B.F. Skinner and the Metaphysics of Darwinism

Michael T. Ghiselin¹

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Abstract B. F. Skinner viewed behaviorism not as the science of behavior, but a philosophy of that science. Such philosophizing is a legitimate part of a scientist's investigative behavior. He sought to eliminate confusion and error by getting rid of objectionable posits such as homunculi, vital forces, intentionalities, purposes and essences, sticking to overt behavior and spurning "mentalism." Skinner believed that there are hard analogies between learning and natural selection, such that what is appropriate in the study of one may be appropriate in the study of the other. Dispensing with teleology is but one example. Where there is selection by consequences, variation has to be taken seriously. Essentialism or typology screens out variation and leads to stereotypes. It may be viewed as treating individuals (in a broad, philosophical sense) as if they were classes. Individuals are concrete, particular things, including species and many other groups, whereas classes are abstract. Individuals can engage in processes, such as behavior. But they do not have definitions (or essences), and there are no laws of nature for them. Trying to find a definition, or an essence, for the human species is trying to find a definition for an indefinable instead of a description for a describable. Idealism has introduced a kind of mentalism into behavioral discourse that behavior analysts should scrupulously avoid. There are no laws for individuals, only for kinds of individuals, and care needs to be taken to avoid confusing laws of nature with contingent, historical fact. Skinner was a (perhaps somewhat inconsistent) realist who presupposed the uniformity of nature in his investigations. Investigative behavior may be more lawful than even he maintained.

Keywords Essentialism · Functional analysis · Variation · Selection · Realism · Idealism

In its December, 1984 issue, *The Behavioral and Brain Sciences* re-issued six "Canonical Papers of B. F. Skinner." They were accompanied by much invited "peer commentary" and responses to it by Skinner. My comment (Ghiselin, 1984) on his

✉ Michael T. Ghiselin
mghiselin@calacademy.org

¹ California Academy of Sciences, 55 Music Concourse Drive, San Francisco, CA 95436, USA

“The Phylogeny and Ontogeny of Behavior” was titled “B. F. Skinner versus Dr. Pangloss.” Here is Skinner’s response, quoted in its entirety:

“I have complained of the extent to which many of those who have commented upon these papers have simply misunderstood what I have said and the extent to which I am forced to offer correction. If all the other contributors understood my position as well as Ghiselin, I should have found myself with little or nothing to say. His commentary might well have appeared first as a general introduction to my paper. It would have made my position clearer than I myself have been able to make it, and a more productive discussion might then have followed.” (Skinner, 1984)

The title of my commentary was evoked by an earlier exchange in the same journal between the philosopher Daniel Dennett and me, on what has been called “adaptationism” (Dennett, 1983; Ghiselin, 1983). “Panglossian” adaptationism alludes to a follower of Leibniz in Voltaire’s *Candide*. Dennett defended what he called the “Panglossian Paradigm.” I suppose it is much easier for a philosopher like Dennett, than an anatomist like me, to believe that this is the best of all possible worlds, in spite of possessing a human back, two knees and a prostate.

I was a graduate student in the Department of Biological Sciences at Stanford University from 1960 to 1964. My doctoral dissertation was a study on the evolution of a group of sea-slugs called opisthobranch gastropods, based upon a functional analysis of their reproductive systems. Such a functional analysis is a kind of physiological anatomy aimed at figuring out how the parts of an organism work (how they function). It should not be confused with the kind of ‘functionalism’ that has sought to identify the functions of parts. Skinner, in his books on *Science and Human Behavior* (1953) and *Verbal Behavior* (1957), used the term ‘functional analysis’ for his causal-analytic approach to behavior. My approach was also causal-analytical, but the causes included evolutionary ones.

When I was at Stanford, there was a great deal of agonistic behavior going on among comparative anatomists and other systematic biologists about the philosophy of the subject, especially the methodological and epistemological aspects. I was caught in the cross-fire. As a result I became seriously involved in the philosophy of evolutionary biology and pursued that interest during three years of post-doctoral fellowships, first at Harvard and then at the Marine Biological Laboratory in nearby Woods Hole. Needing good examples of how to do science properly, I studied the works of Charles Darwin and wrote a book about him and his accomplishments (Ghiselin, 1969). That project involved a ‘conceptual analysis’ of his theories and systems. Such historical research may be characterized as a study of ‘investigative behavior’. I mean nothing particularly arcane or technical about that. Dogs sniff and people snoop. It involved serious efforts to understand what Darwin was trying to accomplish. Because a major goal was to understand and explicate the text, I have compared my commentary to literary criticism (Ghiselin, 1976). [Herein I have found it convenient to cite only the first editions of Darwin’s books. For a more complete bibliography and exegesis see my *Reader’s Guide* (Ghiselin, 2009).]

From the time that Darwin was a student at Edinburgh and Cambridge, he was deeply interested in the philosophy of science as it was emerging in his day. As a young geologist he was involved in controversies about methodology. He was a supporter of Charles Lyell and his “uniformitarian” approach to reconstructing the history of the Earth. After circumnavigating the globe (1831–1836) Darwin realized that his

specimens provided evidence for evolution. He began to speculate and take notes on all sorts of topics. In addition to evolution and its possible mechanisms, those topics included psychology, which, although it was also associated with physiology and medicine, was still considered very much a branch of philosophy. He treated it, however, as a natural science, and pursued that interest throughout his long career (Ghiselin, 1973). Behavioral themes pervade his writings, especially his *Journal of Researches* (Darwin, 1839) and *The Origin of Species* (Darwin, 1859). He was a great comparative and experimental behavioral physiologist. Several of Darwin's "psychological" works were devoted to the behavior of plants: *Climbing Plants* (Darwin, 1865), *The Power of Movement in Plants* (Darwin, 1880), and *Insectivorous Plants* (Darwin, 1875). His work on human beings and other animals is presented in *The Descent of Man* (Darwin, 1871), *The Expression of the Emotions in Man and Animals* (Darwin, 1872), and his little book on earthworms (Darwin, 1881). These books provide examples of how an evolutionary approach can guide scientists to ask the right kind of questions and come up with the right kind of answers.

There has been a long tradition of treating the scientific method from a naïve inductionist point of view: as if it were a matter of patiently collecting "facts" and bundling them into generalizations. Given a wealth of examples, I made it abundantly clear that Darwin was a practitioner of the hypothetico-deductive method. That means that he invented conjectures (hypotheses) and tested them. Skinner was rather dismissive of theories and what passed for the hypothetico-deductive approach. He tended to attribute his own success to his skill as an experimentalist gadgeteer.

Many students of behavior have been "methodological" behaviorists. Skinner insisted that he was not one of them, and that seems to have been one reason why he called himself a "radical behaviorist." Rather early in his life (about 1932) he described that position as denying the "existence of subjective entities" (Skinner, 1979). But does that make him what I (Ghiselin, 1969) have called a "metaphysical behaviorist?" In the social science literature a distinction is often made between "methodological individualism" and "methodological holism." In my book on *The Economy of Nature* I characterized my own philosophical position as "radical individualism" and treated it as a metaphysical thesis (Ghiselin, 1974). Although at the time I emphasized the selection of organisms rather than genes or species, I did not deny the existence of any of these units.

Skinner begins his book *About Behaviorism* (Skinner, 1974) as follows: "Behaviorism is not the science of human behavior; it is the philosophy of that science." Perhaps calling it *the* philosophy rather than *a* philosophy goes too far; and restricting it to human behavior might seem arbitrary or gratuitous. In fact he earlier called it "a philosophy of science concerned with the subject matter and methods of psychology" (Skinner, 1963). Calling it a philosophy seems appropriate, for one of the goals of philosophy in science is to bring clarity into areas where there has been confusion. Skinner's paper on *Phylogeny and Ontogeny* is a fine example. He was highly critical of various kinds of intellectual detritus, much of which has been with us for a very long time. That includes 'homunculi' and their little friends who have been thought to populate spermatozoa, the brain, and, more recently, the genome. Etymologically, an homunculus means a little human being, but we might extrapolate to scales larger than the human body. Some cosmological traditions have invoked a relatively large anthropomorph, one who sits on a throne up in the sky and governs the World. Among other

things, Skinner wanted to get rid of vital forces, intentionalities, purposes, and essences. Eliminating “mentalism” was just part of that agenda.

The rationale for Skinner’s position in his paper on the phylogeny and ontogeny of behavior is not that there are parallels between the evolutionary history of lineages on the one hand, and the developmental (embryological) history of the organisms of which those lineages are composed on the other. Rather, it is that there are “hard” analogies between the causes, or mechanisms if you prefer, of (sometimes adaptive) change in evolution on the one hand and learning on the other. By a hard analogy, I do not mean what is sometimes dismissed as ‘metaphor’ in academic gut-fighting. If I say that bromine and iodine are both halogens, I am subsuming two classes of naturalia under a more general one, not using a figure of speech. In both behavior and evolution, according to Skinner, there is selection by (virtue of) consequences. That mechanism rules out teleological interpretations. The organisms do what they do because of the contingences of selection (including reinforcement). Animals are not trying to maximize their survival, or even their reproductive success. Rather, they are doing what tended to maximize their ancestors’ reproductive success. Evolution and learning have enough in common that what is appropriate in studying the one is likely to be appropriate in studying the other. On that point Skinner’s position would seem to have been basically correct. But a lot more might be said.

Of course, I am familiar with much of the literature on the parallels between organic and behavioral evolution in which various analogies are entertained. I have nothing fundamentally new to add. Nonetheless it seems worthwhile to remind the world that both Darwin and Skinner studied pigeons in the laboratory, with valuable results. Artificial selection is very much like operant conditioning: one varies the conditions of existence, perhaps deliberately. Sexual selection, whether by male combat or female choice is, conceptually, akin to artificial selection. And then there is what Skinner called “superstition” or “adventitious conditioning.” One might try to condition for one response and unintentionally condition for something else that the animal happened to be doing at the same time. Superstition was seriously criticized from an evolutionary point of view by Staddon & Simmelhag (1971; Staddon, 1993). What remains interesting, to me at least, is an analogy with what are called pleiotropic byproducts. (A pleiotropic gene is one that affects more than one trait.) Darwin attempted to find laws or principles of variation, and discussed these at great length in his book *The Variation of Animals and Plants under Domestication* (Darwin, 1868). He used the term ‘correlated variability’, which he explains as follows (*Variation*, 2, 313) “when one part is modified through continued selection, either by man or under nature, other parts of the organization will be unavoidably modified.” He gave the example of white cats with blue eyes commonly being deaf (*Variation*, vol. 2, p. 322–323).

Variation is a necessary condition, albeit not a sufficient one, for evolution by natural selection to occur. The same is true of artificial selection and sexual selection, and for “blind variation and selective retention” processes in general. Once that was understood, characteristics of organisms that had long been considered trivial or uninteresting could be seen in a very different light. If we are to make sense of a world that is diverse because it diversifies, we should pay attention to the variants without which that diversification cannot arise. This leads us to the topics of essentialism and essences.

Essentialism, or what many biologists call *typology*, has often been characterized as screening out variation, or thinking in terms of stereotypes like “the behaviorist” or “the

white rat.” These are good symptoms of the ailment, but what we really need is an etiology, and, if possible, a prescription. So let us consider what happens if we say that maybe some classes have essences, but individuals definitely do not, and that treating individuals as if they were classes is much of the problem. But there is another problem here because it turns out that species are precisely the biological units that need to be interpreted as individuals, not classes. I am largely responsible for this “individuality thesis,” albeit not in the sense that it was original with me. I first proposed it upon encountering what was called a version of ‘nominalism’ with respect to species. According to traditional nominalism, individuals are real, but classes are not real. If one accepts that position, then species might even be dismissed as figments of the imagination—which seems a bit odd if they evolve and become extinct. Rebuttal of nominalism in taxonomy often has involved taking what is called a realist stance: classes are real, if perhaps not real in the same way that individuals are. (There are various kinds of realism, and about all they have in common is affirming the existence of something that others deny.) I turned the problem on its head, and solved it by proposing the individuality thesis: species are individuals, so their reality is no problem. But that created another problem: a pedagogical, rather than a philosophical, one. In philosophy courses species have routinely been used as examples of classes. And in biology courses ‘individual’ has been treated as a synonym for ‘organism.’ So it seemed intuitively obvious that the individuality thesis had to be false, and it took years to get people to pay attention to it, or even start thinking about it. Finally I managed to explain what an individual is, and why it matters, in a paper titled “A radical solution to the species problem,” in which I compared the species of evolutionary biology to the firms of economics (Ghiselin, 1974). That publication initiated serious discussion among evolutionary biologists. Since then I have found a way to explain what it means to be a class or an individual that works with broader audiences (Ghiselin, 1997).

When one gives a lecture there is usually a chair handy. A chair is a good example of an individual, in the sense of a concrete, particular thing. I can sit on it. I can pick it up, maybe smash it to pieces. In other words, it can participate in processes – do things, or have things done to it. A chair also has a definite position in space and time. It is always in one place or another. It had a beginning, and ultimately it will come to an end. But how about ‘chair in general’ or in the abstract? In other words, how about the class of chairs, of which individual chairs are instances? I cannot even imagine what it would be like to sit on chair in the abstract. The same may be said of the class of furniture, which is even more abstract. Perhaps the most important point about individuals is that, unlike classes, they do not have instances. I can point to “this chair,” but not to “a this chair.” Remember that for Skinner, the act of pointing is a good example of verbal behavior (Skinner, 1957). If we talk about an (individual) organism who sits on a chair we would not say that “this is an I” or the like. We should be careful not to confuse an instance of a class with a part of a whole individual. Neither my right hand nor my left hand is a Michael Ghiselin, and if both were such instances of me, we would be driven to the paradoxical result that I am two in number.

‘Michael Ghiselin’ is obviously a proper noun, i.e., the name of an individual. That brings us to defining properties. The name of the class of chairs might be defined (inadequately to be sure) as a kind of furniture upon which one can sit. If so, then something upon which one cannot sit is, by definition, not a chair. But consider me. I do not have a definition: there is no set of properties that are both necessary and

sufficient for me to be Michael Ghiselin. “The bearded man giving this talk” will not do. I could have shaved this morning, and it would not do for me when I was ten years old. I can be described, but not defined. Such defining properties are sometimes called “essential” ones and are said to make up the essence of the kind. So I have pretty well unpacked what I mean by an essence and am ready to go on to essentialism. But first one more point: the laws of nature. The laws of nature are its uniformities that occur irrespective of time and place, and are about kinds (classes) of individuals, not about any individual in particular. They do apply to individuals. If someone throws you out of an airplane, the laws of physics describe how you will accelerate as you fall. This is because they describe how any object will fall. To summarize, I provide a list.

Classes or kinds	Individuals
Abstract	Concrete
Do not participate in processes	May participate in processes
Spatiotemporally unrestricted	Spatiotemporally restricted
May have instances	Do not have instances
May have defining properties	Do not have defining properties
Laws of nature refer to them	Laws of nature do not refer to them

Now let me give a list of some important classes with instances that are individuals.

Classes or Kinds	Individuals
Genus	<i>Homo</i>
Species	<i>Homo sapiens</i>
Organism	Charles Darwin
Organ	Charles Darwin’s right hand

It should be obvious that Charles Darwin was an organism, just as *Homo sapiens* is a species. It may be considerably less obvious that he was a specimen of *Homo sapiens*, rather than a *Homo sapiens*. Now let me flesh this out by adding some more kinds of individuals. Each kind is a level in a so-called hierarchy:

Genus
Species
Population
Organism
Chromosome
Gene

Note that ‘population’ is equivocal, insofar as it can mean any or all of a series of levels, including the species and smaller units of which species are composed.

Finally, having said that only individuals engage in processes, I list some processes in which the individuals that are instances of each class participate.

Kinds or levels	Relevant processes
Genus	None!
Species	Speciation
Population	Evolution
Organism	Mating
Chromosome	Meiosis
Gene	Transmission

Note that the levels, being classes, do not “do” anything: it is the individuals ranked at those levels that engage in processes. I am notorious for the aphorism “organisms copulate, and species speciate, but genera don’t generate.” Organisms do copulate, and species and other populations do indeed evolve by natural selection. Genera, however, do not evolve. The reason is that the only thing that they share is common ancestry. They lack the kind of cohesion (stick-togetherness) that species have due to their sexuality. It is very important that we give some serious thought as to what the basic units are, and what they do. If somebody were put on trial for rape, and argued that it was not he, but his genitalia, that committed the crime, he would have a hard time convincing the court.

Charles Darwin stood to *Homo sapiens* as his right hand stood to his entire body; in other words as part to whole, and not as instance to class. (Or inclusion of a class within a class, e.g., chairs within furniture.) Such whole-part relations are a common source of confusion. In dealing with natural selection we may refer to the “environment” when the “conditions of existence” would be more appropriate. ‘Environment’ tends to be interpreted as ‘surroundings’ and that gives the impression that two different things are involved, even when one is an integral part of the other. 1) One’s self and 2) one’s ecosystem is not a list of two different individuals. Likewise the habit of treating the world as if there were an internal and an external one makes it seem like a jug that contains something rather than a sponge that is saturated with it.

In general, and as Skinner maintained, it is individual organisms that do the behaving. That provides one argument for taking a “molar” rather than a “molecular” point of view in behavior analysis (Baum, 2002). The metaphors here are not to my liking. Molecules are particles whereas molarities are concentrations. Be this as it may there is the question of whether we should apply the term ‘behavior’ only to activities of entire organisms and not to those of their parts. Restricting the definition that way creates problems with organisms the parts of which may act as independent agents by breaking off from the larger whole. Many nudibranch gastropods have dorsal processes (cerata) that break off (undergo autotomy) when the animal is attacked by a predator. The cerata may writhe and crawl about, distracting the predator while the rest of the body escapes.

Ever since Darwin it has been recognized that only individuals can get selected, and that, with the noteworthy exception of families, these individuals are organisms. “Group selection” was deemed implausible except under extraordinary conditions. But when the individuality thesis was proposed, Niles Eldredge realized that entire species might be selected after all. This insight evoked a great deal of theorizing about

macroevolution and what might be going on “at higher levels.” The discussion continues (Eldredge, Pievani, Serrelli, & Taemkin, 2016).

Using a somewhat different jargon, in which we substitute ‘nature’ for ‘essence’ or ‘defining property,’ no species, including our own, has one. Human nature is a metaphysical delusion, one that results from trying to define the undefinable rather than describe the describable. That has not stopped all sorts of people, including some who at least pose as evolutionists, from writing books and papers about it. Human nature allows “experts” to discuss, and perhaps rule upon, who or what gets to be called human. Symptomatic of such discourse are efforts to find “universals” for our own species. These would be properties that can be predicated of all human beings, and preferably of nothing else. In principle, at least, the same exercise could be carried out for each and every one of the millions of species that inhabit, or have inhabited, the Earth.

Philosophers and theologians have often invoked language as the essence of humanity, and many still affirm that it is a good move. For example, Norman Malcolm (1964, p. 153) writes “If a study of mankind does not regard man’s possession of language as an essential difference between man and the lower animals, then I should not know what was meant by ‘essential’.” And yet it is at least theoretically possible, indeed quite likely, that language evolved before the common ancestral population from which we are all descended had become reproductively isolated from the rest of our genus. Furthermore, language might have evolved independently (polyphyletically) at a later stage in the history of our species. Such essentialist reasoning reinforces the attitude of superiority of us *Uebermenschen* to our inferiors. On the other hand it may seem weaker grounds for exalting ourselves to point out that *Homo sapiens* differs from other apes by having a chromosome number $2n = 46$, not $2n = 48$.

There are no properties of individual human beings that make us human. What does make each and every one of us a human being is being an organism-level component of *Homo sapiens*. Of course nobody would want to deny that some of the changes that have occurred since our ancestors emerged from the *Urschleim* are scientifically interesting, including those that have been important in giving them a competitive edge over their neighbors. Language is a good example, as are upright posture and an opposable thumb. But why not just tell the story as it at least seems to have happened, draw attention to the more interesting and important events, and dump the pretentious metaphysics?

In a co-authored book chapter, Eldredge and Gould (1972) presented the notion of punctuated equilibria, according to which evolution has periods of stasis and periods of relatively rapid change. They and others gradually developed a body of (largely) macroevolutionary theory. Toward the end of his life, Gould presented his views in a massive tome, which I can only describe as one long commentary on the individuality thesis (Gould, 2002). Although he rejected essentialism, Gould seems to have been unable to give it up altogether, and suggested, for example, that Darwinism has an essence. This in spite of the fact that the philosopher David Hull (1988) had published another long book, in which he applied the individuality thesis to the evolution of scientific theories and other conceptual entities. If theories are lineages with variants and gradually change over time, then we should only expect that, say, the Darwinism of Darwin would be very different from that which I learned as a student, and from the

current version. And just as species at any given time can consist of subspecies and other subunits, Darwinism has been polytypic.

“Mental” phenomena often serve as essences. In the 2nd edition of his textbook titled *Understanding Behaviorism* Billy Baum (2005, p. 53) writes as follows: “An ancient Zen Buddhist puzzle goes, ‘If a tree falls in the forest, and no one is there to hear it, does it make a sound?’ The behaviorist’s answer is no, because a sound exists only as part of an action of hearing.” Here we are reminded of Bishop Berkeley, and other idealist authors. To be is to be perceived. Berkeley solved the problem by invoking the Great Homunculus. God is omnipresent, and He hears. Now, I happen to live in a forest, one in which trees occasionally fall. I do not think that such events go unheard by the deer, raccoons, bats, birds and insects that live there, for they use sounds as affordances of behavior. Sounds are vibrations, and they existed long before our ancestors perceived them and long before they invented God in their own image. If hearing or anything else is an adaptation brought into existence by natural selection, the cause has to exist before it can produce the effect.

I am not just quibbling. I looked into some standard references on the topic of idealism in philosophy, and came up with the following (Rescher, 1995, p. 357). “Mind-requiring processes are needed for something in the world to be discriminated as a rose and determined to bear certain features. Identification, classification, property attribution are all required, and by their very nature are all mental operations. “There is no good reason to doubt that a professional botanist can identify a rose, or that a professional malacologist like me can identify a stylommatophoran pulmonate, or that either of us might engage in some rather sophisticated investigative behavior in making such identifications. But what went on before botany, zoology, and the mind were invented? Both animals and plants are able to distinguish organisms of their own species from organisms of other species. And they do not have to give it much, or indeed any, thought. They do not need nervous systems to do so. It can simply be a matter of responding to the presence of a chemical, such as a pheromone. And the reason why they make such distinctions is quite simple. Crossing with organisms of another species is apt to give sterile, or not fully viable, offspring. Classificatory behavior is the product of evolution by natural selection.

To adopt idealism is to have the mind do the kind of metaphysical work that Skinner wanted to exclude from science. What is the obvious alternative? Realism, of course, and I mean realism in the sense that scientific research is about objective reality. That Skinner was a realist (if perhaps inconsistently) is clear from what he says in *Science and Human Behavior* (1953, p. 139) “Once we have responded to such an object in apparently inconsistent ways, we may be less confident in saying ‘square’ to any one set of visual stimuli, but we have no reason to argue that our original visual response was not to the object ‘as it really is’. We operate in one world—the world of physics. Organisms are part of that world, and they react to it in many ways.” The version of realism that I advocate flatly asserts that the truth is objective, and denies that what is true or false is dependent upon anything that people believe or opine. The issue cannot be resolved by claiming that scientists are not interested in truth. Things are only made worse by replacing the pursuit of truth with the application of methodological rules. My taking a realist stance has in part resulted from coming under skeptical attack by amateurish philosophers of taxonomy. Skepticism is an impregnable position, because it affirms nothing, including the possibility of scientific knowledge. But that does not

prevent the rest of us from affirming the principle of non-contradiction, which tells us that if there is a real contradiction between two propositions, at least one of them must be false. It is neither wishful thinking nor academic ideology that leads a reasonable person to believe that the number 13 is a prime.

Skepticism is not the same as *criticism*, which includes the testing of hypotheses — in other words treating them as conjectures that we try to refute as best we can. And the justification of scientific reasoning does not just depend upon culling out what we think is error. We sometimes encounter situations in which several lines of evidence lead, unexpectedly, to the same basic conclusion. This is what William Whewell (1847) called ‘consilience’. A discovery can take us by surprise, and if we are surprised, we may be stimulated to revise our theories and enrich them. There are plenty of examples of this in the history of Charles Darwin’s investigative behavior. There were major changes when he found out that the birds of the Galapagos had diversified within the archipelago and when he read Malthus on population.

I can also illustrate what this means by an example from my own career. My genealogical tree of the opisthobranch gastropods, based on the anatomy of the reproductive system, revealed to me that the various lineages had specialized on different kinds of food. Some years later it became apparent that the opisthobranchs were feeding upon distasteful and toxic items such as sponges and using chemicals derived from food in their own defense. I collaborated with D. John Faulkner, a natural products chemist, and we were able to show, among other things, that chemical defense evolved very early, before the ancestral snails lost their shells and became slugs (Faulkner & Ghiselin, 1983). A few years later I joined forces with another natural products chemist, Guido Cimino, and his team in Italy. We were able to show in great detail how chemical defense has been the driving force behind the evolution of the group and worked out the evolutionary history in great detail (Cimino & Ghiselin, 2009). That research led us to some considerations about chemoreception, and the differences between taste and smell in aquatic and terrestrial animals (Mollo, Garson, Polese, Amodeo, & Ghiselin, 2017). In effect we are redefining ‘taste’ and ‘smell’. So one thing leads to another, but our inability to predict exactly what will happen as one problem gives rise to another is not a weakness of our investigative behavior. It is a strength.

One of the most interesting features of science is that it evolves. So too have the various sciences, and the philosophies that have shaped them. Essentialism has been affecting our thinking about what science in general, and also its various branches, are supposed to be, at least as far back as our records of such matters go. Having inherited an approach to classification that was derived from essentialist philosophers, it should hardly be surprising that mediaeval, renaissance, and baroque thinkers tried to arrange their materials on the basis of their supposed essences. Things did not change with the emergence of modern science. Its more impressive accomplishments, especially those of Galileo and Newton, came to be treated as exemplars of what science is supposed to be. The search for laws of nature came to be treated as the essence of investigative behavior. And, as with much essentialist thinking, essences served as norms and standards. So it makes a certain amount of sense that psychologists wanting to enhance the scientific performance of their field would tend to imitate the physical sciences. Therefore we find the early behaviorists trying to discover laws of nature. Behavior analysts often mention laws, such as the law of effect. And yet, not all scientific

research leads to the discovery of laws of nature. It often leads to classifications, and to narrative history. One wonders what the history of behaviorism would have been like had its early practitioners tried to ape the geologists rather than the physicists.

When I first proposed the individuality thesis, David Hull rejected it. However, he encountered the views of another philosopher, J. J. C. Smart, who argued that biology is not a science, because it does not have any laws of nature (Smart, 1963). Smart gave the example of there being no laws for *Homo sapiens*. Hull realized that there are no laws for the planet Saturn, either, and the reason is that there are no laws for any individual whatsoever. Laws are only “about” classes, or kinds, of individuals. Laws apply to individuals, but they do not refer to any of them. So what might these biological laws and kinds be? I looked for laws about kinds of species and other (kinds of) populations. I was able to think of some, but they tended to be statistical laws and not very exciting ones (Ghiselin, 1989). For example, as the effective population size goes up, the frequency of fixation of alleles by sampling error goes down.

Could one use evolutionary theory to justify realism in science? Of course I do not mean the kind of naïve realism according to which our brains are inhabited by thoughts about ice that are cold – like little ice-cubes in the cocktails of homunculi. Panglossian adaptationists might tell us that, because this is the best of all possible worlds, sooner or later our heads will get filled with the best of all possible representations of that world. As a student, Darwin was much impressed by the *Natural Theology* of William Paley (1802), who argued from the fact of adaptation, to the existence of God. There cannot be design without a designer, Paley said. This is true, and it is true by definition. A design logically entails a designer (Ghiselin, 2011). But, as Darwin later showed, there can be the appearance of design, without a designer. Of course appearances can be misleading. To treat natural selection as a designer ignores Paley’s arguments and turns the process into an homunculus.

There have been some philosophers and philosophical biologists who have used natural selection to justify something like realism in the sense of a close correspondence between the supposed inner (mental) world and the outer (physical) one. We might call such persons “evolutionary realists.” One of the “architects” of the Synthetic Theory, Bernhard Rensch (1900–1990), writes “Hence, we may assume that all evolution of thought and reasoning must of necessity be confined to certain directions, since wrong abstractions of facts were invariably followed by severe corrections from the ‘external world’, so that it was necessary for thinking to be in close correspondence with the material world (Rensch, 1959: 348).”

Animals have been remarkably successful at evolving sensory organs and behavioral capacities that give them an increasing ability to obtain an advantage in the struggle for existence. Likewise science has given rise to successions of theories that are increasingly satisfying to its practitioners. Such apparent progress has happened in spite of the limitations of instrumentalities that are more than just instruments. So I would suggest that something like naïve realism has been with us for a very long time, and is the ancestor of the more sophisticated version that we keep longing for. Of course identifying the ancestor is only a beginning. We need to ask what have been the conditions of existence. I think that Skinner would have invoked the uniformity of nature. It would seem that our ancestors have long existed in a lawful environment, and became adapted to it. The laws of nature are nothing more than some of its uniformities. However, there are uniformities other than laws. When we formulate a law of nature,

we mean something that is necessarily true, irrespective of time and place, of everything to which it applies. But, as I have explained, many uniformities, such as those which allow us to forecast the behavior of an individual animal, are matters of contingent fact: they might be otherwise. Given that distinction, we are in a somewhat better position to analyze whatever behavior may happen to interest us. Investigative behavior may be more lawful than even Skinner believed.

SUM, ERGO COGITO

Compliance with Ethical Standards

Conflict of Interest The author declares that he has no conflict of interest.

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