

## **THE WHAT, THE HOW, AND THE WHY: THE EXPLANATION OF ERNST MACH**

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**ABSTRACT:** Putative distinctions between explanation and description constitute a very old issue in the sciences. Behavior analysts commonly call their science “descriptive” as opposed to “explanatory.” One obvious difficulty here is to achieve any agreement on the meaning or use of these terms. Without some agreement, debate is pointless. I examine various uses of these terms and consider whether they have any clearly distinctive meanings in the actual conduct of a science. Many behaviorists are inspired (via Skinner) by Ernst Mach to view science as description, but I will argue that Mach’s use of the term “description” corresponds to what most scientists would call “explanation.” Thus, on this basis at least, behaviorists are unjustified in their opposition to science as “explanation.”

*Key words:* explanation, description, Mach, Skinner, physics

I was enormously privileged to participate in the celebration of the life of U.T. Place, surely one of the most stimulating and delightful men of my experience. He is one of three or so people who got me interested in conceptual issues related to behavioristic positions (Peter Harzem and Robert Arrington were two others). Prior to that, I was primarily an experimentalist (though with some philosophical training) who gave much more attention to what seemed to be interesting experimental questions as opposed to philosophical ones. Given the opportunity, Ullin and I each attended each other’s talks and he was especially helpful in patiently clarifying issues and questions related to my particular concerns, largely dealing with relations between behaviorism and behavior analysis and the conduct and perspectives of other sciences, particularly physics. For nearly two decades, I enjoyed his wisdom and company in meetings from Mexico to Germany, and California to Spain, where I last saw him. It was strange not to see him in Amiens; a brilliant light has gone out.

In deciding what to discuss, I elected not to focus on this or that particular contribution of U.T. Place, but rather to deal with an issue in radical behaviorism in a manner reflecting his more general contributions to the behaviorist program, and, in particular, my own discussions with him over the years. At least two principal influences are reflected here. First, his wide and deep interest in verbal behavior is manifested by my attempts to explore certain aspects of the verbal behavior of the scientist. Second, he was a powerful advocate for behaviorism; but,

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as only a proper advocate can be, he was also its trenchant critic, especially of the sort of conceptual shoe-horning and doxological exercises some behaviorists seem compelled to display. “Pragmatism” versus “realism” and “mechanism” versus “contextualism” are two major examples. In his spirit, but wholly without his facility, I want to pursue a third so-called distinction, that between “explanation” and “description” by briefly examining the views of Ernst Mach, an undisputed pillar of Skinnerian behaviorism.

The issue of description versus explanation has been and continues one of the most contentious in not only a relatively obscure field like behavior analysis, but in science in general. For a time in the twentieth century, one could have made a career out of formalizing the meaning and application of these terms. The efforts by very clever people to place description and explanation into the cold bosom of logic seemed never to pass what might be called a fundamental behavioral criterion: Do the ways logicians treat terms like “description” and “explanation” find a home in the activities (never mind the interests) of the practicing scientist? As with most attempts by philosophers of science to formalize the practice of science, this effort failed. It failed not because logic did not capture some of the uses of the terms, but because it did not capture all those practices considered by scientists as “scientific,” and because, in general, formal philosophy of science has had little influence on the practice of science anyway. My impression is that the formal approaches to the question of what constitutes a description or an explanation has given way to lively discussions of the varieties of each, and how various domains of science might be defined in terms of their use.

### **What Happened?**

Why is the issue important? I believe that for the great majority of successful practicing scientists, the issue rarely, if ever surfaces. Most scientists and thoughtful laymen alike are not greatly confused by distinctions between what is the case, how it came about, and why it occurred. At an intersection I observe two cars, one with a side smashed in and turned over, the other with its front end crushed. This is a “what.” Further evidence may reveal that the first car was hit broadside by the second. This is a “how.” Beyond this it may be determined that the brakes in the second car were faulty, preventing it from stopping at the red light. This is a “why.” I say *a* what, *a* how, and *a* why, because these specifications are not necessarily unique, and certainly not complete. An accident investigator would supply many more details on each. In addition, with respect to the how and the why, physical principles such as Newton’s laws of motion or the conservation of linear momentum might be invoked. The level of detail expressed will depend on who wants to know and why. A newspaper account is one thing, an insurance investigation another. To be sure, sometimes the “what,” the “how,” and even the “why” may be blended or collapsed, as in the above example, when a passerby asks a knowledgeable witness: “What happened?” Nevertheless, a reasonable account should provide a causal sequence addressing the questions of interest. This

kind of analysis goes back to Aristotle, but I will forego all the metaphysical fiddling usually practiced in this context.

In another example familiar to this audience, pigeons whose responding is controlled by concurrent VI-VI schedules typically demonstrate “matching,” that is, they distribute their responses or their time to each alternative in proportion to the frequency of obtained reinforcers from each alternative. The answer to *what is the case* is simply a statement of the Matching Law. *How* may be addressed through a variety of theoretical approaches to the dynamics of choice, but *why* matching occurs is clearly a deeper question. For example, matching might be considered a special case of more general processes imbedded in behavioral ecological or economic theory. These, in turn, may be related to evolutionary selection mechanisms, and so on.

Careful description is, of course, essential to satisfactory explanation. Pigeons responding under so-called matching-to-sample contingencies may demonstrate quite accurate performance. If the behavior is *described* as matching-to-sample, that is, the pigeon is said simply to be responding to the comparison that matches the sample, then, in general, this is not true. For example, if we substitute a new sample and its comparison, choice of comparisons may drop to chance. Control of behavior may be along very different dimensions than a naïve description would indicate. Minimally, explanation here constitutes unambiguous specification of those dimensions through further empirical exploration.

These examples illustrate but a few of the ways questions may be addressed and answered. Contingencies controlling the application of “what,” “how,” or “why” clearly vary within science, and certainly elsewhere. Just what constitutes a description or an explanation is, however, at the heart of the controversies that yet rage among those calling themselves “behaviorists.”

### **Realistic Pragmatism**

Again, why is the issue important? For at least some calling themselves behaviorists, description *is* explanation (e.g., see Catania, 1998). I think this position curious, or, at the least, misleading. Skinner, himself, seems not always to have been clearly consistent on this identity. For example, in *The Behavior of Organisms* (1938) he says: “So far as scientific method is concerned, the system . . . may be characterized as follows. It is positivistic. It confines itself to description rather than explanation.” (p. 44). However, later in *Verbal Behavior* (1957) he writes: “Our first responsibility is simple description. What is the topography of this sub-division of human behavior? Once that question has been answered. . . we may advance to the stage called explanation: What conditions are relevant to the occurrence of the behavior—what are the variables of which it is a function?” (p. 10).

Baum (1994) in his book *Understanding Behaviorism* calls this a pragmatic as opposed to a realist view, and so ties the issue into ontological knots (see, also Zuriff, 1995). What, for Baum is “realism”? One looks in vain for a clearly stated definition. Rather, we are told that realism is about a world “out there,” while our

experiences are “in here,” meaning presumably the private experiences of sensations of the world outside us. We are, moreover, told that we cannot know what the real world is really like, because we can have no direct contact with it. From this it is said to follow that we have no reason for believing that there is a world out there. (I cannot restrain asking the question about how we might know there is a world inside, since I don’t understand what it can mean to have direct contact with one’s sensory experience.) Nevertheless, realism is, according to Baum, tied to a criterion for truth, namely that the principal goal of science would be to discover more and more about what the world is really like, that is, coming closer and closer to the Truth. Baum describes this search for Truth as “slow and uncertain” (p. 19) because we can have no direct contact with what is real. How we could be said to discern any order at all in a world we are shielded from, and whose very existence we may doubt, is not made clear. But somehow with this considerable burden we are told that realism encourages explanation as opposed to mere description. Realists are said to probe deeper than mere appearances. The form of explanation also seems constrained, namely that a realist must be committed to a mechanistic or physicalistic form of explanation. As Zuriff (1995) argues, Baum’s description of realism is a mixture or conflation of a host of different philosophical positions about an equally large number of views on just what realism entails, if anything.

Baum’s treatment of pragmatism fares no better. It, too, is a *gemisch* of differing views under control of different assumptions and purposes. Piercian pragmatism addressed issues of meaning with implications for objective scientific communication. His views could be connected with later developments in operationism. Pierce claimed to be a realist, thus indicating that, at least some forms of pragmatism are not in conflict with realism. James, on the other hand, espoused a more individuated pragmatism, often tied to ethical or moral decisions. “What works best for me?” is the key to truth. One writer (Rescher, 1995) describes how Pierce saw Jamesian subjective pragmatism: “. . . a corruption and degradation of the pragmatic enterprise, since the approach is not a venture in validating objective standards but in *deconstructing* them to dissolve standards as such into the variegated vagaries of idiosyncratic positions and individual inclinations” (p. 712). A lively book on pragmatism by Hillary Putnam (1995) contains a delightful and sympathetic essay on James wherein, in part, he argues that James was a “direct realist.” Indeed, he compares Derrida’s deep skepticism with the general antiskeptical position of both Jamesian and Piercian pragmatism. For James, perception was direct and not mediated by the extra step of private sense data. Moreover, we share and perceive a common world, despite our different routes to truth. Putnam continues:

Pragmatism has been characterized by *antiskepticism*: Pragmatists hold that doubt requires justification just as much as belief. . . ; and by *fallibilism*: Pragmatists hold that there are no metaphysical guarantees to be had that even our most firmly held belief will never need revision. . . as Pierce, James, and Dewey all tried to help us realize—that access to a common reality does not require incorrigibility. . . The fact that perception is *sometimes* erroneous does

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not show that even *non*-erroneous perception is really perception of “appearances.” And it may also help if we realize that access to a common reality does not require access to something *preconceptual*. It requires, rather, that we be able to form *shared* concepts. (pp. 20-21)

In this context, I would argue that the “common reality” is just what confers the practicality of “shared concepts.” Contrary to Baum, realism and pragmatism do not have to be at loggerheads. Indeed, one could argue that pragmatism is in some way parasitic on realism. James, as Baum points out, believed that all scientific theories are approximations. This is certainly in agreement with the foregoing quote by Putnam. But, if they are all approximations, then what are they approximations of? As science advances, is it simply becoming *more practical*? How is that possible? Baum cites Kuhn (1970) as denying progress in science. Whether science is progressive remains a hotly debated topic, though in my view a good deal of the discussion reminds me of Aesop’s fable of two men fighting over the shadow of an ass (see the extensive treatment in Curd & Cover, 1998 and, e.g., Devitt, 1991; Kitcher, 1993; Laudan, 1990). A key issue here is just what “progress” might mean. No one would deny that we now know considerably more about electricity than, for example, Benjamin Franklin. But are we closer to the “truth” about electricity? This raises further issues about truth criteria, and so on. The attribution of successful working depends on a set of criteria and one might inquire whether *they* are chosen pragmatically, and by what criteria. Even Larry Laudan, a major and most eloquent spokesman for modern pragmatism is led to comment about the rules of scientific practice,

. . . there must be something right about the rules. . . since a randomly selected set of rules for judging beliefs would not exhibit the striking success shown by the theories of the natural sciences. Unless the rules of scientific method reflect something about the “facts of the matter,” scientific inquiry would be nothing like as successful as it is. (1990, p. 102. See, also, Kitcher, 1993; Putnam, 1995)

### **The Mocking of Mach**

I want to focus down on the assertion that pragmatism as opposed to realism *implies* a descriptive, and not an explanatory role for science. Baum’s principal authority in this discussion is Mach, whose views will be my emphasis in this little essay.

Now, Mach as a philosopher and historian of science represents a treacherous shoal for interpreters. Somewhat like Wittgenstein, he has been used to justify a host of incompatible points of view. During most of his active career he was a venerated figure, to some contemporaries *the* premier philosopher and historian of science in the 19th century. William James remarked in a letter of the time that, “I don’t think anyone ever gave me so strong an impression of pure intellectual genius. He apparently has read everything and thought about everything. . . .” (cited by Holton, 1970, p. 192). However, in his later years and certainly after his death, Mach was much more vilified than venerated. There is a stereotypical Mach—the

rigidly inductive empiricist insisting that only directly sensed entities or facts belong in a scientific account, and that a scientific theory should comprise only an arrangement of such facts in some economical order. There is at least a scintilla of truth behind all stereotypes, and, indeed, in his cantankerous and painful last days Mach rejected much of what was to become basic to modern physics, namely, atoms and relativity. The reasons for his stand on relativity are complex, if not outright mysterious (see, e.g., Bernstein's Introduction in Mach, 1898/1986 and Holton, 1970), especially since Mach was a major influence on Einstein, having, for example, deeply criticized Newton's absolutism in his classic *The Science of Mechanics*. Einstein, who knew Mach and explicitly recognized his role in the development of relativity theory, ultimately called him a "*déplorable philosophe*" (cited by Holton, 1970, p. 176). As for Mach's stand on atoms, I will address that shortly as it bears heavily on my topic.

### What to Make of Mach

At the outset, I freely admit that each time I attempt to fathom Mach's work I come away with mixtures of awe and perplexity. Part of the difficulty with him (and the same difficulty is shared by Skinner) is that his prose appears far simpler than it is. Take the statement: "Explanation is nothing but condensed description" (quoted in Cohen, 1970, p. 136). What can this mean? The statement itself is a condensation. To explore this statement with any depth would take much more discussion than is appropriate here; just to approach it adequately would require a careful treatment of concepts like "elements," "sensations," "experience," "functional relations," "prediction," "control," "economy of thought," and much, much more. One thing seems clear however; Mach's use of the term "description" goes quite beyond ordinary use. For example, if science is but condensed description tied to our experiences, where is the room for prediction and beyond to control? Mere descriptions could provide nothing but tautologies. The descriptions must include *possible* experiences. The greatest possible experiences encompassed by the most economic means define the best scientific theory. But Mach did not mean that description was defined or limited by experience. As Cohen remarks: ". . . his theory of science enlarges the extension of 'description.' . . . Laws go beyond the given experiences; laws function not only as descriptions but also as prescriptions for descriptions. . . . So laws are condensed and extended description" (Cohen, 1970, p. 137). As such, of course, they cannot be simply descriptions of experiences. Mach expresses this condensation, at its best, as:

. . . a *comprehensive, compact, consistent, and facile conception of the facts*. When once we have reached the point where we are everywhere able to detect the *same* few simple elements, combining in the ordinary manner, then they appear to us as things that are familiar; we are no longer surprised, there is nothing new or strange to us in the phenomena, we feel at home with them, they no longer perplex us, they are *explained*. (Mach, 1893/1960, p. 7)

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To see how Mach applies this idea I quote from one of his lectures:

As soon as we have grasped the fact of the rectilinear propagation of light, the regular course of our thoughts stumbles at the phenomena of refraction and diffraction. As soon as we have cleared matters up by our index of refraction we discover that a special index is needed for each color. Soon after we have accustomed ourselves to the fact that light added to light increases its intensity, we suddenly come across a case of total darkness produced by this cause. Ultimately, however, we see everywhere in the overwhelming multifariousness of optical phenomena the fact of the spatial and temporal periodicity of light, with its velocity of propagation dependent on the medium and the period. This tendency of obtaining a survey of a given province with the least expenditure of thought, and of representing all its facts by some one single mental process, may be justly termed an economical one. (Mach, 1898/1986, pp. 194-195)

Another way of describing what Mach is talking about here is the wave theory of light. At the time Mach delivered this lecture in 1894 physics was still basking in the brilliance of James Clark Maxwell, who had explained light and all the phenomena described by Mach, and much else, as electromagnetic waves in the aether interacting with matter. Within a decade, the wave theory would receive its greatest challenge since Newton, the aether would be gone, and the structure of matter would engender the most vexing perplexities. As a result of these experiences, theory in physics would reach far beyond anyone's experience (Marr, 1995).

The wave theory of light is an example of what Mach called an *indirect description*. *Direct descriptions* such as "red" would be subsumed within the larger theory or theoretical concept of electromagnetic radiation. The latter concept would thus be more economical than a set of direct descriptions that would be encompassed by it. In addition, the power of prediction or control might be afforded. A most lucid discussion of his distinction between direct and indirect description is found in his lecture "On Comparison in Physics" (Mach, 1898/1986) where there is virtually a perfect isomorphism in the distinction between Skinner's (1957) *abstraction* and *metaphorical extension* in the discussion of the tact. These aspects of verbal behavior lie at the foundation of the construction of theoretical models of phenomena, constituting what Mach calls indirect or "condensed description," his description of explanation.

For Mach, the distinction between direct and indirect description was purely quantitative. He says:

... instead of a *single* feature of resemblance culled from memory, in this case a *great system* of resemblances confronts us, a well-known physiognomy, by means of which a new fact is immediately transformed into an old acquaintance. Besides, it is in the power of the idea to offer us more than we actually see in the new fact, at the first moment; it can extend the fact, and enrich it with features which we are first induced to *seek* from such suggestions, and which are often actually found. It is this *rapidity* in extending knowledge that gives to theory a preference over simple observation. But that preference is wholly *quantitative*.

Qualitatively, and in real essential points, theory differs from observation neither in the mode of its origin nor in its last results. (Mach, 1898/1986, p. 241)

Presumably, this expresses a continuity of description and explanation, from which one might conclude the basic equivalence of the two. On the other hand much of what is said here does, in fact, describe theory, in other words, explanation, in terms familiar to most scientists.

### Things of Thought

I want to indicate some confusions centering around Mach on the issue of what Baum (1994) calls “hidden reality beyond experience” (p. 24). Baum and others, as I hinted earlier, want to claim that explanation in a conventional sense demands a particular realist ontology wherein explanation necessarily involves entities or processes beyond experience, the hidden world beneath, or should I say beyond appearances, in other words, the “real world.” Again, I’ll not review all that I think is wrong about this, but rather discuss where this makes clear contact with Mach. The most often cited example of Mach’s recalcitrance and his commitment to a sensationalist account is his opposition to atomic or molecular theories. In his last years when the subject of atoms was raised, he would interject: *Haben Sie einen gesehen?* But the real story is much more complex and interesting as, for example, Laudan (1981) relates in an excellent account in a volume of his essays, *Science and Hypothesis*. MacKinnon’s (1982) *Scientific Explanation and Atomic Physics* and Bernstein’s (1986) Introduction to Mach’s *Popular Scientific Lectures* (Mach, 1898/1986) are also informative treatments. I should point out that these three sources are not always in accord, but the complexity of Mach’s views is not in dispute.

Laudan argues that Mach’s rejection of atomism had little or nothing to do with his sensationalist position, nor even with the scientific evidence. Rather his views were based on the sort of methodological grounds reflecting the usefulness of scientific theories. It is most important to emphasize that Mach did *not* assert that only what was given by the senses be a part of a scientific account. Every theory includes entities or concepts not subject to sensory scrutiny, for example, moment of inertia, potential energy, charge, magnetic field, entropy, etc. As Laudan put it:

He [Mach] has no general axe to grind against theorizing as such, nor against most of the scientific studies of his day, despite the fact that virtually all of them go well beyond what a sensationalist account of knowledge would legitimate. (1981, p. 204)

Moreover, 19th century positivism actively supported theory construction as a means not only to describe present phenomena and to predict new phenomena, but also to frame coherent approaches to an experimental analysis. As Mach (1883/1960) himself said: “Without some preconceived opinion the experiment is impossible. . . . For how and on what could we experiment if we did not previously



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have some suspicion of what we were about?” (p. 161). Later, Einstein would put it this way: “It is the theory that decides what we can observe” (Heisenberg, 1971, p. 63). At any rate, this view dissolves the mythology that Mach proceeded along purely inductive lines, letting a theory emerge like an Aphrodite from a sea of data.

From Mach’s economic perspective, theoretical entities must not be superfluous and must be capable of verification, a view no one is likely to question seriously today. However, he was quite aware that in the development of an account, entities were often hypothesized for *heuristic* purposes. These “things of thought” as he called them could serve like scaffolding to help erect a successful theoretical edifice. Once the edifice is in place (i.e., “familiar”), the scaffolding is no longer needed and should be discarded. My favorite example is electromagnetic theory, along side Darwin’s theory of evolution the greatest scientific creation of the 19th century. Many contributed to it but two names stand out, Faraday and Maxwell. Faraday embraced the notion of “lines of force” in his theoretical and experimental analysis of what was to be known as the electromagnetic field. Maxwell, who was to put all known electromagnetic phenomena into mathematical order, did so in part by attempting to construct an elaborate fluid mechanical model of the aether. Once Maxwell’s equations were derived and their immense power discerned, neither Faraday’s lines of force nor the model of the aether was needed. In fact, today hypothetical constructs such as lines of force survive in some quarters only as pedagogical crutches.

The anti-atomists like Mach and his many famous companions in science believed in the ultimate demise of the concept of the atom. More importantly, even powerful advocates of atomic theory like Boltzmann accepted Mach’s *Wissenschaftstheorie* and believed that atoms would eventually be accepted within a Machist framework. Ironically, it was Einstein who was to provide the first really convincing evidence for atoms with his analysis of Brownian motion (Einstein, 1956). His repudiation of Mach’s philosophy because of Mach’s skeptical view of the atomic hypothesis was thus unfair. There was nothing un-Machian about Einstein’s contribution itself.

Most of the theories Mach talked about are mathematical, for example, analytical mechanics, electromagnetic theory, and classical thermodynamics. Indeed, these theories may be said to *describe* the phenomena of interest; in other words, we often speak of a mathematical description. Moreover, no description can be more condensed or economical than a satisfactory mathematical account. However, this hides more than it reveals. To call a mathematical edifice like Maxwell’s equations a merely a description of electromagnetic phenomena, trivializes the complex history that led to them, trivializes Maxwell’s immense contributions, and trivializes their most astonishing implication, namely the electromagnetic nature of light. Mach, I believe, understood this and that is why he expended so much tortured verbiage on just what he meant by a description.

## Reduction Expanded

The sort of theory Mach seemed to be objecting to in his criticism of the atomic hypothesis is generally termed *reductive*. One interpretation of theory (i.e., explanation) is that it must be a reduction of the known observations at one level of analysis to more basic elements or processes at lower levels of analysis. Everyone in this audience knows at least one such definition: “. . .any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions” (Skinner, 1950, p. 193). This is, in fact, but one kind of reduction.

Nagel (1979) distinguished two forms of reductionism he called *homogeneous* and *heterogeneous* (for a discussion related to behavior theory, see Marr, 1990). Two paradigmatic examples of homogeneous reduction are Newton’s mechanics and what we now call classical thermodynamics. For example, the cosmos of Kepler and the kinematics of Galileo are deducible from Newton’s Laws. Moreover, there is a common terminology: velocity, mass, acceleration, force, etc.; or, more generally, a common set of variables entering into functional relations. In classical thermodynamics (a theory Mach seemed to hold in special regard) functional relations between entities like temperature, pressure, volume, entropy, and enthalpy constitute the theory without hypotheses about underlying (e.g., molecular) processes. Without question this theory has been one of the most productive in physics. This sort of reductive theory is characteristic of behavior analysis. Melioration, optimality, behavior momentum, and various formulations of reinforcement principles are exemplars. Ferster and Skinner’s (1957) analysis of schedules of reinforcement is based on specifying conditions prevailing at the moment of reinforcement, and theories involving feedback functions, linear systems, incentives, etc., are basic formulations of behavior from which the characteristic aspects of behavior are assessed or derived. A common set of variables and terms, for example, reinforcement, rate, time allocation, IRT distribution, etc., prevail.

The notion of a behavioral unit or of a molecular as opposed to molar approach to the analysis of contingencies might seem to imply a heterogeneous form of reductionism, but that is not the case. For example, explaining a particular pattern of responding by demonstrating the effects of differential reinforcement of inter-response times is a strictly behavioral account. Behavior is reduced to behavior—a homogeneous reduction.

Behavior analysts have in common with Mach a concern about forms of heterogeneous reduction of the metaphorical mediational sort where the reductive entities have only the status of mental scaffolding. Few behavior analysts have any problem however with biological reduction, wherein aspects of behavioral processes can be shown to be mediated by physiological mechanisms. Skinner, himself, whose primary training was in physiology, noted the deficiency of a purely functional account:

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The physiologist of the future will tell us all that can be known about what is happening inside the behaving organism. His account will be an important advance over a behavioral analysis, because the latter is necessarily “historical”—that is to say, it is confined to functional relations showing temporal gaps. . . .What he discovers cannot invalidate the laws of a science of behavior, but it will make the picture of human action more nearly complete. (1974, p. 215)

Mach, who taught physics to medical students, had deep interests in physiology, and was influenced immensely by Darwin, wrote much on the topic of the relation between physiology and physics. He was particularly critical of vitalism and teleology, as one might expect. He tells the medical students at the beginning of the physics text he wrote for them: “. . .the phenomena in animal bodies are essentially the same as in inorganic nature” (cited in Blüh, 1970, p. 7). His reductionist perspective is revealed further by his emphasis on physical mechanisms in physiology related to such processes as blood circulation, muscle movement, and osmosis. He also pressed the use of mathematical approaches in physiology and medicine, including, by the way, statistics.

Let me summarize this discussion of Mach and the issue of description and explanation. First, his use of the term description as indicated by the theories he embraced is no way ordinary, and, in fact, corresponds to what even today in most sciences would be called explanation. Certainly, in his time, the greatest theorists in the natural sciences venerated him. Second, Mach’s positivism, like his pragmatism is both sophisticated and slippery. He certainly did not confine his science to directly observables or sense impressions. Nor was he an anti-or even a non-realist. Third, he urged caution about theoretical constructs, yet recognized their possible heuristic value. Fourth, he was an advocate not only of homogeneous, but of heterogeneous reduction as well. Fifth, his advocacy of “economy of thought” is a cornerstone of modern scientific practice, but this view has no necessary relation to the description versus explanation issue.

### References

- Baum, W. M. (1994). *Understanding behaviorism*. New York: Harper Collins.
- Bernstein, J. (1986). Introduction: Ernst Mach and the Quarks. In E. Mach (Ed.). *Popular scientific lectures* (pp. v-xix). La Salle, IL: Open Court. (Original work published 1898)
- Blüh, O. (1970). Ernst Mach—His life as a teacher and thinker. In R. S. Cohen & R. J. Seeger (Eds.), *Ernst Mach: Physicist and philosopher* (pp. 126-164). Dordrecht, Holland: Reidel.
- Catania, A. C. (1998). *Learning* (4th ed.). Upper Saddle River, NJ: Prentice Hall.
- Cohen, R. S. (1970). Ernst Mach: Physics, perception and the philosophy of science. In R. S. Cohen & R. J. Seeger (Eds.), *Ernst Mach: Physicist and philosopher* (pp. 126-164). Dordrecht, Holland: Reidel.
- Curd, M., & Cover, J. A. (1998). *Philosophy of science*. New York: Norton.
- Devitt, M. (1991). *Realism and truth* (2nd ed.). Princeton, NJ: Princeton University Press.

- Einstein, A. (1956). *Investigations on the theory of the Brownian movement*. New York: Dover.
- Ferster, C. B., & Skinner, B. F. (1957). *Schedules of reinforcement*. New York: Appleton-Century-Crofts.
- Heisenberg, W. (1971). *Physics and beyond*. New York: Harper & Row.
- Holton, G. J. (1970). Mach, Einstein and the search for reality. In R. S. Cohen & R. J. Seeger (Eds.), *Ernst Mach: Physicist and philosopher* (pp. 126-164). Dordrecht, Holland: Reidel.
- Kitcher, P. (1993). *The advancement of science*. New York: Oxford University Press.
- Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.
- Laudan, L. (1981). *Science and hypothesis*. Dordrecht, Holland: Reidel.
- Laudan, L. (1990). *Science and relativism*. Chicago: University of Chicago Press.
- Mach, E. (1960). *The science of mechanics*. LaSalle, IL: Open Court. (Original work published 1893)
- Mach, E. (1986). *Popular scientific lectures*. La Salle, IL: Open Court. (Original work published 1898)
- MacKinnon, E. M. (1982). *Scientific explanation and atomic physics*. Chicago: University of Chicago Press.
- Marr, M. J. (1990). Behavioral pharmacology: Issues of reductionism and causality. In J. E. Barrett, T. Thompson, & P. Dews (Eds.), *Advances in behavioral pharmacology*, Vol. 7 (pp. 1-12). Hillsdale, NJ: Lawrence Erlbaum.
- Marr, M. J. (1995). Quantum physics and radical behaviorism. In J. T. Todd & E. K. Morris (Eds.), *Modern perspectives on B.F. Skinner and contemporary behaviorism* (pp. 107-128). Westport, CT: Greenwood.
- Nagel, E. (1979). *The structure of science*. Indianapolis, IN: Hackett.
- Putman, H. (1995). *Pragmatism*. Cambridge, MA: Blackwell.
- Rescher, N. (1995). Pragmatism. In T. Honderich (Ed.), *The oxford companion to philosophy* (pp. 710-713). New York: Oxford University Press.
- Skinner, B. F. (1938). *The behavior of organisms*. New York: Appleton-Century-Crofts.
- Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review*, 57, 193-216.
- Skinner, B. F. (1957). *Verbal behavior*. New York: Appelton-Century-Crofts.
- Skinner, B. F. (1974). *About behaviorism*. New York: Knopf.
- Zuriff, G. E. (1995). A review of Baum's *Understanding behaviorism: Science, behavior, and culture*. *The Behavior Analyst*, 18, 395-400.