

Babies, Bathwater and Derivational Reduction

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There has been a good deal of discussion of the subject of reductionism in the literature of the history and philosophy of science. It would not be an understatement to claim that the standard or received account of the reduction of theories in science, characterized both by its close attention to the supposed connectability of terms between the theories involved in reduction, and, by the prominence assigned to derivation as the core of the reductionistic enterprise [27], has not fared well in recent critical evaluations ([7], [10], [11], [16], [17], [18], [19], [26], [29], [30], [38]). The derivational view of reduction, or at least the version put forth by Ernest Nagel in his book, The Structure of Science, has been criticized as impractical, inaccurate, idealized, distorting, sterile, and even incoherent [38]. Perhaps the most decimating of all the charges brought against the Nagelian account of derivational reduction is that there is not a single instance of derivational reduction to be found anywhere in the entire annals of scientific inquiry ([3], [4], [38]).

Lack of historical accuracy is not the only problem alleged to confront the standard or Nagelian account of theory reduction. The standard account is held responsible by many critics for diverting philosophical attention from the fact that many types or kinds of reduction exist in empirical science ([7], [26], [29]). The standard account is also held liable for promulgating a view of theoretical change and evolution that ignores the dynamic nature of this process in favor of interpretations of theory change as rigid, static, and univocal ([5], [7], [10], [11], [19], [25], [34], [38]). Some critics have gone so far as to suggest that the standard account is not only deficient in terms of historical and descriptive utility, but, is also logically incoherent and conceptually muddled. This charge stems from a lack of specificity about how terms between different theories are to be connected ([10], [11], [38]), and, what sorts of statements or rules are requisite for accomplishing the much ballyhooed derivation of one theory from another ([6], [7], [16], [18], [19], [29], [38]).

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It is certainly true that such criticisms, if valid, would constitute a strong case for abandoning the standard account as a model of any, never mind all, instances of reduction in science. It is also true that the critics of the standard account have scored some telling points against the standard account. For example, there do seem to be interesting examples of theory reduction in science that do not meet the requirements laid out in the standard view ([5], [7], [29], [37]). What is less clear is whether the claims of critics of this view are properly attributed to the Nagelian account of reduction, or, rather, to more general defects in currently available analyses of the structure of scientific theories, theoretical evolution, the criteria for demarcating a body of information as a theory, the criteria for individuating theories, the logic of scientific explanation, and the identification and individuation of scientific laws ([5], [34]), and how closely these defects can be linked to the standard account.

There has been a good deal of quibbling about these and other related points in recent discussions of reduction in science. But rather than take sides in this protracted debate, I shall presume without further argument that there exist more types of reduction in empirical science than were ever dreamt of in any logical empiricist's philosophy. And, it may also be granted that the intentions and motives underlying the standard account of reduction were mixed--that the model was meant to be descriptive as well as prescriptive. Granting all of these points, there can be no doubt that the standard account of theory reduction has come up wanting as a means of understanding the course of scientific events within many areas of inquiry. Nonetheless, the question of central interest still remains: is there a place for derivational reduction in the philosophical taxonomy of theory reductions in science?

Were, and are, proponents of the standard reductionistic model so blinded by commitments to formalism, incrementalism, progressivism and the unity of scientific methodology that they, knowingly or unknowingly, distorted the real facts of theoretical evolution in the history of science to fit their ideological and philosophical biases ([3], [25], [38])? Or, is it the case, as some have thought, that proponents of the adequacy of the standard view knew so little about the nitty-gritty of the scientific enterprise, its history, sociology, psychology and politics, that they arrived at the derivational view of reduction as a consequence of their dependence upon elementary textbooks and oversimplistic narrative reconstructions of scientific history ([4], [7], [18], [19])?

I suspect that none of these charges are true. Indeed, it seems to me that the standard or received account of theory reduction has been overly maligned in recent discussions of the subject. This abuse has not been confined to critics of the theory. Defenders of the applicability and validity of the derivational account have so modified and weakened the model that they run the risk of silencing objections to the standard account at the price of abandoning the distinctive features of this account. Attempts to facilitate the reduction of one theory to

another by concocting 'corrected versions' of these theories, or to substitute furious hand-waving for the demonstration of connectability and derivability between theories, end up depicting derivational reduction solely as an armchair exercise of little moment or import for the actual practitioners of scientific inquiry ([1], [30], [32], [33]).

The remainder of this paper will be devoted to the demonstration of a set of claims about a particular case of reduction drawn from source materials in the history of science. An attempt will be made to show that derivational reduction deserves a place in the panoply of types of reduction countenanced in the philosophy of science. No attempt will be made here to delimit the scope or adequacy of derivational reduction regarding all cases and instances of theoretical evolution in science. This can only be accomplished by a careful analysis of actual cases in the history of science [5]. The intent in this paper is to demonstrate the following points: (1) that at least one case of derivational reduction can be found in the data of empirical science, (2) that in all essential respects the case cited meets the specifications of the standard, Nagelian derivational account of reduction, (3) that derivational reduction, at least in one case drawn from empirical science, did (and still does) play a central heuristic role in guiding and proscribing empirical scientific research, (4) that far from distorting the available data in the history of science, the Nagelian model of derivational reduction helps in some cases to illuminate and clarify such processes, and, (5) attempt to shed some light on the nature of the problems and limits confronting the derivational account that have been raised by critics as well as defenders of this view of theory reduction. The aim in undertaking the demonstration of these points is not a defense or vindication of the adequacy of the derivational model of theory reduction. Rather, it is an attempt to highlight the importance of actual historical case materials in evaluating claims made in the name of any model of theory reduction in empirical science.

The example that will be used to illustrate an instance of derivational theory reduction in empirical science is that of the reduction of a theory in physiology concerning the generalizations and laws appropriate to the contraction of striated muscle in vertebrates, by means of a very specific molecular biophysical theory of muscular contraction. The explanation of the forces, work, and heat generated by the process of muscular contraction in animals is one that has pre-occupied biologists, physicians, and chemists for hundreds of years [28]. In some ways the long history of biological inquiry into the subject of muscular contraction should prove tempting to the philosopher interested in reduction since there is both a long genealogy of theoretical evolution available for critical assessment, as well as a general belief, happily espoused by a number of contemporary myologists, that a number of the key puzzles surrounding the understanding of muscular contraction have been solved ([21], [31], [40]).

The fact that muscular contraction has been long studied, and, in many respects, is currently felt to be well-understood, is of prime

importance in selecting this bit of extant theory reduction for analysis. For it should come as a surprise to no one that what one sees in the history of science will depend greatly on where and how widely one's net is thrown. One is more apt to find connecting links and commonalities drawn by scientists between theories separated by fifty rather than five hundred years. Moreover, connections and derivations are more likely to appear between theories that are viewed as both highly plausible and as relatively insightful in explaining the phenomena at issue. There are relatively few topics in biology and medicine which meet even these minimal guidelines. Many subject areas are spanning new, historically speaking, and, in large parts of molecular biology, developmental biology, neurology, and biochemistry, scientists will, when pressed, admit that uncertainty and tentativeness clouds their work.

But this situation does not prevail in physiology. Much of what was held true about the structure, function, and behavior of the organs and tissues of the body fifty years ago, is accepted as valid today. This is particularly true of the scientific status of physiological knowledge regarding muscles, both striated and smooth [15]. And, given the amenability of muscular tissue to experimental structural, physiochemical, and functional analysis at the biophysical and biochemical levels, great success has been attained in understanding the energetics, chemistry, molecular structure and mechanics of muscular contraction.

Muscle tissue is relatively easy to isolate and manipulate in comparison to other biological materials. These factors have led to the prominence of muscle tissue in the pioneering work of x-ray crystallographers, neurologists, electrophysiologists, biochemists, and biophysicists in their attempt to provide reductionistic accounts of the behavior and function of biological materials throughout this century. An additional motivation for obtaining an understanding of the biology of muscular tissue has been the hope that the understanding of muscular enervation, energetics, and contraction might shed some light on the ailments and diseases of the muscle systems of the human body which afflict many individuals [15]. This pragmatic motivation cannot be emphasized too much since a good deal of the direction, scope, and intent of twentieth century biological inquiry has been fueled by the reductionistic hope that diseases of the body might admit of amelioration or cure through an understanding of the structural and functional component elements of biological materials. While this hope has not always been fulfilled, it is a heuristic research motivation that distinguishes much medical and biological inquiry from work in other domains of science.

Muscles function as highly specialized organic machines. They use chemically stored energy to produce mechanical work and, in the process, produce as by-products heat and certain chemical transformations. Muscles can only pull in order to produce the complex movements of the body, they cannot push. They are composed of thousands of fibers which, when stained, reveal regular striated patterns that divide the fibers into units known as sarcomeres. All striated muscles are com-

posed of these protein units which vary in graininess and are stacked on top of each other like coins in a pile.

The key work in understanding the physiological principles governing muscular contraction was conducted by A. V. Hill in the laboratories of the University College, London. Beginning in 1910 Hill conducted experiments on frog muscle under a variety of electrical (isotonic, isometric, twitch) and chemical (aerobic, anaerobic, lactic acid, caffeine) conditions to study the work, force, heat, and energy generated by muscle under various conditions of load and stretch. This work culminated in Hill's classic paper of 1938 on the mechanics of muscular contraction [14].

At the beginning of his work on muscle, Hill, like other researchers working in this area ([9], [28]), had thought of muscular contraction as a process quite analogous to that of a stretched spring operating in a viscous medium. His initial observations on the heat and force of muscular contraction seemed to support the theory that "muscular contraction consists of the liberation of free potential energy manifested as tension energy in the excited muscle; and that this potential energy can be used indifferently for the accomplishment of work or the production of heat." (Quoted in [28]). W. O. Fenn performed a classic experiment in 1924 [9] that seemed to lend credibility to the visco-elastic theory of muscular contraction. Fenn had found that when a muscle was weighted with a load and allowed to shorten while contracting, it gave out more total energy than when it simply contracted isometrically. The heat of shortening, or Fenn effect, was thought to be a solid empirical verification of the idea that muscle could be understood as a spring in a naturally viscous medium possessing great internal resistance to changes of shape or form. It seemed that muscle exerted a constant force under contraction and that the process of shortening consumed energy in the process of overcoming the 'viscous' resistance of muscle fibers.

By 1938 Hill had perfected a number of measuring refinements that allowed him to make a series of sensitive observations about the contractile properties of muscle. These observations led him to modify the older visco-elastic theory of muscle contraction and to explain a number of experimental observations on the physiology and energetics of contraction that had been made by other workers. Hill found that when muscle contracts and is allowed to shorten during tetanus, the amount of heat induced is independent of the load placed on the muscle. He also confirmed the fact that the amount of energy liberated at any moment during such a contraction, that is, work plus heat, is a linear function of the load placed on the muscle. As the load diminishes, the total energy increases and vice versa. Hill observed also that the heat of shortening for contractions over various distances was very constant and distinctive from one kind of muscle to another [14].

These observations led Hill to formulate an equation for relating the rate of energy liberated as heat and work during contraction to the

speed of shortening under a load. The 'dynamic constants' in a tetanic contraction are, 'a', the shortening heat per centimeter of distance shortened, and ' P_0 ' the force or tension generated by a muscle under full isometric contraction. Obviously a/P_0 must be regarded as a constant and, indeed, it can be computed for various types of striated muscle in various species independent of conditions of load, temperature, or speed of shortening.

If the amount of heat set free by shortening x centimeters is equal to ax and if P represents the load to be lifted or moved by the muscle, then, Hill reasoned, the total work of the muscle could be represented as $(P+a)x$. And the rate of extra energy liberated should, as Fenn had observed earlier, be a function of the distance shortened per unit of time, or, $(P+a)v$ where v is the velocity of shortening correlated with load. As had already been noted $(P+a)v$ is a linear function of the amount of the load, P , present, increasing as P diminishes and being zero when $P=P_0$ in isometric contraction with no shortening. Hill's equation then for relating speed and load in isotonic shortening is $(P+a)v=b(P_0-P)$ where ' b ' is a constant defining the absolute rate of energy liberation.

Hill was able to utilize this powerful equation to obtain a predictive fit with the observations of Fenn and others. Also, he was able to describe the properties of muscles that were lengthened while contracting against loads greater than P_0 . Using the equation Hill was further able to argue that, contrary to the older visco-elastic view, the shortening heat of contraction was a function of the energetics of muscle contraction and not of the internal resistance of muscle itself. When muscle lengthens at a slow speed the rate of heat produced is no greater than that produced in an isometric contraction--which it ought to be on a 'viscous' hypothesis. Additionally, there is a large amount of 'give' in lengthening contracting muscle under load approaching complete relaxation at a load value of double P_0 . This phenomena, akin to the 'give' of a wire under great stress, is also inconsistent with visco-elastic view of muscle [14].

Hill made a number of further generalizations about muscle behavior worth noting in his 1938 paper. He noted that while the Fenn effect seemed valid at slow shortening speeds, at very high speeds of shortening the amount of energy liberated per unit change of length declines. During forced lengthening, the rate of energy liberated, heat minus work done on the muscle, is much less than the rate during isometric contraction. Finally, during tetanic stimulation the amount of tension developed depends on the length at which the muscle is held, declining steeply on either side of an optimum length which is close to the normal resting or slack length of the muscle.

Hill felt that all of these observations and generalizations concerning the properties of muscle would square with a theory of muscle contraction that posited for active muscle (1) an undamped elastic element, and, (2) a contractile element arranged in series and governed by his characteristic equation relating energy, velocity, and load. Using

this theory, he was able to derive three key properties of muscular behavior: (a) the rate of isometric contraction of muscle, (b) the nature of force/length relationships during shortening at constant speed, and (c) the optimal rate for doing muscular mechanical work.²

Nearly twenty years later A. F. Huxley proposed a theory of the biophysical behavior of the contractile element of muscle [20]. Huxley knew on the basis of his x-ray diffraction work that each muscle sarcomere was composed of interdigitating fibrils of contractile protein—actin and myosin.³ The refractive index of these fibrils showed that actin and myosin proteins were confined to different thick and thin fibril elements which seemed to slide past one another during lengthening and contraction. Thin actin fibrils extended from the boundaries of the sarcomere toward the middle of this fibril segment, while thick myosin fibrils are located in the middle of each sarcomere. These distributions of proteinous fibrils accounted quite nicely for the gross appearance of striated muscle tissue. Their shifts relative to each other during shortening of a muscle account for observed changes in the banding patterns of muscle tissue.

Huxley's basic idea was that the observed generalizations of Hill and others concerning force, stretch, velocity, work, and elasticity could be explained by means of a molecular biophysical theory which viewed muscle fibers as a system which interacted in a sequential step-by-step fashion. Each gross contraction of a muscle would be reducible to a set of repeated and identical contractile events at the molecular level -- the attachment, detachment, and reattachment of links between actin and myosin myofibrils in zones where these fibers overlap. The theoretical insight necessary to explain the observed properties and principles relevant to muscular contraction was that a sequential set of steps, rather than the single expansion or contraction of fibrous proteins, could best account for the behavior of a contracting muscle. As H. E. Huxley wrote, "Whatever the details of the mechanism, the same basic property remains: a number of minor cycles of a detailed molecular process take place within the major cycle of contraction and relaxation of the muscle itself." ([23], p. 187). Unlike earlier workers who had viewed muscular contraction as a singular process akin to the contraction of a spring, Huxley was able to show that a molecular theory which posited a set of individualizable sequential contractile events squared best with the observable behavior of a contracting muscle.

Huxley's specific program for reducing Hill's physiological two-element theory of contraction to his molecular biophysical model is avowedly derivational in character ([20], [21], [22]). In his classic 1957 paper he presents a "mathematical treatment of sliding filaments in order to derive formulae for tension and heat production as functions of the speed of shortening, in order to check the relationships (of the sliding filament model) against the relationships found experimentally by A. V. Hill." ([20], p. 280). He argues that there must be points of attachment along the stationary myosin filaments to which projections from the actin filaments may attach. The points of attachment on the

myosin fibre are to be thought of as elastic or flexible 'waving' projections, which are normally maintained at an equilibrium point, but which, under the catalytic effect of the ATPase actin, will be displaced producing a characteristic force, and, thereby draw along attached projections from the actin strands. Actin-myosin links will tend to move toward the equilibrium point and, if the force generated is great enough, will tend to slip past one another under conditions of continuous electrical stimulation and thermodynamic disequilibrium caused by the breakdown of ATP to ADP.⁴

Realizing that the area of overlap is about one-half the distance of each sarcomere, given the distribution of actin and myosin in muscle tissue, Huxley was able to concoct a set of equations for describing the biophysical behavior of sliding filaments. If 'f' equals the rate of actin/myosin attachment and 'g' the rate of disattachment, then the velocity of sliding muscle sensu Hill and his physiological theory should be equivalent to:

$$\frac{sV}{2} \frac{\partial n}{\partial x} = f - (f+g)n$$

where 's' is sarcomere length, 'n' is the proportion of attachments existing relative to the possible attachments and 'x' is the equilibrium distance position of the actin/myosin attachments. If 'L' is the distance which separates actin attachment sides along an actin fibre then it is possible to equate the rate of energy liberation per cubic centimeter of muscle sensu Hill as:

$$E = \frac{me}{L} \int_{-\infty}^{\infty} f(1-n) dx$$

where 'e' represents the number of ergs liberated per attachment site in one contraction cycle.

This equation can be utilized to connect the amount of work done at one attachment site on the biophysical model with the tension of muscle produced in contraction in the sense used in Hill's physiological theory. Huxley's equation is:

$$P = \frac{msk}{2L} \int_{-\infty}^{\infty} nxdx$$

where 'k' represents the flexibility or elasticity of the myosin attachment site in units of dynes/centimeter in accordance with Hooke's law. Further calculation makes it possible to draw identities between tension generability under lengthening, as well as under various loads, and, in equating the shortening heat and energy liberation of muscular work, as represented in Hill's theory, with the sliding filament theory.

These equations can be used in light of the identity relationships established between the physiological and biophysical theories of con-

traction to deduce a number of generalizations observed and posited on Hill's theory. In Huxley's words, "to find out whether the equations can represent the actual behavior of muscle we may see how closely they ...fit the relationships shown by A. V. Hill in 1938." ([20], p. 287). Without going into all the details of this derivation, it is possible to show that if identity statements are posited between Hill's dynamic constants, 'a', 'P₀', 'b', and 'a/P₀' in terms of the characteristic rates of attachment of various muscles, 'b', and 'g', one can easily deduce Hill's characteristic equation, the principle that the rate of liberated heat increases linearly with the speed of muscle shortening, and, the law that the total rate of work of muscle increases linearly as the load is decreased. In the words of H. E. Huxley, "A system of links, acting in parallel and having to break and reform each time a small amount of shortening takes place has another interesting property. If a finite time is required for each link to form, then the number of links in existence at any particular moment, and hence the total tension exerted, will be a function of the speed of shortening. If the system is prevented from shortening, then there will be time for the maximum number of links to form, and the tension will be a maximum. Making a small number of assumptions, it can be shown that in such a system the rate of energy release varies with load in a manner which imitates closely the simple and striking behaviour of muscle itself." ([23], p. 188). In essence, what Huxley does is, using an avowedly reductionistic research model, connect the terms and properties of his biophysical model of sliding filaments with the gross physiological properties of muscle and proceed to derive the generalizations and laws known to govern muscular contraction.

The example, drawn as it actually occurred in the history of physiology, would seem to fulfill all the promises made in the name of derivational reduction at the beginning of this paper. The reduction is done by scientists not philosophers. Connectability and derivability are the distinctive features of the inquiry. Identities are established between the terms appearing in generalizations of two types of theories concerning muscle behavior, and, the laws of the 'higher' theory are deductively explained by means of the laws of the 'lower' theory. Moreover, the avowed intent underlying the scientific inquiry is explicitly that of bringing off a reduction that is in the spirit if not in the letter of the standard or Nagelian account of reduction.

It is of some interest to note that the scientists engaged in this example of theory reduction maintain no prior commitment to the structure of scientific theories, the individuation of theories, or to models of explanation in science. While it is usually presumed in discussions of the standard account of reduction that a person committed to the derivational view must also be committed to very definite views on these other topics, this example would suggest that this need not be the case. It may be possible to separate the properties of connectability and derivability in theory reduction from cognate views on theory structure, theory change, and explanation commonly associated with this model.

Certainly this sort of historical case-study lends some weight to the descriptive utility of the standard account of reduction.⁵ It should also permit some specification of the circumstances under which this type of reduction is most likely to occur and/or be pursued in empirical science. Notice, for instance, that both theories of muscular contraction are highly confirmed and that any 'corrections' that take place concerning the two theories must involve both levels of theory. Finally, it should be evident that it is connectability and derivation which are at the core of the standard account of reduction. These are the sole characteristics manifest in the case considered. No commitment is made to any overarching view of theory structure, the utility of models, theoretical evolution, or theory individuation. This is as it should be for the standard account, while perhaps presuming certain views on all these topics, is not wedded to a particular view on any one of them. I would suggest that the assessment of the standard account's descriptive and prescriptive power will ultimately have to rest on a good deal more historical investigation and a good deal less anti-positivistic emotion.

Notes

¹ I would like to thank Caroline Whitbeck and Walter Bock for helpful comments on an earlier draft of this paper.

² The role played by these generalizations in explaining phenomena such as the efficiency of various muscle arrangements in animals and the behavior of human muscle systems is recounted in [15].

³ See [23] for some discussion of the history of structural and chemical analyses of muscle tissue. There is no doubt that the ability to 'see' the interdigitating fibrils of actin and myosin protein played a great role in directing the nature of Huxley's theorizing at the biophysical level. Structural and functional analyses were closely interrelated throughout the history of molecular biophysics--as they continue to be to this day [8].

⁴ All of Huxley's theorizing concerning the biophysics of muscular contraction was formulated in light of available knowledge concerning the chemistry and energetics of muscular contraction. Much was known then (1957), about the nature of these processes and, for the most part, the chemistry underlying the production of energy for muscular contraction is quite similar to those views available to Huxley (See [2]).

⁵ In his commentary on my paper Kenneth Schaffner forcefully raised the question of the representativeness of this case for understanding reduction in science. This important question is difficult to address for two reasons.

First, the representativeness of the case depends upon the reference frame that is utilized. The example of reduction presented in this

paper may well be typical of reductions in myology or physiology, but, not of reductions in biology, medicine, or other sciences. It may be typical of reductions in twentieth century Anglo-American biomedical science, but not nineteenth century French physics.

Second, it is difficult to determine the representativeness of any case example at present since so few actual examples of reductions are available. For example, most discussions of reduction in the biomedical sciences have been confined to the question of whether Mendelian genetics is or has been reduced to molecular genetics. The resolution of the question of representativeness may simply have to await further philosophical inquiry into other contemporary and historical cases.

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