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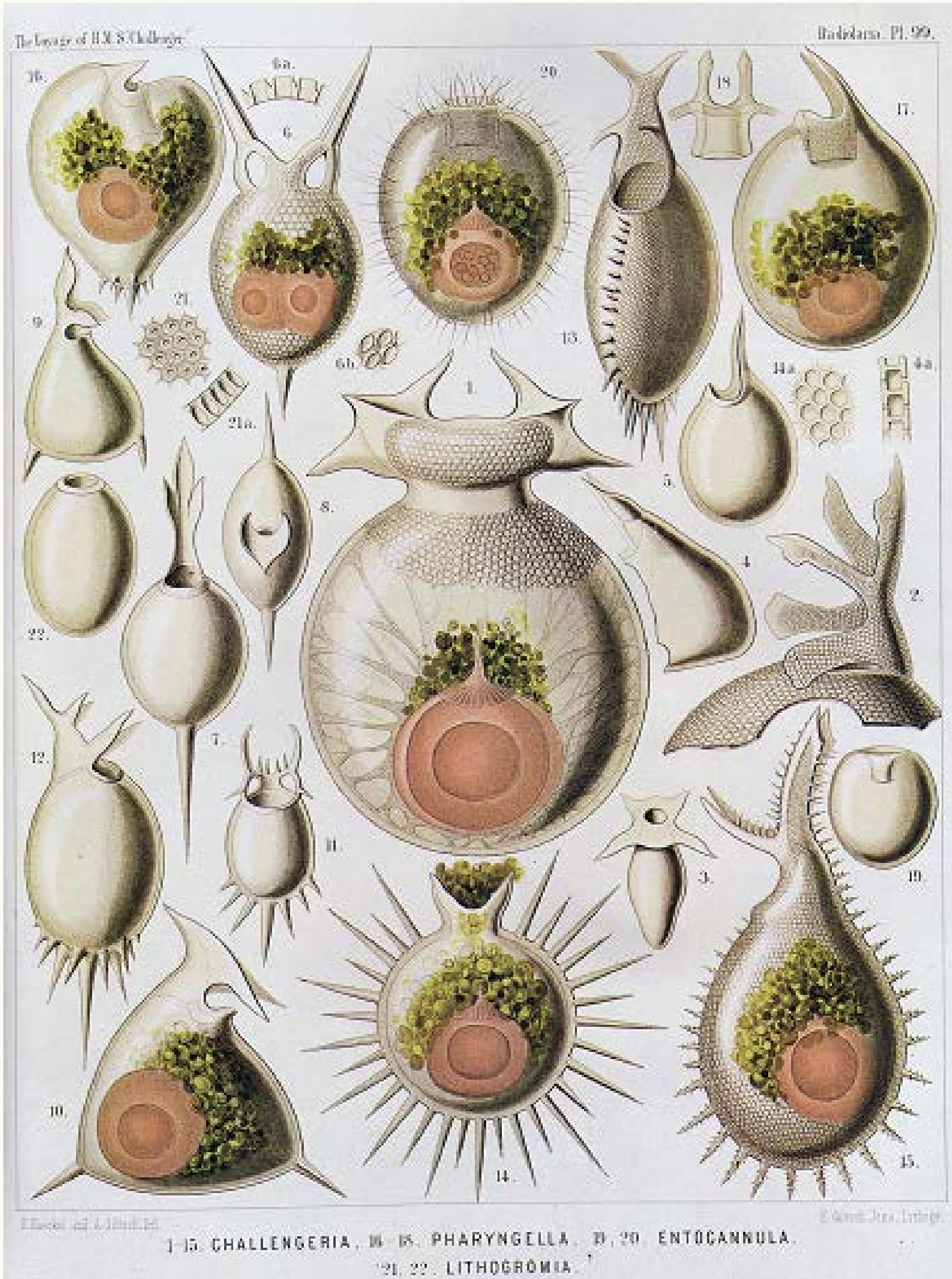

Examples of the variety of shapes of radiolarian skeletons. Source: Ernst Haeckel, *Report on the Radiolaria collected by HMS* Challenger*, Report of the Scientific Results of the Exploring Voyage of HMS* Challenger *during the years 1873–1876*, vol 18, pt 3, plate 99. Edinburgh: Her Majesty's Stationery Office, 1887.

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ARTICLE

More Geological Reasons Noah's Flood Did Not Happen

Lorence G Collins and Barbara J Collins

Introduction

Young-earth creationists believe that there was a worldwide flood covering the earth and that virtually all fossil-bearing sedimentary layers, up until the most recent, were deposited by that Flood in about one year (Genesis 7:11–24 to 8:1–13; Whitcomb and Morris 1961; Morris and Parker 1987). Although the time for deposition by a one-year flood of the nearly 3.75 kilometers of sedimentary rocks on top of the Precambrian strata (Figure 1) to the present seems impossibly short, Oard (2002) argues for Flood Geology nonetheless. His conclusions rest on the notion that the modern evidence cannot be used as a key to the past, as the uniformitarian principle is typically applied (Oard 2002).

He writes

> uniformitarianism is a poor organizing principle and often invalid. For instance, sandstones, which make up approximately 20% of the sedimentary rocks on the earth, are consistently different from modern sand deposits. As an example, pure quartzites (orthoquartzites) are common in the older record but none seem to be forming today. Quartzite is metamorphosed sandstone. (Oard 2002:8)

Of course, even if uniformitarianism (as Oard [2002] has framed it) were completely false, it still would not make his alternative—a world-wide flood—true. He would need other evidence to support his model. So he goes on to say

> Furthermore, in the modern world sand generally accumulates in linear deposits while ancient sandstones form very large sheets: It is noteworthy that the most common sites of sand accumulation in the modern world are linear (beaches and rivers); yet most sands of the past form extensive stratiform deposits (Pettijohn 1975). (Oard 2002:8)

Therefore, he argues, "The evidence is consistent with the global Flood, which would be expected to deposit sand in sheets" (Oard 2002:8).

Thus, he believes that the deposition of the various sedimentary rocks around the world could have occurred in this incredibly short period of one year rather than over thousands or millions of years.

The uniformitarian principle and modern geologic interpretations

First of all, creationists who claim that uniformitarianism is an unreliable basis for interpreting the past do not seem to understand that modern geologists do not apply this prin-

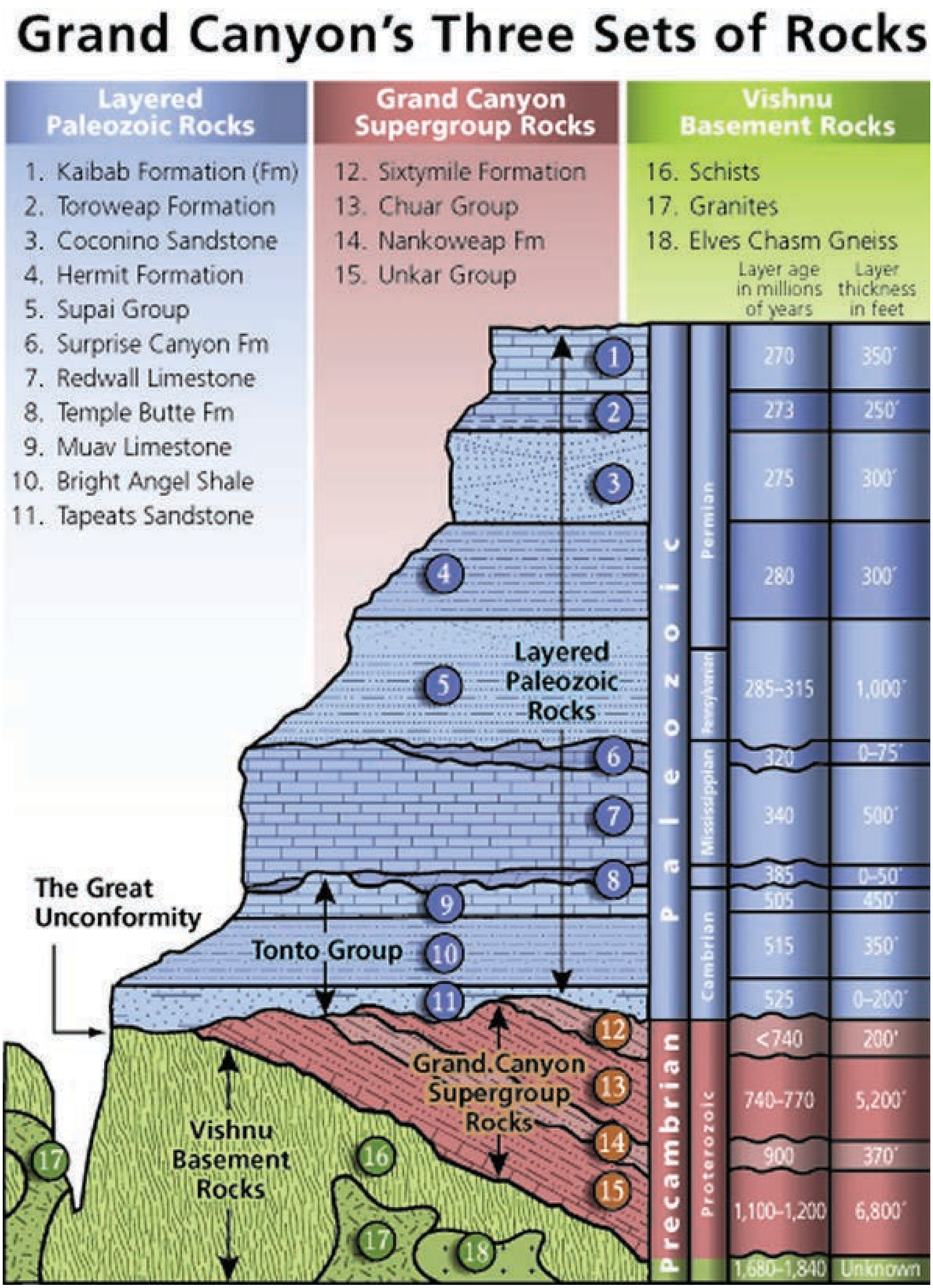

FIGURE 1. *Geologic time scale, showing period names and ages in millions of years of strata in the Grand Canyon. Copyright Grand Canyon Association. Used with permission.*

ciple to every geologic situation. It is merely the philosophical principle of simplicity. As Shea says (1983:105),

> Contrary to creationists' allegations, modern uniformitarianism makes no assertions about nature, but instead, tells scientists to choose the simplest hypothesis that both fits the observations and leads to greatest simplicity in overall theory. What the creationists attack, therefore, is not uniformitarianism as it is used by contemporary geologists, nor uniformitarianism as it has been clearly explained in several careful analyses published since 1965, but a false 19th[-]century uniformitarianism that has been abandoned.

The uniformitarian principle has been a useful idea, but too many examples exist in which the present is not the key to the past. For example, today we have an atmosphere with relatively abundant oxygen. In the past, the atmosphere was likely very poor in oxygen and rich in methane. Therefore, processes happening today would be different from those happening in the past. Modern geologists no longer strictly apply the idea of uniformitarianism except where the formations of rock structures, such as ripple marks, cross-bedding, and mud-cracks, can be observed that look like similar features in ancient rock layers. On that basis, Oard's (2002) harping on the shortcomings of uniformitarianism is both a strawman argument and an example of what Abrahamson and Smith (1993) called a "black/white" fallacy. Neither of these is a valid argument that automatically makes a global flood the only alternative.

Second, Oard (2002) makes some untrue statements about ancient sandstones in the geologic column (Figure 1). If Oard's descriptions of the sandstones were correct ("Quartzite is metamorphosed sandstone"), then these sandstones would be in the form of quartzites associated with marble, slates, schists, and gneisses; yet these co-existing metamorphic rocks do not occur in the geologic sedimentary column (Winter 2001). If the layers of sand in the geologic column were quartzites, as Oard (2002) claims, then they would have no pore spaces in them to enable them to be aquifers or reservoirs of methane gas or oil, as they commonly are.

Sand grains in sandstones in the geologic column are generally glued together by various kinds of cements (calcite, traces of hematite, which make the layers red, and/or secondary silica). Most sandstone layers in the geologic column are cemented by calcite or by calcite plus traces of hematite. Where secondary silica is the glue, the sandstone is generally harder and called orthoquartzite (or quartz arenites). Although orthoquartzites are uncommon in both modern and ancient environments (Scholle and Spearing 1983; Walker and James 1992), clearly Oard (2002) is misinformed about orthoquartzites being formed by metamorphism. Furthermore, orthoquartzites would not have formed under different conditions in the past from what we see today.

Third, it is simply not true, as Oard (2002) claims, that most modern sandstones are linear and ancient sandstones occur in sheets. It is true that along California's west coast, only linear beaches of sand are visible, and those extend from San Francisco to San Diego. Nevertheless, in several places, the drifting sands carried by long shore currents along these beaches drain down submarine canyons and then are deposited as sheets at the bottom of adjacent deep ocean basins (Inman 1980). On that basis, what we observe in ancient sandstone sheets of the supposed Noachian Flood can also be seen in recent basin deposits extending either from former submarine fans or from deltas that connect ultimately to the streams that brought the sands to these basins—in other words, normal transport of sand in water currents and not the result of a sudden or prolonged flood.

Sandstone sheets can be present in recent deposits and not only in ancient sandstones as Oard (2002) claims. Indeed, in the modern world most sands that are deposited are not in linear arrangements but in sheets that are deposited in a multitude of different environments. These include (a) glacial sands in outwash aprons at the foot of a melting glaciers, (b) eolian (wind) deposits in vast sheets of sand, as in the Sahara Desert and in other deserts around the world, (c) sand layers in alluvial fan deposits, d) lacustrine layers of sands in lake beds, (e) fluvial sands in wide flood plains, (f) delta sands, (g) estuarine sand deposits, (h) sands in tidal flats, and even (i) sheets of sand in continental shelf and slope deposits (Scholle and Spearing 1983; Walker and James 1992).

In addition, some linear sandstone deposits occur among the layers of sandstones in the geologic column which Oard (2002) says were deposited during Noah's Flood. An example is the Cretaceous Cardium sands reservoirs of Alberta (as Ken Wolgemuth pointed out to us). These ancient linear deposits are like the offshore barrier bars that occur as the Galveston Island in South Texas. These sandstone bars are bounded on one side by marine shales and on the other side by brackish-water shales of former lagoons. The similar Cardium offshore-barrier bar sands become traps for introduced oil. Thus, there are both linear sandstones and sheet sandstones that occur in the present as well as in the ancient past. Therefore, Oard's (2002) arguments are false, and in the case of sandstone deposits, the present can be the key to the past.

In order to evaluate Oard's (2002) position regarding the deposition of sand during Noah's Flood properly, we need to examine his whole model from a scientific viewpoint. For example, if the average geologic column of sedimentary rock deposited during Noah's Flood is about 5000 meters thick (Morton 2001), and it took less than one year to deposit this column of rock, then 50 meters of sandstone in the column should take less than 4 days to be deposited during the one year of deposition if all the various sedimentary rocks were deposited in 365 days. Is such a rapid rate of deposition of sandstone at all possible?

Before we answer this question, there are several important issues that Oard (2002) does not address. What rate of erosion is required to produce enough sand grains so that they could be part of a sedimentary column 5000 meters thick and be deposited during Noah's Flood? What rate of deposition is needed to deposit the sand grains in a single solar year? Where were all the sand grains before the Flood? Had they been turned into stone? Is Oard (2002) considering the rate of erosion during the first 2000 years prior to the Flood or during the Flood? Oard (2002) does not say if the sand was just lying around the planet until the Flood, which then picked it up and threw it around, or whether the Flood actually eroded granite masses and created the sand during the Flood or some combination of both.

Formation of sandstone

To address these issues, we need to start with how sandstone is formed. Sandstones consist of quartz grains that are produced and deposited by various eroding agents (streams, wave action, wind) and which ultimately are glued into a solid mass by some kind of cement (calcite, hematite, or secondary silica). These quartz grains, however, have to come from somewhere, and granitic igneous rocks are the probable sources. Oard (2002) fails to indicate sources of the sands that are supposedly deposited in one year. It is important to note that coarsely-crystalline granitic rocks (granite, granodiorite, and diorite), on average as a whole, contain about 10% quartz (personal observations; Winter 2001).

When granitic rocks are eroded, the quartz grains are loosened and carried away in streams, and these quartz grains would drop out early, forming layers of sands. How long would it take to make enough sand to form a layer of sandstone 50 meters thick? Because the earth in the creationists' model was formed about 2000 years before Noah's Flood (Whitcomb and Morris 1961; Morris and Parker 1987), a thickness of 500 meters of granite would need to be eroded in 2000 years to release enough quartz grains to enable the production of 50 meters of sandstone. On that basis, the rate of erosion would be 4 meters of granite per year. That is a lot of granite loss in those early days. One study indicates erosion rates of granite today to be about 0.0000137 meters per year (Duxbury 2009). Because sandstone layers that are alleged to have been deposited during Noah's Flood are many times thicker than 50 meters, the erosion rate of granite needed by the creationists to produce 20 percent of all the formations in the geologic column during Noah's Flood would have to have been very fast.

Then, would it even be possible for sand grains, if they were present in sufficient quantities, to be deposited in sandstone layers 50 meters thick during the Flood in just one year? Because sand grains have a relatively large size and are heavy, they do settle out of water readily—if the flood waters could suspend and move that quantity of quartz (including pebbles and cobbles in co-existing sandstone conglomerate beds) in the first place. So if there were sufficient quantities of sand grains available, then they might have precipitated from the flood waters, but the release of enough grains from the earth's rocks in 2000 years so that they were available to be deposited by the Flood would require a rate of erosion slightly more than 29 000 times what we observe today. And even dropping strict uniformitarian expectations, that is an exceptionally rapid rate of erosion. In any case, before or during the Flood, sand grains do not exist without extensive erosion requiring many thousands to millions of years unless the Creator just miraculously produces them, but then that option is not science.

What are the co-existing rocks that are interlayered with the sandstones?

Sandstone layers in the geologic column are commonly interlayered with shales and limestones. Many such shale layers have fossilized mud-crack prints in them, which are also associated with evaporite (gypsum and rock salt) deposits. Both the mud-cracks and the evaporite deposits can have formed only in surface-drying conditions that cannot have occurred if these were under water during Noah's Flood (Collins 2009). Other co-existing rock layers of Carboniferous, Permian, Triassic, and Cretaceous ages contain fossilized charcoal that indicates the prior existence of huge fires that also cannot have happened under water during Noah's Flood (Shen and others 2011; Senter 2011). Still other co-existing limestone layers in some places have caves and collapsed structures (sink holes) of karst topography that can have formed only when the limestone layers were raised above water and exposed to surface weathering conditions—for example, the Redwall Limestone in the

Grand Canyon (Hill and Moshier 2009) and some limestones in the geologic column in the Williston Basin (Morton 2001).

Thus, the many different kinds of geological scientific evidence show that Oard's (2002) belief that the sandstone layers sheets were formed during Noah's Flood has no merit.

Oard's uniformitarian arguments about radiolarian cherts

Oard (2002) also argues that because there are no known radiolarian cherts being formed today, the present cannot be a key to the past. He uses this observation to claim that deposition of the geologic column of sedimentary rocks was different during Noah's Flood from what occurs today. Chert is a rock that commonly occurs as rounded nodes or irregular globs a few centimeters wide and consists of cryptocrystalline quartz in which the fibrous crystals are too tiny to be seen with the unaided eye. This kind of quartz is also called chalcedony or flint when the nodules are found in limestone or chalk. It fractures in smooth curved surfaces, and several cultures have used this rock to make arrowheads, blades, and other tools because of its breakage pattern that produces sharp edges.

Radiolarian chert occurs in two forms: bedded chert associated with volcanic rocks and fine-grained terrigenous clastics (earthy fragmental grains) and nodular chert that occurs in limestone (Schwab 1992). Bedded chert is formed from the recrystallization of masses of radiolarian fossils in oceanic oozes in which the radiolarians have been almost entirely welded together as solid quartz (Chester 2003). In this kind of chert, remnants of the former radiolarians can be seen. We would expect that nodular chert or flint occurring in limestone (or chalk) without remnants of radiolarians would have a different origin, such as from dissolved silica carried into the oceans by continental streams.

Because the solubility of silica is very low at temperatures that are normally found in streams and in the ocean and also quite low where the water is acidic (Rowe and Fournier 1977), the dissolved silica that is carried into the ocean tends to come out of solution quickly where the oceanic water is relatively acidic. Thus, it can coalesce into gel-like masses (opal) that eventually accumulate and harden to form flint. Any dissolved silica that is incorporated into opal in radiolarians is eventually dissolved and precipitated in the chert nodules (Prothero and Schwab 1996).

Because quartz is very insoluble in surface waters, the source of the silica ultimately to form the chert comes from weathered feldspars that release the silica during hydrolysis to produce clay. Like the source of sand for the sandstone layers in the geologic column, which takes thousands of years of erosion of granite to produce the sand grains, both the source and time needed to produce large quantities of dissolved silica also require weathering of feldspars in granite during this same long period of time before the formation of large quantities of radiolarians can occur. As with the erosion of granite in the 2000 years creationists allow before Noah's Flood and the one year during the Flood, the weathering of feldspars cannot produce enough silica in sufficient quantities in all the many different chert-bearing formations in the many different geologic periods (Figure 1). This is another reason why a global Noachian Flood did not happen.

However, there is no geologic environment today that is both supplying abundant dissolved calcium that might be precipitated by organisms in tiny carbonate shells to form limestone or chalk while simultaneously supplying abundant dissolved silica. So there are no places where flint is being formed today in limestone or chalk beds. So what Oard (2002) states is true in the strict sense: the present in this case is not the key to past. But he does admit that radiolarian oozes are currently being deposited on the ocean floor. Nevertheless, in order to examine Oard's (2002) claims properly, we need to look at the whole issue of radiolarians and how radiolarian chert is formed.

What are radiolarians and how fast are they converted into chert?

Radiolarians are tiny organisms that live in the oceans and have skeletal structures (Figure 2) composed of amorphous silica (opal A). To form radiolarian chert, opal A in the radiolarians, after long periods of time, converts to another kind of amorphous silica (CT opal) which transforms into tiny crystals of cristobalite and tridymite (varieties of quartz), then into chert.

Although radiolarian-bearing oozes are not as abundant as other kinds of oozes, they are produced where abundant silica-bearing, hot, hydrothermal fluids come from volcanic sources, such as "black smokers," on the ocean floor (Roberts 2009; Hüneke and Mulder 2011). High temperatures (>350°C) and basic rather than acidic waters are needed to carry significant amounts of dissolved silica (Rowe and Fournier 1977), and these conditions occur around volcanic vents (black smokers) near oceanic spreading centers. These high temperatures and pressures are required to cause these transformations (personal communication from Rudi Pohl; Roberts 2009; Hüneke and Mulder 2011). The absence of these conditions is the reason why Oard (2002) has never observed modern, thick, radiolarian chert beds being formed from radiolarians. Nevertheless, the process of radiolarian-bearing chert begins with the deposition of radiolarian oozes on the ocean floor, and that is going on in the oceans today. Under the right conditions, these deposits eventually will become part of radiolarian chert beds (Roberts 2009; Hüneke and Mulder 2011).

Moreover, there is no logical physico-chemical reason why rates of conversion of radiolarians into chert should be any different today than in the past. Radiolarians today are also being subjected to increasing temperatures and pressures as their exoskeletons precipitate to the ocean floor and are progressively buried to greater depths. Therefore, it is clear that the process that produces the radiolarian ooze—which is the first step in the formation of radiolarian chert—is going on today. So there is no need for any additional explanation, and certainly not a proposal of an accelerated rate of transformation of radiolarians into chert during the supposed single year of deposition during Noah's Flood, but let's examine what that proposal would entail.

Deposition of radiolarian fossils

To produce a 50-meter-thick radiolarian chert layer in 4 days (the deposition rate required for 5000 meters of rock to be deposited during a 1-year flood), one must consider how fast radiolarian skeletons can accumulate on the ocean floor. Takahashi (1981) and Takahashi and Honjo (1983) found that radiolarians will take from 2 to 56 weeks to fall through a water column of 5000 meters, which is far, far longer than the 4 days that are available in the young-earth model used in Flood Geology. The slow rate is because radiolarians have pores or projecting spines in their skeletal structures (Figure 2) that increase friction with the water and will slow their descent. Therefore, if natural physical laws are obeyed, the accumulation of astronomical numbers of such tiny radiolarian fossils to be parts of a

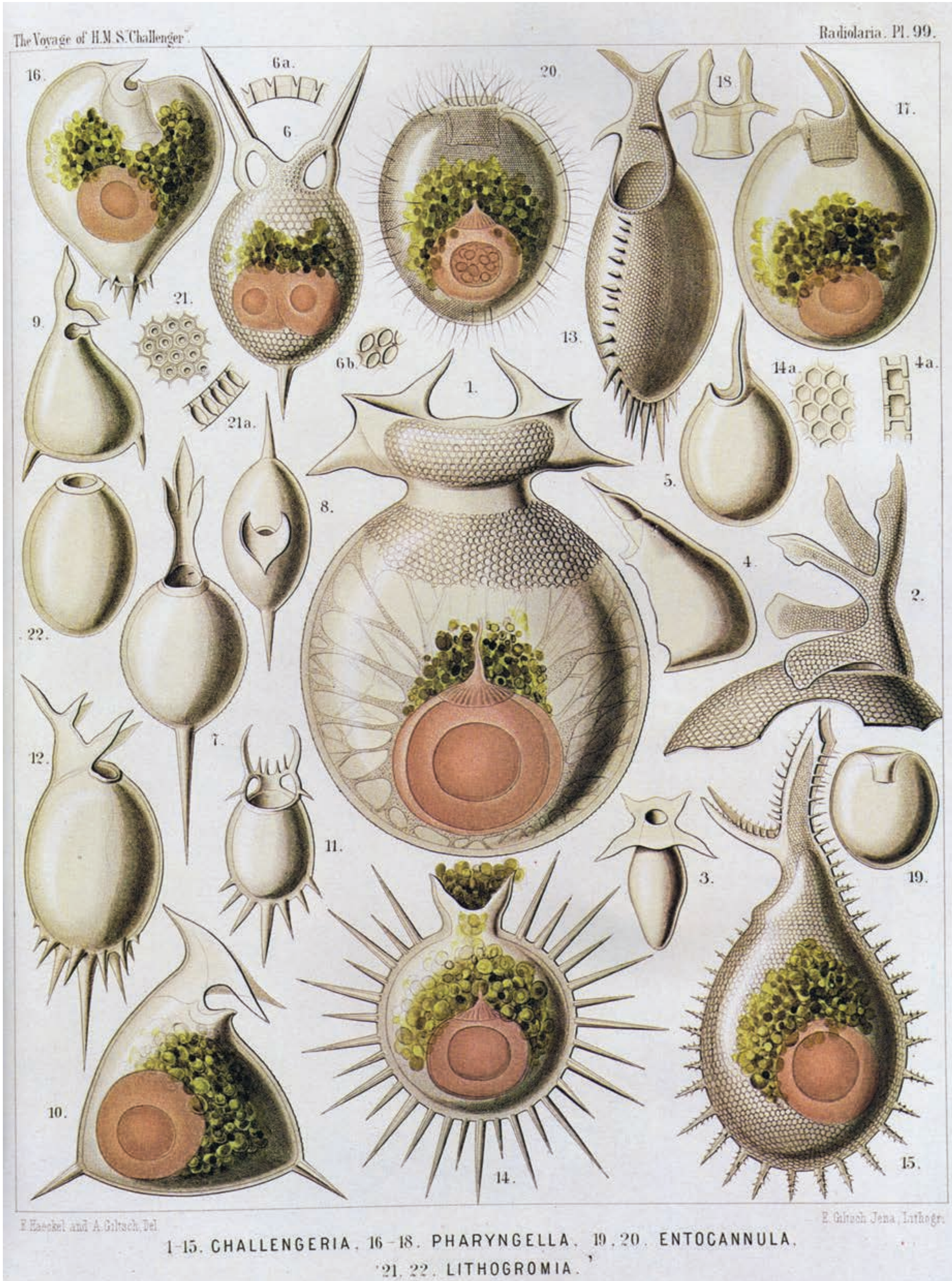

FIGURE 2. *Examples of the variety of shapes of radiolarian skeletons. Source: Ernst Haeckel,* Report on the Radiolaria collected by HMS *Challenger,* Report of the Scientific Results of the Exploring Voyage of HMS *Challenger* during the years 1873–1876, *vol 18, pt 3, plate 99. Edinburgh: Her Majesty's Stationery Office, 1887.*

layer 50 meters thick is certainly going to take much, much more time than the 4 days that would be available during Noah's Flood.

In our modern oceans other deposits also show similar slow rates of accumulation. For example, there are several kinds of other oozes composed of diatoms, coccoliths, foraminifera, globigerina, and pteropods. These oozes occur in different environments from those where radiolarian oozes occur for a variety of geochemical reasons. Such oozes cover as much as 48% of the Atlantic and Pacific Ocean floors. Calcareous deposits (oozes) composed of foraminifera, coccoliths, and pteropods, accumulate at a rate of about 0.3–5 centimeters per 1000 years and range up to 1400 meters thick (Riley and Chester 1971). If this range of deposition rates of the organisms is correct, then it would take several million years to form deposits that are 1400 meters thick. Comparable rates for deposition of radiolarians in modern oceans certainly could not have accumulated in the 4000 years since Noah's Flood or in one year during the Flood. (Note that the Creation Museum's date for the Flood is 2348 BCE.) If Oard (2002) counters with the argument that the deposition rates were different then, those rates would be so very different from any that happen naturally that they would require miracles to occur; and including miracles in the model would not be basing the model on science.

Furthermore, because radiolarian cherts are interlayered with sandstones in the geologic column at many different levels (Grünau 1965), and if both kinds of materials are deposited in one year in a huge "bath tub" of water called Noah's Flood, there are two competing rates of deposition. The "heavy stuff," the sand grains, should settle out relatively rapidly at first and go to the bottom of the column, and the "light stuff," the radiolarians, which settle extremely slowly because of friction, should settle out last and be on top. But that is not what we observe. During a major flood, it would not be physically possible to alternate settling heavy and light stuff. Moreover, Noah's Flood would not be able to sort more than 4000 different species of radiolarians (Takahashi and Honjo 1983)—all of which are essentially the same size—into the evolutionary sequence that appears in the geologic record.

Conclusions

Young-earth creationists need to provide scientific data and research that honestly supports their models. True scientists cannot choose only data that fit their models and ignore data that do not fit. Although Oard (2002) claims he is basing his arguments on science, his model only seems to work because he overlooks or ignores extensive geologic literature. Contemporary geologic research contradicts his creationist model that sandstones deposited during Noah's flood are unique in their characteristics and that radiolarian chert layers can be deposited in one year. He might as well just have said "and then a miracle occurred" as a basis for his Flood model. Of course, there is no scientific argument against a miracle.

The scientific evidence strongly suggests that a global Noachian Flood did not happen. Oard's (2002) model requires not only the rejection of strict uniformitarian models, as he claims, but also a repudiation of practically all the geologic processes that geoscientists have studied and confirmed for decades. A model of geologic processes that only works by rejecting the fundamental knowledge in the geosciences is not a scientific model at all, but little more than wishful thinking.

Acknowledgments

We wish to thank Rudi Pohl for contributions regarding radiolarites, Peter Fischer for providing information about submarine canyon depositions off the coast of California, and Ken Wolgemuth for many suggestions regarding the geology of oceanic oozes and other associated topics as well as many excellent editorial suggestions. We also acknowledge the help of two reviewers whose critical comments markedly improved the article.

Glossary of Terms

black smoker: a chimney-like structure on the ocean floor near volcanically active areas through which hydrothermal fluids move carrying dissolved minerals and gases; often black.

coccolith: a microscopic calcite skeletal plate that protects certain marine phytoplankton and in a fossilized state forms chalk and limestone deposits.

cryptocrystalline: a rock texture composed of such tiny crystals that its crystalline nature is only barely revealed in microscopically thin sections.

diatoms: a major group of algae that has siliceous shells, most of which are single celled.

diorite: a coarsely crystalline igneous rock that contains sodic plagioclase feldspar and ferromagnesian silicate minerals, such as biotite, hornblende, and pyroxene.

Foraminifera: a large group of amoeboid single-celled animals that live in surface waters.

Globigerina: one-celled marine Foraminifera with calcareous shells.

granite: a coarsely crystalline igneous rock that contains quartz, more potassium feldspar than plagioclase feldspar, and ferromagnesian silicate minerals (commonly biotite).

granitic rock: a coarsely crystalline igneous rock that contains varying percentages of quartz, plagioclase feldspar, potassium feldspar, and ferromagnesian silicate minerals, such as biotite and hornblende.

granodiorite: a coarsely crystalline igneous rock that contains quartz, more sodic plagioclase feldspar than potassium feldspar, and ferromagnesian silicate minerals, such as biotite and hornblende.

limestone: carbonate rock; $CaCO_3$.

oceanic oozes: fine-grained sediment that has accumulated by settling of particles through sea water to the ocean floor. The particles can be composed of hematite iron oxide, meteorite dust, clay, radiolarians, diatoms, coccoliths, foraminifera, globigerina, pteropods, and radiolarians.

pelagic: derived from material that has fallen to the bottom from the upper waters of the sea.

phytoplankton: microscopic plant-like organisms.

pteropod: a specialized group of free-swimming sea snails and sea slugs.

radiolarians: one-celled animals that produce intricate skeletons of silica.

uniformitarian principle: "The present is the key to the past."

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FEATURE

People and Places: Miriam "Ma" Ferguson (1875–1961)

Randy Moore

FIGURE 1. *Miriam "Ma" Ferguson. Undated; Library of Congress LC-DIG-ggbain-37272.*

Miriam Amanda Wallace was born in Bell County, Texas, on June 13, 1875. She attended Salado College and Baylor Female College, and married James Edward “Pa” Ferguson in 1899. Miriam became the First Lady of Texas when her husband was elected governor in 1915. As a result of Miriam’s dedication to her husband and two daughters, as well as the combination of her initials, she became known as “Ma”.

When James Ferguson was impeached during his second term, Miriam announced her candidacy for governor. Her campaign, which was meant to vindicate her husband’s reputation, was fiscally conservative, anti-KKK, and anti-prohibition. In November 1924, she handily defeated Republican nominee George Butte. Although Ferguson was the first woman to be *elected* governor in the United States, she was *inaugurated* 15 days after Wyoming’s Nellie Ross, and thereby became the second female governor in US history.

Ferguson entered the evolution–creationism controversy in October 1925, when, as head of the state textbook commission, she banned the use of biology textbooks that included evolution in public schools. That year, the state adopted Truman Moon’s *Biology for Beginners*, but only on the condition that its three evolution-related chapters be removed. Ferguson threatened to fire and prosecute any teacher who used an unapproved book, and she justified her edict by reminding Texans that she was a Christian and a mother. For the next several decades, the ban imposed by Ferguson forced publishers to produce special editions of their biology books for Texas classrooms.

Ferguson was criticized for granting contracts to her friends and political supporters, as well as for pardoning an average of 100 convicts per month. These controversies, as well as the opposition of anti-evolution crusader (and fellow Texan) Frank Norris, helped Attorney General Daniel Moody defeat Ferguson in 1926, and in 1930 Ferguson was defeated in another campaign for governor. In 1940, the 65-year-old Ferguson accepted a draft for the gubernatorial nomination, but lost to incumbent governor W Lee O’Daniel, and never again ran for public office.

“Ma” Ferguson died of heart failure on June 25, 1961. She rests beside her husband in the Texas State Cemetery in Austin, Texas.

About the author

Randy Moore is the HT Morse–Alumni Distinguished Professor of Biology at the University of Minnesota. His latest book, coauthored with Sehoya Cotner, is *Arguing for Evolution: An Encyclopedia for Understanding Science* (Santa Barbara [CA]: Greenwood, 2011).

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RECAPITULATIONS

Reply to Laurence A Moran's review of *Evolution: A View from the 21st Century*

James Shapiro

Before I saw Laurence A Moran's book review (Moran 2012), I wrote the following: "It is a shame that NCSE chose Larry Moran to review my book; not because of anything he said in the review but because he is hostile to new ideas and perspectives."

A year ago, Moran posted a piece entitled "Physicists and biologists" on his Sandwalk blog (http://sandwalk.blogspot.com/2011/08/physicists-and-biologists.html). In this post, he ridiculed the enthusiasm I expressed in the book for physicists coming into evolutionary studies and bringing new skills and new ideas.

> Meanwhile, I welcome all those physicists who know nothing about evolution, protein structure, genetics, physiology, metabolism and ecology. That's just what we need in the biological sciences to go along with all the contributions made by equally ignorant creationists.

What a great way to make new friends for evolution science—equating physicists with creationists and calling them "equally ignorant"!

The scientific community is engaged in an important struggle to convince the public of the reality of evolution and the importance of evolution science. NCSE is the organization entrusted with representing us. The shame in NCSE's choosing Moran as a reviewer is that he seems to seek to alienate everyone not educated in a certain way; hardly the best choice to convince the public that evolutionists are open-minded and that evolution science is an active, exciting and forward-looking field.

Now that I have seen the review, I have to conclude that my expectations were, sadly, fulfilled. Let me illustrate what I mean by summarizing what I tried to say and giving a few quotations from the review.

My argument is that molecular research over the past sixty years on DNA change processes has taught us that virtually all genetic variation results from the action of regulated cell biochemistry, including a wide array of cutting, splicing and polymerizing functions that I summarize under the term "natural genetic engineering". I assert that this realization represents a fundamental shift from the conventional view that genetic change is a random, accidental process.

I discuss these molecular discoveries, which continue well into the 21st century, in detail in Part II of my book, entitled "The genome as a read-write (RW) storage system." I used this title because another way of stating the conceptual change I see is to say that we have to substitute a RW view of the genome for the conventional notion of a "read-only memory"

(ROM), which changes only by copying errors. As far as I know, others had not made this argument before I started writing about it in primitive fashion almost 30 years ago (Shapiro 1983). I suspect the idea of a RW genome is still new to most readers of RNCSE.

In his review, Moran tells us "*I have to confess that I skipped most of this chapter* [that is, Part II, emphasis added]. I know about genome rearrangements and so does everyone else who has read a textbook in the past forty years" (Moran 2012:9.2). Frankly, I am not aware of textbooks that have routinely covered mutator polymerases, diversity-generating retro-elements, retrosplicing group II introns, CRISPRs, SINE elements and many other natural genetic engineering systems over the past 40 years. In fact, one of the reasons for writing the book was that people who had seen my journal articles would often ask, "Is there a book where I can read more about this?"

Moran goes on to write scornfully about the large amount of tabulated information I included, "A litany of examples is not only overkill, it smacks of an agenda" (Moran 2012:9.2). I did have an agenda, to be sure. As I told the reader in my introduction,

> The goal of this book is to acquaint you with previously "inconceivable" but currently well-documented aspects of cell biology and genomics so that you will be ready for the inevitable surprises in evolutionary science waiting for us as this new century runs its course. (Shapiro 2011:5)

How else to do this but by laying out the facts exhaustively and organizing them in a way that lets them tell a coherent story by themselves? Moran, by acknowledging that he did not read the most detailed part of the book, demonstrated his lack of interest in learning what the facts or my interpretation of them might be.

Ignorance of what I actually wrote in detailed support of my argument is not the only shortcoming of Moran's review. He makes a number of erroneous statements that clearly seek to minimize the evolutionary importance of what I had to say in the book.

For example, I cited whole genome duplications deduced from sequencing as a key part of the DNA evidence for abrupt, multi-character changes in evolution. Such duplications have been fully documented in yeasts and other fungi, in protists, in an extremely wide range of flowering plants (Darwin's "abominable mystery"), and at the origins of vertebrate evolution.

To counter my position, Moran writes,

> His main thesis seems to be that such mutations are not random as neo-Darwinism demands. Genome duplication is one example. There may have been two genome duplications in the vertebrate lineage. Both of them occurred in fish. (Moran 2012:9.2)

This is wrong and misleading. There were indeed two genome duplications in the history of teleosts, at key points of phylogenetic diversification, but they were far from unique in vertebrate evolution. I was quite explicitly referring to the pair of duplications that, successively, coincided with the origins of all vertebrates and then of all jawed vertebrates (Nakatani and others 2007). I think *RNCSE* readers will agree that these certainly constituted major events in animal evolution.

Moran continues to depict what I had to say about the evolutionary role of natural genetic engineering as exaggerated:

> Another example involves transposons. In the hominid lineage there may be evidence of a few transposon-related genome alterations that turned out to be beneficial and subsequently became fixed in the population. That's a rate of approximately one every million years or so. (Moran 2012:9.2)

This downplaying of the role of transposons (a class of mobile genetic elements) is quite an ironic assertion. The rate with which "transposon-related genome alterations" are being discovered by parsing genome sequences is truly astonishing. At the end of last year, a group of bioinformaticians published a *Nature* paper examining the human genome as compared to 29 other aligned vertebrate genomes. They said:

> We report … 280,000 non-coding elements exapted from mobile elements and more than 1,000 primate- and human-accelerated elements. (Lindblad-Toh and others 2011:476)

Perhaps Moran would not have made his tendentious error about the rarity of "transposon-related genome alterations" if he had not have skipped so much of the core of my book.

Finally, since I spoke of cell sensory mechanisms and cognition, Moran pulled out the "intelligent design" card and made disparaging use of the fact that I published two peer-reviewed papers on the importance of repetitive DNA in 2005 with Richard von Sternberg (Shapiro and Sternberg 2005; Sternberg & Shapiro 2005). Sternberg turned out to become something of an ID cause célèbre the following year.

> Shapiro's views seem to be philosophically similar to those of Richard Sternberg (Richard von Sternberg)—the two of them published several articles together a few years ago. (Moran 2012:9.3)

What Sternberg's personal views have to do with these papers or the contents of my book, readers can judge for themselves. I am happy to stand by their scientific validity. The fact Moran chose to use a "guilt-by-association" approach to criticize my book speaks volumes about the character of his review.

Let me reiterate in closing that it is a shame NCSE chose someone who wrote such a closed-minded and ill-informed review of my book as Larry Moran did. This review will only help the opponents of evolution science. Moran's review fits the creationist cartoon of evolutionist views all too well: prejudiced, uninterested in facts, and unwilling to change positions in the face of new ideas and data.

The truth is that this happens to be one of the most exciting periods in evolutionary science because of all the revolutionary new molecular data. I invite *RNCSE*'s readers to find some of it in my book or in the copious reference lists I have posted online at http://shapiro.bsd.uchicago.edu/evolution21.shtml.

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Note from the Editor

As is our policy in *RNCSE*, authors whose books have been reviewed have an opportunity to respond to the reviews that we publish. James A Shapiro submitted this response to the review of his *Evolution: A View from the 21st Century* by Larry Moran. It is also our standard practice to give the author of the review an opportunity to reply to the book author's response. In this case, Moran did not respond to our offer to publish his reply to Shapiro's response. Therefore, we print Shapiro's comments here without a reply from Moran and consider that the issue is closed with respect to further exchanges in *RNCSE*.

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REVIEW

Evolutionary Theory: Five Questions

edited by Gry Oftedal, Jan Kyrre Berg O Friis, Peter Rossel, and Michael Slott Norup
Copenhagen, Denmark: Automatic Press, 2009. 245 pages

reviewed by Robert Arp

Reading just a bit of Darwin and a few decent explanations of evolutionary theory can be life-changing for a person. For me, by the time I finished a few chapters of Robert Pennock's *Tower of Babel: The Evidence against the New Creationism* (1999) while in grad school trying to solidify a philosophy dissertation topic, I had been born again. Along the way, however, Francisco Ayala's "Teleological explanations in evolutionary biology" (1970), Michael Ruse's "Function statements in biology" (1971), Richard Dawkins's *The Blind Watchmaker* (1996), and Tim Berra's *Evolution and the Myth of Creationism* (1990) were significant in setting the stage. *Evolutionary Theory: Five Questions* only serves to solidify the conviction that I am not alone in my conversion.

What I communicated in the previous paragraph—namely, a bit of my personal journey—is similar to part of what the reader will find in *Evolutionary Theory: Five Questions*, with some of the greatest evolutionary theorists sharing their experiences of how and why they were drawn to Darwin. The book asks the same five questions of well-known people doing work in (or directly connected to) evolutionary theory, and the reader is then privy to the part-informative, part-explanatory, part-argumentative, and even part-sentimental reflections of these people. The questions in the book are:

1. Why were you initially drawn to discussions and research on evolution (or evolutionary aspects of your field)?
2. What does your work reveal about biological evolution (or evolutionary aspects of your field) that other academics, citizens, philosophers or biologists typically fail to appreciate?
3. What, if any, practical and/or social-political and/or moral obligations follow from your work on evolution?
4. What do you see as the most interesting criticism against your position in the biological or philosophical discussion of evolution?
5. With respect to present and future inquiry, how can the most important problems concerning evolutionary theory (or evolutionary aspects of your field) be identified and explored?

And the people answering the questions are Patrick Bateson, John Tyler Bonner, Terrence W Deacon, Daniel C Dennett, Douglas J Futuyma, Peter Godfrey-Smith, Brian Goodwin, David L Hull, Eva Jablonka, Philip Kitcher, Ulrich Kutschera, Richard Levins, Elisabeth A

Lloyd, Stuart A Newman, Samir Okasha, Susan Oyama, David C Queller, Michael Ruse, Geerat J Vermeij, Andreas Wagner, and David Sloan Wilson. (The reader will note that the responses of Bonner, Futuyma, Kitcher, Vermeij, and Wagner follow the general pattern and spirit of the five questions, but the section headings are different in their chapters.)

There are a few reasons why neophytes and experts, as well as critics, of evolutionary theory should read this book.

Neophytes in evolutionary theory should read this book for the simple fact that—given the breadth and depth of scholarship represented—they will find many of the standard topics and current controversies in evolutionary theory presented. For example, the late Brian Goodwin makes an important clarification about how natural selection works, noting that because behaviors and morphologies emerge in developing organisms, natural selection "has an important role to play in evolution as a dynamic stability testing process, but it is not responsible for producing the distinctive forms of living organisms in the first place. [Natural selection] accounts for the differential abundance of different species, not their origins" (p 45). At first blush, this sounds somewhat controversial, and causes the reader not only to want to read on in the chapter (and in the book), but also to investigate the fairly recent science of evolutionary developmental biology (evo devo) to see the ways in which natural selection is being recast in the biological sciences (see, for example, Carroll 2006).

Neophytes also should read this book to see what motivates someone to pursue research in evolutionary theory in the first place. As David Queller notes, the attraction includes the fact that evolution is a "big theory ... Darwin was the Newton of the living world." And just to sweeten the pot, he also notes that one need not be a "math geek in order to understand it" (p 171–2 … of course, no offense to mathematicians). In other words, Queller is communicating that the basic principles of Darwinian evolution are not, well, "rocket science." The neophyte will also find more than one theorist in the book claiming something similar to Samir Okasha's point, namely, that "Darwinian evolution is an extremely powerful idea, with an appealing simplicity and generality" which can "shed light on diverse topics" (p 143). And many of the theorists explain clearly and exactly what areas are bathed in that Darwinian light.

Experts in evolutionary theory should read this book for the vicarious nostalgia they will experience as the theorists recall life in graduate school, or in the laboratory, or at conferences, where they were first impacted by Darwin. There's a bit of the "Where were you when X happened?" that will no doubt strike a chord with the reader. When I read Michael Ruse recounting that "evolution was just the most exciting idea I had ever encountered" (p 188), for example, I was taken back to the summer of 2000, while I was sitting in my easy chair with Pennock's *Tower of Babel* in my hands. Also, though we may know a theorist's writings and ideas inside and out, some will be pleasantly surprised to read a bit about a theorist's journey that led up to a famous book or article written by her/him. I found that, on more than one occasion, I said to myself: "Ah, so that's where she got her ideas" or "I see now … that's how his work began. Neat."

Further, because it's difficult for someone to be a master of everything associated with a particular discipline, experts in evolutionary theory should read this book not only to get a sense of the present debate concerning a particular area in evolutionary theory, but also to see where a particular position or idea has its failings and criticisms. On these scores, the reader will find that a theorist's responses to the second and fourth questions (mentioned above) are particularly enlightening.

The second and fourth questions are also a reason for critics of evolutionary theory to read this book. Of course, there are legitimate critics who, for example, question the current explanatory mechanisms associated with evolution and the biological sciences: such criticism is always welcome, as when David C Queller (agreeing with part of Stephen Jay Gould's criticisms) claims that scientists "use mechanisms to try and purge bias and approach the truth" (p 179). Then there are those not-so-legitimate critics of evolutionary biology—discussed by Ulrich Kutschera—who maintain that "scientists will never know everything; therefore, Biblical myths may be true and become part of evolutionary biology" (p 100). This brand of critics may find responses to the third question off-putting or even infuriating. In his response to the third question, Kutschera claims that his work had led to the cessation of anti-evolution films produced by a company in Berlin. This may be worrying even for non-creationists, for, in the spirit of John Stuart Mill's arguments and justification for public discourse in his famous work *On Liberty*, it is important for all opinions—even patently false ones—to be expressed. In any case, there are plenty of legitimate criticisms of evolutionary theory mentioned and explained in this book.

In the book, Elisabeth A Lloyd claims that the "best way to identify and explore the most important problems concerning evolutionary theory is to talk with and hang around evolutionary biologists" (p 121). While maybe not the second, or even the third, best way, reading *Evolutionary Theory: Five Questions* offers a decent opportunity to identify and explore evolutionary theory, as well as make you feel like you're hanging around with some pretty special people.

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Robert Arp is the author of *Scenario Visualization: An Evolutionary Account of Creative Problem Solving* (Cambridge [MA]: MIT Press, 2008). He is also the editor, with Francisco J Ayala, of *Contemporary Debates in Philosophy of Biology* (Malden [MA]: Wiley-Blackwell, 2009) as well as the editor, with Alexander Rosenberg, of *Philosophy of Biology: An Anthology* (Malden [MA]: Wiley-Blackwell, 2009). For details, visit http://www.robertarp.webs.com/.

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REVIEW

Transformations of Lamarckism: From Subtle Fluids to Molecular Biology

edited by Snait B Gissis and Eva Jablonka
Cambridge (MA): MIT Press, 2011. 432 pages

reviewed by Francesca Merlin

In June 2009, the 23rd Annual International Workshop of the History and Philosophy of Science took place in Jerusalem to celebrate the 200th anniversary of the publication of Lamarck's *Philosophie Zoologique* (1809). The workshop resulted in a volume: Snait B Gissis and Eva Jablonka's *Transformations of Lamarckism*. More than just celebrating the 200th anniversary of the publication of *Philosophie Zoologique*, the main aim of this collective volume is to cast a light over the contemporary relevance of some of Lamarck's ideas—specifically, ideas about the generation of developmental variation and its role in the change of organisms—to recent advances in biology. The central message of the volume is that a Lamarckian perspective should be taken into account in biology in order to produce a new evolutionary synthesis that would describe and explain the biological world better than the classical theory of evolution (that is, the Modern Synthesis).

As conveyed by the title, Lamarckism includes very different perspectives, not all of which necessarily correspond to the historical Lamarck's ideas. That is why, in this volume, the term "Lamarckism" refers to a general stance about the way organisms change over time. More precisely, "Lamarckism" here is focused on the generation of developmental variation: the causal role of the environment in development is one of its main themes. "Darwinism" here is defined as a view attributing a central role to natural selection, thereby stressing the causal role of the environment in the selective process. As explicitly stated in the preface, no contributor means to oppose Lamarckism to Darwinism. There are advantages and disadvantages to this terminology, as I shall explain.

The volume is structured into five parts, preceded by two introductory essays dealing with the new perspectives opened by Lamarckian problematics (Gabriel Motzkin) and with Lamarck's life and way of thinking (Pietro Corsi) respectively. Each part explores the topic of the volume from a specific point of view expressed in its title: "History", "The Modern Synthesis", "Biology", "Philosophy", "Ramifications and future directions". All these aspects are essential in order to understand the transformations of Lamarckism over time and its relevance with respect to the new developments in contemporary biology.

The first part—"History"—presents the historical transformations of Lamarckism, from the 1820s to the 1940s. It addresses in particular the diversity of Lamarckian ideas and their relation to Darwinism in different countries and over time (Sander Gliboff; Charlotte Weissman; Laurent Loison). It is worthwhile reading it for the following reasons. First, it gives the chance to have a fairly comprehensive image of the varieties of Lamarckism. In particular, it shows that the common tendency to identify it with the inheritance of acquired characters is wrong-headed. Actually, the inheritance of acquired characters was a widespread and uncontroversial idea in the 19th century (Richard W Burkhardt Jr). Second, it explores the relationship between biology and other dimensions and disciplines, for instance, the impact of the political situation of a nation on the acceptance or the rejection of Lamarckism (Nils Roll-Hansen), and the way evolutionary (Lamarckian) ideas as a metanarrative had an influence on the emerging field of sociology during the second half of the 19th century (Snait B Gissis).

The second part—"The Modern Synthesis"—is about the classical theory of evolution, which emerged between the 1920s and the 1950s as the synthesis of Mendelian genetics and Darwin's vision of evolution by natural selection. The volume rightly stresses the fact that the Modern Synthesis was constructed in opposition to Lamarckism: it relied on a Weismannian view of Darwinism and therefore centered on a rejection of the inheritance of acquired characters (supposed to be typically Lamarckian). More broadly, the Modern Synthesis represented a rejection of soft inheritance, defined as the gradual change of the hereditary material by use and disuse, by some internal progressive tendency, or by the direct effect of the environment (Marion J Lamb; Adam Wilkins). In so doing, biologists of the Modern Synthesis produced the split between development and heredity, which characterized biology until the 1980s (Scott Gilbert). This second part of the volume already conveys its central message: that it is now time to articulate the study of development and evolution in a broadly Lamarckian way in order to describe and explain the way the biological world changes over time more adequately.

The third part—"Biology"—is pretty technical. It deals with the nature, origins, construction, and inheritance of developmental variation, that is, phenomena that biologists often describe as Lamarckian. It is composed of a series of informative articles introducing new research advances in biology about the following topics: developmental and phenotypic plasticity (Stuart A Newman and Ramray Bahat, Erez Braun and Lior David, Sonia E Sultan, Marcello Buiatti), different forms of epigenetic inheritance (Eva Jablonka, Minoo Rassoulzadegan, Peter D Gluckman, Mark A Hanson and Tatjana Buklijas, Marcello Buiatti, Moshe Feldman and Avraham A Levy, Jan Sapp, Scott Gilbert), stress responses (Arkady L Markel and Lyudmila N Trut; Sivan Pearl, Amos Oppenheim and Nathalie Q Balaban). It exhaustively shows why, in the light of such new developments, biology needs a revival of a Lamarckian perspective, in particular the idea of soft inheritance reformulated in modern epigenetic terms. More precisely, it provides good evidence that the inheritance of non-genetic factors and, more broadly, development can play an explanatory role in biology.

The fourth part—"Philosophy"—contains some reflections on the possible integration of a Lamarckian perspective in contemporary biology and on its implications, both from a theoretical and a methodological point of view (Ayelet Shavit and James Griesemer, Paul E Griffiths, James Griesemer, Ehud Lamm, Evelyn Fox Keller). It addresses the urgent question of whether new discoveries on developmental variation can be integrated into current evolutionary models or are definitely incompatible with them. Like the "Biology" part, the fourth is pretty technical, discussing in detail how biologists should proceed in order to integrate such new developments into the traditional framework. Indeed, Gissis, Jablonka, and the contributors to this fourth part explicitly claim that there is no clear-cut distinction between philosophy of biology and theoretical biology.

The fifth and last part—"Ramifications and future directions"—is very short and deals with specific topics that have not been addressed before in the volume. A few short chapters discuss Lamarck's take on the mind-body problem (Simona Ginsburg), the Lamarckian theme of progressive evolution (Francis Dov Por), epigenetic inheritance in prokaryotes (Luisa Hirschbein), and the implications of a return to a Lamarckian view for the societal role of modern biology (Raphael Falk, Alfred I Tauber). This final part ends with a critical discussion of the idea of epigenetic inheritance (Adam Wilkins), which, unfortunately, is not developed enough.

The best way to read *Transformations of Lamarckism* is to look first at the introductory essays, which provide an analytical presentation of the topic under discussion in each part and chapter, and to decide where to go from there. In fact, the volume is not particularly designed to be read from the beginning to the end, as one long argument; rather, readers are allowed, and even invited, to choose chapters guided by their own specific interests. A glossary that clarifies the notions repeatedly invoked through the entire volume will aid readers.

The main strength of the volume is that it indeed provides, as promised, an analysis of the relevant dimensions (historical, biological, and philosophical) of Lamarckism. This is meaningful because all these aspects should be taken into account in order to evaluate the contemporary relevance of Lamarckism with respect to the new developments in biology. The volume succeeds pretty well in addressing these three dimensions, in particular the historical and the biological. The "Philosophy" part could have been further developed in order to deal with some other central issues raised by the return of a Lamarckian perspective in biology (such as the role of chance in development and evolution). Additionally, by emphasizing that Lamarckism, as defined here, is not in opposition to Darwinism, the book acknowledges that Lamarck and Darwin had many points in common, in particular the 19th-century idea of the inheritance of acquired character, and helps the reader to appreciate that the Lamarckian and Darwinian visions can be complementary rather than mutually exclusive.

As to the weaknesses of the volume, I would like to underline two among them. First, while there are advantages to the broad sense given to "Lamarckism," there are disadvantages, too. Although the contributors explicitly disclaim any necessary opposition between Lamarck and Darwin, the terminology is so entrenched that it is hard to avoid misunderstanding. Moreover, it is not clear what insight or understanding is to be gained from associating Lamarck's name with some of the research described. Indeed, I consider that Lamarck's ideas are not needed in order to characterize recent research advances in biology and to show their novelty with respect to the traditional evolutionary theory. Second, the question of the relative importance of epigenetic inheritance is not explored enough. The issue is raised in the last part by Wilkins, who plays the role of the devil's advocate. Nevertheless, more critical remarks would have been welcome to make the central message of the volume more convincing.

In conclusion, *Transformations of Lamarckism* represents a complete overview of the spectrum of Lamarckisms and its diversity. It also provides an interesting discussion of the recent research advances in biology (about developmental and phenotypic plasticity, epigenetic inheritance, stress responses, niche construction, and so on) that should be integrated into the contemporary models in biology. This is the reason why it is an essential volume for any people interested in Lamarckism or in current developments in evolutionary theory. Last but not least, this volume provides an outstanding starting point for investigating further the implications of such changes in biology from a theoretical, a conceptual, and a methodological point of view.

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REVIEW

Naming Nature: The Clash between Instinct and Science

by Carol Kaesuk Yoon
New York: WW Norton, 2009. 299 pages

reviewed by Andrew J Petto

It was a truism often heard during the sesquicentennial celebration of *On the Origin of Species* that Darwin's work changed everything about how we think about the history and diversity of life. In her very approachable and cautionary account of how this view of life was incorporated into the biological sciences, Carol Kaesuk Yoon shows that the change in the way that scientists identified related groups of organisms may be more tectonic than seismic. In a sense, Yoon has discovered ethnobiology and, in applying it to evolutionary biology, has learned that scientists are not immune to umwelt—the cognitive filter that shows us order in nature.

The impact of umwelt and what it means for understanding and practicing science is the subject of a number of books (for example, Dunbar 1999; Atran 1993). More broadly, anthropologists have explored and reported the names and naming categories used to describe the natural world and how those names reveal the perception of the natural order. It is not that the data themselves are so different—humans in all cultures quite reliably sort living things into quite similar groupings—but how those groupings are related to each other and to life in general can be quite different (see, for example, Begossi and others 2008; Petto and Meyers 2004). It is umwelt that tells us what seems "natural" in the order and relationships among living things, and Yoon argues that this perception is challenged by the requirements of phylogenetic or "tree" thinking.

From animal rights to xenotransplantation, umwelt underlies our understandings—and misunderstandings—of what Douglas Adams called "life, the universe, and everything". And what Yoon demonstrates so clearly in this book are two important points: (1) that scientists are not immune to umwelt; and (2) that the sciences are still in the process of moving from a pre-evolutionary umwelt (still reflected in the basic taxonomic framework that we use to locate species on the tree of life) to a phylogenetic view of life even a century and a half after the *Origin*. Edward Sapir's admonition is as true of this transition in the sciences as it was almost a century ago for anthropology: "The worlds in which different societies live are distinct worlds, not merely the same world with different labels attached" (1929: 209). So, what is the different world that emerges from tree thinking?

The concept is simple: the story of the history and diversity of life is a saga of descent from shared ancestral populations. Therefore, our way of naming organisms ought to reflect those biologic relationships. Yoon highlights the ways in which scientists have tried to apply this charge and how biology struggled with the pre-evolutionary umwelt as it brought to bear a deeper and better understanding of the processes of evolutionary change and of phylogenesis. She marks the turning point—the breaking away from an umwelt shared with the general public—this way: "Here was the mother of all clashes: cladists declared that the proper evolutionary ordering of life revealed that the group 'fish' did not exist" (p 7).

The "common sense" ordering of life—that there are fishes and birds and reptiles and mammals and crustaceans, and even "wugs" (a term introduced by Brown [1979], including worms and "bugs" and other miscellaneous crawly items)—was out the window because these groupings did not reflect evolutionary history. Phylogeny was about ancestry—grouping together organisms that shared a line of biologic descent—not about form, function, evolutionary "grade", or any of a number of other ways of organizing living things (even though these observations could provide evidence to infer common ancestry). It was not that scientists did not recognize that there were bony, scaly, gilled things swimming in the world's rivers, lakes, and oceans. But for "fish" to be a valid biologic category, it must include all of the fishes and all the descendants from the common ancestor of all the fishes. In other words, the title of Neil Shubin's book *Your Inner Fish* is not just catchy; it reflects the fact that we all share a common ancestor with fishes and are highly modified, air-breathing, terrestrial fishes—in the strict phylogenetic sense of the category.

Yoon uses a technique here that is common in science writing (and reporting) but is often misunderstood. When she explores the scientific work and theoretical orientations of researchers who figured prominently in the process of trying to organize and make sense of the history and variety of living things, she is providing neither a complete exposition nor a systematic scientific critique of their positions. Rather, she uses them more as exemplars of the stages in the maturation of a phylogenetic understanding of the relationships among living things. There are no demons or villains in the story, and each exemplar's contributions are given their due.

Yoon's book is a great introduction to the changing umwelt of the life sciences, focusing on major shifts in thinking about how best to identify phylogenetic relationships and to apply them to the study of living things. The result of these shifts affects everything from what we call organisms to how we understand their evolutionary history. Perhaps most important for those of us interested in helping the public learn and understand the sciences is that our repetition of scientific facts may never succeed in convincing nonscientists of anything; they see the same facts as we do. We need to find a way to explain the value of a different umwelt (and as readers of *RNCSE* know, that value is found in biotechnology, genetics, medicine, agriculture, ecology and conservation, and so many other fields that affect everyday life). So, from now on, there will be a sign over my desk: *ITUS: It's the umwelt, stupid!*

And if this book ever becomes a movie, my suggestion for the title would be, *Thanks! And So Long to All the Fishes!*

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REVIEW

Did Darwin Write the Origin *Backwards? Philosophical Essays on Darwin's Theory*

by Elliott Sober
Amherst (NY): Prometheus Books, 2011. 225 pages

reviewed by Doren Recker

Elliott Sober's most recent book covers a variety of topics in the history, philosophy, and practice of Darwinian evolutionary biology. It contains five chapters, on: (1) the relationship(s) between common ancestry and natural selection in the *Origin*, (2) group selection, (3) sex ratio theory, (4) naturalism, and (5) a postscript, providing a more detailed, formal account of several issues contained in the first four chapters. The style is fairly informal, with statistical analyses sprinkled here and there, but the main text is understandable without knowledge of formal probability theory. The main audience would be philosophers and evolutionary theorists, though there are some interesting claims concerning more historical issues as well.

Sober is certainly among the leading philosophers of biology of this generation, and much of his work has also been influential among practicing evolutionists. His *The Nature of Selection* (Sober 1984), for example, remains one of the best introductions to and analyses of selection theory, and his co-authored book with David Sloan Wilson, *Unto Others: The Evolution and Psychology of Unselfish Behavior* (Sober and Wilson 1998), pretty much resurrected the concept of group selection as a serious evolutionary force. Both issues are revisited in his new text, with some new wrinkles added. I will concentrate on a few areas of interest to give some idea of the contents, rather than reviewing each chapter.

Others (including myself, Recker 1987) have noticed that there are at least two main positions defended in the *Origin*: (a) that common ancestry accounts for the main patterns of data within comparative anatomy, paleontology, biogeography, systematics, and so on; and (b) that natural selection provides the chief means by which populations change over time. Evidence for natural selection is predominantly provided in the first four chapters of the *Origin*, while common ancestry is defended in most of the remaining chapters. What is the relationship between the two, and how do they both relate to the argument structure of the *Origin*?

Sober argues plausibly that these two main concepts are "entangled," and that natural selection has *causal* priority while common ancestry has *evidential* priority in Darwin's work (p 33–34). That is, while natural selection is the main ("ultimate") cause of species change over time, the main *evidence* that these changes occurred at the macro level is the evidence provided for common ancestry. While no one has seriously denied that natural selection occurs (that is, denied its status as a *vera causa*, supported by Darwin primarily with examples from and facts about domestic cases, and by more recent experimental work on natural selection—differential populational resistance to pesticides, antibiotics, and so on), opponents have always claimed that it is unable to produce *large* phenotypic changes. Evidence that *these* have occurred is, again, largely attained from comparative anatomy, systematics, paleontology, and so on. So Darwin's "extrapolationist" (p 21) claims about macroevolution, based on evidence provided in chapters five through thirteen of the *Origin*, also provide the main support for the causal efficacy of natural selection.

The situation here is similar to opponents of evolution claiming that "it's not possible for natural selection to produce x." An appropriate response involves what Philip Kitcher has called "Darwinian Histories" (Kitcher 1985), which are basically the "adaptationist, just-so stories" that have elicited such ire among many evolutionary theorists (for example, Gould and Lewontin 1979). But as possible *hypotheses*, as appropriate responses to *impossibility* claims, good Darwinian Histories play an important role in defending the causal efficacy of natural selection. To further *support* such adaptationist stories, however (that is, to provide reasons to believe that such *have* occurred), independent evidence is required (such as appropriate fossil evidence or DNA comparisons). Similarly, evidence that natural selection *occurs* and can be shown to produce appropriate changes in certain populations does not, by itself, show that it can account for all or most current biological phenomena. *That* requires support from the various areas of biology. Natural selection may be the primary causal factor in evolutionary change, but evidence that it *has* acted on a grand scale requires support from the various areas of biology (again, it is causally prior, while support for common ancestry is evidentially prior).

The idea of group selection was anathema to most evolutionary biologists at the time I began reading books and articles on natural selection. One of the main reasons for this was the great influence of George C Williams's *Adaptation and Natural Selection* (Williams 1966), followed by the popularization of "gene-selection" models (such as Dawkins 1976), which seemed to bring the "Paradox of Altruism" back under evolutionary control. Roughly, this paradox is: (i) altruists are (by definition) less fit than selfish individuals within the same group; (ii) due to natural selection, fitter traits increase while less fit traits decrease in frequency within a group; so (iii) natural selection cannot cause altruism to evolve (p 57–58). Both premises are true. So if traits and behaviors cannot evolve for the "good of the group," then how does evolutionary biology account for obvious altruistic behaviors throughout the animal kingdom? Gene-selectionism dealt with this problem by arguing that traits and behaviors which are comparatively deleterious at the level of the individual organism can be beneficial at the level of individual *genes* (which are shared by multiple individuals, especially by close relatives—kin selection). Or among non-relatives, a relatively deleterious trait or behavior can still "pay off" for an individual in an act of cooperation, provided the individual can expect similar acts of cooperation from other members of the group over time (reciprocal altruism), as long as the cost/benefit ratios come out positive in the long run. So, the story went, we don't need group selection as an explanation for altruistic behaviors.

One of the most striking aspects of Sober's and Wilson's reanalysis of group selection (Sober and Wilson 1998), was to show that kin selection is an example of group selection. Sober again argues for this in chapter two. Basically, when an individual sacrifices some of her fitness for another member of the group, this "pays off" at the group rather than the individual level. That's what group selection *means*. Paying off for other carriers of specific genes is still paying off at the group level (here, if you want, a group of close relatives). What matters in all discussions of levels of selection is what is selected *for* (whose fitness is enhanced, at whose expense?). For group selection to occur, there has to be competition between groups, and this has to outweigh the deleterious effects on individuals within groups. Sober again provides scenarios where these conditions can be met, and argues (rightly!) that discussions concerning levels of selection need to be data-driven (with evidence concerning *particular* cases), rather than theory-driven (where the theory requires that one or another "level" is always preferred). This is an important point, and an important chapter.

Readers of *Reports of the NCSE* may be most interested in Sober's discussion of naturalism. He rightly points out that there seem to be a number of theological arguments in the *Origin*, preferring God as a "majestic law-giver" rather than a persistent "meddler" in the biological realm; the problem of evil being better addressed by a law-giver than one who individually creates each organic being; and God's goals being hidden, rendering Paleyian design arguments untestable (among others, p 123–130). Does this sacrifice the scientific status of Darwin's work? No. Methodological naturalism is not concerned with whether God or supernatural entities of any kind are *mentioned*, or even with whether or not they *exist*. They cannot be used as scientific *evidence* for a claim, nor can they be used to rule out scientific evidence for a claim. And contrary to some recent disclaimers (such as Dawkins 2006), the existence or non-existence of God is not relevant to "good science" either (p 130–133). That is, *metaphysical* naturalism is not a commitment of science, and evolutionary biology is neutral with respect to it (p 134).

On at least some interpretations of the status of mathematical entities, science may also be committed to the existence of *some* "supernatural" entities. That is, if "supernatural" is defined as being "outside spatio-temporal boundaries" (p 134), then mathematical entities (on some standard interpretations—for example, mathematical Platonism), are supernatural. And yet, of course, science uses mathematics all the time! While I am not a fan of mathematical Platonism as the best account of mathematics, Sober's point here is nevertheless a good one. It would not "destroy science" if mathematical Platonism turned out to be true. Nor are a number of beliefs about God's existence and role in the universe in themselves "science stoppers". Scientific claims need to be *testable* at some level and to some degree, and *that's* essentially methodological naturalism (since only "natural" entities and processes have been susceptible to scientific testing).

Hence, divine intervention isn't part of science, *in so far as* no one has been able to make such claims empirically testable (nor does the prospect seem promising). But the theory of evolution does not necessarily entail that no such interventions occur. If you believe they do, you believe this for non-scientific reasons. And if you want to do science or get a hearing for a scientific hypothesis, you cannot transcend the limits of methodological naturalism. This seems to me to be all that is needed to protect science from the various flavors of creationism. And it has the virtue of not disenfranchising the myriads of scientists (including evolutionary biologists) who *also* have religious beliefs of various kinds (one important example is Ken Miller). We shouldn't let extraneous programs and prejudices interfere with biological (or *any*) science. But we shouldn't extend science beyond its proper boundaries to place extraneous limits on philosophy or religion either. Good fences make good neighbors (and vice versa).

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REVIEW

Defining Darwin: Essays on the History and Philosophy of Evolutionary Biology

by Michael Ruse
Amherst (NY): Prometheus Books, 2009. 271 pages

reviewed by Brian Regal

Reviewing a book by Michael Ruse can be a bit of a challenge. He writes so well and makes his arguments so well, and has done it so consistently over the course of so many titles, simply saying how good it is sounds redundant: of course it's good. *Defining Darwin* is another in a long line of works geared towards general audiences to help them understand the various complex issues involved in evolutionary studies and history. Rather than a single book-length work, this is a collection of essays, some of which have been published elsewhere and all of which have been updated here. They are arranged chronologically beginning with an examination of Darwin's *Origin of Species*. He then addresses early ideas on evolution and transmutation by Kant through the co-discoverer of evolutionary mechanics, Alfred Russel Wallace. He then goes on to some larger issues, including evolution and literature, finishing with two essays on the creationist critique. On the whole, it is a nicely arranged collection, giving a fairly broad sweep to the material.

The essays can be read individually or taken together as a connected narrative. For those new to the subject, Ruse writes in a conversational style which takes complex ideas and makes them easier to understand. Rather than bogging the stories down with excruciating footnotes, Ruse draws upon his deep and profound knowledge of the topic presenting the material in a relaxed way as if in a lecture hall. This is because most of the essays began as class lectures. Ruse presents the material in a reasonable way, observing, for example in "Adaptive landscapes and dynamic equilibrium", that not all scientists agree on the details or mechanisms of evolution and that this shouldn't be seen as proof of the perfidy of evolutionary science—as creationists claim—but of an example of the way science works.

In "Evo-devo", Ruse states, "These are exciting days for evolutionary biology" (p 177). Interest is high both for and against. With such force being expended to punish belief in evolution, to excise it from the schools and generally make belief in evolution one step away from devil worship, Ruse's approach helps the grand melee which is the current "discussion" of evolution seem a little less strident. Ruse does not go for the shocking and the intentionally provocative as Richard Dawkins does. His approach is somewhat more circumspect, but just as powerful and convincing. Michael Ruse has spent a career producing works on evolution for public consumption that are both informative and thought-provoking. *Defining Darwin* is another in this line. If you know someone—even an anti-evolutionist—who needs to know more about this subject, get them a copy.

About the author

Brian Regal is Assistant Professor for the History of Science at Kean University. His latest book is *Searching for Sasquatch: Crackpots, Eggheads, and Cryptozoology* (New York: Palgrave Macmillan, 2011), a history of monster hunting and its place in the history of science.

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REVIEW

The Cambridge Companion to Darwin, second edition

edited by MJS Hodge and Gregory Radick
Cambridge: Cambridge University Press, 2009. 548 pages

reviewed by John S Wilkins

As the sesquicentenary of Darwin's *Origin of Species* in 2009 showed, there is an enormous amount of material one might have to become familiar with if one wants to have an informed view of Darwin, and so a standard reference book is required. This is that book —the second edition of the volume, updated somewhat and with new essays. In conjunction with another volume on the *Origin* itself, most students of Darwin would have little need for any other introductions to the historical context and development of the theory of evolution.

Part I includes pieces by Phillip Sloan on how Darwin theorized evolution, Jon Hodge on the Notebooks and the years Darwin spent in London after the voyage of the *Beagle*, and essays on Darwin's views on heredity (Jim Endersby), on mind and the emotions (Robert Richards) and the argument structure of the *Origin* (Ken Waters). All of these are excellent and nuanced, and well referenced, written by leading specialists on each topic. Endersby's essay in particular introduced me to material I hadn't previously encountered.

Part II looks at the historical, cultural and religious contexts, again by leading specialists. Gregory Radick asks an interesting question that concerns many: is the theory of natural selection tied into its cultural and political context? That is, does it rely on a "Victorian view of society?" He concludes that the origin of the notion of natural selection is inseparable from Malthusian and other contexts, but that this does not imply that it is *merely* a social construct.

David Hull discusses the philosophical context of Herschel, Whewell, and Mill, and situates Darwin as a Herschelian and Whewellian exponent of the idea of a *vera causa*, or "true cause" account of explanation in science, making their reticence to accept the arguments of the *Origin* interesting. He also considers how it is that Mill is popularly thought to have accepted the *Origin*, when in fact he hadn't.

John Hedley Brooke, the doyen of Victorian religion and science studies, discusses both Darwin's religious development from deist to agnostic (but unfortunately accepts the mistaken but prevailing view that his daughter Annie's death contributed to his agnosticism), and the response of the religious to Darwin's theories. It should surprise nobody that the response was mixed, but it may surprise some that overall, the churches did not object to the idea of evolution, nor even of selection. He concludes with a discussion of Darwin's role (minimal) in the spread of secularism and skepticism.

Diane Paul discusses in a measured way an issue that has become more urgent now in the light of claims by anti-evolutionists that Darwin led to the Holocaust—whether or not Darwin was a social Darwinian. She observes that many of the founders of what came to be called "social Darwinism" were people Darwin cited extensively in *The Descent of Man*, such as WR Greg and Darwin's cousin Francis Galton. Darwin, she concludes, wavered on the matter, and was overall not optimistic about the future of the European people. However, Paul notes that both socialists and capitalists found source material in Darwin's work, and that most of what they are remembered for lacked any specifically Darwinian content. Eugenics owes most to Galton and subsequent genetics, and German militarism to the liberalism of the day, which was strongly authoritarian and nationalist.

A new essay for this edition by the editors discusses and dismisses John Dewey's 1909 claim that Darwin overturned Greek essentialism. Instead, they show that Greek formalism was opposed by Greek atomism and Epicureanism, and that the tension between these two traditions persists and is largely unaffected by Darwin. However, while they rightly point out that Linnaean classification was not Aristotelian, in mentioning that he followed Aristotelian conventions in defining *per genus et differentia*, they imply, I believe wrongly, that Linnaeus thought classification was about definition rather than identification. Whewell's reaction to the *Origin* is shown to be a case of treating it as Epicurean, which is the all-purpose heresy of the Christian west. I suspect that Whewell was more right than he realized, and that this was a good thing; Epicurus gets bad press. The essay ends in a plea not to oversimplify our historiography of the sciences and intellectual movements post-*Origin*.

Part III brings together a number of modern themes about Darwin in philosophy: the development of evolutionary theory from Darwin to today (Jean Gayon), metaphysics and epistemology (Elliott Sober), mind (Kim Sterelny), moral and social theory (Alex Rosenberg), and religious belief in modern society (Michael Ruse). Each of these is worth the price of the book alone, I think. Gayon's essay on the meaning of "Darwinism", the tree of life metaphor, the centrality of natural selection in Darwinian theory, group selection and self-organization, and macroevolutionary challenges to "Darwinism" is impossible to summarize. Sober's is easier to summarize, in part because he is himself summarizing ideas he has widely discussed for the past quarter century. He holds that "Darwinian theory" consists in two parts: the tree of life and natural selection, and that probability is a core aspect of the theory. He adopts the "essentialism story" that the editors deprecate in their essay above, unfortunately, but this is consistent with the view he has propounded for a very long time, as are his arguments in favor of likelihoodism against parsimony, and of optimality. The essay is a good summary of Sober's ideas, if not a general philosophical consensus of the modern evolutionary theories in play.

Kim Sterelny discusses the general role that Darwinian ideas play in modern philosophy of mind and cognition, which is appropriate given his contributions to that field. In the context of the tension between folk psychology and eliminativism, he notes that Darwin is an equivocal source, on the one hand promising an account of "intensionality", how beliefs can be "about" the things they are about (through a process akin to selection, called "teleosemantics"), but on the other hand undercutting our folk psychological categories like the emotions and agency. These issues underpin (and to an extent undercut) the recent attack on Darwinian thinking by Jerry Fodor. He expands on these matters under the rubric of evolutionary psychology, and also the evolution of language.

Alex Rosenberg, a noted evolutionary naturalist, argues in favor of what he calls "Darwinian morality," in which ethical judgments have their foundation in our biological and culturally evolved natures. In particular he discusses a "Darwinian meta-ethics" developed by Alan Gibbard, and argues that natural selection brought about norms of cooperation. He concludes with a discussion of the revival of group selectionist accounts by Edward O Wilson and David Sloan Wilson, and of Brian Skyrms's "stag hunt" model of cooperative behavior as a replacement for Prisoner's Dilemma accounts, and what it all means for "hidden hand" accounts of social order.

Michael Ruse discusses various religion-specific issues, such as design, teleology, divine intervention in evolution, progress and providence, the problem of evil (which exercised Darwin himself no little bit), the existence of the soul, morality and freedom of the will, and divine mysteries, concluding that it is a hard thing, but possible, to be religious and a Darwinian. The essay is valuable both as a summary of Ruse's own published arguments, and for its survey of the issues and subsequent taxonomy, whether you agree with Ruse or not.

Part IV is titled "Philosophical prospects", which is possibly the most unifying label for the three following essays. Daniel Dennett asks where we are given that Darwin overturned the Cartesian tradition. I am uncertain that Darwin achieved this feat myself, but given that the Cartesian tradition has been overturned, and Darwinian resources are used to do so, the topic remains salient. The abandonment of the centrality of design and intention in the natural order is a great problem for many, and Dennett is unapologetic about this. He gives a précis of his arguments in *Darwin's Dangerous Idea* in favor of a "design space" explored by trial and error processes, using a mythical play, *Spamlet*, which warms the heart of every Monty Python aficionado. It is Dennett at his best, accessible, deep, and controversial.

Owen Flanagan considers the "manifest image of humankind", taking a phrase from Wilfred Sellars. We were supposed to be a "little less than the angels", made in the image of God. Now we find that we are animals, modified and unique, but animals nevertheless. What does this do to our self-image? Flanagan distinguishes between etiologies of ethics and moral value, and their justification, and considers ethics as a form of "human ecology", in which values that contribute to human "flourishing" survive; this is, he notes, consistent with Aristotle's ethics of the "political animal", as well as Hume's view of us as having "sympathy" for our fellows. He considers the recent work on emotions that derive from Darwin's *Expression of the Emotions*, and a Strawsonian view of them as "reactive attitudes." He closes with a section on the "Darwinian Good Life."

Finally, in a new essay for this edition, Simon Blackburn investigates whether there even *is* such a thing as "human nature", and whether the Hume-Darwin view survives. He discusses Fodor's attack on Darwinian naturalism, in a timely manner, and how Darwinian Hume really is (or how Humean Darwin really is).

The *Cambridge Companion* series is dedicated to individual philosophers, and while Darwin is not a philosopher, the concerns discussed here are primarily philosophical rather than scientific. This explains why the contributors are either philosophers or historians of ideas, rather than scientists. As such, it is the best entry point into the many debates and issues of the so-called "Darwin industry" that was set in motion 50 years earlier at the centenary of the *Origin*, and which continues unabated today. It should be in every interested person's personal library.

About the author

John S Wilkins is an honorary research associate in the Department of Philosophy at the University of Sydney and the author of *Species: A History of the Idea* (Berkeley [CA]: University of California Press, 2009).

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