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A Letter on:

Rationales for Mathematical Modeling, and Points of Terminology

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Michael Best opens his *Critique of Memetic Models* letter (Best, 1998) with what might be taken as a broad swipe at new science being done under the heading of `memetics'. Yet Best has himself published in this journal (Best, 1997), using a very recent definition of meme and very recent methods. So it might seem surprising that he should now proclaim a sweeping `weakness' in memetics models or cast doubts over thoughts of `new science' in the field.

The letter does not mention, however, that my *Units, Events, and Dynamics* paper (Lynch, 1998) finds a wide range of meme definitions, including his, functionally encumbered and contrary to Dawkins (1982). In particular, the considerations in section 12 of my paper find an unrecoverable problem in the broader usefulness of definitions based on the embedded theoretical construct of `unit size', because sets of proposed cultural units can in general only be considered partially ordered in the set theoretic sense when applied to human memory items. This affects both `smallest units' and `largest units' definitions of the word `meme'. My paper also maintained that to avoid terminological confusion, we should remain consistent with Dawkins's clarification that memes are informational replicators *residing in the brain*. These were matters of terminology, rather than criticisms of Best's empirical netnews findings or the findings of many others who have used vastly divergent definitions of the word *meme* or different words altogether. I even proposed a more general term than memetics, namely, *cultural repliconics* to encompass the study of divergent kinds of non-brain based replicators. I also favor inserting the word *and* into the title of this journal to emphasize its inclusion of the broader scope of evolutionary cultural replicator theory: *Journal of Memetics* ***and*** *Evolutionary Models of Information Transmission*. Still, it is possible for one to misinterpret a merely terminological discussion as a declaration that researchers along the lines given by Best are `memetics outsiders'. One might further conclude, then, that papers such as Best's cannot, by definition, be considered exemplars of `weaknesses' that he presently claims to see in memetics. This possibility diminishes the surprise we might feel about broadly condemnatory tone in Best's opening remarks. The rest of his essay, however, shows a variety of serious flaws which I address severally.

Best's first specific complaint was that the *Units, Events, and Dynamics* paper should have cited Campbell (1974) for first proposing the term `mnemon', even though Campbell does not use this term. Campbell (1974) does use the phrases "*nonmnemonic problem solving*" and "*mnemonically supported thought*", but does not use or introduce the word "*mnemon*". Nor does Campbell indicate that the substring `mnemonic' means anything other than `pertaining to memory'. The term `mnemonic' is sufficiently old that derivatives such as `nonmnemonic' and `mnemonically' do not warrant special citation in the context of my paper. Nevertheless, a few words about the derivation and history of the word `mnemon' are in order. Although I formed the word from its Greek roots, this should be recorded as

a reinvention of a word that was once coined before in a physiology article about engrams (Cherkin, 1966). That 1966 term, which does not seem to have gained wide use, was originally defined as "*the minimum physical change in the nervous system that encodes one memory*". It is not defined as something that is `the same' from one organism to another. Because I derived the term "mnemon" from its Greek roots and gave it a different meaning, it would have been inaccurate to attribute my usage to Cherkin. Nevertheless, my paper might have benefited from some mention of the prior use of `mnemon' by Cherkin, especially as this may have prevented any suggestion that I was plagiarizing Campbell.

It would also have been interesting and informative to include a paragraph calling attention to the mathematical work of Cavalli-Sforza, Feldman, Boyd and Richerson, and Lumsden and Wilson in the preface to section 16 of my own paper. This is not because the equations I present are derivable from equations by these other authors, but rather, to point out that a range of models exists and to make it clear to readers that the present model differs from the others. Still, my own paper is not in any way offered as a review of literature, and should not be critiqued as such. There is much that can be said about the relative merits of different mathematical models, and why my own model is warranted in light of earlier work, but proposing a new model does not of itself require taking on the lengthy task of sorting through older models. The equations in section 16 are quite different from those of the other authors that Best has cited. They were also developed `from scratch', rather than by borrowing from the other authors. Had it been otherwise, then failing to cite the other authors would have been a serious error indeed.

I chose to use the language of mathematics to discuss the quantitative consequences of phenomena discussed earlier in the paper. However, the transition into the mathematical language does not represent any kind of `magic moment' at which one must always pause to give recognition to other cultural scientists who have spoken in mathematical language, when what is presently being said is not based on the previous works. Likewise, if one voices original arguments in prose, the author should likewise not be faulted for not citing non-source works that happen to use English prose for substantially different arguments. The scholarly courtesy of citing other mathematical models of cultural evolution is a good suggestion, but not mandatory in the present case. Original arguments in the language of mathematics do not have a different standard for "mandatory" citations than do original arguments expressed in prose. Unlike the physical sciences, where the use of mathematics is routine, there may be an irrational mystique about mathematics in the social sciences, but it is unwarranted.

Michael Best complains that my model fails to "*describe empirical data (present or sought)*", to which I invite readers to read the original article and consider whether host populations of memes should be considered empirical data or not. He also asserts that the equations shed no explanatory light on phenomena despite the fact that they express changes in meme prevalence in terms of rate parameters for such constituent processes as having and teaching children. As for using the equations to model a specific meme propagation phenomenon, this is indeed a more extensive project than merely publishing equations. Nevertheless, the equations are far more usable when published than when unpublished. Best fails to see the publication of equations as part of a larger process. For this, we can examine the field of theoretical physics, where equations are frequently published without yet having a specific plan to test them. We may also wish to reexamine the other works of mathematical culture modeling to see numerous examples of equations published without specific testing plans. I will not do so here, however, as I am still not offering a review of literature.

Best has further faulted the equations for modeling everything as continuous functions. Best goes on: "*...even within his formalisms, Lynch is compelled to discretise time.*" This is confusion. I have chosen the year as an arbitrary unit of time, but I have nowhere indicated that time is an integer or an integer multiple of any quantity. Nor have I imposed any kind of discrete reproductive generations on a population. In an actual study, it will generally be necessary to choose a finite number of times at which to measure host populations, and so forth, but these details are not built into the equations themselves.

They are left to the discretion of potential users of the equations. The corresponding approximate evaluation of integrals as finite summations, and derivatives as ratios of differences is also left to potential users of the equations. Why Best chooses to demand formal inclusion of a scheme to "discretise time" for my equations in advance of specific application eludes me. Would he demand the same from certain other mathematical models of culture or indeed, mathematical models of physical phenomena?

Best has asked why the age of a parent (more exactly, a meme host of a given age who may have zero to many children) is relevant. The reason is that human reproduction is closely tied to the phases of our species' life cycle, so I have chosen age as the variable with which to describe phase in the life cycle. A 5-year old meme host will generally have about zero children per year, as will a 95-year old meme host. Mortality is also modeled as age-dependent, largely for biological reasons beyond the scope of this journal. Yet once the number of hosts at a given age needs to be known for one term in the equation, it must be modeled in all terms of the equation. As it happens, the rate at which many memes are learned from parents also depends on the age of the child, even if the parents' age matters little. As I have pointed out in the paper, the model can be simplified to treat each meme as being learned at just one effective childhood age and at a rate such as k11 that is invariant with respect to parent age. Treating k_{11} as invariant with respect to time, location, socioeconomic status, etc. is of course a simplification too.

Still another complaint that Best lodges against $K_{11}(p, a)$ regards its definition as a ratio rather than a probability. It is true that I could have defined it as the probability per unit time of meme transmission from a parent of age p to their child of age a. Correspondingly, $N_1(a, t)$ and $N_2(a, t)$ would have been defined as expectation values of host population-age profiles. This is a matter of taste. Probabilities, however, are empirically measured by taking ratios. The probability per unit time of a child of age a acquiring a meme from an age p parent, for instance, is measured by taking the ratio in a representative sample of population of those age a children with age p parents acquiring the meme in a representative time interval to the total number of age a children with age p parents. There is, however, a branch of "*arm-chair theorizing*" known as statistics that concerns itself with such questions as how likely it is that a parameter measurement taken on a `representative sample' matches the parameter for the whole population to a given degree of accuracy. I have deliberately left this sort of analysis for future work. Section 16 of my paper serves only as a starting point for a particular line of mathematical modeling in population memetics, and was certainly not offered as a complete and final treatise.

As I stated clearly in the paper, "*equation 1 and equation 2 model fairly ideal cases of the two-idea propagation problem*." Thus, I do not include certain complications that can arise from biparental transmission, such as memetically mixed marriages. So the present model works better when the proportion of meme-discordant marriages is either quite low (e.g., with certain religions) or remains constant, with reproductive rates of mixed families well modeled as the average of the two meme-concordant cases. In the constant intermarriage case, the meme-1 partner is still credited with an $R_1(a)$ reproductive rate and the meme-2 partner is credited with an $R_2(a)$ reproductive rate. However, because some meme-1 parents' children are being partly raised by meme-2 spouses, and vice versa, we expect the measurement of $K_{12}(a, p)$ and $K_{21}(a, p)$ to show significant effects of crossover inculcation. (The reduction of K_{12} and K_{21} figures is one of the evolutionary pressures favoring intermarriage taboos in religions.)

In cases where the proportion of intermarriages changes substantially over time, or where reproduction rates are poorly modeled as a by averaging the two unmixed cases, a more elaborate modeling of parental transmission is called for. For example, a set of four equations could model the population of female meme-1 hosts, male meme-1 hosts, female meme-2 hosts, and male meme-2 hosts. A model for intermarriage rates as a function of (for instance) $N_1(a, t)$ through $N_4(a, t)$ is required, as are additional

K(p, a) and *R(a)* values for meme-concordant and meme-discordant parents. (More generally, for all possible parental meme combinations being modeled. Again, the simplified k_{11} etc. parameters may be used.)

Still further elaborations can be pursued for increasingly sophisticated and realistic models. Yet even when the simpler system of equations can be used, the application of the model to a specific meme is an extensive project requiring further resources and collaboration. It is unreasonable to demand that such a project be well underway before a system of equations can even be published, but the question of how to handle two parents and meme discordance is appropriately raised at this stage. Presenting a first-order model of biparental transmission may seem a bit like treating a person as a point mass in a physics problem, but even the point-mass person is a good approximation in certain situations. However, for the most ambitious investigations of meme propagation, one should be prepared to face a degree of mathematical and computational complexity rivaling the modern models of terrestrial weather.

Best apparently demands that I model parent to child meme transmission in terms of a whole "host of factors" without being specific. Does the system of equations need to take account of every conceivable variable, or else nothing at all? If so, this could demand a degree of accuracy that would have prohibited early models of the atom as well. I agree that a whole host of variables can be added to the model, but how can one reconcile demands for a host of added variables while at the same time complaining about the "*sheer complexity*" of the model? I can only conclude from these incongruous demands that Best would have rejected any model I had proposed, regardless of potential utility. And I must ask him if he knows of a mathematical model that actually meets all of his demands, to please identify it.

The purpose of my present reply to Best's essay is not to provide details for the next and subsequent levels of elaboration to the mathematics in section 16 of my paper. At 17,100 words, the *Units, Events, and Dynamics* paper is already quite lengthy for a journal article, and elaborated models require entire new articles at the very least. Another kind of paper is one that simplifies the equations, as by taking lumped, single-event models of reproduction and child inculcation. The value of such simplifications would be to demonstrate that a particular magnitude of reproduction difference, for instance, can in principle lead to significant meme proliferation even if there are also non-parental transmission processes at work. Alternately, if even large reproduction differences never produced much long-term effect, then this may be taken as a falsification of the hypothesis that reproduction differentials sometimes account for large relative gains in the host populations of memes. The presentation of the population memetics section in mathematical terms should be seen as an invitation to investigate such matters quantitatively, rather than by sheer guesswork. It should also be seen as a demonstration that methods do exist for translating specifically memetic theory into mathematical/quantitative terms, which is widely deemed essential to taking any variant of scientific theory seriously. An absence of such work among those concerned with "memes" could easily have been taken as a signal that in order to see serious mathematical reasoning, one must ignore those using the word "meme" in favor of those using words conceived in association with highly sociobiological tenets, such as `culturgens' (Lumsden and Wilson, 1981). A deep suspicion exists among many scientists for lines of thought advanced without any hint of possible mathematical expression, for such efforts are widely seen as vague and evasive. In any case, Best falls far short of justifying his assertion that the mathematical model "*does damage to the problem*." Because such phrases may discourage people from pursuing the mathematical investigations further, any claim of `damage' should really be withheld unless strongly justified--unlike the present case where the term `damage' is not even remotely warranted.

Best comes very close to asserting that no new mathematical models should ever be developed directly from a particular combination of concepts including `transmission event', `host population', `reproductive rate', `child inculcation rate', and so forth. Instead, he seems to assert that one must limit modeling to efforts to building upon past equations developed around substantially different premises and parameters.

For this, he gives no justification, theoretical or otherwise.

Best has also seriously misread the transmission of a negatively defined mnemon (e.g., the lack-of knowledge of birth control knowledge spreading by way of more numerous childbirths) as a "forgetting" event. Individual forgetting events are of course possible, and are a subset of individual dropout events generally (e.g., A --> ~A). One can also persuade a peer to drop a belief, for instance. But nowhere in the paper is there a notion of peer to peer `forgetting events'. Subsequent to his letter, Best explained to me that he got the idea of the transmission of lack of birth control information from his reading of the clause "*people can be taught about birth control far more easily than they can be made to forget about it*." Apparently he didn't notice that while knowledge of birth control is discussed as transmitted, the lack of such knowledge is not. To *make* someone forget about birth control could involve drastic measures such as concussion or drug induced amnesia, or even lobotomy. Such extreme measures do not constitute `transmission' of lack of knowledge, because `transmission' presumes that lack of knowledge existed in the person who performed the lobotomy, drugging, etc., and that such lack of knowledge played a role in causing or directing his actions. Moreover, nothing in my paper suggests that lack of knowledge can be *taught.* Given this wild misreading of part of a sentence in section 5 of my paper, I can understand how Best would expect the worst from the whole rest of the paper. This and other misunderstandings show that before concluding that a refereed paper proposes an outlandish process divorced from reality, a few well-placed queries to the author may help.

Best barely touches upon the original disagreement he had with me, namely, the definition of `meme'. After praising Richard Dawkins (1976), he asserts merely that `meme' describes "*replicating cultural units*." Yet Best cannot flatter Dawkins for being so nonspecific as this, for Dawkins has distinctly clarified (1982) that the word he coined refers to brain-based replicators only. Ironically, Best, whose paper and letter overlook the specificity in Dawkins (1982), now calls out for "*an appreciation of past art*". Yet this disagreement between me and Best is only a matter of terminology. We may have a deeper disagreement over the value of doing new work on theoretical frameworks in memetics. Yet regardless of the broader nature of our disagreements, more careful readings of my own paper and related works such as Campbell's are in order.

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