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Correspondence

ALAN GIJSBERS

More on Memes

An illusion called Philip Bligh watches as the memes in his body churn out a complexly argued case about this grand illusion called the self. The final attractive meme is that the self is self-in-narrative. This is then published by a further meme indwelling the body of the Editor of *Science & Christian Belief* who then watches helplessly as his meme 'decides' to publish the letter on the illusory self (*Science & Christian Belief* (2004) 16, 56). My memes respond. However if the illusion called Philip takes exception, he cannot hold the illusion I call me responsible. It was not me, it was my memes!

There was another illusion called Descartes whose memes said 'I think therefore I am.' The modern debate on the self then started in earnest. Indeed the meme of self-actualisation, self-development and equality of selves was a major enlightenment meme that created the modern world with its strong commitment to democracy. Here each illusory person was regarded as equally valuable. Some illusory people found the meme of equality in the Christian gospel which said that all illusory people are sinners and all equal recipients of God's grace. But it also created a harmful self-referentialism which regards each self as the final arbiter of truth.

Another illusion called Freud contained memes telling of a conflict within an illusion of the memes of id, ego and super-ego. Self-identity, false and true selves are powerful memes, therapeutically useful in settling illusory selves into a more comfortable illusion of themselves. An illusion called Taylor has written one of the most important books on the philosophy of the self outlining the long history of thought about such an important topic: self is so by the choices it makes, the way it relates, its inwardness and the celebration of the self in ordinary life.

This powerful meme of a sense of self has been strengthened by the development of cognitive behavioural therapy which encourages illusionary selves to think about their thoughts, feelings, and behaviours and seeks to modify the latter by changing the memes! Dialectical behavioural therapists talk more radically of a rational self in dialogue with the emotional self to gain some sort of resolution. Practically while there are some forms of reflex behaviour, in therapy we ask patients how they feel, and whether they have thought through the consequences of their addiction. We ask what they themselves are doing about the situation they see themselves in. The choices-consequences model is alive and well in addiction medicine. The self is also a major theme of psychiatric and neuroscience inquiry.

I do not just think. I perceive, I feel, I fall in love, I relate, I choose, I am responsible and society holds me responsible for the actions I perform, even when sometimes those actions are driven by forces (spirits, demons, in the original senses of the words) more powerful than I can cope with.

This letter started by trying to keep me separate from my memes. That is impossible. The self rules!

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COLIN REEVES

Van Till and Probability Theory

Statistical concepts are notorious for tripping people up, and Howard Van Till (*Science and Christian Belief* (2003) 15, 117-140) is not the first to misunderstand the nature of conditional probabilities. In his article he opposes William Dembski's assertion that a proliferation of naturalistic explanations for complexity does not necessarily lead to an increased probability of the event requiring explanation. Van Till wishes to calculate (in his terminology) $P(X|n)$, where n is some 'natural cause' for a complex system X. He believes that if he were further to include some other natural cause m, the combined probability would necessarily be larger.

Dembski is quoted (p.135, note 42) as saying 'that's not how probabilities work', a statement with which Van Till takes issue. Unfortunately, Van Till leaves several ambiguities as to how exactly he supposes these causes would interact. In this article, I propose to examine the three possible cases in which n and m can jointly contribute to a combined probability concerning X, and consider the question whether the combined probability necessarily increases – as Van Till argues it must.

Case I

Van Till frequently refers to a 'list' or 'menu' of causes, which is most naturally read as being a set of disjoint alternatives. (A 'menu' brings to mind a restaurant, with a long list of entrees from which I choose one.) If that is really what he means, then the probability he has in mind (written in standard probability-theoretic terms) is $P(X|n \text{ OR } m)$, and Dembski is correct, as is easy to see with a little mathematics. Suppose we denote $P(X|n)$ by p_n, and $P(X|m)$ by p_m. Then, by straightforward application of the laws of probability,

$$P(X|n \text{ OR } m) = wp_n + (1 - w)p_m, \quad (1)$$

where $w = P(n)/(P(n) + P(m))$. (Clearly $0 \le w \le 1$, which is all that matters for our purposes here – we don't need to know what the prior probabilities $P(n)$ and $P(m)$ are.) Clearly, the combined probability can only exceed p_n if $p_n < p_m$.

How might we apply this to the ID argument? Well, suppose n is Darwinism, which (in the form 'genetic variation plus natural selection') seems to be the preferred 'natural cause' for generating biological complexity. This is presumably because biologists generally (with a few exceptions) rate this explanation for the actualisation of complex biological systems more highly than others – they believe $P(X|n)$ to be higher than it would be for any alternatives to n. If this is the case, then adding to the menu of natural causes *less* plausible models is only going to shrink the combined probability, not increase it. So Dembski can quite justifiably say 'give me your best shot: propose an n that you think is most likely to challenge my criterion'. If he can show that it fails, merely lengthening the list of causes will not help his opponents.

There is a response, of course: n was not the best shot after all, just the best we can come up with at the moment. Getting his retaliation in first, this seems to be what Van Till argues for on p.135. However, to rely on ignorance and wishful thinking – the future discovery of an as yet unknown natural cause – is hardly science. It sounds more like the empty rhetoric of King Lear:

> I will do such things,
> What they are, yet I know not,
> but they shall be the terrors of the earth.

Or, at least, the terror of the ID movement.

Case II

There is a *caveat* to case I, but it does not provide much comfort for the opponents of ID. Equation (1) assumed implicitly that n and m are *mutually exclusive*, i.e., the occurrence of n precludes that of m. If we remove this condition, the probability of the combined event $(X|n \text{ OR } m)$ is modified as follows:

$$P(X|n \text{ OR } m) = w'p_n + (1— w')P(X|\bar{n} \text{ AND } m), \quad (2)$$

where $\bar{n}$, in set-theoretic notation, denotes 'not-n'. (Note that the relative weighting factor is also modified, but as its value does not matter we can ignore the details.) In this case, even if n is the best single candidate (i.e., $p_n > p_m$) it is conceivable that including m in the analysis might increase the probability $P(X|n \text{ OR } m)$. However, it is not the case that such an outcome is necessarily true, and counter-examples to Van Till's belief can easily be constructed. This can be generalised to an arbitrarily long list of such causes: $P(X|\ n_1 \text{ OR } n_2 \text{ OR } \ldots)$.

So the mathematics says that Dembski is right, and Van Till is incorrect.

However, perhaps it is more that Van Till was unclear – his imprecise use of words like 'list' and 'menu' have clouded the picture. We thus turn to the last alternative.

Case III

Maybe Van Till does not really mean to imply a list of alternatives, despite his terminology, but rather an integrated set of natural causes, so that the problem with n is merely that it is incomplete. So while the probability of n alone causing a complex system is small, perhaps if its action is modified by other causes, the probability would be increased. This is implied by his later characterisation of $P(X|n)$ as due to the *joint* action of all relevant natural processes. In this case, he would be asking for $P(X|n \text{ AND } m)$, (e.g., 'Darwinism and self-organisation', *à la* Stuart Kauffman, or 'Darwinism and natural genetic engineering', *à la* James Shapiro), or more likely $P(X|n_1 \text{ AND } n_2 \text{ AND } n_3 \ldots)$.

Now, it is often the case that (quite generally – I am not referring specifically to ID here) refining the conditions under which we might observe X in this way does indeed lead to increasing probability. Diagnosis of a specific disease, for example, is usually strengthened by observing the conjunction of several symptoms rather than relying on one only. However, this is not *necessarily* true, and it is again simple to construct counter-examples where the probability decreases. (Technically, in both Cases II and III, the salient point is whether, and how much, the causes are *associated*. Nowhere do we have to assume, as Van Till mistakenly believes, that the probability of $P(X|n)$ has been incorrectly computed.)

Conclusion

Paraphrasing his argument slightly for the sake of brevity, Van Till states (p.135, Note 42) that the 'discovery of a new m will necessarily add a positive contribution to $P(X|n)$'. I have argued that this rests on a naive and false idea of the nature of probability calculus, and the best we can say is that it is possible for such a discovery to have such an effect. Every case would have to be treated on its own merits.

Even in Case I, calculating probabilities in the context of biological complexity is clearly a hazardous business, but confronting the current best natural explanation will do for the moment. The opponent of ID only has the King Lear defence: much hand-waving and vague supposition, more 'just-so' stories, but nothing concrete.

Calculating probabilities in Cases II and III would be even more difficult, but arguably it would be redundant anyway. Another name for an integrated set of causes is a complex system, and we know very well that complex systems can beget complexity. Thus this approach, if it is what Van Till intends, merely displaces the problem of complexity's origin from one system to another, and

fails to address the underlying question.

The case of evolutionary algorithms (EAs) is pertinent here. The theory and practice of EAs, which Van Till pleads in his cause, actually provides evidence against it. Twenty years of research into EAs has shown that you get out what you put in: the performance of EAs is disappointing unless a large amount of (human) design enters into their implementation at almost every level – choice of encoding, design of operators, sizing of populations, optimal application rates of the operators, manipulation of fitness functions etc. – and furthermore, we have so far almost no theoretical principles on which to base these decisions.[1] In other words, a successful EA is itself a complex system.

I have not addressed the question of the plausibility or otherwise of Dembski's calculations in respect of the bacterial flagellum; I happen to agree with Van Till here that Dembski may not be asking quite the right question. However, I note that Van Till doesn't attempt a calculation based on *his* model. Perhaps that is just as well: it is hard to see how he could even begin to make a calculation based on nebulous statements such as the following –

> Many scientific hypotheses regarding the manner in which various transforma-tional processes may have contributed to the actualisation of some new biotic structure….(p.126)

especially when you realise that William Dembski is right to at least this extent – Van Till's idea of probability theory is **not** how probabilities work.

Colin Reeves, Professor of Operational Research. Coventry University, UK

1 For a recent study of EA theory, see.Reeves, C. R. &.Rowe, J. E. *Genetic Algorithms – Principles and Perspectives*, Norwell, MA, Kluwer Academic Publishers (2003).

HOWARD J VAN TILL

ID's Vulnerability to False Positive Indicators:
A Response to Reeves

My thanks to Professor Colin Reeves for his interest in my evaluation of probability claims made by ID proponent William Dembski. I believe that his comments can be constructively incorporated into this discussion, but first let me provide a brief summary of the context in which I made the particular argument (appearing primarily in a footnote) to which Reeves takes exception.

Here, then, is the sequence of critical considerations:

1. Although it may be difficult for many readers to detect in Dembski's writings, I find the most common operative meaning of the term *intelligent*

design, when employed as the name of an action, to be *some non-natural, form-conferring intervention performed by an unidentified, unembodied, choice-making agent in the course of actualising some biotic structure or system, X.* In the spirit of the ID movement, such action is judged to be necessary in certain instances for which the system of natural causes is demonstrably inadequate to accomplish the task of actualising X.

2. The foundation of Dembski's claim for the necessity of this *intelligent design* action rests on his ability to demonstrate that there are biotic structures, X, for which $P(X|N) < 10^{-150}$, where $P(X|N)$ is the probability that X could have been actualised *by the joint effect of all natural causes, both known and unknown*, here designated as 'N'.If this probability condition is satisfied, then Dembski considers X to be sufficiently 'complex' to satisfy the complexity portion of his 'complexity-specification criterion' that, in his judgment, signals the need for non-natural 'intelligent design' action. In this context, the 'complexity' of X is taken by Dembski to be a measure of the difficulty of actualising X by means of the natural causal system, which Dembski often characterises as the forming of X 'by chance'.

3. In my essay I argued that the computation of this probability requires an exhaustive knowledge of the system of natural causes, N, which is simply unattainable. The best that could be computed is $P(X|n)$, the probability that X could have been actualised *by the joint effect of all known natural causes operating in known ways*, here designated as 'n'.

4. Furthermore, I have pointed out that the only known causes that can be included in 'n' for this computational test (a test devised by Dembski himself) are those for which probability values can actually be computed. Plausibility arguments (often denigrated as 'just-so stories') regarding some conjectured formational pathway leading to the forming of X, no matter how reasonable they might seem to a biologist, cannot be considered here unless firm probability values can be computed. In application, this restriction is especially effective at eliminating from serious consideration those natural processes that ID proponents label 'non-Darwinian', which means that they would fall outside of the category of strict Darwinian gradualism.

5. Given that any computation of the probability for the natural forming of X must necessarily rest on an incomplete knowledge of the system of natural formational pathways, the principal point of my consideration of Dembski's probability computations was to argue that Dembski's test for 'specified complexity' and the purported necessity for 'intelligent design' action is highly vulnerable to false positive indications. Factors contributing to this problem include these: not all natural causes, whether they be Darwinian or non-Darwinian, are known; some known causes might operate in ways not yet conceived; some plausible biological pathways do not lend themselves to calculable probability values of the sort

that Dembski needs in order to make his case.

6. My specific conclusion, as stated at the end of my essay, was that 'claims by the ID movement that some non-natural Intelligence must have intervened in the past to inject specific biotic forms into a universe otherwise incapable of actualising them have failed the test of careful scrutiny.' (140)

In the course of my argument I offered the judgment that 'the more we learn about the self-organisational and transformational feats that can be accomplished by biotic systems, the less likely it will be that the condition for *complexity* – in the particular way that Dembski employs this term in relation to *specified complexity* – will be satisfied by any biotic system.' (135) In the footnote to that judgment I made a statement equivalent to $P(X|N) \geq P(X|n)$, where N represents the joint effect of *all* natural causes, both known and unknown, and n represents the joint effect of *known* causes operating in *known* ways.

Professor Reeves argues that this probability relationship *might be true* in some instances, but that it is *not necessarily true* in all cases. His analysis consists of three 'cases', each representing a different interpretation of what I meant by the notation $P(X|N)$ and $P(X|n)$. Reeves' first two cases consider the relationship among probability values for situations in which a choice of two specific *alternative* natural causes, say *n* and *m*, is available. I do not understand, however, why Reeves chose to explore these two cases. I thought that I had made it amply clear, a) that *N* represented *the joint effect of all natural causes*; b) that *n* represented *the joint effect of all known causes operating in known ways,* with the additional constraint that the relevant probability values must, in fact, be computable; and c) that *n* was consequently a subset of *N*. That being the case, Reeves' Case I and Case II strike me as potentially valid explorations of the relationship among probability values computed for *alternative* natural causes, but *not directly relevant to anything that I had said in the paper under scrutiny.*

That leaves Reeves' Case III to be examined. Says Reeves, 'Maybe Van Till does not really mean to imply a list of alternatives, despite his terminology, but rather an integrated set of natural causes, so that the problem with *n* is merely that it is incomplete.' Aha! Now the picture seems to be in focus. Furthermore, what I label *n* cannot simplistically be identified with Darwinism, as Reeves suggests a number of times, because we have no assurance whatsoever that all relevant Darwinian processes (however defined) are known or lead to computable probabilities. Reflecting on this, Reeves notes that, '…it is often the case that…refining the conditions under which we might observe X in this way does indeed lead to increasing probability. … However, this is not *necessarily* true….'

Point well taken. Reeves' point would be especially relevant if the contribution of each newly discovered natural cause, *m*, were to be considered individ-

ually, as his conclusion implies. Strictly speaking, however, when I suggest that $P(X|n \text{ AND } m) \geq P(X|n)$, the *m* with which I am concerned is not some single newly-discovered natural cause, but rather the *sum of all natural formational processes yet to be discovered*. Under those conditions, the possible exceptions to which Reeves calls attention will, I believe, be exceedingly rare and perhaps non-existent.

Consequently, I believe that my conclusion regarding the *vulnerability of Dembski's computational strategy to false positive indications* stands, even with this modification. ID proponents simply cannot claim that they have unequivocally demonstrated the necessity of 'intelligent design' action. As I have noted on other occasions, the strongest case that ID proponents can actually sustain is, in effect, this one: *In the absence of exhaustive knowledge that demonstrates beyond question the adequacy of natural cause to bring about the actualisation of biotic structure X, it is logically permissible to posit that the actualisation of X required, in addition to natural causes, one or more episodes of non-natural, form-conferring intervention performed by an unidentified, unembodied, choice-making agent.*

That may be true, but it was known to be true long before the ID movement came along to embellish it with claims employing incalculable probability values.

Finally, Reeves complains '…that Van Till doesn't attempt a calculation based on *his* model'. Why that is the case should, by now, be obvious to most readers – I do not have, nor have I ever claimed to have, exhaustive knowledge of the system of natural causes. I don't mind admitting that. That is why, in response to the question, 'But what if our knowledge is inadequate to do the probability calculations?' I replied, 'As I see it, the only honest thing to do is to say so with candour, and admit that no definitive conclusions can be reached.' (135) What I ask of ID proponents is precisely the same admission.

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