if we did not
firstly examine Haldane's basic premises
which _just might_ provide a reason as to
WHY he got it wrong BEFORE we rush to the
very convenient white wash that it was all
just "too hard" so nobody was really to
blame?

Yes or No?

> JM:−
> If the question is why Haldane's error was ignored until
> the 1970's, then again the answer is obvious. It is only
> a slight exageration to say that population genetics in
> the 1950's consisted of Fisher, Wright, and Haldane, and
> Fisher and Wright weren't talking to each other.

JE:−
I wonder WHY! Fisher was a racist right wing
bigot. His bigotry was reflected in his models.
He didn't just delete all gene fitness epistasis
to make his models work, it was also a convenient
exercise to allow racist bigotry. Only epistatic
gene fitnesses (which remain the only _empirical_
fitnesses available to this very day!) can stop
racist arguments in their tracks. It is my
contention that Fisher understood this fact
so he milked it for all he could get out of
it. Ironically the same error (the deletion
of all gene fitness epistasis) is milked by
Hamilton et al (Dawkins etc) who happen
to be biased towards the Looney left that
worship the god of altruism.

> JM:−
> It
> wasn't until the 1970's that the population explosion of
> pop gen experts happened to notice the problem. Well,
> I guess that Kimura had a lot to do with that.
> The reason why it has mostly been ignored since then,
> from the seventies to the late nineties, say, is that
> Kimura won (for reasons having nothing to do with the
> dilemma) and people lost interest. And as to why

> ReMine's attempt to resurrect the issue has been
> ignored, well Edser's explanation is as good as any.
> There apparently just aren't enough genes so that
> ReMine's challenge is a serious one.

JE:−
ReMine's charge is NOT serious because
the genomes of man and ape have been proven
to only be tiny with just a few genes that
are different within each so the dilemma
and ReMine (who is entirely innocent) fade
into just magnificent non starters. This
creates a much larger dilemma in its
wake which I will call Fisher's
Dilemma: If only lineal gene information
is heritable how can 30,000 or so genes code f
or all the known human heritable traits that
exist? The corollary: How can just a few
genes separate man from chimp?

> JM:−
> But I agree with ReMine that Haldane's original attempt
> to understand the problem was flawed, as were many of
> the 1970's attempts to improve on Haldane. I hope to
> someday see how ReMine has corrected these flaws. If
> he has.

JE:−
I agree. Although I do not
understand Felsenstein's
argument or ReMine's rebuttal
of it, the way Felsenstein
evaded questions when the first
debate raged here between Felsenstein
and ReMine inspired me to make
my own investigation of what was
really behind the matter. My
conclusion: Fisher's deletion
of epistasis which most importantly
included ALL EMPIRICAL GENE FITNESSES
because all of them ARE EPISTATIC.

I hope Felsenstein will stop using
pretexts to evade questioning on such
BASIC issues so they can be decided.
Once again I implore ReMine
to provide Felsenstein's argument
in his own words in the most simple
way possible. I assure him that I
will attempt to be scrupulously unbiased
in my appraisal of his subsequent

rebuttal. Obviously we cannot understand his rebuttal if we cannot firstly understand the argument to be rebuked. So lets get on with it..

Regards,

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