Studies in Flood Geology: Clarifications Related to the 'Reality' of the Geologic Column

JOHN WOODMORAPPE

ABSTRACT

Recent papers in the ***Creation Ex Nihilo Technical Journal*** *have focused on the presumed reality of the geologic column, and the need for Flood advocates to accommodate it. After clearing up several misconceptions about my research, it is demonstrated that long-distance tracing of strata is not an internally-consistent line of evidence for its reality. Also countered is the claim that Lower Palaeozoic strata need to be treated as a unitary depositional horizon. Finally, some little-appreciated factors which obviate the need for long periods of time in the formation of successive footprint horizons and dinosaur nests are discussed.*

A good many issues concerning Flood geology have been raised in the book review of my **Studies in Flood Geology,**[1] as well as the several papers in the **Creation Ex Nihilo Technical Journal,** volume 10, number 1. I would like to respond to some of these issues.

ANCIENT HUMANS IN PHANEROZOIC ROCK?

Does it actually beg the question to suggest that ancient humans have not been found in the early geologic record because they occur at very low frequencies? I think not. It is like saying that we should not expect seas of molten metal at the Earth's surface because the air temperature never exceeds some 50°C, whereas most metals don't melt below thousands of degrees C. As it utilises the same logic, my argument is neither logically nor epistemologically circular. After all, I present independent evidence on the volumes of sedimentary rocks along with plausible population figures for the antedivulian world. Also, the claim that we should have at least found a few truly ancient humans is, at best, a *non-sequitur.* To use the needle-in-haystack analogy, we would not expect any needles to be found if the haystack is large enough (say, many kilometres across), even if all the able-bodied people on the planet Earth had spent their entire life-times rummaging through it.

Of course, various claims have been made in the creationist literature of human remains found in 'ancient' strata. These have to be carefully evaluated, but should not be rejected out of hand merely because anti-creationists are very antagonistic towards them.

Is it unjust to suggest that evolutionists ignore any finds of Early Phanerozoic human remains? I think not. One should reread the section of my work to see that I provide ample evidence that evolutionists only reluctantly accept the existence of fossils in 'wrong' strata. In addition, I showed in my work on the cephalopods[2] that certain belemnites were discounted or ignored when found in an unacceptable level in the geologic column. If this is still insufficient, consider also a more recent example:

> *'The occurrence of a labyrinthodont in the Jurassic was not considered likely by most workers, and the report was ignored (as much as possible) until 1967*[3]

No doubt evolutionists would be less than objective and candid if they ran across a human fossil in Palaeozoic strata! Of course, such finds are probably explained away on a case-by-case basis, as is the anomalously-young fossil amphibian quoted above, and need not imply a collective evolutionistic conspiratorial silence about ancient human remains.

THE CONTINUITY OF SEDIMENTARY STRATA: AN INTRODUCTION

By way of introduction to Snelling's suggestion that the properties of sedimentary strata accredit the geologic column, there is no doubt that there is similarity in lithologies and geologic ages. This is a foregone conclusion, as comparable lithological sequences (regionally as well as globally) were preferentially assigned to the same age as long as they didn't come into conflict with index fossils. This is a holdover of the Wernerian System.[4] Decades after the geologic systems had been named, Lapworth[5] complained that the divisions of the Lower Palaeozoic relied too much on lithic features of assumed universality and contemporaneity. (Of course, there are also many contradictions between lithology and biostratigraphy, and these are discussed in the ensuing sections.)

Let us look at some 'global' sedimentary fades.[6] All coal-bearing sequences were once assumed to be Carboniferous in age,[7] so it is hardly surprising that coal measures assigned to the Carboniferous Period can be observed in many different parts of the world.[8] Of course, they must be put in perspective. As shown in my work on cyclic sedimentation,[9] coal-bearing sequences are found in all the geologic systems from Devonian through Tertiary. More important, 85 per cent of Upper Carboniferous sedimentary rocks, by global volume, are **not** coal-bearing.[10] The continuity of individual coal beds over vast, subcontinental areas must also be taken with a 'grain of salt'. In my work on cyclic sedimentation,[11] I pointed out that coal seams which trend thousands of kilometres aren't observable in continuity, but did recognise their widespread occurrence and the superiority of the Flood model in explaining their extent.

An analogous chain of reasoning holds for the Upper Cretaceous chalk facies. This distinctive facies can be seen on different continents.[12] However, it needs to be kept in perspective, in that coccolith-bearing rocks occur from Jurassic through Tertiary, as shown in my work on the antediluvian biosphere.[13] At the same time, 75 per cent of Upper Cretaceous rock (worldwide, by volume) is non-carbonate;[14] far more is non-coccolith.

Recently, claims have been made of the possibility of highly detailed global chronostratigraphic correlations based upon sequence stratigraphy. Miall[15] provides a devastating critique of these claims. He shows that the range of errors in biostratigraphy, magnetostratigraphy and isotopic dating are such that the precision claimed is a physical impossibility. Furthermore, he highlights the role of circular reasoning and fortuitous correlations in, for instance, the belief that there are 40 discernible global events within just the Cretaceous Period.

There is also at least some element of circular reasoning in even the first-order cycles of the Vail curve. The times inferred for even first-order global regressive sequences contradict each other, and depend upon which database is used.[16] Of course, the sea-level curves should not be directly imported into Flood geology. For instance, 'marine regressive' sequences need not be accepted as such in Flood geology, but may be interpreted as giant reverse-graded beds laid down by Flood waters that are increasing in velocity at that point of deposition.[17]

DOES LATERAL CONTIGUITY OF SEDIMENTARY STRATA ACCREDIT THE GEOLOGIC COLUMN?

My brow was furrowed as I read Snelling exhorting us to *'bury our myth of the non-reality of the geologic* column'.[18] In actuality, the real myth — the temporal continuity of strata — had been exploded over a century ago. Long ago, Spencer[19] had chided the 19th century geologists for tacitly assuming that similar lithologies were contemporaneous (whether regionally, subcontinentally, or intercontinentally). Spencer saw this type of fallacy as a ghost of the Wernerian system, wherein it had once been imagined that all of the primary lithologies had formed at the same time. More about the history of the geologic time-scale later. Let us begin by focusing on those sedimentary beds which cover vast areas. To begin with, any extensive bed that comes into conflict with local biostratigraphy is not recognised as such: it is broken up into local units. Alternatively, its extensive nature is recognised, but localised potential extensions of it are assigned to divergent ages depending upon the dictates of local biostratigraphy. For instance, a sandstone unit in Ohio is lithically identical to the laterally-extensive St Peter Sandstone (Ordovician), of the central United States, but is constrained by other factors to be dated as Upper Silurian.[20] Conversely, just because a lithological unit is consistent with biostratigraphy throughout its geographic extent, and is mapped as one contiguous unit, does not necessarily make it the 'same' stratum. For instance, the 'sheet sandstone' concept has recently come under criticism,[21] because sandstones previously mapped as one contiguous lithology often turn out to be a series of overlapping (or even non-overlapping) units called shingles. It has also been known long ago that the tracing of sedimentary sequences often fails to agree with time divisions, based on index fossils, even on a regional scale:

> *'Correlation by superposition, however, is a method fraught with grave dangers. Thus a succession of formations from sandstones to shales and limestones in one part of a province is not necessarily the same as a similar series in another part of the same province, and most probably not the same as a similar series in another geologic province.'* [22]

The fallacy of the premise that lithological successions can be dated independently of index fossils is not only of historic interest, but also continues today. For instance, in my diluviological treatise,[23] I gave the following modern examples: It was supposed that a Palaeozoic lithological unit in north-eastern Asia and the Japanese Islands could be

Figure 1. *A hypothetical sequence of three strata of different lithologies deposited on a regional scale.*

unambiguously correlated with very similar strata on Sakhalin Island, until some Jurassic corals were found in the Sakhalin lithologies. At a more local level, a phyllite unit in eastern North America had been mapped as part of the adjoining Carboniferous succession until typically Cambrian trilobites were found in the phyllite, instantly ageing it by a few hundred million years.

If it were actually true that sedimentary strata could be dated reliably by examining successional relationships, this should show up in Precambrian geology, where relative dating by biostratigraphy is commonly ruled out by a scarcity of fossils. Instead, major problems result by tracing lithologies, either singly or as successions, even on a regional scale. In my radiometric dating study,[24] I cited the warning that 'layer-cake stratigraphy' leads to errors. Also, I documented the fact (also verified in my independent field work) that a seemingly 'compelling' correlation of two nearly-identical lithological successions (quartzite/ carbonate/banded iron formations/slates) in Wisconsin, United States of America, separated by only a few hundred kilometres, was contradicted by isotopic dating.[25] Those nearly identical successions, despite being intuitively obvious correlatives, were instead placed hundreds of millions of years apart in time.

DOES SUCCESSIVE OVERLAP OF SEDIMENTARY STRATA CONSTRUCT THE GEOLOGIC COLUMN?

To begin with, I am well aware of the fact that there is a certain element of reality behind the stratigraphic succession of different fossils. If I were unaware of this, or refused to recognise it, I most certainly would not have spent five years and hundreds of hours developing my diluviological treatise on the stratigraphy separation of fossils![26]

However, Snelling goes beyond this, asserting that the geologic column as a whole must be accepted as reality because strata overlap each other, and thus build up (as do shingles on a roof). I now explain my disagreement with his premise. Consider Figure 1. The term iithology' may refer to a sedimentary unit (for example, sandstone) or distinctive succession of lithologies (for example, sandstone/ shale). There is no doubt that the law of superposition dictates that (B) was deposited before (A) at the point of their mutual contact (barring tectonic effects, of course). But beyond that, all is inference. For instance, (C), (B) and (A) might be Cambrian, Ordovician and Silurian, respectively. Following Snelling's reasoning, we would also be forced to infer that a stratigraphic succession of (A), (B) and (C) is a reality, despite the fact that nowhere are (A), (B), and (C) superposed in any one spot.

Is such reasoning invariably justified, and does it accredit the geologic column? Far from it. To begin with, if stratum (B) trends a great distance, changes significantly in thickness and in composition (say, from a quartz arenite to a sandy shale, and has intercalations of other sedimentary rocks within part of its lateral extent), are we still justified in concluding that stratum (B) is the 'same' bed throughout its extent? Is it still a valid 'bridge' relating strata (A) to (C)?[27] There is no objective standard I know of which spells out how much a given stratum can change with distance and still be mapped as the same lithological unit or formation. Rather, it depends on the local stratigraphy, the region, and the geologists doing the work.

More fundamentally, would uniformitarians invariably recognise strata (A), (B) and (C) as belonging to three different geologic periods? The answer is a loud negative. Anywhere from one to several geologic periods could be represented in the succession depicted in Figure 1, whatever the *ad hoc* dictates of uniformitarian time designations. Moreover, time divisions need not even coincide with any of the lithological boundaries! In other words, any or all of the three strata pictured could be considered 'time transgressive', according to the *ad hoc* needs of the uniformitarian and his manufactured 'global onion skins' of geologic time, as dictated by his index fossils. What if, for example, stratum (B) contains Cambrian trilobites in the part which overlies (C), and Ordovician trilobites in the part which underlies (A). This would not faze the uniformitarian in the slightest. He would either split up stratum (B) into two different units (examples of this given in the next section), or consider stratum (B) to be a so-called time-transgressive unit: to have been laid down partly in the Cambrian and partly in the Ordovician.

As a matter of fact, Conybeare[28] states that most lithological units are diachronous, which is another way of admitting that lithological units and lithological successions do **not** usually coincide with divisions based on time. The implications of this are obvious. Since lithostratigraphic successions, whether continuous or overlapping, are interpreted inconsistently by uniformitarians, they cannot logically count as evidences supporting the validity of the uniformitarian geologic column. Otherwise, we unwisely fall for the claims of uniformitarian geology by buying into its characteristic special pleading and circular logic. Of course, the use of index fossils itself is subject to its own

body of self-serving illogic, but that is another topic.

Nor is it true that creationists have ignored the physical reality of overlapping and/or contiguous strata. We have in fact considered the 'continuous strata' argument long ago, finding it illogical and internally inconsistent. For instance, Price[29] had called attention to lithological units (for example, carbonates) so patently similar throughout that they had been mapped as one unit or formation, until a conflict with index fossils caused them to be arbitrarily split up and assigned to different geologic periods (even remotely different ones, separated by so-called paraconformities). Later, Whitcomb and Morris[30] had added to the expose of the 'physical continuity of strata' argument, documenting and discussing the fact that time boundaries do not coincide with lithological successions. I myself have updated this discussion with several recent examples (see below), and have in my article on cyclic sedimentation[31] discussed 'time-transgressive units', which, as we have seen, are a smokescreen for contradictions between lithostratigraphy and biostratigraphy. In my diluviological treatise,[32] I considered the question of stratigraphic continuity of fossiliferous strata, documenting its inconsistency.

Of course, many if not most sedimentary formations cannot be traced long distances to begin with. We see the concept of 'fades change' employed to snow-over the lateral fusing of diverse lithologies into the horizontal time-planes manufactured by uniformitarian geology. For instance, attempts to correlate the Lower Palaeozoic of North America, based on overlapping lithological successions, met with failure:

> *'As the Palaeozoic formations of other districts of North America were studied, it was found that the correspondence between them and the New York formations was not as close as could have been hoped. Not only did the lithic character of the strata change when traced away from the type locality, but the superposition did not, in many cases, correspond, some formations being absent altogether, while others were found to be united in a single unit, often of slight thickness. Even the fossils which had gradually come to be looked upon as the surest indicators of position in the geologic scale, appeared in horizons not known to contain them in New York.* '[33]

DID THE GEOLOGIC COLUMN ORIGINATE BY THE TRACING OF SEDIMENTARY STRATA ACROSS CONTINENTS?

Snelling informs us that the 19th century European geologists were able to physically trace the stacking of the continuous sequences of strata from nation to nation, and then encountered similar sequences on other continents. The actual history of the development of the Phanerozoic geologic column appears to be substantially different. If anything, the history of the tracing of sedimentary beds and sequences across continents shows the miserable failure of such efforts in the development and corroboration of the geologic column.

To begin with, the observing of how the strata physically relate to each other on a subcontinental scale was **not** a prerequisite for the naming of geological systems. Consider the history of the naming of the Lower Palaeozoic systems:

> *'The Devonian thus went its way in peace, but problems awaited to plague the Silurian and Cambrian. From the first both systems had been imperfect: groups based upon rocks exposed in different regions, without known overlap and with nothing but inference to tell which was the older.* '[34]

As we can see, the Silurian and Cambrian were named as systems **before** their relative positions had been fixed through superposition. Were such a superposition not discovered, it is acknowledged that the Cambrian would have been recognised as a facies of the Silurian.[35]

Nor is it correct that the lithologies could be traced, bed by bed, over long distances in order to decipher their respective stratigraphic relationships. Long ago, Spencer[36] had cogently dismissed the 'continuous strata' argument, as a putative validation of the geologic column, by noting the impossibility of tracing strata from continent to continent due to the presence of intervening oceans. However, the force of Spencer's reasoning is not confined to trans-oceanic correlations of strata. For instance, the presently-accepted divisions of the Lower Palaeozoic had been worked out by comparing the **local** stratigraphic successions found in the British Isles, Scandinavia, Bohemia, and North America.[37] The tracing of strata between these key localities would have been physically impossible if only because of the fact that each one of the localities is separated from the other three by bodies of water. Already by the mid-19th century, both regional and transcontinental correlations were primarily based upon index fossils.[38]

What about regions which occur within contiguous land masses? The long distance tracing of strata would have been physically impossible, even if outcrops had been universally available at will, simply because the strata representing the type localities peter out. I have already discussed the difficulties of correlating the Palaeozoic of New York with that of adjoining US states. The same holds for the classic western European type localities. For instance, lithologies within the British Isles had initially been assigned to different ages because of their great differences over short distances.[3,] The notion of tracing lithologies thousands of kilometres is pure fantasy. Even if Murchison *et al.*,[40] had attempted to trace the lithologies in central Russia, step by step, back to western Europe, they could not possibly have done so. This is because **no** Permian strata is continuous, either directly or indirectly, from the Urals back to western Europe. Neither, for that matter, are the Carboniferous, Silurian, Ordovician, or the Cambrian.[41]

Let us now review the first attempts to correlate the sedimentary lithologies of Europe. The fact that lithological and stratigraphic successions do not 'standalone' apart from biostratigraphy is proved by the many lithic correlations

which had to yield to biostratigraphic re-correlation. For instance, lithologies in the Baltic states and adjacent Russia had been correlated with the New Red Sandstone based on lithological similarity and the shared intercalations of gypsum and halite. This seemed self-evidently valid until the discovery of Lower Palaeozoic-type fossils in them.[42] Correlations based on similar successions of strata fared no better. For instance, when on his famous trip to Russia that resulted in the naming of the Permian system, Murchison *et al.*[43] observed a succession of coal-bearing rocks atop of red beds (at Karakuba, central Russia). He naturally thought that this sequence was an obvious Russian counterpart to the coal measures (Carboniferous) and underlying (Devonian) Old Red Sandstone in England. However, both Russian units had to be assigned to the Carboniferous because the red beds were regionally found to overlie beds with Carboniferous fauna.

Let us now move on to the first efforts to 'export' the geologic column from western Europe to North America. Snelling's supposition that the same or similar lithologies could be traced from one continent to another is at variance with the facts. Initially, the Silurian Rochester Shale of New York had been correlated with Lias (Lower Jurassic) of England, based on lithic composition, superposition, and structural character of the rocks.[44] In a similar vein, Ordovician and Silurian sandstones in east-central Canada were initially correlated with the Devonian Old Red Sandstone of England.[45]

The failure of correlations based upon lithological successions failed even on a local level. Spencer[46] provided examples of backpedalling on lithostratigraphic correlations, within the British Isles, imposed by biostratigraphic contradictions. The errors also occurred on a subcontinental scale within North America no less so than they had in Eurasia. For instance, based on similarities in lithology and superposition, a Cambro-Ordovician sandstone of the central United States had been correlated with the Triassic sandstones of the eastern United States.[47] Many other examples are given by Grabau, to which I refer the interested reader.

DO 'GOOD' ISOTOPIC DATES VALIDATE THE METHODS?

Overall, Snelling comes across as far more sympathetic to isotopic dating when reviewing my article[48] than he does in his own research on the U-Th-Pb systems,[49] and, for that matter, in his review of Dalrymple's book.[50] He is of the position that we cannot discount isotopic results because many results are 'good'. In private conversation, Snelling informed me that he did not mean his remarks about my work personally, or even against my work *per se.* However, the reader gets the unavoidable impression that I 'buried my head in the sand' and ignored 'good' isotopic results, and am in need of being 'shocked' by their ubiquity.

I therefore feel compelled to set the record straight. I did in fact explicitly consider the 'there-are-more-good-than-bad-results' argument,[51] finding it wanting (more on this in the ensuing paragraphs). I also considered the 'consistency and concordancy of isotopic results' argument, finding it invalid.[52] For instance, concordant results are rejected by uniformitarians whenever they don't 'fit'. Clearly, if even concordant results are rejected by uniformitarians when they don't suit their preconceptions, neither should we buy into their special pleading by accepting concordancy as proof of validity.

Far from supposing that 'good' results don't exist or are uncommon, I had cited the numbers of tie-points then available for the Phanerozoic geologic column.[53] Ironically, I had intended to compare the relative numbers of 'good' and 'bad' dates but was forced to drop that approach upon learning, and then documenting, that most discrepant results go unpublished.[54] I also discussed the difficulty in discriminating between 'good' versus 'bad' dates in view of the fact that most igneous bodies have wide biostratigraphic brackets, so could yield a variety of isotopic results without any of them being 'bad'.[55] A further complication is the fact that existing biostratigraphic brackets of plutons can be re-interpreted in order to 'resolve' conflicts between the bracket and newly-acquired isotopic results.[56] Finally, it is difficult to determine how many Rb-Sr isochrons are 'real', because, among other things, the slope (age) can frequently be altered by selectively excluding certain points from the isochron.[57]

Despite all these obstacles, I did come to grips with the 'good' results, estimating the relative frequency of 'good' versus 'bad' dates when discussing the suitability of glauconite dating versus that of biostratigraphically-bracketed igneous bodies.[58] According to some estimates, more than 50 per cent of results from the authigenic mineral glauconite are 'bad', and isotopic results from tightly-bracketed igneous bodies yield 'bad' results more frequently than does glauconite. If this chain of reasoning is valid, then more than half of all radiometric results are 'bad' relative to biostratigraphy. If not, it at least demonstrates that many uniformitarian geologists are willing to accept a dating method as reliable even though upwards to half (or even more) results from it are 'bad', so long as it suits their preconceptions.

Snelling is, of course, correct that I did not present a comprehensive theory accounting for all radiometric results, mainly because I have insufficient background in geochemistry (especially isotopic and trace-element geochemistry). I believe, however, that the answer will come through the study of isotope geochemistry. For instance, creationists (including myself[59]) have considered mixing lines as non-temporal causes for isochrons. As for K-Ar dating, the amount of argon may be inversely related to the rate of magma emplacement. For instance, modern magmas are produced at low rates, hence usually can degas all their argon and yield zero ages. During the Flood, magmas were generated at such prodigious rates that much argon remained

entrapped in them, hence multi-million-year apparent ages.

Let us look more closely at Snelling's position that we cannot discount isotopic results because many results are 'good'. 'Good' based on what? Agreement with uniformitarian preconceptions, of course. Let's remember that the great antiquity of the Earth and relative ages of strata had been a mainstay of uniformitarian geology long before radioactivity had even been discovered. These 19th century concepts served as a filter as to which methods and results would be accepted as valid by uniformitarians. Furthermore, one wonders what percentage of isotopic results would have to be 'bad' before Snelling would be satisfied that all isotopic results are discredited? And on what basis?

In conclusion, I maintain that we are excessively conciliatory towards uniformitarian geology if we accept the premise that most results must be 'bad' before we reject the credibility of all the isotopic results. In fact, there are many situations in the evaluation of evidence where only a minority of 'bad' results discredit all the results. In a court of law, one lie by a witness is sufficient to discredit everything that the witness has said. It is unnecessary to prove the falsehood of all or most of the remaining testimony of the witness. Likewise, since the isotopic results impugn the very credibility of the Word of God, we should at very least hold the methods to the most stringent possible tests, including the refusal to accept them *en toto* even if hypothetically only a significant minority of them are 'bad'.

Theological issues aside, there are several purely scientific arguments for an overall jaundiced eye towards all isotopic results. One obvious one is the many layers of unproved assumptions and extrapolations inherent in isotopic dating. Uniformitarians themselves have brought upon themselves a very heavy burden of proof vis-a-vis dating. They commonly equate the certainty of the great antiquity of the Earth with the certainty of its sphericity. On that ground alone we should hold them to a very stringent standard of evidence, one that would, for example, allow even a significant minority of 'bad' dates to discredit the hypothetical majority which consists of 'good' ones. Secondly, uniformitarians once dogmatically called their isotopic results 'absolute ages', something they have since backpedalled from. What else aren't they telling us? Thirdly, there is usually no independent textural evidence to justify the rejection of 'bad' results,[60] so results are arbitrarily rejected on an 'after-the-fact' basis. For that reason alone, the 'good' results are questionable at best. At least some may be fortuitous.

Until we see all the results, and standardise our sampling of igneous bodies for isotopic dating, we cannot know about the relative frequency of 'good' and 'bad' results. Of course, 'bad' results may not be in the minority in the first place, as noted earlier. Finally, I documented the fact that, because of the frequency of 'bad' results, some uniformitarians think little of isotopic dating as a whole.[61] Evidently they are not impressed by the 'there-are-many-good-results' argument. Neither should we creationists. At very least, I see no reason to buy into the transparent special pleading of uniformitarian geology, including its isotopic dating.

FUTURE RESEARCH

With reference to the ongoing project of studying stratigraphy through the use of computers, I have the following concern. Since computers do not think, I hope that uniformitarian assumptions are not tacitly fed into the programme, thus guaranteeing results unfairly congenial to conventional uniformitarian geology.

Meanwhile, a computerised study could be attempted with the data I have gathered. Table 1 of my essential nonexistence chapter[62] could be entered into the computer, either by being manually typed-in or imported from the printed page with optical-scanning software. A programme such as Dbase4 could then be used for complex searches involving different combinations of geologic periods, taking into account the adjacent squares of continental areas, etc.

FLOOD VERSUS POST-FLOOD: CLARIFYING MY POSITION

The remainder of this essay is an answer to many of the claims in the Creation Ex Nihilo Technical Journal, volume 10, number 1. I conclude that almost the entire geologic column, before Miocene and Pliocene, was deposited during the Flood year or perhaps within a few years thereafter. The excellent paper by Holt[63] should not obscure the fact that commonsense alone dictates that the post-Flood could not do geologic work comparable to a global Flood, at least on a young-Earth time-scale. Otherwise, we would need either or both a pre-Flood near-global Flood, or a post-Flood one. Both are clearly unscriptural. A contemporaneously published paper by Dr Henry Morris demonstrates that the Cambrian-Pliocene Flood makes the most sense biblically.[64] The principle of Occam's Razor should dictate that one Flood should be recognised as the main cause of the Earth's crustal geology. Whatever problems there are with such a position should be solved with careful research, not by jumping at *ad hoc* post-Flood floods or large-scale post-Flood changes.

Allowing much of the Miocene and Pliocene to be post-Flood obviates the 'problem' of the animals having to return to the same places they had lived before the Flood. (Note that only Miocene and Pliocene contain modern genera of mammals, and in nearly the same geographic locations as their live counterparts live today).

I have been misunderstood by some authors. When I wrote my article on diluvian interpretation of ancient cyclic sedimentation,[65] I assigned the Carboniferous coal-bearing rocks to the recessional phases of the Flood. But this was true of coal-bearing rocks, and was never meant to suggest that most or all Carboniferous is Flood-recessional. In fact, where Carboniferous is overlain by Mesozoic and/or Cainozoic (especially a thick mantle of the same),

Carboniferous may be mid-Flood or even early-Flood. In my essential nonexistence work,[66] I had toyed with the idea of **some** of the Mesozoic and Cainozoic being post-Flood, but never suggested that most or all of it was post-Flood.

Overall, I must say that I find Robinson's cavalier dismissal of one-year explanations as *ad hoc* especially galling in view of the fact that it is he who makes the most transparently and absurdly *ad hoc* speculation of all, repeating Glenn Morton's canard about the Flood obliterating **every trace** of land animal and plant.[67] Indeed, such a thorough annihilation would be expected from something like a Mars-sized impact on Earth (which would have destroyed the Ark and virtually sterilised the planet), not a global Flood. Then again, one-year suggestions are not *ad hoc*, but are supported by scholarship.

Proponents of the Meso-Cainozoic post-Flood insist that assigning such things as hardgrounds and dinosaur nests to the Flood year itself is impossible, or does 'not do justice' to the evidence. On the contrary: until we have **hard evidence** on such things as how long it takes a hardground or dinosaur nest to form (and under what environmental conditions), such claims can only be baseless. Of course, with extinct animals, we may never know. And since assigning much of the geologic column to the post-Flood causes vastly more problems than it ostensibly solves,[68-69] the burden of proof should be placed on those who insist that it must be done, not on those who try to fit nearly the entire geologic column into one year.

We obviously also need a thorough understanding of depositional factors during the Flood. To insist that pure chalks could not have been transported without becoming mixed with elastics[70] implies some kind of detailed knowledge of the precise sedimentological conditions compatible with long-distance transport of pure chalks. Tyler provides no evidence of this. Perhaps chalk particles could have been transported in a slurry or turbidite-like fashion (it is common knowledge that turbidites can get transported long distances without becoming admixed with foreign materials). Provenance effects are also paramount. If the accumulation of chalk that got eroded had been particularly thick, there might have been little or no elastics around to get admixed with the chalk particles. Then again, the purity of chalks must be kept in perspective in that many or most chalks are not particularly pure. Hence the western European chalks and their purity may have been the result of an unusual set of within-Flood circumstances.

The Meso-Cainozoic post-Flood proponents dust off some old arguments against the workability of such things as hydrodynamic sorting, differential escape, and ecological zonation in the stratigraphic differentiation of fossils. In my diluviological treatise[71] I have demonstrated that most index fossils do not directly succeed each other in the same location. For that and other reasons, it is not necessary for such things as differential escape or hydrodynamic sorting to have been particularly efficient in order to have added up to the highly-differentiated stratigraphic appearance of fossils that is observed.

My TAB model[72] is not complex nor hard to understand. In summary, it states that living things were passengers on sinking blocks of crust, and there was a preferential association of type of living thing and the relative tendency for the crust to downwarp during the Flood. Consequently, there is no need at all to be perturbed about such things as chalks not appearing until the upper part of the conventional geologic column. The same holds for such things as land-vertebrate footprints, dinosaurs and dinosaur nests, etc. Indeed, the TAB concept explains the stratigraphic differentiation of fossils within and between **both** Palaeozoic and Meso-Cainozoic deposits. There is no need to imagine fantastic (in fact, magical) occurrences such as the total obliteration of land remains during the Flood, as Robinson does.[73]

Furthermore, the TAB concept readily explains why each geologic period (and **not** only the Lower Palaeozoic ones) contains **marine** faunas that are substantially different from that of every other geologic period. The Meso-Cainozoic post-Flood notion, by contrast, is totally incapable of accounting for this fact.

SOME LITTLE-APPRECIATED FACTORS TO CONSIDER

Those who doubt that repeated horizons of footprints or dinosaur nests can happen within the Flood year seem to have little inkling of the ease with which vast areas of land could have been exposed, and then re-inundated, during the Flood. Consider the Flood waters being a maximum of 1km deep. A **minuscule** one degree difference in slope over a transect 100km long, caused by the most minor of tectonic movements, translates to a 43 km swath of exposed land. It is obvious that small, medium, and large areas of exposed land (for brief to longer periods) could **easily** have formed during the Flood year itself. Such exposures not only covered large areas, but readily migrated over considerable geographical distances. For this reason, there is no need to be perturbed by the fact that footprint-bearing horizons commonly overlie appreciable thicknesses of previously-deposited sediment.

It is also evident that these large exposed areas, owing to their periodic geographic distance from active Flood action, must have been at times compatible with normal egg-laying and strolling behaviour. Of course, this concedes the validity of the premise that dinosaurs could not lay eggs under stressful conditions, something that we do not know and probably never will know.

As for footprints in the geologic record not usually being indicative of running, this argument falsely assumes that animals must have been constantly frightened during the Flood, and/or never got habituated to Flood-related stresses. If there were fairly large areas temporarily exposed, there is no reason why the animals could not have calmed down and made normal footprints. Of course, if dinosaurs were

ectotherms or part-ectotherms, they could not have run for significant intervals at a time without experiencing exhaustion. Clearly, normal strolling behaviour among dinosaur footprints is not evidence for the Mesozoic being post-Flood.

Another factor to consider is the rate of footprint formation. A single horse can produce over 10,000 footprints on one day.[74] Thus there is no need for large numbers of individual vertebrates to have still been alive by the middle of the Flood. On the contrary; relatively few surviving individual land animals could have formed the fossil footprints that we find. This fact becomes even more significant when we keep extensive footprint horizons in perspective. Note that most known footprint horizons in the geologic record **don't** cover vast areas nor have considerable numbers of footprints. Those large ones (such as in western North America, discussed by Oard[75]) are exceptional, and could have formed as a result of atypically-favourable conditions during the Flood year.

Since dinosaur eggs can survive transport without disintegration, as cited by Garner,[76] it is possible that at least some so-called dinosaur nests are allochthonic assemblages of once-floating eggs. If so, whirlpools of Flood water could have transported them into spherical or ovoid assemblages, or into windrows (so-called rookeries). Until we have flume and/or field experimental data on the patterns that can be created by water transport, the argument that water transport could not have deposited eggs into nest-looking configurations will carry no weight. The claim that there are superposed horizons of dinosaur nests[77] must also be carefully re-examined. If valid, we must abandon the mental straightjacket that insists that each horizon was laid down before the overlying one (much as occurs with sedimentary layers on the ocean floor). On the contrary; if sediments had been deposited in a prograding fashion, it is likely that at least some of the dinosaur nests were contemporaries of each other.

Proponents of the Meso-Cainozoic post-Flood have uncritically accepted the prolonged altriciality of dinosaur young [hatched very immature and have to be fed in the nest by the parents] as proven fact. Now a recent study[78] challenges this. The growth plates of juvenile dinosaurs turn out to be similar to those of precocial birds [hatched with a complete covering of down, and able to leave the nest at once and seek food], and comparative reproductive biology of birds and reptiles indicates that the nest-attending behaviour of mothers does not imply altricial young. If correct, the study implies that the laying of eggs, hatching of young, and fully independent behaviour of dinosaur hatchlings all must have taken place within a very short period of time during the Flood year. It also underscores once again how proponents of the Meso-Cainozoic post-Flood are prone to jump to conclusions, favourable to long periods of time, based on tenuous or questionable evidence.

THE CONTINUOUS LOWER-PALAEOZOIC FALLACY

Robinson[79] would have us believe that the entire Lower Palaeozoic had to be deposited before any Mesozoic and Cainozoic were deposited above it. This position is an unwarranted and inexcusable sop to uniformitarian geology. Indeed, in the absence of correlation by index fossils, there is no basis whatsoever for concluding that the Lower Palaeozoic had to be deposited everywhere at the same time. As noted earlier, lithological similarity is not proof of coeval deposition. Separate tectonic events could have formed even the sheet sandstones, as discussed earlier.

Long-distance correlations of bentonites have also been cited in support of essentially-instantaneous deposition of Lower Palaeozoic over vast areas. However, it can be shown that even medium-distance correlations of bentonites can be equivocal, and there are examples of bentonite beds whose correlation had to be reinterpreted as a result of conflicts with index fossils.

Robinson's map showing the Lower Palaeozoic of the central United States,[80] is essentially a cartoon, as it completely glosses over the many differences within Lower Palaeozoic strata on a transcontinental scale. In reality, the Lower Palaeozoic is **not** a monolithic carpet that had to be laid down as a unit before anything was emplaced above it anywhere. Robinson cites Sloss to the effect that cratonic sediments are very similar throughout their extent. I have observed these rocks, and have, instead, become impressed by the **differences** seen in outcrops as little as tens of kilometres apart. Clearly similarity and difference, like beauty, are in the eye of the beholder.

Robinson has argued against the validity of the TAB concept as an outgrowth of his continuous Lower Palaeozoic fallacy. In reality, Lower Palaeozoic strata, along with anything deposited above it, could have been laid down at different times in different sedimentary basins and geosynclines, subject only to the Law of Superposition at a **local** or **regional** level (certainly **not** the continental or intercontinental level as demanded by uniformitarian geology or its speeded up version employed by the Meso-Cainozoic post-Flood advocates).

Another frivolous argument is the one about the Tertiary lasting 200 days[81] if Oard's model is correct. In actuality, nothing constrains the Flood to particularly narrow time intervals for deposition or erosion at particular regions during the Flood year itself. One Jurassic rock could be laid down during the Flood while an Ordovician rock was deposited elsewhere. Likewise, one Jurassic rock could be late Flood, whereas another Jurassic rock could be early Flood. To insist that the geologic systems followed each other in the order of deposition on a global basis, during the Flood, as they do in standard uniformitarian geology (albeit seven to eight orders of magnitude faster), is completely unjustified by any empirical evidence.

'REWORKING' RATIONALISATIONS FOR STRATIGRAPHICALLY-DISCREPANT FOSSILS

In my second anthology,[82] I have tabulated over 200 instances of stratigraphically-discrepant fossils. Robinson[83] has attempted to belittle this evidence by asserting that this phenomenon

(1) is rare,
(2) usually involves only one geologic period,
(3) almost always involves microfossils (and thus is expected), and
(4) shows reworking much more common than downwash.

All of his arguments are based on specious reasoning. To begin with, Robinson provides no evidence to support his assertion that reworking is a rare occurrence. By contrast, I discuss and document, in conjunction with the table of 200 discrepant fossils, that this phenomenon is rated **common,** often not reported, etc. I also document the fact that often stratigraphers don't know whether to accept an anomalous find as a stratigraphic-range increase, or to attribute the anomalous occurrence to reworking. This hardly would be the case if 'reworking' was a trivial problem, as Robinson claims. More recently, Miall[84] referred to (alleged) reworking phenomena as posing a 'great difficulty' for biostratigraphers, and gave examples of **entire formations** that have more 'reworked' than ostensibly indigenous fossils! All this hardly is consistent with the premise that 'reworking' is an uncommon or freakish occurrence.

Yet even if it was an infrequent phenomenon, it would not diminish its importance in the slightest. Everyone familiar with biostratigraphy knows that index fossils may be uncommon in a formation, and sometimes an entire formation is dated by the presence of a solitary index fossil. Therefore, to the extent that 'reworked' fossils may be rare, it must be kept in perspective of the fact that 'correctly-emplaced' fossils are also often rare.

As for the fact that 'reworking' usually involves only one geologic period (assuming that it is not an artifact of reporting), Robinson's argument completely ignores the transitive property of 'reworked' faunas. For instance, if Permian faunas are 'reworked' into Triassic and Triassic faunas are found 'reworked' into Jurassic, then, following the usual logic and methodology of using index fossils, the Permian-Triassic-Jurassic junction alone is made contemporaneous.

The claim that 'reworking' almost always involves microfossils and not macrofossils is probably nothing more than a self-fulfilling practice, based on the **preconception** that microfossils are easy to rework, whereas macrofossils are not. In other words, a stratigraphically-discrepant microfossil is liable to be blamed on reworking, whereas a stratigraphically-discrepant macrofossil is much more likely to be recognised as a stratigraphic-range extension. While not included in my table, there are many occurrences of 'reworked' ammonites. This is documented in my work.[85]

Of course, all the foregoing is rather academic in view of the fact that there is a very prosaic explanation for the microfossil/macrofossil imbalance in my table. I had constructed this table primarily by examining micropalaeontological journals, so the microfossil/macrofossil trend in it, which is so exciting to Robinson, is hardly surprising or significant.

The preponderance of reworking over downwash may also be little more than indicative of geologists being more willing to invoke reworking over downwash. Indeed, I have shown in my diluviological treatise[86] that biostratigraphic conflicts are usually resolved in a younger direction. For instance, if a uniquely-Cambrian and uniquely-Carboniferous fossil is found to coexist, it is much more likely that the Cambrian fossil will have its range extended upwards into Carboniferous, or the Cambrian fossil will be labelled 'reworked' into Carboniferous, than the opposite (that is, having the Carboniferous fossil's range extended downwards into Cambrian, or labelling the Carboniferous fossil 'downwashed' into Cambrian).

Finally, the entire question of 'reworking' must be kept in perspective in that many if not most fossils found ranging beyond one geologic system are not considered reworked, but merely subject to stratigraphic-range extensions. This matter is discussed and quantified in detail in my diluviological treatise. Long ago Spencer[87] pointed out how illogical biostratigraphy really is: certain fossils are picked out as time-markers (with long-ranging forms arbitrarily ignored), and often so-called index fossils are found to coexist with those of other geologic systems upon further collecting. Index fossils can come and go, but the core premise of 'global onion skins' of fossils is protected from serious re-examination.

CONTRASTING PRE- AND POST-FLOOD HUMAN REMAINS

Why should we suggest that human remains have not been preserved in Flood-deposited sediments in view of the fact that they obviously occur in post-Flood sediments? The answer to Robinson's objection[88] is obvious: the two are entirely different. There is a vast difference between the dilution rates of human remains entombed in Flood versus post-Flood sediments. The skeletons we find in post-Flood sediments occur in caves, gravels, etc. Very little sediment has accumulated around them, so their burial is essentially two-dimensional (a matter of area, with negligible sediment overlying them). During the Flood, by contrast, the human remains were buried in widely different layers **in addition to** widely different areas. Since their burial is three dimensional (area plus considerable thickness), they are diluted by hundreds of millions of cubic kilometres of sediment. The probability of encountering even one pre-Flood skeleton is therefore very small, if not nil.

CIRCULAR REASONING

Robinson denies that there is circular reasoning in the establishment of the geologic column. His denial can be refuted by pointing out the admissions by uniformitarians (these are cited in many creationist publications). The fact that the Law of Superposition does **not** circumvent the circular reasoning of using index fossils is discussed in my diluviological treatise,[89] as well in my anthology 1.[90]

I document the circular reasoning in use of biostratigraphic ranges of fossils, and in multiplying taxon names at different stratigraphic intervals, notably at system boundaries.[91,92] Oard[93] provides another example of circular reasoning: a rock dated as Tertiary instantly became Mesozoic upon the discovery of a dinosaur remain within it. Earlier, I had documented a similar example involving dinosaurs.[94]

As for overthrusts, Robinson[95] asserts that these features were first discovered by geophysical means. In doing so, he is egregiously misrepresenting the facts. Many earlier geologic works (diluvial as well as uniformitarian) make it clear that overthrusts were inferred because fossils were in 'wrong' order, not from physical evidence. As to whether or not the degree of evidences for deformation provide independent justification for concluding that an overthrust took place, it is a matter of judgment. I have done field work on an overthrust in Tennessee (USA). Evidence for motion (for example, slickensides) was there, but they were quite meagre considering the colossal masses of rocks which supposedly slid one over the other. I have observed more dramatic slickensides within Carboniferous coal-bearing sequences (where no overthrust is claimed) than in the contact zone of the overthrust in Tennessee.

In my anthology 2,[96] I considered alleged overthrusts in Canada and Russia. There was little folding seen, and gouge was either minor or absent. In addition, the overall amount of deformation was admitted to be small. In conclusion, while there is **some** evidence of motion, it is admittedly meagre, and the contact between the supposedly-inverted formations is admitted to be often no more impressive than a mere bedding plane.

MORE-THAN-ONE-YEAR 'EVIDENCES': THE INCONSISTENCIES

It is interesting and ironic to see how the proponents of the Meso-Cainozoic post-Flood highlight the supposed evidences for more-than-one year burial in Mesozoic and Cainozoic, all the while glossing over or ignoring the same for Lower Palaeozoic rocks.

They emphasise the biostratigraphic differentiation seen in the Meso-Cainozoic, while glossing over the same in the Lower Palaeozoic. In truth, the biostratigraphy within the Lower Palaeozoic is no less complex than that of the Meso-Cainozoic. If, for example, such things as the disappearance of coal near the P-Tr boundary demand a post-Flood explanation, then why does an Ordovician trilobite horizon not demand the same? Conversely, if the Flood can account for biostratigraphic differentiation in the Lower Palaeozoic, it can also do the same for the Meso-Cainozoic. Either way, the Meso-Cainozoic post-Flood position has no coherence and no validity.

Similar considerations are applicable to hardgrounds. They occur not only in the Meso-Cainozoic, but also the Lower Palaeozoic. For instance, there exists a widespread hardground horizon within Ordovician carbonates over much of the Illinois Basin.[97] Alleged *in situ* fossils occur in the Palaeozoic no less so than the Meso-Cainozoic.[98] 'Fossil soils' occur not only in the Meso-Cainozoic, but also the Palaeozoic (for an Ordovician example, see Driese and Foreman[99]). Will people like Robinson and Tyler now insist that even the Lower Palaeozoic must be post-Flood, eliminating the Flood altogether (see below)?

Now let us consider iridium anomalies. They occur not only at the K-T and P-Tr boundaries, but also within the Palaeozoic, including the Lower Palaeozoic.[100] Again, if the Flood can account for them in Palaeozoic, it can also do the same in the Meso-Cainozoic. As for iridium anomalies being concentrated at system boundaries, we have to first control for different sampling intensity. Not until **all** sedimentary rocks have been sampled at equal intensity can we know if iridium anomalies are indeed confined exclusively to system boundaries.

THE VANISHING FLOOD

Where does all this lead? To become consistent, one has to apply the same standards to the **entire** sedimentary record. The classical diluvialist strives to understand the entire Phanerozoic record as the result of one Flood. He is consistent. So is the anti-creationist in his face-value acceptance of more-than-one-year evidences for Meso-Cainozoic as **well** as for the entire Palaeozoic.

The gratuitous acceptance of uniformitarian claims can only cause the Flood to become progressively diminished in importance as a factor in Earth's history, until it is squeezed out altogether: hence the vanishing Flood.[101] This first occurred in the 18th and 19th centuries. The Flood was relegated to the Quaternary (whence the still-used term diluvium) before being abandoned altogether. In the 20th century, the Meso-Cainozoic post-Flood position has tended to be a way station prior to the complete rejection of the Flood. For instance, the Flood was first relegated to Palaeozoic-only by people like Davis Young and Glenn Morton before being rejected entirely by them. At least one thing can be said in their favour: their thinking is now logically and methodologically consistent.

I hope that diluvialists will recognise that the only sensible understanding of geologic phenomena is the recognition that the Noahic Flood was the cause of nearly all the Phanerozoic geologic column.[102] To assign much of this geologic record to either the pre- or post-Flood does not

make sense, either scientifically or scripturally. Of course, any and all problems should be dealt with by research, not retreats to acceptance of the geologic column (or its speeded-up version).

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John Woodmorappe has a B.A. and M.A. in geology, and a B.A. in biology. He is a science teacher and also a research fellow at a university in the USA.