Part I

The Problem of Form

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Introduction: Forms and Kinds

Modern biologists . . . exhibit a positive dread of form.

Aristotle and Modern Biology

Marjorie Grene's (1974, p. 408) remark is made in the course of a discussion of Aristotle and modern biology and especially of Aristotle's concept of *eidos.* Grene is not primarily concerned with the exegesis of Aristotle, a difficult enough task in itself, but rather with an attempt to draw attention to issues in biology which she regards as important and in relation to which Aristotle's concepts remain, at least potentially, significant. Her stimulating and scholarly discussion provides a convenient means of entry to the problem of form.

While *eidos,* according to Grene, is a single, univocal concept, it is used by Aristotle in two apparently different contexts. In the first, where the sense seems to be relatively straightforward, *eidos* is contrasted with *hyle;* form as against matter. In the second, where the sense is perhaps less transparent, *eidos* is contrasted with *genos;* the concept here is usually translated as species.

Consider first *eidos* and *hyle.* Here we have a pair of concepts which can be used to analyse a variety of systems. In the traditional example of Socrates' nose, snub is the form of the matter: flesh and bone. But at another 'level', bone is the form of whatever elements compose it – earth and fire, say. As Grene notes, in this context, the concept of form functions in much the same way as the concept of organisation in modern biology. The form of an entity or process denotes its principle of organisation. Thus, systems which lend themselves to analysis in these relative terms have a double aspect. On the one hand are the 'laws' or constraints arising from the nature of the material elements of which the system is composed; on the other, the

'laws' or constraints that arise from the order or 'arrangement' of these elements. A form–matter analysis is, therefore, antireductive in the sense that it denies that systems of the relevant kind can be completely understood in terms of the elements of which they are composed. As Grene points out, ''the higher level, though dependent on the lower, is both epistemologically and ontologically prior to it'' (p. 413). To know the system is to identify the kind of system it is. To explain it is to understand the constraints on the components, which make it a system of a particular kind. Since 'matter' could, in terms of its own principles, take on many 'forms', it is the existence of this organisation which makes the system the particular kind of system that it is.

In this context we can see some evidence of that ''dread of form'' to which Grene refers. For example, the 'revolution' in biology consequent upon developments in molecular biology is often characterised as a triumph of 'physico-chemical reductionism'. But this is to ignore its real significance. The central concepts of code, message, information and so on, are concepts pertaining to form not matter. The analysis of metabolism in terms of feedback and regulatory circuits is an analysis of the organisation of the cell, not just of the elementary chemical reactions which occur in it. In fact, Sibatani (1985), a 'first-generation' molecular biologist, has recently argued that the revolution of molecular biology ''was a structuralist rather than a physical-reductionist one as had been generally believed.'' Webster and Goodwin (1982) independently make a similar point. The very title of Jacob's (1974) book, *The Logic of Living Systems,* which is primarily devoted to the development of molecular biology, implies a central concern with formal aspects of biology.

While there might be argument as to the exact manner in which the preceding point is best made, I do not think there would be much argument about the point per se. Why then the pronounced tendency to characterise molecular biology solely in terms of material reduction? The answer is not difficult to find. A position which is antireductionist in the sense noted and which implicitly rejects a 'crude' materialism in favour of a concept of 'informed matter' is one which asserts that living things are specific forms of being and that biology is, therefore, an autonomous science concerned with a domain of being which has its own laws; it is in effect a 'vitalist' position in the most general sense of that term (see Driesch, 1908, 1914). In the case of many biologists, a characterisation of their theoretical beliefs in such explicit terms is apt to be received with horror or derisive denial, for vitalism is the love that dare not speak its name. Moreover, the conception of biology as an autonomous science implies the rejection of a positivist conception of the unity of science. In either case, attitudes and beliefs which are deeply

entrenched and which, no doubt, have an important ideological function, involving issues of identity, power and the like, are called into question.

So much for the present on *eidos* in the context of form and matter. Let us turn now to *eidos* as contrasted with *genos* – *eidos* as species. While the phenomenal entities with which most biologists (and layfolk) deal are individual organisms, such individuals are neither all identical nor all totally dissimilar. ''Common Sense'' (Atran, 1990) conceives them as individuals of such and such a kind possessing underlying natures. Individuals are what they are because they are individuals of a certain kind, because they 'belong' to a certain species and consequently possess a characteristic nature. And this species is what it is because it 'belongs' to a more inclusive kind, to a certain genus. Aristotle's work can be seen as a systematic explication and development of these ''common sense'' notions (see Atran, 1990). Within this Aristotelian tradition or paradigm, therefore, a major concern of biology was to discover the structure of real kinds. To identify the natural kinds of living things and to determine what makes them the kinds of things they are – that is, to determine their essential as opposed to their accidental properties. The end result of such an investigation is a definition, a statement of what a thing is, and this definition has a sort of explanatory power as a consequence of the subsumption of kinds. Thus, to say that ''man is a rational animal'' is to 'account for' the characteristics of the human species since these 'follow from' the kind of thing a human being is, from the genus and differentia. When considered outside Aristotle's conception of the teleological structure of nature, this may not seem very impressive as an 'explanation' and we might be more inclined to think in terms of 'making intelligible' rather than 'explaining'.

This question aside, if *eidos* means 'form' and also means 'species', it would seem that for Aristotle 'organisation' and 'kind' are closely linked if not identical notions. From this perspective, biological kinds cannot be characterised in terms of unstructured clusters or aggregates, for this would be an inadequate characterisation of an organism in the same way that a 'cluster of chemical reactions' would be an inadequate characterisation of the Krebs cycle. What confers upon the Krebs cycle its qualitative identity as a particular kind of metabolic cycle is its form. Likewise, the properties of the individual organisms of primary interest to biologists seem to be of a special sort in that they must be characterised in terms of a form or pattern of organisation: a whole with its parts and an 'arrangement'. The property of 'rationality', for example, might be characterised as a specific form of behaviour in which the separate acts are so ordered as to comprise a whole which can be understood in relation to an intended goal. A property like

'animality' involves a certain form of bodily structure, a certain form of physiology, a certain form of behaviour and so on.

For Aristotle, then, the being-what-it-is of an individual is its form. For Driesch (1908), ''It is *form* . . . which furnishes the foundation of all biology'' (p. 17). A significant, if partial, aspect of form is the bodily structure of an individual, its morphology, and, traditionally, morphology has been the basis for the identification and classification of kinds. In this context, we must think not just of a collection of parts but of an 'arrangement'. This formal aspect of individual organisms is emphasised by Bateson (1894). As he puts it: ''In the bodies of living things Heterogeneity is generally orderly and formal; it is cosmic, not chaotic. . . . anyone who has ever collected . . . animals and plants . . . knows how the eye is caught by the formal regularity of an organised being . . . contrasting with the irregularity of the ground.'' (pp. 19–20). It is significant that Bateson's emphasis on the formal aspects of organic beings is made in the context of a critical discussion of Darwinian theory.

As Grene observes, modern biology, at least in practice, is in broad agreement with Aristotle as regards form and matter. As she also observes, there is a parting of the ways when it comes to Aristotle's concept of kinds. This, in large measure, is due to the influence of a theory – Darwinism – which for many scholars is *the* unifying theory in biology. This theory requires that the notion of natural kind be replaced by that of historical lineage and the concept of essential nature be replaced by that of the accidental collocation of properties; in traditional terms, the concept of *forma essentialis* is replaced by that of *forma accidentalis*. It is probably not coincidental that this theory has developed historically in parallel with a positivistic conception of epistemology and ontology with its notion of the independence of coexisting properties (see Harré and Madden, 1975).

It is with the notion of *eidos* as form and kind that the first part of this book is concerned. The initial objective is to examine how the traditional view has been undermined. The second objective is to determine to what extent some view can be sustained which, if not identical with that of Aristotle and ''Common Sense,'' has at least some affinities with it.

The discussion in this part of the book largely takes the form of a critique of Darwinism. It must be emphasised that the point of such a critique is not, as many Darwinists (and anti-Darwinists) may initially suppose, the development of some new 'theory of evolution'. In recent years, and in large measure as a result of the penetrating analysis of David Hull, there has been a significant clarification of the conceptual structure of Darwinian theory and

of the roles which *scientific* and *narrative* explanations play in it. In particular, it has been argued that the theory of evolution considered as a *scientific* explanatory theory is concerned with the *species category*. The present discussion has nothing whatsoever to say about the theory of evolution in this sense. Rather, it is concerned with the implications of the theory as regards *taxa,* which, from a Darwinist perspective, are not natural kinds. Consequently, they are not susceptible to scientific explanation but must be explained by means of narrative. Hence, a primary focus of critical attention is the ontological status of taxa in the context of a possible science of biological form; the arguments of David Hull provide a means of organising this discussion. The attempt to ascertain the nature and limits of Darwinism has the object of determining a ''space'' in which a scientific explanatory theory of form might flourish, and in which a new dialectic between a theory of this kind and a rational systematics might come into being.

The nature of such a scientific theory is plain. As Driesch (1908) observes, a living organism does not possess its 'typical' form throughout its life; rather the form comes into being by a process of development. ''So the living form may be called a 'genetic form' . . . and therefore *morphogenesis* is the proper and adequate term for the science which deals with the laws of organic form in general'' (p. 20). Thus, a scientific theory of form will be a theory of morphogenesis.

There is nothing particularly novel about this approach per se. It can be regarded, on the one hand, as a revival of the pre-Darwinian tradition of 'Rational Morphology' and, on the other hand, as an attempt to continue the work of those scholars of the 1930s who were concerned to develop a satisfactory 'Concept of the Organism' in opposition to the prevailing mechanistic and atomistic view which characterised the Darwinist tradition.

In effect, the first part of this book can be read as a series of footnotes to, or glosses on, the work of Driesch, Bateson, D'Arcy Thompson, Woodger, Waddington and Goldschmidt, among others, written in the light of more recent considerations.

organise the diversity of beings into a unified system of kinds, it sought to reveal, in phenomena which at first sight might appear to be free and unconstrained, some kind of inherent necessity.

Driesch (1908, 1914) reconstructs the pre-Darwinian study of morphology as the beginning of an attempt to develop a science of morphology which will be comparable to the existing natural sciences but which will, at the same time, take due account of the specificity of the biological object. He contrasts pre-Darwinian 'Rational Morphology' with the Darwinist historical project:

> The old morphology had sought by means of anatomy and embryology to establish the laws, if any, which actually controlled morphological phenomena. It sought, in fact to discover what morphogenesis really was. It sought, moreover, to construct what was typical in the varieties of forms, into a system which should be not merely historically determined, but which should be intelligible from a higher and more rational standpoint. (Driesch, 1914, p. 149)

As reconstructed by Driesch, therefore, Rational Morphology addresses two distinct but related issues which are, on his view, the two problems addressed by any natural science: the nomothetic problem and the systematic problem (see Driesch, 1908). According to Driesch, the nomothetic problem, concerned with ''generalities,'' addresses itself to establishing the link between the General and the Particular, the ''typical'' and the individual. The laws Driesch refers to are, firstly, those governing the process ''by which the type is realised for the time being in the individual'' – this is what morphogenesis really is, and, secondly, ''those governing how it [the type] changes its *specificity,* if such a change, i.e., a descent, is . . . assumed'' (Driesch, 1914, p. 94). Thus, this first problem is primarily a causal/explanatory problem.

The second problem, concerned with ''diversities,'' is a systematic problem; the question of kinds and the relations between kinds. Is it possible to discover or construct an ''intelligible'' or ''rational'' system of forms? By this Driesch seems to mean an attempt to discover whether there is a principle of order in the relations between apparently diverse forms; whether there are 'laws of form'. In other words, the goal of Rational Morphology is the construction not merely of an empirical classification, though this is a necessary stage, but of a rational system which would embody an understanding of the raison d'être which lies behind or beneath the empirical classification. As Driesch puts it, an empirical taxonomy is a ''classificatory *preparation* for the knowledge of . . . the rational in the forms of nature'' (Driesch, 1914, p. 140). A knowledge of the ''rational'', of the 'laws of form', would enable

us to understand that there could not exist more than a certain number of diverse forms or, alternatively, that there could exist an indefinite number whose diversity was, nevertheless, related in a lawful fashion (see Driesch, 1908). Thus the project is to determine to what extent, if at all, the *merely empirical* classification of diverse individual forms can be theoretically elucidated in such a way that it, or some aspects of it, can be shown to have a systematic, formal structure, analogous to that of certain kinds of logical system, so that the diverse forms can be *intelligibly* related. I take it that the question of rational systematics is a particular instance of the more general question of the ''logic of morphology'' as it is referred to in the continental tradition (see Cassirer, 1950).

Driesch attributes the concept of what is called ''a type'' to Cuvier and Goethe and defines it as ''a sort of irreducible arrangement of different parts.'' However, he emphasises that ''all such statements [concerning 'types'] are empirical and have their limits''; nevertheless, ''it is important that they are possible'' (1908, p. 248). It appears, therefore, that for Driesch 'type' simply denotes the existence of *some* degree of empirical regularity in form and, therefore, *some* indication of the existence of laws or intrinsic constraints on the possible. It should also be born in mind that Driesch is writing within the paradigm of late-nineteenth-century experimental morphology (*Entwicklungsmechanik*) where the term 'typical' seems to have been used to denote *any* forms, normal or abnormal, at any stage of the 'life cycle', which might be regarded as characteristic variants of a kind insofar as they are forms which arise from the intrinsic nature of the kind as opposed to those which are externally induced and organised (see Roux, 1905; Driesch, 1908; Churchill, 1969). Consequently, the 'typical' properties of a kind need not be universal properties in the sense that they are actually possessed by every member of the kind and possessed at all times. This notion of the 'typical' does not ignore variation but rather involves the assumption that normal, variant and 'monstrous' forms are all law governed, a view also adopted by Bateson (1894; see Webster, 1992). This position, therefore, differs in crucial respects from the essentialist notion of type criticised by Mayr and Hull. This will be considered further below.

The concepts of 'type' and 'typical' have, to an extent, fallen into disrepute, largely as a consequence of the Darwinist polemics of Mayr (1959, 1963, 1966, 1976; see below), for whom 'type' is a four-letter word. However, *some* concept of the 'typical' is central to the goal of developing a *science* of form. While the realist perspective adopted in this book does not equate laws with empirical regularities and consequently does not regard the absence of such regularities as necessarily providing grounds for the absence

of laws, it remains the case that, in the absence of *any* preliminary indication of empirical pattern or regularity of form, we have no prima facie grounds to suppose that there is anything to be *scientifically* investigated and explained.

The Philosophical Perspective: Realism

The philosophical position from which Driesch writes (or claims to write) is that of Transcendental Idealism. My own position is that of Realism (see Harré, 1970, 1986; Harré and Madden, 1975; Bhaskar, 1978), where scientific practice is conceived as being concerned with kinds and causes; that is, with discovering the kinds of things that exist in the world and explaining how they act. From this perspective, the two distinct aspects of any science which Driesch characterises as the concern with ''generalities'' and the concern with ''diversities'' are intimately related.

In contrast to the Positivist ontology of events, this form of Realism proposes an ontology of things or continuants – ''powerful particulars'' (Harré and Madden, 1975). From this perspective, the manifest properties of entities which are available to experience in the material practice of science (Harré, 1986) – that is, a practice involving the identification and classification of particulars – as well as experimental practice, have to be understood as the realisation of dispositions which are grounded in the natures of the particulars in question. Natures are determined by structures which are ''hidden'' and, as such, not (immediately) accessible to experience; they have to be constructed by the speculative work of theoretical imagination. The goal of the systematic enterprise of science is the construction of a real order of discrete natural kinds or taxa; as Harré (1986) argues, if taxa are to be reliable they must mark off natural kinds. Natural kind terms thus function in two dialectically related contexts: the context of material practice and that of theoretical practice.

Bhaskar (1978) distinguishes three stages in any scientific investigation. Following Locke, a distinction is made between nominal and real essence. The nominal essence of a thing consists of the behaviour or properties the manifestation of possession of which is necessary for a thing to be identified, in material practice, as being of a certain kind. Nominal essences are therefore involved in the construction of empirical taxonomies. From a realist perspective, an empirical taxonomy represents the first, *Humean,* stage which occurs in the development of any science; that is, the descriptive characterisation of putative natural kinds in terms of empirical regularities or 'proto-

laws'. As I indicated earlier, it is only on this basis that we can, in the first instance, suppose that there is something to be *scientifically* explained.

Real essences, however, are those underlying structures and generative (causal) mechanisms – imagined but not necessarily imaginary – by virtue of which a thing manifests or possesses a particular behaviour or property. The discovery of natures or real essences is, therefore, an important goal of science, for these provide explanations. This is the *Lockean* level of knowledge. Thus, according to current theory, a sample of gold has the manifest properties it does because it has a particular atomic constitution which is the real essence of gold. These properties cluster because they are the properties of a kind. The real difference between gold and silver does not lie in their manifest or observable properties but in that which serves to explain these properties, that is, in what they are. Thus we arrive at the *Leibnizian* level of knowledge, for knowledge of real essences permits the formulation of real definitions, so that natural kind terms, insofar as they are based on such definitions, group together those entities whose manifest properties have a common explanation, that is, whose nature or real essence is the same. While theoretical explanations are necessarily relative to descriptions, the reality they refer to is independent of both description and explanation. However, both taxonomic and explanatory knowledge must be conceived as defeasible. As Bhaskar (1978) puts it: ''Science consists of a continuing dialectic between taxonomic and explanatory knowledge; between knowledge of what kinds of things there are and knowledge of how the things there are behave. It aims at real definitions of the things and structures of the world as well as statements of their normic behaviour'' (p. 211). From a realist perspective, to classify something is to commit oneself to a particular line of inquiry into the real essence. Not all general terms stand for natural kinds because not all features of the world have a common explanation and not all sets of properties individuate one and only one kind of thing. In this sense, classification is not just a matter of convenience or convention, as Darwin (1859) sometimes implies; it is correct or incorrect. Moreover, empirical taxonomies are subject to revision in the light of explanatory theories since the theory may lead to a change in the criteria for identity consequent upon which empirical properties the theory deems essential or necessary, that is which properties the thing manifests in virtue of its nature. Hence, in the dialectic between theoretical and material practice, theory always has the last word.

From this perspective, causal laws are not statements describing empirical regularities. Rather, they are statements about how things act, or rather tend to act, and things tend to act in particular and 'typical' ways because of the