

devolved their claims of punctuation from an "alternative" to being "complementary" [to gradualism].<sup>1</sup>

Wise wants to divorce PE from biological mechanisms for speciation. However, more than half of Eldredge and Gould's original 1972 paper is devoted to allopatric speciation and re-interpreting some of the published fossil data in the light of this **biological** theory. Indeed they tried to claim that the concepts of abrupt appearance and stasis were palaeontological predictions derived from their understanding of allopatric speciation. In 1977 they wrote, 'Our model of punctuated equilibria is a hypothesis about **mode**.'<sup>2</sup> (Emphasis mine.) That is, they saw it primarily as a biological mechanism of evolution. Later, they wrote of allopatric speciation models as being '... the very heart and soul of punctuated equilibria.'<sup>3</sup> This does not sound like an optional component of PE, as claimed by Wise. However, I agree with Wise that PE is basically a palaeontological theory, or rather observation, but Eldredge and Gould have tried to cast it as a prediction of the biological theory of allopatric speciation.

Wise also wants to completely divorce PE from macroevolution, but this is not possible either. Again the original Eldredge and Gould paper devoted a section to 'some extrapolations to macroevolution'. In their 1977 paper they wrote at length on 'Punctuated equilibria as the basis for a theory of macroevolution: the speciation theory.'<sup>4</sup> Furthermore, in the summary to their 1993 review they stated two significant implications of PE as:

*'the recognition of stasis . . . and . . . the recasting of macroevolution as the differential success of certain species'*.<sup>5</sup>

Wise wants to recognise the first but ignore the second. It is well to remember that macroevolution still entails speciation.

Wise says that, to his knowledge, Gould 'has never . . . claimed there are no stratomorphic intermediates between higher groups . . .' In my paper

I cited Gould as saying,

*'The absence of fossil evidence for intermediary stages between major transitions in organic design . . . has been a persistent and nagging problem for gradualistic accounts of evolution.'*<sup>6</sup> (Emphasis mine.)

I stand by my review. I believe I have accurately reviewed PE, **as perceived by Gould and Eldredge**, although I concede that this may encompass more than the PE of Wise.

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## REFERENCES

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4. Gould and Eldredge, Ref. 2, pp. 139-145.
5. Gould, S. J. and Eldredge, N., 1993. Punctuated equilibrium comes of age. **Nature**, **366**:223.
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## SPEED OF LIGHT DECAY

Dear Editor,

Lots of statistical treatments, based mainly on regression analysis, have been submitted to this journal claiming that the hypothesis of Barry Setterfield that *c*, the speed of light, has decreased in the recent past is without proper statistical and scientific foundation. I cannot recall that any author has submitted the historic measurements of *c* to an analysis of variance. This test has been used to establish the significance or otherwise of regression equations but has not been applied directly to the *c* data itself. I have therefore performed an analysis of variance on this historic data. This analysis compares the variance **within** each individual determination of *c* with

the variance **between** each individual determination. The result of this test indicates that there is a far greater statistical variation within the estimates of *c* than there is between the estimates. The result is highly significant ( $p = <0.01$ ). This result is not surprising when one considers that the early measurements using very crude methods carry very large standard errors compared with the measurements made post-1947 using electronic methods and whose standard errors are very small. This result supports the claim of Aardsma, Brown, Evered and others that *c* is a real constant, and means that the claim of Setterfield, Norman and Montgomery that *c* has undergone a statistically significant reduction in the last 300 years cannot be substantiated when the true nature of the scatter of the data is taken into consideration.

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## NAMES FOR GOD IN GENESIS

Dear Editor,

May I be permitted to add something to Dr Taylor's paper on Genesis, which appeared in **CEN Tech. J.**, **8**(2):204-211? Dr Taylor has done us all a great service in helping to restore our faith in the truth and integrity of the book of Genesis. We are, however, left with one big question. How did the covenant name for God find its way into Genesis? For we are plainly told, when God appeared to Moses, that the name Jehovah (YHWH) was a new revelation, not before revealed to the people of God. In Exodus 6:3 we read, 'I am the Lord (Jehovah or YHWH). I appeared to Abraham, to Isaac and to Jacob as God Almighty, but by my name Jehovah I did not make myself known to them.' This seems plain enough, and on the basis of this statement we should not