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by Michael Polanyi

KNOWING LIFE

When we accredit our participation in the act of knowing, we also commit ourselves to a belief in the existence of the personal facts which we thus know. Indeed, everything we may say about personal knowing implies something about the nature of the things known; if we demonstrate that our personal knowing is unspecifiable, we demonstrate at the same time that certain things cannot be exhaustively represented in terms of their particulars.

So long as we are dealing with our personal knowledge of the patterns or the probabilities found in the inanimate world, this dual aspect of knowing is unobtrusive and without great interest. But when we turn to living beings, there emerges an important additional feature in the structure of personal knowledge which radically changes the situation. This feature is our knowledge of the individuality of living beings.

Life does go on in tissue cultures and viruses which are not segregated in the form of individuals, and the germ plasma transmitting heredity also presents a continuously extended form of life that transcends the individuals, through which it passes. In plants and the lower animals like protista, hydrozoa and worms, many different parts of an individual are viable in themselves and to this extent the individuality of the parent organism is blurred. Yet notwithstanding, these cases of incompletely individualised life we find that the bulk of living matter is embodied in a finite set of individuals circumscribed in space and of limited duration in time. Each has come into existence at a definite moment, to remain alive for a certain period, after which it will die.

The acknowledgement of the existence of individuals, makes the dual implications of personal knowing at which I have hinted before, effectively manifest. I am myself an individual being. Therefore as I gave instances of my personal knowledge and analysed its general structure in previous lectures, revealing my essential participation in it, I have already been describing a living being, and crediting it with certain acts of doing and knowing of which I believe myself possessed.

From here we can pass on to other people. I believe that other men are like myself and ascribe to them powers of personal knowing similar to those which I claim for myself. Since these powers are unspecifiable, I can appraise their existence only by an act of my own personal knowing. Such an appraisal of the powers of an other individual differs from the appraisal of the orderliness of an inanimate pattern by the fact that it appraises its object both from the point of view of the appraiser and from that of the individual who exercises the powers in question. We may show this by affiliating such cases of personal knowing once more to a process of reflection.

Remember that our recognition of an orderly pattern is performed by pouring ourselves into a subsidiary awareness of its particulars and thus

achieving a focal awareness of the whole. For example, by dwelling in an harmonious sequence of sounds, we acknowledge the meaning they jointly possess as a tune. This meaning they possess in themselves; it is an existential meaning. Similarly, we recognise a living individual by appraising in him a significant orderliness which means something in itself: which has an existential meaning. But now this existence possesses a focus, a centre, to which its meaning is ascribed and which exists by possessing this meaning; it is the centre of individuality in an individual living being. Thus we recognise individuality and appraise its manifestations from its own point of view by identifying ourselves with it, and only after that do we achieve a knowledge of an individual as different from ourselves as if we were splitting our united selves into two--one part of us knowing the other--exactly as we split up ourselves in the process of reflecting on any intelligent powers of our own.

This raises a curious point. Logicians discriminate sharply between our knowledge of things and our reflections on our knowledge of things. Science is a knowledge of things and so scientific knowledge about science is held to be quite distinct from science, and is called "meta-science." In this sense, science is placed on a separate logical level from that on which are the things to which science refers and meta-science is once more removed to yet another logical level. A first floor is thus established for the objects of science, a second floor for science itself and a third floor for meta-science. But what if the objects with which science is concerned are themselves persons and science is interested in the knowledge possessed by them? Take for example a psychologist studying the process by which an animal learns to recognise the ringing of a bell as a sign indicating an imminent electric shock. The bell and the electric shock will obviously occupy the lowest logical level and the animal itself will have the second floor assigned to it, while the psychologist will be on the third floor. And anyone examining the science of psychology, the way I am doing at this moment, would be found to be pursuing this meta-science on a newly opened fourth floor.

In other words, a science dealing with living persons, appears logically different from a science dealing with inanimate things; in contrast to the two-storied logical structure of inanimate science, biological science is seen to possess a three-storied logical structure. Its problems should be logically on a par with those of the theory of knowledge pursued by meta-science.

I shall develop the implications of this important point at a later stage; at the moment I want to concentrate only on the paradox presented by the fact that an additional logical level appears to be inserted in the course of an evolutionary process which forms a continuous transition from the inanimate stage to that of living and knowing persons. It would be clearly useless to speculate at what exact point of the evolution of life the new logical level is inserted into our knowledge of life. I should be prepared to rival in this respect the efforts of historians, who are indefinitely pushing each stage of history further backwards, so that (I understand) for example, the Industrial Revolution has now been shifted right back to the time between Clovis and Charlemagne; in this way it should not prove impossible to reveal the first

signs of personhood even in the inanimate world, as for example in the process by which a crystallographic system governs the appraisal of specimens to which it is applied. But the correct inference seems rather to be that we regard personhood as varying gradually from a rudimentary level, at which it may be safely overlooked to its fully developed forms which dominate altogether the lives of its bearers. Throughout these stages the distinctive logical structure of biological knowledge can be acknowledged conditionally by saying that to the extent to which we regard the manifestations of life as achievements of an individual, our knowledge of these is three storied; which means that it is guided by standards of achievement which the observer accredits as appropriate to the individual under his observation, in view of the nature of this individual, as assessed by the observer himself.

I think that this conceptual structure can be properly applied to the very lowest forms of life, even though this may put some strain on its meaning. A plant is an individual, but it lacks an active centre for it merely vegetates, and hence it is not possible to distinguish in a plant the logical levels of the individual from the level of things on which the individual operates. Yet there is something remarkable that even a plant achieves, namely that it displays a significant shape, which characterises it as a member of a particular species. This fundamental fact of morphology may be regarded as the minimum achievement of every living being, and the establishment of this fact outlines already the typical three-storied problem of biology. In order to sort out living beings into species, we have to establish standards of normal shapes characterising each species and then appraise individual specimens as normal or abnormal; mutilated or complete; healthy or deformed; in the light of the standard of its own species. This involves three logical levels which I shall now proceed to elaborate further.

The classification of living beings according to typical shapes is the task of taxonomy. We should realise at the start the magnitude of this task and the remarkable mental powers available for facing it. Take the shape of the living being with which we are most familiar, the shape of man. How wide are the variations to which the human shape is subject? It changes according to age and race; then abnormally, through malformation or mutilation; through diseases which may shrivel the body or else swell it and distort it; by malignant excrescences which may weigh many pounds. Yet the Common Law makes the crime of murder and hence also punishment for murder, dependent on the human shape of the individual whose death has been caused. It demands that through all its variations we should identify the presence of what the law calls the human shape; and this demand seems hardly excessive since one has not heard of a case in which an accused had pleaded failure to recognise the human shape of something he had killed.

Yet it would seem impossible to devise a definition which would cover the whole range over which human shape may vary, and it is certain that those who recognise it are not in possession of any such definition.

Instead they have a belief that human beings exist and they continue to build up their awareness of the human type by noticing individual human beings. The process is of the same kind by which our focal awareness of a comprehensive feature is generated from a subsidiary awareness of its parts. We have seen how the attention by which we concentrate on this focal meaning may assimilate further parts of it which fit into the whole without our ever noticing the act except in the corresponding modification of the comprehensive feature to which it contributes.

The science of taxonomy has exercised this faculty of comprehending distinctive types from an aggregate of varied specimens over an enormous field of study. The British Museum has a collection of 15 million insects and 5 million plants, which it can use for matching any new specimen either in order to identify it as one of the million known species of insects or some hundreds of thousands of species of plants, or else as representing a new, not yet recorded species. Equipped with its unique collection of type specimens, the staff of the British Museum is recognised internationally as an authority on the taxonomic classification of insects. It was reported for example on Nov. 5th, 1951, that "American zoologists found a strange flea in Utah and sent it to Dr. Carl Jordan, the British Museum expert for his opinion. Dr. Jordan confirmed the Utah flea as a new species." Such are the functions of the expert taxonomist. The peculiar faculties that went to the building up of this storehouse of taxonomic connoisseurship and are exercised in a minor way in the practice of this connoisseurship can best be appreciated by remembering the work of some naturalists who displayed them in a high degree. Famous among these was Sir J.D. Hooker, who in 1859 brought together and published evidence of nearly 8000 species of flowering plants in Australia, more than 7000 of which he had himself collected. The 8000 generic entities which Hooker derived from the individual specimens coming under his notice, have been recognised as valid in the vast majority of cases by subsequent observations of botanists. Hooker's special gift for recognising types have accordingly received high praise. "Few have ever known or will ever know plants as he did", wrote his biographer, "He knew his plants personally." *

The personal character of the knowledge embodied in the identification of different species was clearly revealed in the transactions of the Fifth International Botanical Congress of 1930 held in Cambridge partly for the purpose of finding a definition for a species. Thus Professor C. H. Ostenfeld responded to the problem of the Congress by stating emphatically that a species consists of all the individuals the character of which are in all main points the same so far as the characters which we consider essential are concerned. It is obvious that the 'we' in this definition refers neither to the speaker alone nor to the whole of mankind, but to skilled botanists accredited as such by the speaker. A.S. Hitchcock revealed the situation more frankly by saying "The concept of most species must rest on the judgment and experience of the individual botanist".

*F.O. Bower, "Sir J. D. Hooker," in Makers of British Botany, (1913), p. 303, and "Hooker's Position as a Botanist" in Lady Huxley, Life and Letters of Sir J.D. Hooker, (1918), p.412

A species is admittedly defined in each case by certain distinctive key features which facilitate the subsequent identification of a specimen as belonging to a species; but this procedure presupposes our anterior acknowledgment of the species of which the key feature shall be accepted as a true characteristic. It would be absurd to go about collecting specimens which have some particular feature in common in the hope that this will establish a species of which the feature in question would be the key feature.

Moreover, key features themselves are variable in shape and hence, reference to them represents once more a claim to the recognition of a typical shape which transcends such variations. We cannot escape this necessity by reference to the characteristic details of the key since this would merely shift the problem to a further stage. It is unavoidable that ultimately the anatomy of a species should rely on reference to details which are not further specified in detail, and since these ultimate details will still be manifestly variable such knowledge as we claim to have of them must rest on our estimate of what are things of the same type in different forms. When key features of plant species are ultimately characterised by different authors as "ovate, oval, lanceolate, hirsute, ciliate . . ." said T. Wilmot at the Fifth International Congress of Botany, these authors may have quite different attributes in mind. For the meaning of such elementary terms is itself the product of a process of generalisation over a series of variable instances which were inevitably evaluated somewhat differently by different botanists.

Finally, the art of knowing a species depends on the use of the highly personal skill of examining specimens. You have no effective knowledge of a particular detail of anatomy--say the anterior tibial artery in man--unless you can isolate it yourself by dissection, and this requires a delicate and not specifiable skill. More familiar illustrations may be borrowed from the art of the medical diagnostician. In order to judge the quality of a pulse, you must know how to feel for it. Similarly, you hear clearly enough the changes in the percutory sounds elicited by a practised teacher of medicine, but find that you cannot reproduce them. What you do will in general determine what you observe; and no specification can define the right mode of doing.

I have spoken earlier of the use of maxims, the interpretation of which is part of an art, as a guide to the practice of an art. The knowledge of a systematic key plays the part of a maxim in the identification of specimens; it is useless to a person not trained in the art of identifying the key features within the context of which it forms part.

In any case, the identification of specimens in the field may often successfully dispense with the observation of any key features when these are located inside the specimen's body. The distinguished zoologist C.F.A. Pantin has described, in a memoir which I have read in manuscript,

1) S.C. Harland, "The Genetical Conception of the Species", Cambridge Biol.Rev. Vol XI. (1936) p.83-112

the way he identifies certain species of lizards by their manner of crawling; much as Claus Fuchs identified Greenglass, whom he had failed to recognize from his photographs, the moment he saw a film showing him walk about.

These principles of taxonomic classification are most clearly revealed when taxonomists are attacked on the grounds of practising them. The essentially personal and authoritative definition of a species suggested at the Fifth International Congress of Botany was made fun of six years later by the distinguished geneticist S.C. Harland. He recalled that in Fanny's First Play, the dramatic critic replied to the question whether the play was a good play, that if the play was by a good author it was a good play. "The situation would appear to be somewhat similar," says Harland, "in regard to what constitutes a species." He himself attempts to correct this situation by analyzing some species in genetic terms. Other biologists would eliminate connoisseurship from taxonomy by restricting the conception of a species to a group within which cross-fertilisation produces fertile offspring. By applying this more impersonal standard, they would challenge the scientific value of identifying species according to typical shapes. However, the lack of fertility in cross-breeds can only be observed by first establishing distinctive morphological types between which cross-breeding can be said to take place. In fact, interspecific infertility can be used merely as a subsidiary criterion of morphological classifications; as such a criterion it serves to bear them out and so, add new weight to them.

The same holds of other tests drawn from genetics, or else from bio-chemistry, ecology, etc. They may all be useful as subserving the purposes of morphology, but they cannot be substituted for it. The suggestion to ignore the immense quantity of morphological knowledge accumulated by naturalists from a preference for the greater objectivity of other tests can in my view only be regarded as an interesting expression of a misguided prejudice against the true nature of science. We should meet the challenge by re-affirming boldly that this immense spectacle of natural order rests and must continue to rest on our personal comprehension of typical shapes as culled from a vast multitude of individuals exemplifying them; or else on our personal appraisal of another person's capacity for seeing such shapes. Harland's mockery should be answered in his own terms, by replying that good science is - exactly like a good play - inherently the work of a master, and can be upheld only by a continued respect for scientific mastery, while continuing to practice the art of knowing as exemplified by the masters.

Biologists have indeed felt that the recognition of the typical shape of living beings is akin to the appreciation of a work of art. They have told us that only the naturalist who loves plants and animals will acquire a personal knowledge of them, and unless he can contemplate them with joy he will never be able to observe them with

sufficient persistence. "I confidently assert," writes Konrad Z. Lorenz¹ "that no man, even if he were endowed with a superhuman patience, could physically bring himself to stare at fishes birds or mammals as persistently as is necessary in order to take stock of the behavior patterns of a species, unless his eyes were bound to the object of his observation in that spellbound gaze which is not motivated by any conscious effort to gain knowledge, but by that mysterious charm that the beauty of living creatures works on some of us!"

We find accordingly Agnes Arber suggesting in her book "The Natural Philosophy of Plant Form" (C.U.P. 1950, p.210) that morphological insight in botany is aided by artistic powers; while the zoologist, C.F.A. Pantin² gives examples of the aesthetic appreciation of animal forms leading to the appreciation of anatomical features.

This aesthetic appreciation operates on three logical levels as follows: Prompted by his admiration for the harmonious shapes and functions of animals and plants, the naturalist sets up standards of harmony for the several kinds of living beings; standards, which he regards as typical or normal for them. Any new specimens that come to hand are identified as belonging to one of these types or else are acknowledged as the prototypes of new species. Specimens which are identified, are at the same time assessed as more or less true to type. Some variations of them are accepted as normal, while others are condemned as mutilated or pathological forms, or as outright monstrosities. When the taxonomist surveys a system of species, grading them, for example, according to their level of differentiation, he is operating on the highest logical level; when appraising an individual specimen according to standards which he regards as normal for it, he operates on the logical level below this; while the specimens themselves are still further down, at the lowest logical level. This is the three-storied structure of biological science, in which living beings are criticised from a point of view which is ascribed to them as their own. The three-fold stratification does not yet reach its fullest expression here, for the possession of a typical form is not something actively done by the individual and therefore the individual's success or failure in being true to type cannot be brought home to any active centre operating within it.

Yet the degree to which the perfection of specimens is assessed from the individual's own point of view is distinctly more marked in this case than in the application of crystallographic standards to individual crystal specimens, which process approaches it most closely in the inanimate sciences. For space-groups are derived geometrically from a summary experience of crystals in general, while morphological types are constructed from a close observation of individual specimens and are continuously subject to revision in the light of further observations of this kind.

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- 1) Konrad Z. Lorenz in "Physiological Mechanisms of Animal Behavior" Cambridge 1950.
 - 2) C.F.A. Pantin, Presidential Address to the Zool. Sect. British Association 1951.

I have explained earlier on that every act of personal knowing, whether it lies in the performance of skill or in the giving of an expert opinion, sets up a standard for appraising that which it knows. The self-set standards of personal knowledge gain a new structure when the biologist sets up his morphological standards on one logical level and pronounces his judgment on individual specimens from the next lower logical level. The paradoxical fact that the knower is responsible for both processes at the same time is brought out by the different effects which the identification of every new specimen entails on both levels of judgment. When the expert in the British Museum identifies a specimen of a rare insect with a known species, the emphasis lies of course on the lower level, from which he classifies the specimen by pre-given standards. But at the same time his expert knowledge of this rare insect is being enriched and to this extent its accepted morphological standard is being modified. It is even conceivable that the expert would hesitate to identify the new specimen as a member of the species in question, but for the fact that he has slightly modified his conception of the species by the very act of accepting the specimen as a member of it.

An explorer may of course seek his way by the aid of an inaccurate map, which he continues to revise in the process of using it; but with the taxonomist every act of map reading entails the valuation of a specimen and every act of mapping the revaluation of the standard of valuation, the two going on concomitantly.

No wonder that philosophers, reflecting of this process without acknowledging the justification of personal knowledge, have not been able to make sense of it. Take the morphological type of the coelacanth of which two living specimens have been observed so far. There are two strictly impersonal interpretations of this situation. Either the name 'coelacanth' denotes the pair of curious specimens recently discovered, in which case it ignores the coelacanth as a species; or the name 'coelacanth' refers to the Platonic ideal of the coelacanth in which case we may doubt whether we know anything about it, since both our specimens may be quite abnormal. The dilemma is averted by acknowledging taxonomy as a personal process of setting up standards on the grounds of experience and judging experience in the light of these standards.

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The notorious logical stumbling block in the biological sciences has of course always been teleology and its relation to natural causation. Now we can see that the logical problems of biology arise at an earlier stage already from the mere accrediting of typical shapes to plants and animals. In fact the idea of purposefulness can be introduced by merely passing from the science of typical shapes to the science dealing with the coming into being of these shapes. This takes us from the appraisal of living forms by the standard of their types to the appraisal of the processes of regeneration or embryonic growth, by which such forms are achieved; in other words, from the science of morphology to the science of morphogenesis. The transition from the appraisal of being to the appraisal of purpose is thus quite easy.

Let me give now a few characteristic samples of the kind of knowledge that we possess of the morphogenesis of animals on which we wish to reflect. Protozoa regenerate from any small piece containing a fragment of the nucleus. Ascending to multicellular types, we find that hydra and many other cœlenterates are able to develop new individuals from cuttings. Certain fresh water worms (*Lumbriculus*, *Stenostomum*) may regenerate from a piece 500 times smaller than their bulk.

In higher animals we still observe the regeneration of a limb or a sense organ. Thus the arms of the squib, the eye-bearing tentacles of snails, the legs of spiders, the claws of lobsters, the lens in the newt (Cf. Paul Weiss¹ 459 ff.) all appendages of insects and the limbs of fish and amphibians grow again after amputation. Mammals cannot be regenerated, all that remains is only a process of continuous repair and the healing of wounds. Young animals regenerate better than old ones. The frog is unable to replace a limb whereas as a young tadpole it is able to regenerate the same member.

The sexual propagation of the higher animals may be regarded as a form of regeneration in which a new individual grows out of two fused cells of two adults of opposite sex. The embryonic stage of this growth leads up to the formation of a youthful animal which then grows into a mature individual capable of reproducing itself in its turn. The fragments detached from embryos of certain lower animals have the capacity of regenerating the whole embryo and of producing normal individuals. This ontogenetic principle was first discovered by H. Driesch in the embryo of the sea urchin. Throughout its cleavage stage any cell or combination of cells detached from the embryo will develop into a normal sea urchin. Driesch characterised these regenerative powers of an embryo by describing it as an "harmonious equipotential" system. The apparent tendency of the germ to build up a normal embryo in spite of severe amputations is more generally referred to today as "morphogenetic regulation".

Other embryos show more firmly localized potentialities from the very first. If the fertilised Ascidian egg in the 2 or 4 cell stage is cut in two, each half develops only into half an embryo. Though this type of ontogenesis is never free of regulative tendencies, its principle can be clearly distinguished as a pattern of independently proceeding processes of growth; by which the organism is built up in sections which must fit together and be ready to function together when the moment arrives for it. Such a mosaic of independently proceeding interlocking sequences corresponds to the conception of ontogenesis which Roux and Waisman had formulated and made universally current before Driesch's observations on equipotentiality.

Spemann's discovery of organisers in the ontogenesis of vertebrates presents us with a combination of both principles. He found that in the

1) Principles of Development (1939).

next at the gastrula stage of the embryo there is situated at the edge of the blastopore, that is the entrance of the cavity representing the primitive gut, a region which dominates the further segmentation of the embryo. If the embryo is cut up, any part of it in which this dominant region is included or in which it is engrafted, will proceed to develop further, while in the embryonic tissue from which it is eliminated, individuation comes to a stop. Thus the dominant region, which is the seat of the organiser, moulds a whole region under its control into one complete embryo, irrespective of any otherwise predetermined character of its several components cells, which are induced to respond equipotentially to its stimulus.

This early equipotentiality is superseded in the further process of embryonic maturation by the formation of sub-centres of organisation. Each of these separate organisers controls the development of one section of the embryo in relative freedom from regulative influence of other parts of the embryo which are developing under the control of other organisers. The first organiser having split into a whole hierarchy of secondary, tertiary, etc. specialised sub-organisers each of these controls the development of a limb, or part of a limb or of some other organ or feature emerging from the progressive differentiation of the individual. A segregated area provided with its own organiser may be cut off with it and go on differentiating in isolation; as has been proved for the case of limb and tail buds of newts and lizards. A tail bud transplanted, after a certain maturation, into the place of an amputated limb will still produce in this anomalous position the determinate tail. At this higher stage the development of the embryo may be regarded as a mosaic of interlocking independent sequences, each controlled by its organiser, while equipotentiality has been reduced within the narrow confines of the several regions controlled by their separate organisers.

This picture of ontogenesis, as the invasion of embryonic tissues by a multiplying array of organisers, represents the outcome as entirely determined by the action of the organisers, to which the tissues respond passively. But embryonic tissues do not always submit unconditionally to the field of an organiser. Grafting experiments have shown that "a field cannot make any cell produce any specific response unless that cell is intrinsically prepared to do so."¹ This preparedness is called the 'competence' of the tissue.² Indeed, the part played by the organiser may be reduced to a mere evocation of the potentialities preformed in the tissue subjected to its influence. Tissue taken from the embryo of species A (newt) may fail to produce certain features that are characteristic of species B (axolotl) when subjected to the organisers of species B; while in the reverse case, these organs will make their appearance in the engrafted tissue of species B in response to the organisers of the species A - the species which lacks that organ. Experiments of this kind

1) P. Weiss, Principles of Development (1939) p.359.

2) Waddington's term: See H. Spemann, Embryonic Development and Induction, New Haven, 1938, p. 341.

have revealed a wide range of rivalry between the morphogenetic tendencies inherent in an embryonic tissue and those induced in it by the organising influence of adjacent tissues.

I have given you here illustrations for several principles covering a wide range of experimental results obtained since the experimental study of morphogenesis started towards the end of last century. Let me run through them once more as follows. (1) Any small cutting of certain animals will regenerate the whole animal. (2) Any cell or group of cells detached from the embryo of certain species at the stage of segmentation may form a complete animal; in contrast to which (3) the fertilised eggs of other species appear to form a mosaic, the isolated elements of which tend to form only a segment of the complete animal. Furthermore (4) the early development of some animals proceeds under the control of a single organiser which subsequently splits up into a large number of independent sectional organisers; and (5) the morphogenetic effect of any organiser is restricted by the competence of the tissue on which it is acting so that (6) the organiser may even produce a feature found only in the species to which the responding tissue belongs and lacking in the species to which the organiser belongs.

Every one of these principles describes a function, that is an event defined by some measure of success achieved by it. The success is measured in terms of right form-achievement, be it in the regeneration of a mutilated organism or within the embryonic development of a normal individual from the germ cell. Thus the morphogenetic principles discovered by modern experimental embryology derive their meaning entirely from their contribution to the normal processes of embryology as described by classical descriptive embryology.

This situation has evoked the usual antagonism on the part of the scientists who would rebel against their actual dependence on personal knowledge. They want to break out of the purposive framework with reference to which the meaning of their observations is defined. Thus the recent book of C.H. Waddington, entitled "The Epigenetics of Birds" (1952) pointedly defines epigenetics as "the science concerned with the causal analysis of development".¹ Such a claim is correct only if 'causal analysis' is used in a sense that would also apply to the process of finding out how a machine works. For in both cases we must start with the assumption that we are faced with an achievement and then proceed to analyse the principles which may account for this achievement; and in both cases the causal particulars to which we advance in either case are meaningful only in their bearing on the presupposed achievement while any exhaustive causal analysis of these systems carried out without relation to this achievement would totally dissolve our original knowledge of the systems in question.

The study of morphogenesis, like that of morphology, implies our appreciation of typical living shapes, but adds to this appraisal a further judgment on the aptness of the processes by which shapes are achieved, be it by regeneration or by embryonic development. The personal knowledge involved in appreciating such morphogenic time sequences will be best discussed further by affiliating it to other instances of time sequences

1) Comp. also A.M. Daleq, "Form and Causality in Early Development" (1938)

usually described as the behavior of animals. This will bring out fully for all such time sequences the typical three-storied structure of biological knowledge and prepare the ground for the curious problems to which this logical identification of biological knowledge with the process of epistemological reflection will be seen to give rise.