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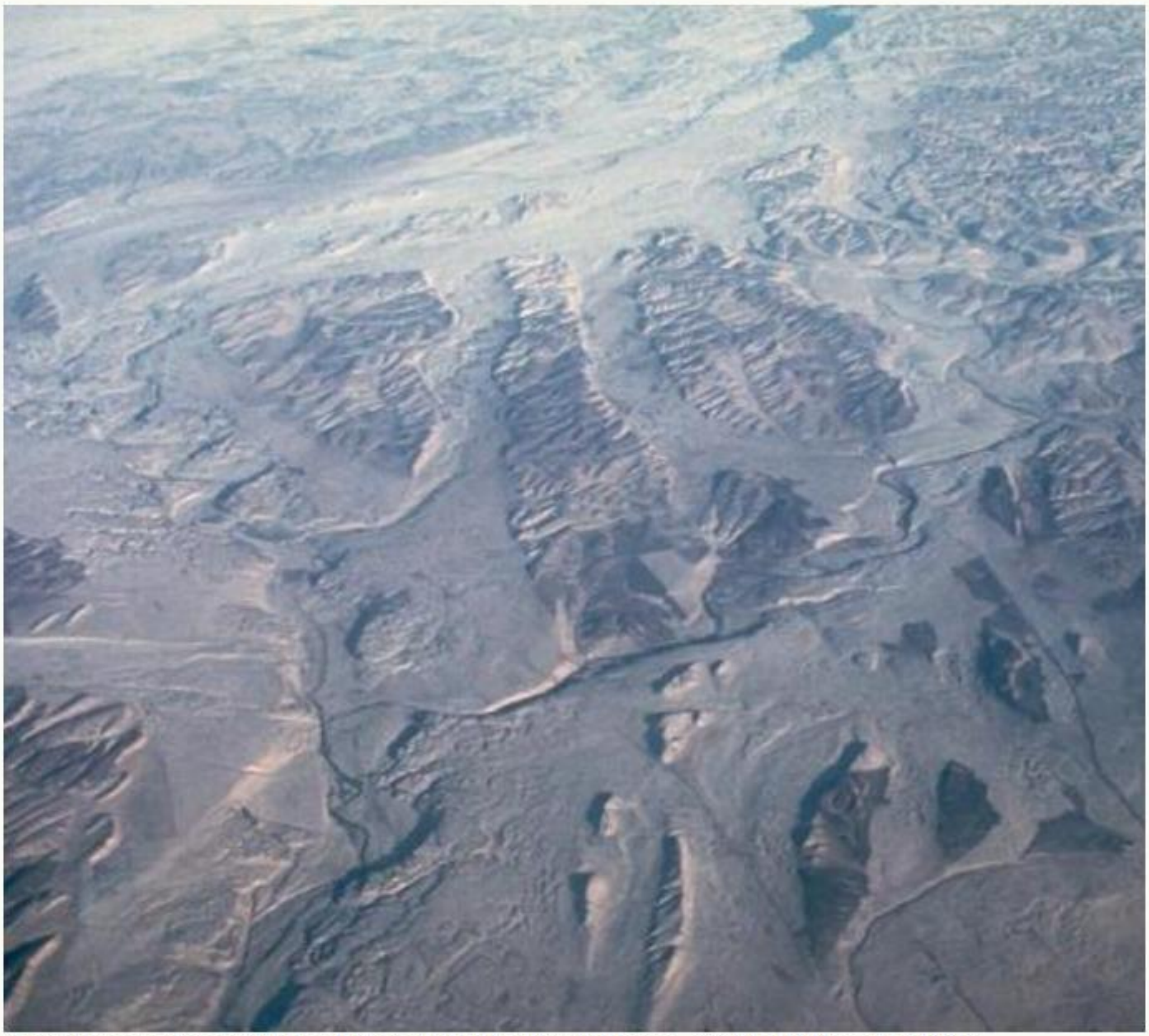
TABLE OF CONTENTS



Aerial view of the Channeled Scablands, Washington. © Marli Bryant Miller, http://www.marlimillerphoto.com. Used by permission.

Vol 31, No 3 (2011)

Table of Contents

Articles

Features

Reviews

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ARTICLE

The Defeat of Flood Geology by Flood Geology

The ironic demonstration that there is no trace of the Genesis Flood in the geologic record

Phil Senter

Introduction

According to the young-earth creationist (YEC) paradigm, the narratives recorded in the biblical book of Genesis are accurate historical records of actual events. Within that paradigm, the Flood of Noah is considered to have happened as described in chapters 7 and 8 of Genesis. According to the narrative, the rain of the Flood began in the second month of Noah's 600th year. The rain lasted 40 days, at the end of which the water level was more than 6 meters above the height of the highest mountains. All humans and non-aquatic animals perished, except those that were on the Ark with Noah. The earth remained flooded for 150 days, but by the end of that period the waters had receded enough for the Ark to rest on the "mountains of Ararat" (not a single Mt Ararat, as is commonly but incorrectly assumed). About two and a half months after the Ark came to rest, the waters had receded enough to expose the tops of mountains. By the end of the second month of Noah's 601st year, "the earth was completely dry" (Genesis 8:14, New International Version). The account therefore describes a flooding event in which water rose for 40 days and receded for the rest of a single year, during which recession the planet was completely submerged for 150 days.

In 1961 Whitcomb and Morris published *The Genesis Flood*. The authors presented the hypothesis that the Flood was responsible for the deposition of all Phanerozoic sedimentary strata stratigraphically below the Quaternary. They also questioned the validity of the stratigraphic principles upon which the geologic column—the sequence of time divisions to which geological deposits are assigned—is based (see Figure 1). Their publication was not the first to espouse these views but its popularity precipitated a deluge of Flood-related research by young-earth creationists in an attempt to find support for the book's conclusions. Ironically, that outpouring of research has ultimately led to the falsification of most of the book's geological interpretations.

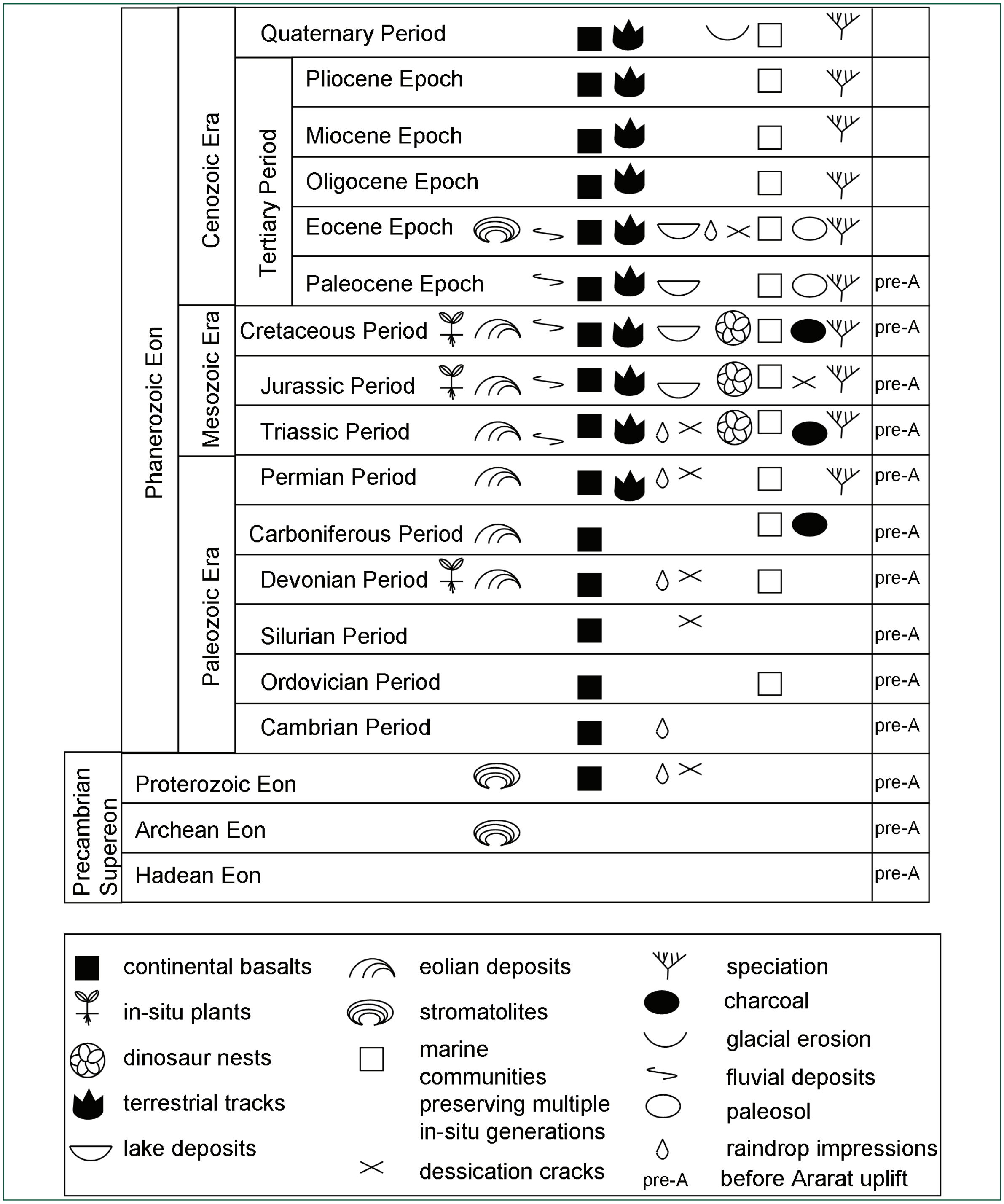

Figure 1. *Stratigraphic distribution of sedimentologic and other geologic features that Flood geologists have identified as evidence that particular strata cannot have been deposited during a time when the entire planet was under water (middle column) and distribution of strata that predate the existence of the Ararat mountain chain (right column). Note that data collected by Flood geologists show that a period of worldwide submergence cannot have spanned the entirety of any period of the Phanerozoic Eon, nor any epoch of the Tertiary Period.*

The body of work exemplified in that maelstrom of research, in which researchers have attempted to reconcile geological data with the Genesis Flood story, has come to be called Flood Geology. Through its history Flood Geology has had four main foci:

(1) the attempt to connect any bit of geological evidence of ancient catastrophe or high-energy water activity to the Flood or its potential causes or effects (for example, Akridge 1998; Sigler and Wingerden 1998; Oard and others 2007),

(2) descriptions of hypotheses of geological causes of the Flood, often ad hoc and with little or no supporting physical evidence (for example, Woodmorappe 1998; Brown 2003; Samec 2008),

(3) attempts to reconcile the biblical description of the Flood with contradictory physical evidence such as appearances of long passages of time (for example, extreme thicknesses of strata, beds of invertebrate exoskeletons representing multiple *in situ* generations, and so on) or subaerial deposition in the geologic record (for example, Oard 2006; Matthews 2009), and

(4) attempts to locate or narrow down the pre-Flood/Flood boundary or Flood/post-Flood boundary in the geologic record (for example, Scheven 1990; Oard 2007; Whitmore and Garner 2008).

By the 1990s, after much fieldwork and theoretical study, most Flood geologists had conceded that mainstream stratigraphic principles are valid and had accepted the sequence of time periods in the geologic column, although most continue to maintain that those time periods together total little more than 6000 years (for example, Robinson 1996; Tyler and Coffin 2006; Whitmore and Garner 2008). A few holdouts continue to doubt the validity of the geologic column and maintain that "Mesozoic" and "Cenozoic" strata were deposited simultaneously (for example, Woodmorappe 1990; Oard 2001; Reed and others 2006; Matthews 2009), but they are now the exception rather than the rule.

The post-1980s recognition among most Flood geologists of the validity of the geologic column and the stratigraphic principles upon which it is based led to a plethora of studies in the 1990s and beyond that fall into the fourth category above. Such studies are of particular interest here. The fact that alleged Flood deposits are stratigraphically scattered between deposits indicating subaerial exposure or long periods of calm has not been lost on the Flood geologists who have performed such studies. As a result, the long-cherished hypothesis that most Phanerozoic strata are Flood deposits (Whitcomb and Morris 1961) is questioned by many of today's Flood geologists, and debate rages as to which portions of the geologic column represent the Flood year.

There is general consensus that the beginning of the Flood is recorded in Precambrian strata (for example, Robinson 1998b; Wingerden 2003; Wise and Snelling 2005; Tyler 2006), but that is where consensus ends. As early as 1982 Morton gave several reasons to consider all Phanerozoic strata post-Flood, and a few subsequent Flood geologists eventually expressed agreement (for example, Robinson 1998b; Tyler 2006). However, that view is not popular among Flood geologists. Some have expressed agreement that Cenozoic—and according to some, Mesozoic—strata are post-Flood but maintain that most of the Paleozoic Era represents the Flood year (for example, Garner 1996a, b; Garton 1996; Robinson 1996;

Whitmore and Garner 2008). Others continue to espouse the view that most Phanerozoic strata are Flood deposits (for example, Holt 1996; Oard 1996, 2006, 2007; Matthews 2009).

Several Flood geologists have presented geologically sound reasons why strata assigned to specific parts of the geologic column cannot have been deposited during the Flood year or at least during the part of it when the entire planet was under water, hereafter called the PWS (period of worldwide submergence). In fact, compilation of such studies shows that together Flood geologists have eliminated the entire geologic column as having any record of a PWS. Here, I review the evidence against a PWS record that has been presented by the Flood geologists themselves.

Subaerial deposits

Deposits that are demonstrably subaerial (that is, deposited on exposed ground, not under water) obviously cannot have been deposited during a PWS. The PWS, if it existed, has to have occurred before the deposition of the earliest subaerial deposit, after the deposition of the last one, or between the deposition of two such deposits. Flood geologists have accepted that many such deposits are subaerial and that therefore the corresponding portions of the geologic column cannot record a PWS.

Several Flood geologists have noted that desiccation (drying out) cracks indicate extreme shallowness or exposure to air and have cited their presence in certain strata as evidence that those strata were not deposited during a PWS. Rupke (1966) noted the presence of desiccation cracks in the Triassic Muschelkalk deposits of the Netherlands. Lammerts (1966) noted their presence in the Proterozoic Altyn Limestone of Montana. Morton (1982) noted their presence in Silurian limestone. Scheven (1990) noted that desiccation cracks are present in Devonian "Old Red" and Permian "New Red" deposits of Europe. Tyler (1994) noted the presence of desiccation cracks in Middle Jurassic deposits of England. Whitmore and Garner (2008) cited the Green River Formation (Eocene) as an example of a formation that contains desiccation cracks.

Oard (1993), an advocate of the hypothesis that most of the Phanerozoic column represents the Flood year, has expressed doubt that the identification of desiccation cracks in the geologic record is correct, noting that similar features can occur underwater. However, other Flood geologists have noted that many of the deposits identified above as having desiccation cracks also exhibit impressions of raindrops, which can be made *only* on exposed surfaces. Even within the paradigm of Flood Geology the association of raindrop impressions with cracks is diagnostic of true desiccation cracks and can be used to eliminate similar crack types from consideration (Whitmore 2009). Rupke (1966) noted the presence of raindrop impressions in Cambrian strata, the Upper Devonian of Belgium, and the Triassic Muschelkalk. Lammerts (1966) noted their presence in the Proterozoic Altyn Limestone. Scheven (1990) noted that raindrop impressions are present in Europe's Devonian "Old Red" and Permian "New Red" deposits. Even Oard noted the presence of raindrop impressions in the Eocene Green River Formation (Oard 2006).

Basalt, a type of volcanic rock, can be deposited subaerially or under water. Unlike basalts that are deposited under water, continental basalts (basalts that are deposited subaerially) exhibit laterally widespread flow, columnar jointing, and a lack of pillow structures. Nevins (1971, 1974) noted that continental basalts are present in a number of Cenozoic formations in North America including the Eocene Clarno Formation, the Oligocene John Day Formation, the Miocene Columbia River Group, and the Pliocene and Pleistocene Mesa basalt. Garner (1996a) noted that such basalt flows are present at the Paleozoic–Mesozoic boundary and are stratigraphically and geographically widespread through Mesozoic and Cenozoic strata. He also noted that continental basalt flows are present in Proterozoic and Cambrian strata.

Holt (1996) includes a figure showing the stratigraphic distribution of continental volcanic deposits. According to the figure, continental volcanic deposits occurred during all Phanerozoic periods, including each Tertiary epoch. This information precludes the PWS from having spanned more than one Phanerozoic period or Tertiary epoch. Although Holt (1996) nevertheless insisted that the entire pre-Quaternary, Phanerozoic rock record represents the Flood year, his own figure eliminates the span of any Phanerozoic period or Tertiary epoch as having been deposited entirely during the PWS.

Land plants cannot germinate and grow in place while under a meter of water. Accordingly, several examples of *in situ* fossil land plants have been cited by Flood geologists as evidence that given strata do not represent the PWS. Morton (1982) noted the presence of small plants preserved upright where they grew, in the Devonian Rhynie Chert. Tyler (1994) noted that *in situ* plant roots are present in several stratigraphic levels within the Middle Jurassic of England. Robinson (1996) specifically mentioned six such Middle Jurassic stratigraphic levels and also noted the presence of *in situ* root beds in Upper Cretaceous deposits in western Europe.

Morton (1982) cited hatched dinosaur eggs from the Cretaceous of Montana as evidence that the area could not have been under water at the time of deposition. Garner (1996b) and Robinson (1996, 1998a) noted that strata containing *in situ* nests with dinosaur eggs are known from Upper Triassic, Lower Jurassic, Upper Jurassic, and Upper Cretaceous strata. Such strata must have been deposited subaerially, because dinosaurs would not have made nests or laid eggs underwater. As Robinson (1998a) further noted, at least one Upper Cretaceous dinosaur nesting site in Europe is overlain by an *in situ* fossil root bed, indicating that plants germinated and grew after the nesting site had been buried. This cannot have taken place during worldwide submergence. Robinson (1996) also cited a Triassic insect nest as an example of a nest that cannot have been made underwater.

Northrup (1986) noted that there is geographically widespread evidence of subaerial erosion by glaciers during the Pleistocene. He therefore argued that the Pleistocene Epoch must be post-Flood. There is wide agreement among Flood geologists that the Pleistocene glaciations were post-Flood, but most do not explicitly mention glacial erosion as evidence of subaerial exposure. The existence of pre-Quaternary glaciation is doubted by most Flood geologists. Several have presented alternate explanations, usually involving underwater deposition, to explain pre-Quaternary deposits that mainstream geologists consider glacial (for example, Oard 1994, 2009a; Sigler and Wingerden 1998; Wingerden 2003).

Williams and Howe (1993), Williams and others (1993), and Holroyd (1996) described large amounts of fusain (fossil charcoal) from Upper Cretaceous deposits in western North America. Williams and Howe (1993) also noted the presence of fusain in the Triassic Chinle Formation. All these authors further noted that charcoal is created by fire, which cannot occur under water. Holroyd (1998) noted that fusain is also known from the Pennsylvanian (late Carboniferous) of Kentucky.

Eolian sandstones are remnants of ancient dunes deposited subaerially by wind. Robinson (1996, 1998a) noted that Upper Cretaceous dinosaur fossils from Mongolia are often entombed in eolian sandstones, indicating that they perished in terrestrial sandstorms and were not underwater. He and Northrup (1990) also noted that eolian red beds are present in Devonian, Carboniferous, and Permian strata and all through the Mesozoic. The PWS can therefore not have spanned any of those time periods.

Whitmore and Garner (2008) noted that soils form on land and that paleosol (fossil soil) therefore indicates subaerial deposition. They cited paleosols in the Green River Formation and the Paleocene Fort Union Formation as evidence that those formations cannot be Flood deposits. Flood geologists generally doubt that pre-Tertiary "paleosols" are correctly identified, and Klevberg and others (2009) list several reasons that the identification of paleosols is problematic.

Northrup (1990), Garton (1996) and Robinson (1996, 1998a) argued that trackway evidence eliminates the entire Mesozoic and Cenozoic portions of the geologic column as having PWS strata. They noted that tracks of terrestrial reptiles and mammals are absent in pre-Permian strata, whereas they are present in Permian strata and are stratigraphically and geographically widespread through Mesozoic and Cenozoic strata. Scheven (1990) also noted the presence of vertebrate tracks in the Permian. Such tracks are produced by live, air-breathing, terrestrial animals and cannot therefore be produced during a PWS. In addition, Robinson (1996a) and Garner and others (2003) cited the presence of over 160 successive track-bearing horizons in a Chinese Cretaceous locality and of over 300 successive track-bearing horizons in a series of Cretaceous strata in Korea as examples of evidence that is difficult to reconcile with deposition during the Flood year. Because track-bearing strata are stratigraphically and geographically widespread through the Mesozoic and Cenozoic, Garton (1996) and Robinson (1996, 1998a) argued that any Flood strata must be pre-Mesozoic.

Flood geologists who claim that the entire Paleozoic and Mesozoic represent the Flood year simultaneously acknowledge that subaerial deposits are present at many stratigraphic levels therein. There is broad agreement among such researchers that sediments bearing dinosaur nests, eggs, and tracks were deposited subaerially but that this occurred during the early stages of the Flood before the entire globe had been covered in water (for example, Holt 1996; Oard 1996, 2009b; McIntosh and others 2000). Such researchers explain the existence of multiple track-bearing strata between multiple water-deposited strata by hypothesizing that tectonic activity raised and lowered the land and/or sea level in various areas several times during the first 40 days of the Flood. According to this hypothesis, during the first 40 days of the Flood a given area of land might experience several cycles of exposure and track-making followed by submergence and deposition of water-borne sediment (McIntosh and others 2000; Woodmorappe and Oard 2003; Oard 2009b).

Other Flood geologists have pointed out several reasons that such hypotheses are unsatisfactory. First, myriad track-bearing sediments are present through the Mesozoic and Cenozoic, up to strata representing the present, as Garton (1996) and Robinson (1996, 1998a) noted. According to Robinson (1998a:59), this means that if these are Flood deposits, animals were "trying to escape the deluge right to the time Noah steps out of the Ark," which contradicts the Genesis account. Indeed, if all track-bearing stratigraphic levels were deposited during the early stages of the Flood, this pushes the stratigraphic level representing the PWS all the way up the geologic column and beyond it into the future! Second, such hypotheses cannot accommodate eolian deposits, charcoal, or the presence of *in situ* roots. No Flood geologist has explained away the charcoal and root problems, and the closest any has come to explaining away the eolian deposits is the puzzling objection by Oard (1996) that the terms "fluvial, lacustrine, and eolian are purely uniformitarian environmental interpretations that have little to do with a Flood paradigm." Third, as Lawrence (2003) noted, if all Phanerozoic strata were deposited in the first 150 days of the Flood, the enormous average daily sedimentation rate indicates such catastrophic conditions that all air-breathing organisms would have been destroyed much too quickly to have left such a vast and stratigraphically extensive ichnological record. Oard (2003) pointed out that Lawrence's estimated average daily sedimentation rate (100 m/day, based on an average continental thickness of 15 km accumulating in 150 days) is too high because the average Phanerozoic continental thickness is 1.5 km, not 15 km. However, even so, an average sedimentation rate of 10 m/day still indicates conditions catastrophic enough to validate Lawrence's point. Fourth, such hypotheses fail to explain why dinosaur tracks are found only in Mesozoic sediments and large mammal tracks are found only in Cenozoic sediments (Garton 1996). Fifth, as Robinson noted (1996), there is no post-Silurian stratigraphic level at which there is geological evidence that the entire globe was simultaneously under water.

Low-energy deposits and long passages of time

Tyler (1996) made a fourfold argument that Cretaceous chalk deposits must have taken longer than a single year to form. First, he noted that those deposits are full of hardgrounds. The process of sediment hardening, erosion, and encrustation that results in these hardground deposits requires weeks, months, or possibly even years to form. Second, Tyler noted that the alternation of soft-sediment faunas with hard-sediment faunas within the chalk deposits indicates "cyclical geological history involving numerous stationary surfaces". Third, he noted that the sequence of *Micraster* (a group of extinct sea urchins) fossils in the chalk shows speciation within the genus, which requires years to occur and therefore cannot have happened within the single Flood year. Fourth, there are over 100 bentonite beds in the Niobrara Chalk of North America, indicating over 100 periods of deposition during calm periods that are inconsistent with catastrophic Flood conditions. Woodmorappe (2006) and Matthews (2009) argued for the rapid formation of hardgrounds, claiming that burrows within them indicate that these sediments were soft at the time of deposition, and Matthews (2009) further expressed skepticism that the Cretaceous chalk deposits took longer than a year to form. However, neither author addressed the other indicators of long passages of time that Tyler (1996) noted: bentonite beds, *Micraster* speciation, and cyclical formation of multiple surfaces.

Robinson (1996, 1998a) listed several stratigraphic levels containing fossil communities that include multiple generations and therefore must have taken multiple years to grow. Examples include Ordovician and Middle Jurassic hardgrounds, a Lower Jurassic starfish bed, Upper Permian algal growths, Middle Triassic and Upper Cretaceous bivalve beds, and a Permian reef. Scheven (1990) cited examples of Triassic reefs that show evidence of several generations of clams. Brand (2007) noted that reefs over 100m thick are known from all post-Silurian periods.

Wise and Snelling (2005) noted that *in situ* stromatolites (sedimentary deposits formed by slow-growing beds of cyanobacteria) are abundant in the sediments of the Proterozoic Chuar Group in the Grand Canyon. Stromatolites form in low-energy, shallow marine environments, and the sedimentology of the Chuar Group sediments containing them is consistent with this. Those sediments therefore cannot have been deposited during the Flood year, as Wise and Snelling (2005) note. Wise (2003) also noted that the Crystal Spring and Beck Spring Formations in Death Valley, both Proterozoic, contain stromatolites. Dickens and Snelling (2008) noted that stromatolites are also present in other Proterozoic deposits and also in Precambrian deposits of late Archean age.

The Green River Formation is a series of Eocene strata in Wyoming, Colorado, and Utah. Whitmore (2006a, b) presented evidence from lithology, sedimentology, taphonomy (post-depositional alteration), and geochemistry that the Green River Formation is a series of lacustrine (made by a lake) strata, not a Flood deposit. The overall shape of the deposit and its lateral and vertical distribution of specific sediment types are consistent with lacustrine deposition. The presence of multiple stromatolite horizons and the spatial distribution of intact and disarticulated fish skeletons are inconsistent with catastrophic deposition and indicate deposition over a period of years. The presence of thick deposits of the mineral trona (a form of sodium carbonate formed by the evaporation of water) is inconsistent with mixture with seawater. Whitmore (2006a, b) noted that all this evidence demonstrates that the Green River Formation was not deposited during the Flood year.

Brand (2007) also mentioned the Green River Formation. He noted that the presence of stromatolites in several different horizons within the Green River Formation indicates a periodically expanding and shrinking lake, not a single, catastrophic Flood. He further noted that stromatolites are stratigraphically widespread and that this is a problem for the hypothesis that most of the Phanerozoic represents the Flood year.

Whitmore and Garner (2008) also noted that lacustrine deposits cannot have occurred during a PWS. They cited lacustrine deposits in four Cenozoic formations of North America—the Fort Union (Paleocene), Wasatch (Eocene), Green River (Eocene), and Bridger (Eocene) Formations—as evidence that those formations are post-Flood. Whitmore and Garner (2008) also noted that fluvial (stream- or river-deposited) deposits cannot occur during a PWS. They cited fluvial deposits in the aforementioned four formations and the Late Cretaceous Lance and Mesaverde Formations as evidence that those deposits are post-Flood. These authors also noted that both lacustrine and fluvial deposits are found in the Early Cretaceous Cloverly Formation and the Late Jurassic Morrison Formation and that the Triassic Chugwater Group includes fluvial deposits. A PWS therefore cannot have spanned the Triassic, Jurassic, or Cretaceous Periods.

Diversification of terrestrial animals

The concept of fixity of species was discarded early by the YEC movement. YECs have long recognized that speciation can take place within each "created kind". Robinson (1996) noted that Permian and Mesozoic strata record diversification of various categories of non-dinosaurian reptiles, Mesozoic strata record diversification of various categories of dinosaurs, and Cenozoic strata record diversification of various kinds of mammals. Because such speciation cannot occur during a single year when the entire planet is underwater and during most of which the relevant animals are dead, he argued that the entire post-Carboniferous portion of the geologic column must be post-Flood.

Whitmore and Wise (2008) noted that the Eocene Green River Formation contains multiple species within each of several mammalian families. Because "created kinds" recognized by YECs usually correspond to taxa of family rank or higher, they argued that this formation must have been deposited after enough time had passed for the descendants of the single pair of each "created kind" on the Ark to have undergone diversification. The Green River Formation, they insisted, must therefore have been deposited sufficiently long after the Flood.

The Mountains of Ararat

Holt (1996) argued that the Flood year cannot have ended before the Mountains of Ararat existed, because the Ark rested upon those mountains. He further argued that the Ark cannot have rested there during major volcanic episodes, because it would have been destroyed. Any post-Flood deposits must therefore be stratigraphically above the Ararat mountain chain. He noted that the uplift that created the chain occurred in the Eocene, Miocene, and Pliocene Epochs. Because Noah looked out onto dry land before leaving the Ark (Genesis 8:13–14), the Ark's landing had to have occurred after an Early Pliocene marine transgression that occurred in this mountain chain. According to this argument, the end of the Flood year cannot have been before the Pliocene Epoch. Robinson (1998b) advocated post-Flood status for the entire Phanerozoic Eon. To reconcile this notion with the fact that the Ararat chain did not exist before the Cenozoic, he suggested that the Ararat mountains mentioned in Genesis are not the chain that today is known by that name. Indeed, within the YEC paradigm that is the only possible solution to the dilemma.

Discussion

It should be noted that mainstream geologists have identified a much wider stratigraphic distribution of desiccation cracks, raindrop impressions, *in situ* plant fossils, fusain (fossil charcoal), eolian deposits, paleosol, lacustrine deposits, and fluvial deposits than Flood geologists typically recognize. Fossil charcoal, for example, is known from all post-Ordovician periods (Scott 2000; Scott and Glasspool 2006), which by itself is enough to eliminate any of those periods from having been completely spanned by a PWS, because fire cannot burn underwater. But even without recognizing the complete stratigraphic distribution of any of these indicators of subaerial deposition, Flood geologists have still managed to confirm, with sound sedimentological reasoning, that no Tertiary epoch, Phanerozoic period, or post-Haden eon was spanned by a PWS (see Figure 1). This means that—according to the results of the studies by Flood geologists themselves—if the Flood occurred during Phanerozoic time then all Flood deposits are stratigraphically sandwiched between a pair of non-Flood deposits within the stratigraphic span of a single one of the geologic periods. If this is the case, then the Flood left little if any geologic evidence of its occurrence. Flood geologists have difficulty accepting that a worldwide cataclysm would leave but a small geological scar, but they themselves have provided evidence that either such is the case, or the Flood was pre-Phanerozoic, or it is mythical.

An entirely pre-Phanerozoic Flood is accepted by a few Flood geologists (for example, Morton 1982; Robinson 1998b; Hunter 2000; Tyler 2006). According to the view expressed by those authors, God's carrying out of his threat in Genesis 6:13 that the Flood would destroy the earth (the land, not the entire planet earth, because the writers of Genesis did not know they were on a planet) did indeed completely destroy all pre-Flood land, thereby eliminating the geological record of a pre-Flood world. In this view, Hadean and early Archean igneous deposits record the geological catastrophe that accompanied the onset of the Flood, and the unconformity between such igneous strata and subsequent sedimentary strata marks the wiping out of the pre-Flood land by the Flood. It could be that such authors represent the vanguard of a paradigm shift. If so, then acceptance that the entire Phanerozoic has no worldwide Flood deposits will be the consensus of the next generation of YECs. In light of the current state of Flood geologic research it will be the most realistic consensus possible within the YEC paradigm.

Unfortunately for the proponents of that view, the hypothesis that a Precambrian Flood occurred and left no sedimentary strata is less scientific than the hypothesis that most or much the Phanerozoic sedimentary column was Flood-deposited. This is because the latter hypothesis is testable and falsifiable—and has been tested and falsified by the Flood geologists themselves—whereas any hypothesis that a phenomenon occurred but left no evidence for its occurrence is an untestable, unfalsifiable hypothesis. Some may argue that the igneous Hadean and Archean deposits are evidence of the geological catastrophe that caused or accompanied the onset of the Flood, but the equation *catastrophe* = *Flood* is fallacious. No recorded geological catastrophe has caused worldwide flooding.

The majority of Flood geologists continue to maintain that a large portion of the Phanerozoic column represents the Flood year, although they have falsified that position themselves. As shown in Figure 1, this is an untenable position even within the paradigm of Flood Geology, because the collected evidence from five decades of research in Flood Geology demonstrates that a PWS cannot have spanned any Phanerozoic period. Even if the Mesozoic and Cenozoic Eras were simultaneous (for example, Oard 2001; Reed and others 2006; Matthews 2009), Flood geologists have rendered untenable the hypothesis that the Flood year spanned much of the relevant slice of time, by demonstrating that too much Mesozoic and Cenozoic sediment deposition was subaerial or was prolonged for years. The continued denial of the implications of their own findings is an example of what I call the gorilla mindset: the attitude that if something looks like a duck, walks like a duck, and quacks like a duck, but religious dogma says it is a gorilla, then it is a gorilla.

It is noteworthy that the gorilla mindset is steadily diminishing within the ranks of the practitioners of Flood Geology. Fewer and fewer researchers in that field deny the accumulated evidence of subaerial deposition or of deposition for longer than one year for large portions of the Phanerozoic column. As mentioned above, some have already rejected the hypothesis of a Phanerozoic Flood in favor of the hypothesis of a Precambrian Flood, despite the fact that such a hypothesis necessitates acceptance of a lack of sedimentary deposition by a Flood in the geologic record. In the words of Flood geologist Max Hunter (2009:88), "It is somewhat ironic…that, almost a half century after publication of *The Genesis Flood* by Whitcomb and Morris in 1961, the geologic record attributed to the Genesis Flood is currently being assailed on all sides by diluvialists…[and] there remains not one square kilometer of rock at the earth's surface that is indisputably Flood deposited."

Flood Geology began in order to find support for YEC doctrine but ironically it has now produced an impressive body of evidence against it. The defeat of Flood Geology by its own hand is a great example of how the practice of sound geology leads to correct geological conclusions.

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FEATURE

People and Places: Paul Kammerer: 1880–1926

Randy Moore

FIGURE 1. *Paul Kammerer. (Undated photograph from the Bain News Service, courtesy of the Library of Congress, LC-DIG-ggbain-37807.)*

Paul Kammerer (Figure 1) was born on August 17, 1880, in Vienna, Austria. He studied music at the Vienna Academy, but eventually graduated with a degree in biology. Kammerer studied Lamarckian inheritance and claimed that many genetic traits had been suppressed by animals' lifestyles. Kammerer studied this by investigating how the breeding of amphibians in unusual habitats affected their offspring. Kammerer claimed many successes, and biologists from throughout Europe visited his lab. He delivered popular lectures about how humans might become super-humans.

Kammerer's most famous claim involved midwife toads (*Alytes obstetricians*), a group so-named because males carry fertilized eggs on their backs and hind legs. Most toads mate in water and have black, scaly nuptial pads on their hindlimbs that help them cling to each other while they mate in their slippery environment. However, midwife toads mate on land and lack these pads. When Kammerer forced midwife toads to mate in water, he reported in 1919 that they laid fewer eggs and developed the black pads. Kammerer's apparent confirmation of Lamarckian inheritance made front-page news throughout the world. Because Kammerer's claims supported socialist ideals and were consistent with Lysenko's Lamarckian version of genetics, Kammerer was accepted a job offer in Moscow.

Kammerer was hailed as a successor to Charles Darwin, and his work was described as revolutionary. However, Kammerer's facts weren't true; other biologists could not replicate his work. The situation was further complicated by the loss of his original research during World War I. In the August 7, 1926, issue of *Nature*, G Kingsley Noble—Curator of Reptiles at the American Museum of Natural History, who had examined Kammerer's frogs—claimed that Kammerer's data were fake (Noble 1926). William Bateson agreed, claiming that the alleged nuptial pad were merely ink. Subsequent examination of one of Kammerer's pickled toads showed that the black pads—that is, the acquired trait that had allegedly been inherited—were nothing more than black ink that had been injected into the toad's foot. Kammerer's reputation was destroyed.

Kammerer claimed to be astonished by Noble's accusation, and denied any wrongdoing. However, six weeks later—on September 23, 1926, just before he was to begin work in Moscow—Kammerer committed suicide. While on a walk in the Theresien Hills, he shot himself in the head.

After Kammerer's death, the Soviet Union produced a film titled *Salamandra* that ended with Kammerer's triumphant arrival in the Soviet Union. In *The Case of the Midwife Toad*, Arthur Koestler (1971) speculated that Kammerer's results might have been planted by Nazi sympathizers or Darwin supporters intent on discrediting Lamarckian inheritance. More recently, Alexander Vargas (2009:671) speculated that "rather than being a fraud, Paul Kammerer could be the true discoverer of non-Mendelian, epigenetic inheritance."

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REVIEW

Time Matters

by Michael Leddra
Oxford: Wiley-Blackwell, 2010. 269 pages

reviewed by Steven Dutch

This book describes the development of the concept of geologic time and the conflicts that took place in the process. The book is aimed at British audiences and emphasizes British events, which is fitting since so much of early geology developed in Britain. Admirably, Leddra repeatedly exhorts the reader to imagine living in the past and to try to interpret the evidence as it then existed without the benefit of modern data. Leddra is well aware that it can be all too easy to look smugly upon early scientists who were doing the very best they could with what they had. Unfortunately, the book is severely compromised by almost exclusive reliance on secondary sources, some problems of organization, and a few startling and egregious errors.

The opening chapter reveals the book's strengths and weaknesses in microcosm. When Leddra discusses the famous age of creation given by Archbishop Ussher, he stresses correctly that far too many modern writers sneer condescendingly at Ussher, whereas he was regarded as an eminent scholar in his day, as well as being moderate and willing to compromise. An empathetic view of early science is probably the strongest virtue of *Time Matters*.

However, there are a number of glaring errors in the descriptions of the calendar cycles that were used in calculating dates. We read that one of the cycles was "[t]he Solar Cycle, which refers to the 28-year cyclic behaviour of sunspots." This statement is absolutely wrong. The sunspot cycle was unknown in Ussher's day. The Solar Cycle actually refers to the cycle of calendar dates falling on the same day of the week. Leddra also misstates the Metonic Cycle, a 19-year cycle in which lunar phases repeat on the same calendar date. It is possible that these gaffes are just unfortunate lapses, but the opening chapter is the worst possible place to make them.

On page 49 we read that most ^{40}K converts to "^{40}C (carbon) [*sic*], which is non-radioactive and is therefore of no use in dating rocks." Actually, of course, most ^{40}K decays to *calcium*-40, and the stability of daughter products is irrelevant to their usefulness in dating. One might imagine a typographical error changing ^{40}Ca to ^{40}C, but how do we account for explicitly writing "carbon" in parentheses? Then Leddra uses the symbol "St" for strontium instead of "Sr," and reverses the proportions of ^{235}U and ^{238}U. Elsewhere he says, "If there was ever a case for making a natural feature a World Heritage site, surely Siccar Point should be high up on the list of candidates" (p 90). I absolutely concur, but there are already 180 natural World Heritage sites. He states that the first paleontological museum was established by the "Emperor Augustinus," which of course should be "Augustus."

Chapter 3, "The origins of the geologic time scale," describes how each geologic period was defined. The organization is puzzling, since it follows the chronological order that the periods were named, but that can only be determined by close reading. Since the periods were defined piecemeal and out of geologic sequence, this arrangement serves only to confuse the reader. Remarkably, Leddra does not explain how some of the names were derived, for example that "Cambrian" derives from the Latin name for Wales, or that "Cretaceous" comes from the Latin "creta," meaning "chalk." He does make the vital point that the sequence of periods was defined long before Darwin wrote about evolution, and (a connection I hadn't made), mostly even before Charles Lyell introduced uniformitarianism.

There are far too many urban legends about history, especially the history of science and religion, floating around and being parroted in textbooks and on-line, even after they've been debunked in the scholarly literature. Therefore, if I'm going to tell a class that the Comte de Buffon self-censored his results out of concern for religious reprisal, or that Lyell had to soft-pedal his ideas to get his chair in geology and that "ladies were forbidden to attend his lectures because of his shocking views," I want to be confident that it really happened that way, and that means using primary source materials, or at least a source that cites them liberally. Since Leddra relies almost entirely on other recent writers, it's difficult to check the reliability of his accounts.

Chapter 7, "Evolution versus creationism," mostly deals with how views of fossils changed over time and describes some famous British controversies, especially the Huxley-Wilberforce debate. Creationism in America is described in about three pages, much of it dealing with the Scopes trial. Considering how much effort Leddra puts into detailing the milieus in which other conflicts played out, the absence of any historical background of American creationism is glaring. Perhaps the worst failing of scientists who take on creationism is their lack of understanding of the cultural forces at work, and *Time Matters* does nothing to cure that problem.

I would have grave reservations about using this book as a text. I would be hard put to explain to a class why I was using a text that incorrectly said early dates for creation made use of the sunspot cycle, or that potassium-40 decays to carbon-40. I admire Leddra's willingness to put himself in the minds of people long ago and his efforts to convey that attitude to the reader. I also like the copious high-quality illustrations of many of the people mentioned in the book, which serves to humanize the story. The book could be useful to someone who wants an overview of controversies in the history of geology, but for any serious application, I would feel compelled to refer to the sources Leddra cites, and perhaps even the primary sources themselves, and that largely defeats the purpose of the book. At the very least, the book needs a new, thoroughly checked edition.

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REVIEW

Life from an RNA World: The Ancestor Within

by Michael Yarus

Cambridge (MA): Harvard University Press, 2010. 208 pages

reviewed by Arthur G Hunt

In the past several years, RNA has crept ever so subtly into the lexicon of the popular press and the lay public. No longer is RNA thought of as simply the intermediary between DNA and proteins. Instead, there is increasingly a greater appreciation for the ways that RNA is intertwined throughout life. From the reporting of recent Nobel Prizes that have been awarded in the recent past for work related to RNA, to the forays that mainstream news publications make into RNA-related subjects (such as epigenetics), to more specialized coverage of the medical applications of RNA silencing, to the (perhaps overblown) controversies that concern the primacy of the so-called RNA World in the origins of life on earth, RNA has entered the public's mind in a number of fascinating contexts. Michael Yarus's book *Life from an RNA World* is a welcome and timely contribution, one that brings many different themes together in an understandable and coherent package.

Yarus takes on an ambitious task—to summarize the excitement and curiosity of RNA research for a broad audience that includes the informed lay public as well as life scientists. On top of this, he is faced with the unenviable but inescapable task of explaining some of the fastest-moving and -changing areas in science. But Yarus succeeds in explaining the remarkable nature of RNA, and how this singular molecule ties together the present and the very distant past.

The setting, as it were, for Yarus's book is decidedly evolutionary; basically, Yarus sets out to place his explanations and illustrations in a context of the origins and evolution of life on earth. This context takes up the first section (roughly a third) of the book, in which he presents his own take on evolution and the origins of life. Casual readers and critics alike would be mistaken in thinking that the first six chapters are presenting a comprehensive review of current modern evolutionary theory. Rather, these chapters are telling us something about the author—his own biases, interests, and focus, and the route he is choosing to take through the evolutionary landscape, the destination of which is an accessible description of the RNA World.

The rest of the book (save for one chapter) describes the RNA World. Yarus puts life in its appropriate context in chapter 9 (the appropriate context being that life is all about RNA), going so far as to replace the familiar picture of the central dogma with a revised version, the center of which is rightfully RNA. This chapter is refreshingly current, and should stand the test of time, even though new developments will add even more to the text and figure 9.2, the recasting of the central dogma. (Yarus allows for this, with a well-placed "??" denoting the likelihood of new discovery and understanding.) Chapter 10 is a well-crafted, accessible description of the structural and chemical diversity of RNA. Readers whose chemistry is rusty may have to dredge up some basic chemical principles to follow the discussion, but it should be possible for informed and interested readers to pick up the basic concepts and to follow some of the subsequent discussion of RNA function. Chapter 11 recalls some of the early speculations on the existence of the RNA World at the dawn of life, interesting ideas bounced around by the likes of Carl Woese, Leslie Orgel, Francis Crick, Harold White III, and Walter Gilbert. In chapter 12, Yarus broaches the subject of the relative inaccessibility of the distant past vis-à-vis the prospects of understanding the details of the prebiotic (RNA) world. Chapters 13 and 14 bring us into the world of test-tube biology and evolution, with, of course, a special emphasis on RNA. These chapters serve as introductions for the more current aspects of RNA research that are explored in chapters 15 (self-replicating RNA), 16 (the various biochemical capabilities of RNA), 17 (the peptidyl transferase), and 18 (the origins of the genetic code). The book concludes, in its last two chapters, with some clever speculation into the possible nature of the so-called ribocyte (a hypothetical organism with a RNA-based genome and cellular systems) and some future directions of research into the realm of the RNA World.

These two sections are separated by a chapter (chapter 7: "Tornadoes in a junkyard") that is both curious and illuminating. This chapter is the author's brief discussion of the intersection between his own work and the criticisms of the RNA World (and evolution in general) that are bandied about by leaders of the "intelligent design" movement. I think that this chapter is interesting in that it helps to portray the reach of antievolutionary thought: long enough, evidently, to pique the interest of one of the leaders in the field of RNA research. As with the first six chapters, this effort helps us to understand the author. However, as a contribution to the "field" (as it were) of criticism of "intelligent design," it is probably not as informative as other, lengthier treatments of the subject.

Although perhaps not written with this in mind, this book gave me a sense that I (the reader) was being recruited—to science, to RNA, and even to the author's lab. I came away with a favorable impression of Yarus's personality, and I was impressed by what his account revealed about the personal connections between him and other giants in the field as well as about the actual experimental and conceptual advances to which his lab contributed. Yarus deliberately chooses not to swamp the reader with a long list of citations, and instead picks and chooses the highlights of the field over the past several decades. This makes the book relatively easy to read. Readers who are familiar with RNA research will recognize the suggested readings, and may appreciate the broader context in which they are presented and tied together.

The highlights of this book, for me, were the personal feel that underpins the first seven chapters of the book, the informal and colloquial discussion that reveals much about the sense that a scientific leader makes of the larger field of evolutionary biology (as well as the more curious "field" of "intelligent design"); chapter 9, an excellent recasting of molecular biology in an RNA-centric fashion; and chapter 18, an entertaining and clearly-written account of research, much of it from his own lab, that is suggestive of a plausible mechanistic link between the biochemical properties of RNA and the genetic code as we know it. But those items are what I most liked about the book. Other readers will likely find other gems and insight in what is an accessible account of *Life from an RNA World*.

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REVIEW

Not a Chimp: The Hunt to Find the Genes that Make Us Human

by Jeremy Taylor
New York: Oxford University Press, 2009. 256 pages

reviewed by Jonathan Marks

Our genetic similarity to the apes—known for over a century from serological work, protein work, and DNA work—has been interpreted in several ways. First, as indicating that genetic data are unreliable as stand-alone guides in assessing the overall relationships of species (GG Simpson); second, that the genetic similarity and the anatomical difference constitute a paradox in need of empirical resolution (Allan Wilson); and third, that we are really chimps and should simply be reclassified as such (Morris Goodman). Coincident with the rise of the Human Genome Project, Goodman's view came to predominate by the end of the 20th century, for example in Jared Diamond's pop-science hit, *The Third Chimpanzee* (1992), and it is specifically that interpretation with which this book takes issue.

Our genetic similarity to the chimpanzee was famously quantified in the 1970s at 98–99%, a value that has remained remarkably robust despite changes in technology and measurement. Jeremy Taylor argues that (1) we are genomically more different than the 98–99% datum has indicated; (2) we are cognitively and behaviorally more different than the inhabitants of the post-Goodall world have been led to believe; and (3) the elision of human and chimpanzee, as animal-rights advocates have promoted, is unwarranted. He documents all three points admirably.

There is only one weakness to Taylor's argument. The relevant science pivots on a fundamental question, but it is neither about people, nor chimps, nor evolution. It is: To what extent do we privilege genetic data, and what they reveal, over other kinds of data and what they reveal? Relationships inferred from genetic data tend to encode ancestry; relationships inferred from ecological data tend to encode divergence. So what is worthier of our attention—how similar we are genetically to chimps, or how different we are ecologically? It's not really about how we evolved, but about how we compare biological entities. Why should we suppose that genetic relationships are "realer" or just "more important" than other kinds of relationships? Taylor takes the privileged position of genetics for granted, rather than engaging the cultural issue (as in the 1995 classic *The DNA Mystique*, by Nelkin and Lindee). After all, by a base-for-base comparison you are statistically constrained to match a carrot's genome over 25% of the time (since there are four bases in DNA, and you do share a remote ancestry with carrots), but you are obviously not one-quarter carrot, and anyone who says you are is teasing you, biologically incompetent, or barking mad. Comparisons of biological objects cannot be sensibly reduced to comparisons of their DNA. It's just not a whole lot more complicated than that.

This failure to interrogate the reductive assumption at the core of the claim leads Taylor into some familiar territory: that humans actually do have genetic properties that the apes lack, and whether humans actually do have mental powers and cranial anatomies that apes lack. Obviously we do—that's what evolution is all about, after all—the naturalistic production of difference. To deny difference is to deny evolution itself, as the synthetic theorists rightly maintained. So noting that we aren't apes—genetically, mentally, neurologically—is ultimately a trivial proposition. The interesting research problem is—as Allan Wilson framed it—that genetically we seem to be apes, while every other way, we don't seem to be. At very least, apes seem to be adapted for hanging and swinging in the trees, and we seem to be adapted for walking on the ground. So why doesn't that show up genetically?

Oddly, evolutionary processes get scant discussion here, save for mutation and crossing-over. Genetic drift gets neither a role nor a mention, nor does François Jacob's classic metaphor of genomic evolution as working like "a tinkerer, not like an engineer". Consequently, the author's view of evolution is largely an automatic selectionist/adaptationist one. It's not necessarily bad, but I think it's a bit conceptually narrow at this late date. Taylor nevertheless very competently reviews recent literature, highlighting the theoretical and empirical work of anthropologist/psychologist Daniel Povinelli.

Much of the book is concerned with establishing that our genomes are indeed different; attempting to identify the specific genetic changes that resulted in us being human, with a lot of correlational behavioral genetics; and highlighting the intellectual failures of apes. Since we know so little about how to build a normal person from a set of genes, and even less about how to build two different normal people from two different sets of genes, it shouldn't be at all surprising that we have only the faintest glimmer of an idea how to build two different normal species from two different normal sets of genes. It's not an embarrassment; it is just a fact that is easy to lose track of, in an age of hyperbolic "geno-hype".

The overall product, then, is a good read, covering some familiar ground, and competently exorcising some demons from the literature of the 1990s. There is, however, a unique and valuable lesson in *Not A Chimp*—that one can be an evolutionist and yet still appreciate the differences between human and ape—which has had an unfortunate tendency to get lost in the derivative literature. And so, as long as you are not caught sleeping in a tree while wearing a fur coat, it is unlikely that anyone will be mistaking you for an chimpanzee anytime soon.

Unless they are molecular geneticists, that is.

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REVIEW

Evolution: The Extended Synthesis

edited by Massimo Pigliucci and Gerd Müller
Cambridge (MA): MIT Press, 2010. 504 pages

reviewed by Anya Plutynski

In July 2008, at the Karl Lorenz Institute for Evolution and Cognitive Research in Vienna, sixteen biologists and philosophers met to discuss an "extended" evolutionary synthesis. The meeting resulted in a book: Massimo Pigliucci and Gerd Müller's *Evolution: The Extended Synthesis*. This engaging volume surveys novel empirical and theoretical advances in biology since the Modern Synthesis, some of which add to, and some challenge, its central tenets.

Scientists disagree about many things, and any disagreement among evolutionary biologists, especially involving claims to the effect that "tenets" of evolutionary theory are being challenged, is all too often taken to imply that evolutionary biology is "in trouble," and this makes for big headlines. The truth is, perhaps, rather more mundane. Pigliucci and Müller, and the contributors to this volume, do not intend to challenge the fact of common descent, or evolution by natural selection as one of several mechanisms of descent. By claiming that there is a call for an "extended" evolutionary synthesis, they explain that there is no "fundamental crisis in the structure of evolutionary theory" (p 10).

Instead, the aim of an extended synthesis is to include under the umbrella of evolutionary theory patterns and process previously considered to be at the "margins": plasticity, accommodation, evolvability, epigenetic and niche inheritance, and multilevel selection. Essays in the volume review how and why genomics, molecular biology, and development have transformed our understanding of evolutionary pattern and process. The volume is comprehensive and almost impossible to survey in less than 1000 words; I will focus below on two key concepts that each play an important role in the "extension" of the synthesis, according to Pigliucci and Müller: plasticity and evolvability.

Plasticity of a genotype is measured by a norm of reaction (a relationship between the phenotypic expressions of a genotype over a range of environments). This notion is not new; as Pigliucci remarks, plasticity has just turned 100 years old. What's novel is the role that plasticity plays in evolution, as Mary Jane West-Eberhard has argued. "Phenotypic accommodation" is the adaptive adjustment of an organism over the course of its lifetime, to novel internal or external environments. This idea is not new; biologists were, arguably, aware of examples of this in the 19th century and earlier. What's more controversial is whether, and how, phenotypic accommodation "becomes" genetic accommodation—that is, whether a novel phenotype generated by phenotypic accommodation may be "stabilized" or "fixed" by natural selection through the alteration in "genetic architecture" (p 368). A central tenet of the synthesis was that germ and soma are distinct, and only genes may be passed on, not any features of the phenotype gained over the lifetime of the organism. "Lamarckian" inheritance was thoroughly debunked by the synthesis's architects; most took Mendelian genetics as providing a solid basis for heritable variation, a necessary condition on selection. Pigliucci argues that there are several ways that phenotypic plasticity, as well as phenotypic and genotypic accommodation, could come to play a major explanatory role in evolutionary biology; for instance, plasticity might factor in niche construction or serve as a driver for speciation. Some of these claims may be more plausible than others. As Pigliucci acknowledges, "we need further—and better characterized—examples of genetic accommodation" (p 372). While there is ample empirical data on phenotypic plasticity and accommodation, genetic accommodation is still controversial.

Another controversial idea that receives some attention in this volume is "evolvability"; roughly, this is defined as the capacity for a species to evolve. Surely, the authors of the synthesis were interested in this property, and even measured it; additive genetic variance is a measure of the ability of a population to respond to selection. Günter P Wagner and Jeremy Draghi stress that the idea of evolvability can be seen as extension of similar concepts in contemporary quantitative genetics—one might measure mutation rate or mutational variance or covariances, or, at "lineage" level, the capacity to evolve given some measure of genetic variation, variability and selection. This "integrative" approach is rather different from, for example, John Gerhart and Marc Kirschner's notion of evolvability. They argue that there are certain developmental and molecular features of organisms—weak linkage, modularity, robustness—which make lineages "more evolvable"—that is, more likely to diversify. They appeal to this notion as an explanation for the "explosion" of diversity in eukaryotes, and call their theory "facilitated variation." Where this notion of evolvability becomes controversial is whether, and if so, how, evolvability itself evolves. How, if at all, does selection act at the lineage level, and if it does, does it promote "evolvability" or is evolvability a byproduct of selection (or drift) at lower levels of analysis and shorter timescales? It's not clear that this debate has been entirely resolved, and this volume does not definitively answer this question.

Advances in genomics, molecular genetics, and developmental biology have made evidence available that the synthesis's architects of the 1940s only dreamed of discovering. The contributors to this volume correctly claim that this new evidence bears significantly on our understanding of the patterns and processes of evolution. Anyone interested in becoming aware of both what we know now and what theoretical advances may come from this new data for evolutionary theory should take a look through Pigliucci and Müller's superb collection.

About the Author

Anya Plutynski is Associate Professor of Philosophy at the University of Utah. She edited, with Sahotra Sarkar, *A Companion to the Philosophy of Biology* (Malden [MA]: Wiley-Blackwell, 2008).

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REVIEW

Written in Stone: Evolution, the Fossil Record, and Our Place in Nature

by Brian Switek
New York: Bellevue Literary Press, 2010. 320 pages

reviewed by Pat Shipman

I looked forward to reading this book, after reading glowing blurbs on the back cover from paleontologists and science writers whom I respect. Unfortunately, I was disappointed in the book.

Switek opens with an excellent account of the last overhyped fossil to hit the newspapers, *Darwinius masillae,* a new primate fossil from the Messel shale quarry in Germany. The preservation of fossils at Messel is truly extraordinary: not only are complete skeletons of many 47–million-year-old species preserved, but so are their fur and their stomach contents. And in 2009, the popular media announced that Messel had produced the Missing Link, the creature at the base of the human lineage.

Darwinius is indeed a beautiful specimen, with a long tail, and gripping hands with nails, not claws. Identified as an extinct sort of lemur known as an adapiform and nicknamed Ida, *Darwinius* was promoted as the missing link, the "first link to all humans … the closest thing we can get to a direct ancestor." The first publication of *Darwinius* was a disaster. The lead scientist, Jørn Hurum, was a dinosaur expert who had urged his employer, the University of Oslo, to fork out $750 000 for the fossil slab in 2006. Since 1991, the other, less complete counterslab had been languishing in the Wyoming Dinosaur Museum, "enhanced" by skillfully filling in the missing details based on the better slab. Such enhancements, of course, tiptoe closely around the edge of forgery. The two halves were reunited and written up in a hurry, because Hurum was developing plans to make a documentary and put out a mass market book. This meant the scientists were unable to do a thorough job and make a detailed point-by-point comparison of the fossil to other known fossils and the paper made claims that were unsubstantiated by the evidence cited within. This incompletely studied specimen was unceremoniously dethroned by an analytical paper on the jaws and teeth of a very similar fossil from Fayum, Egypt, that showed *Darwinius* was no closer to the base of the human lineage to any other adapiform. *Sic transit gloria* "missing link," to paraphrase.

After an exciting beginning, Switek embarks on a series of chapters about missing links—or fossils identified as such—that would stand better as separate brief essays than a book. It took me several chapters to figure what the organizational theme of the book was; scientists are introduced, briefly sketched, their role in some debate over a missing link explained, and then Switek jumps into a new chapter involving a new lineage, often with yet again the same players. I wish that he had told us from the outset that chapters 1–9 were each going to focus on a different lineage or a different evolutionary transition—from ocean-dwellers to land-based animals, from birds to dinosaurs, from hoofed artiodactyls to whales, from unimpressive *Eohippus* to modern horses, and of course, from apes to humans. Though the chapters are arranged roughly in chronological order based on the lineage that is the main focus, Switek's narrative hops from Charles Darwin and roughly contemporary anti-evolutionists like Philip Gosse and Robert Chambers, then jumps the Atlantic to consider Othniel Charles Marsh, leaps forward to modern-day scholars like George Gaylord Simpson and Philip Gingerich, then reverts to William Buckland, Eugène Dubois, and other nineteenth-century scientists again, and then wanders back to the twentieth- and twenty-first–century paleontologists. I find it nearly impossible to weld these disparate narratives together into a whole story, either of evolution or of evolutionary thought. Each chapter's narrative is good on its own, with very few errors that I detected, but taken as isolated chapters I find that the available information on context, personality, and facts underlying debates are very thin.

The result is a choppy book, good in parts, but without any overall insight into our ideas of missing links and our treatment of fossils. Those hoping to learn about exciting discoveries and advances in paleontological techniques will, I fear, need to look elsewhere.

For a student wanting to brush up quickly on, say, human or horse evolution, this book will be a treasure trove. For an evolutionist seeking factual ammunition to battle creationists who pooh-pooh the idea of whale evolution or mock diagrams of elephant lineages, this will be a handy source. But for someone reasonably knowledgeable in the field, this book offers nothing new and for those who are interested but wish to learn more, the organization of the book is downright confusing.

What plagues me most about this book is how I can be so dissatisfied with it when other colleagues praise it heartily. After reading most of it, I put the book away and returned to it weeks later, hoping my judgment would return with the passage of time or that I would appreciate its merits more. I still don't like it much, I am sorry to say.

About the author

Pat Shipman is Adjunct Professor of Biological Anthropology at Pennsylvania State University and the author of many books, including *The Man Who Found the Missing Link: Eugène Dubois and His Lifelong Quest to Prove Darwin Right* (Cambridge [MA]: Harvard University Press, 2002).

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REVIEW

In the Light of Evolution: Essays from the Laboratory and Field

edited by Jonathan Losos
Greenwood Village (CO): Roberts and Company, 2011. 330 pages.

reviewed by Marvalee H Wake

In the Light of Evolution: Essays from the Laboratory and Field, edited by Jonathan Losos, is a wonderfully rich and diverse collection of essays that illustrate the way evolutionary biologists *think and work*—how they develop questions and hypotheses about evolution and how it occurs, how they test their hypotheses, why both lab and field work are important to resolution of many questions, and why the answers usually open new questions—and why that is useful for the progress of science. The authors of the essays present a wide range of exploration of several major areas of evolutionary biology, and of research on a great diversity of organisms. The essays are fast-paced and clearly written, accessible to any interested reader, no matter the level of either training in or skepticism about evolution. Virtually every author has framed his or her contribution with a background statement about Darwin's approach to similar problems, and how their work is able to extend his approaches because of new tools and expanded knowledge—and they often point out Darwin's prescience as he conjectured about pattern and process of evolution, especially natural and sexual selection. In fact, the book is part homage to Darwin, published two years after the bicentennial celebration of his birth, and part elucidation of Darwinian principles in today's scientific vocabulary. Nearly all of the contributions treat the origin, maintenance, and increase in diversity of organisms, their parts, and their behaviors, ranging from the evolution of weapons (such as horns and poisons), immunity, selection of mates, and on and on … They emphasize that evolutionary biology is multidisciplinary in approach, and synthetic in analysis. The contributions illustrate current ideas and theory of evolution, using tools and technologies drawn from many subfields of biology, and chemistry, physics, computer science, social science, history, and philosophy. Each contribution includes a list of suggested readings, mostly the authors' own papers, a Darwin or two, and a few additional references, all useful in giving additional information to the interested reader.

Losos selected a magnificent group of contributors—all experts in their own domains, hardworking scientists who report on their own work, which typically is integrative and synthetic, with emphasis on trying to understand diversity and unity, mechanisms, and history, illustrating that evolutionary biology is historical, observational, and experimental. "Being and becoming" is now testable in the lab and in the field. It had long been a premise that evolution is only historical, that it happened once, usually long ago, and that one must use clues to reconstruct pattern and process. Several of the contributions illustrate the new understanding and perspective that evolution is ongoing, and can be rapid, controlled, or capricious in the response of selection to environmental (external and internal) change. Have we made progress in understanding the theory of evolution and how natural selection operates? This volume presents a resounding "yes" to that question. We can test how natural selection works, we can assess the genetics of change, the rate of evolution, and the effects of inter-individual through environmental interactions, how species arise, and what effects diversity and how it is maintained.

The interested reader should be able to find questions and/or organisms that stimulate thought—there is something for everyone, and mastering one essay is likely to lead to needing to read others! Organisms of study are mostly vertebrates (fishes, birds, amphibians, mammals including humans, lizards and snakes), but also beetles, ants, and butterflies. The volume opens with a foreword by David Quammen that puts current study of evolutionary biology in the context of Dobzhansky's oft-quoted comment that "Nothing in biology makes sense except in the light of evolution"—the theme of the volume. The first contribution is an examination of Darwin as a "traveler, author, and naturalist"—that is, a working scientist—by the erudite historian and scholar of Darwiniana, Janet Browne. Essays follow by James Curtsinger, Carl Zimmer on Rich Lenski's work on microbial evolution, Daniel Lieberman on bipedalism in humans, Jon Losos on the diversification of island anolis lizards, Butch Brodie III on the "arms race" between newts and snakes, Naomi Pierce and Andrew Berry on nitrogen scarcity and the evolution of mutualism between caterpillars and ants, Luke Harmon on Wallace and island biogeography, Doug Emlen on horn evolution as weapons of sexual selection in dung beetles, Marlene Zuk and Teri Orr on ornamentation, sexual selection, and female choice, Mike Ryan on sexual selection in frogs via calls (and their consequences), David Reznick on studying natural selection in guppies, David Queller on the evolution of altruism in honey bees, Axel Meyer on speciation in cichlid fishes, Hopi Hoekstra on the genetics of color adaptation in mice, Ted Daeschloer and Neil Shubin on fossils and the origin of tetrapods, and Harry Greene on cows, deer, and the evolution of "wild".

I will describe, briefly, two of the essays; I could have chosen *any* two to illustrate the scope, complexity, and clarity of the research areas described, but those by Dan Lieberman and by David Reznick provide excellent but very different examples—do read most or all of the essays, though, to get the depth of "flavor" of modern evolutionary biology, and the way it can and should inform so many of our current social constructs.

Both students and skeptics of evolution are interested in new research on the evolution of our own species. Lieberman introduces his discussion of human bipedalism with a thoughtful examination of Darwin's perspective on human evolution. He points out that Darwin had far fewer facts at hand about human evolution than he had for many other species when he published *On the Origin of Species* in 1859, and his few comments were diffident. But by 1871, in order to prepare *The Descent of Man and Selection in Relation to Sex*, he had put together evidence for the relationships of humans to other mammals. He also speculated that humans evolved in Africa, and that they are most closely related to the great apes. Darwin reasoned that human bipedalism was a key innovation that set humans on a course that resulted in the evolution of speech, intelligence, and morality. The freer use of hands led to tool use, defense, prey attack, and so on. At the same time, Darwin apparently accepted that humans are "special", and wondered how becoming special occurred as a consequence of their evolution. How did selection work on variation in humans? Darwin realized that natural selection is a highly contingent process in which change is influenced by previous events—that is, the cumulative history of the entity. Lieberman then examines Darwin's prescient ideas in the context of the current genetic evidence for the relationships of humans, and compares the hypotheses. He notes that there have been few hominoid fossils until quite recently, and he compares australopithecines with hominids regarding two issues—brain size and walking/running ability. Both aspects are major foci of his research program. Lieberman carefully and lucidly tests hypotheses regarding the evolution of brain size and the timing of the evolution of various capacities, such as tool use, and of the evolution of endurance running. He presents a fascinating scenario, based on two ideas supported by fossil evidence: at first, bipedalism was slow and awkward; large brains evolved well after hunting did. Given that, Lieberman postulates that the evolution of endurance running released a constraint on brain size, allowing persistence hunting, the evolution of language, aggressiveness, moral sense, concealed ovulation, and so on. Lieberman asserts that the meshing of these features that occurred based on chance events, contingency, and selection may indeed make the human species "special." (I would argue that the species is unique, like all species, but different, again like all species …) The key is that humans are "us" and we do not always recognize our place in nature and the effect that we have on it. Lieberman's ideas provide much food for thought!

David Reznick has developed a wide-ranging, intellectually challenging, diverse and synthetic research program that is devoted to understanding natural selection as a process, and the nature of adaptation. He tests experimentally many aspects of evolutionary theory. His essay showcases the course of his thinking about *how* to test evolution, and not in the lab, but in the field so that he could explore and manipulate variables in the actual lives and habitats of organisms. He notes that his early ideas came out of digesting the lab experiments on evolution in *Drosophila*, and his desire to test selection in the natural world. It was becoming clear that evolution *is* testable, that change doesn't happen just once, back in distant time, but is ongoing and can be fast. Reznick is also interested in life history theory, including mate selection and aspects of fitness, such as numbers and sizes of offspring. His beautifully organized contribution presents the background for the thesis that natural selection is the cause of evolution. He next discusses how to develop ways of experimentally testing principles explicitly in nature, including choice of study organisms and sites. His rationale is that nature does experiments, as Darwin recognized. Consequently it should be possible to replicate the parameters of those experiments in the lab, and to manipulate them there and in the field. Life history theory predicts how the risk of death alters the way organisms allocate resources for life. Reznick reasoned that the numbers of predators at a locality establishes the level of risk. In situations with many predators, the prediction is that natural selection would result in early maturity, and high fecundity—lots of babies. Conversely, few predators would allow the population to delay maturity and devote resources to its own growth and maintenance. Reznick then thought about what species and what localities would allow him to test these predictions. He had visited Trinidad, and knew the species of guppies there. He also knew that there were different predators in kind and number and therefore different risks at the headwaters of streams in the mountains versus those at lower levels; therefore each stream constitutes a natural experiment. Guppies are viviparous, and have superfetation (clusters of embryos in the ovary that can be at different stages of development, resulting from different fertilization times and even different fathers), and high genetic diversity. A single female can start a new population if she moves to a new site, because of the genetic diversity of her offspring. These factors, together with the presence or absence of predators in different streams, became Reznick's natural laboratory. Reznick then set to work, manipulating guppy populations and predators and their numbers of species and individuals in Trinidadian streams. He found that the rate of evolution (trait change) in both field and lab was fast, but often at erratic rages. He found the imprint of predation on every trait that he tested—guppy locomotion, resource allocation, and so on—and thus determined the scope of adaptation by natural selection. His choice of species and sites made major theoretical discoveries possible. This description short-cuts years of thought and work, and doesn't do it justice. For example, Reznick and his team started by doing mark and recapture studies for which each fish was marked, and photographed; he now removes two scales from each fish and genotypes the fish based on the DNA from the scales. They census the populations each month and they genotype new recruits and baby guppies, thus developing pedigrees of evolving populations, allowing new experiments in natural selection to be designed. Reznick concludes his essay with an inspiring discussion of the ways we can now test evolution as a process, and his view of what future research will be, for him and for the science. Biologists can now quantify fitness and assess the features of organisms, develop a quantitative theory of evolution that is predictive regarding species' responses to their environments and changes in them, develop experimental designs that make natural selection observable and quantifiable (change over time), and work with contemporary, not just historical, data.

Each of the essays in this book is as thoughtful, informative, and perceptive as those of Lieberman and Reznick. They would be splendid points-of-departure for seminars on natural selection, biodiversity, evolution, or even research styles. An understanding of how scientists think and work is fundamental to our developing conception of the complexity of life and how it evolves. Losos and his fellow contributors reveal the histories, personalities, and futures of the scientists and the science in honest and engaging essays.

About the author

Marvalee H Wake is Professor of the Graduate School at the University of California, Berkeley; a past president of the International Society of Vertebrate Morphology, the American Institute of Biological Sciences, and the International Union of Biological Sciences; and a Supporter of NCSE.

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