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This investigation attempts to demonstrate that the development of the early 20th century biological enterprise involved a striving for a unifying theoretical perspective. A skeletal substantiation of this is given by an overview of early 20th century biology in which some biologists began recognizing a multiplicity of different orders of concern against the background of the fractionation of biology into specialized empirical domains. More importantly this investigation attempts to show that the attainment of such a unifying theoretical perspective in early 20th century biology necessitated first establishing justified and independent supra-empirical orders of discussion, ultimately of communal status. This second yet primary concern is approached here by tracing the emergence of supra-empirical order <u>concerns</u> in the thought of an individual biologist of the period, J. H. Woodger. It was

found that in the transitions in his writings from empirical to theoretical and philosophical biology, there was a persistent attempt by him to found and secure supra-empirical orders of discussion for the theoretical and philosophical aspects of biology. Woodger's claim was that such orders of discussion must ultimately be of communal status since the framing of a theoretical biology cannot arise from any single empirical domain. To test the relevance of his claim, a set of communication which the editors of Nature entitled 'Biological Fact and Theory', is examined with respect to Woodger's criticisms. This leads to a discussion in the Epilogue of Woodger's later work and the question of the viability of his goal.

J. H. Woodger and the Emergence of Supra-empirical Orders of Discussion in Early Twentieth Century Biology

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I. INTRODUCTION: BACKGROUND AND FOCUS

The Early 20th Century Biological Enterprise

The biology of the early 20th century was an enterprise of fundamentally different character and aim from that of the 19th century, in fact they stand as widely differing points of view on the meaning and study of living phenomena. While it is not difficult to see that the transition involved spirited conflict between the veterans of the "old guard" and the students of the "new biology," it is more difficult to pinpoint the root issues undergoing change or the manifold character of the enterprise in transition. We might seek the recorded reflections of a biologist of this transition period, but if we do we tend to find only short-range histories of one empirical domain or another rather than broad views of a changing biological enterprise.[1] Of broader scope is

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[1] For example see E. B. Wilson, "Some Recent Studies on Heredity," Harvey Lectures 2(1906/07):200-222; Hans Przibram, "Transplantation and Regeneration: Their Bearing on Developmental Mechanics (A Review of the Experiments and Conclusions of the Last Ten Years: 1915-1924)," Journal of Experimental Biology 3(1925):313-330; F. G. Hopkins, "Some Chemical Aspects of Life," Nature(supplement) 129(1933):381-394; C. H. Waddington, "Twenty-five Years of Biology," Discovery 16(1935):134-137.

the account of Edmund B. Wilson (1856-1939) in his presidential address to the American Society of Naturalists in 1900.[2] Wilson was a biologist of wide vision and experience. Having studied and done research in America, England, and Continental Europe he was familiar with the empirical domains and national "schools" of biology.[3] His self-concept as "surveyor" of the field of cytology is amply manifested in his monumental volumes The Cell in Development and Inheritance.[4]

Wilson's topic in his 1900 address was the "Aims and Methods of Study in Natural History" and his intention therein was to discuss the major changes going on in that enterprise. In Wilson's view the natural history of the mid-19th century was guided by the aim of observing vital phenomena with the greatest possible accuracy and then the arrangement and classification of these observations to discover the "natural affinities" of living things. When natural history fell under the spell of the theory of evolution its motive changed to one of interpreting these natural affinities in accordance with evolution theory.[5] But not all of biology fell under this spell, in fact the

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[2] E. B. Wilson, "Aims and Methods of Study in Natural History," Science 13(1901):14-23.

[3] T. H. Morgan, "Edmund Beecher Wilson, 1856-1939," National Academy of Science, Biographic Memoirs 21(1941):318-320, 335.

[4] E. B. Wilson, The Cell in Development and Inheritance (New York: MacMillan Co., 1896 (1st ed.); 1900 (2nd ed.); 1925 (3rd ed.).

[5] Wilson,"Aim and Methods," p. 15.

unifying drive of the evolution doctrine seems to have been limited to the concerns of comparative morphology (i.e. systematic zoology and botany; geographical and geological distribution of animals and fossils; comparative anatomy and embryology). Comprehensive embryological genealogies and ordering schemes in comparative anatomy were carried out but they shed little light on the major evolutionary questions.[6] As a result the interest in the morphological assault on the broad problems of genealogy waned, while interest in the other, "uninvolved" lines of 19th century study received new impetus. Among these, two were of major consequence: the development of the cell theory and a wide-spreading interest in the experimental methodology of the physiologists. In a very short time most morphological studies (and empirical domains) changed their approach to the experimental one. The focus of study became not what organisms do, in the natural history sense, but rather how they do what they do--a concern requiring the control of study conditions.[7]

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[6] Ibid., p. 15-16.

[7] Ibid., p. 18-19. The accuracy of Wilson's statement about the magnitude of changeover in methodology probably requires clarification. Certainly the "experimental method" was being used by biologists at least as early as the beginning of the 19th century, for example in experiments in biogenesis and plant and animal breeding as well as in vivo physiological studies. At least with respect to morphological studies, Wilson's claim gains support if we consider that all the empirical domain's energy in the early 20th century biological enterprise followed the experimental approach. Moreover, I have asked Woodger about turn of the century biology and his major recollection is this rapid changeover in methodologies.

While the immediate impact and interest in the evolution doctrine waned, interest in the cell doctrine eventually raised the latter to the same status as the former as a unifying theoretical generalization. The development of chemical and optical techniques invigorated cytological research so much so that by the beginning of the 20th century the race was on to identify the fundamental cell constituents (e.g. nucleus, centromere, chromatic granules, golgi apparatus) for all the kinds and species of cells.[8] Then, in its theoretical status, the cell became the basic unit of biological organization and the base to which all living processes could be reduced. Cytological studies pervaded the old embryological and physiological concerns and as experimental investigations they produced an abundance of information.[9]

While Wilson was aware of the fractionation and specialization in the new, experimental biology his attitude toward the new methodology was that biologists should welcome each and every new method by which knowledge of living nature could be gotten and unified. Others did not agree. H. S. Jennings (1868-1947), a well known American zoologist, felt biologists should spurn such "borrowed" methodology and that not every empirical domain of biology was amenable to study by the experimental approach. Perhaps the most notorious discussion

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[8]Wilson, The Cell (3rd ed.), p. 1-18.

[9]Charles Singer, "History of Biology," Encyclopedia Britannica (14th ed.), vol. III, p. 617-618.

arising from problems over the experimental method (or rather the interpretation of the results of its use) was the mechanism-vitalism controversy of Hans Driesch and his opponents. To Jennings no one seemed to appreciate the basic fact of biological experimentation: "The same materials, under the action of the same agents, respond in most diverse ways, depending on how the materials are arranged."[10] In other words, living phenomena do not respond as physical phenomena do. Moreover the reaction to the infusion of the experimental method in biology was not wholly internal. The 'antivivisection' issue was social dissatisfaction over the cruelty of animal experiments of such magnitude as to bring those experiments under strict legislative control.[11]

This new biology was not the diffuse morphologically-centered natural history, but rather experimental studies carried out in laboratories under the most controlled of environments. Out of this change in methodology and specialization of study came a newly structured biological enterprise with a host of new empirical domains--genetics, linking cytological studies of the germ cell nucleus with the breeding experiments of the 19th century to give a materialistically-based theory of inheritance[12]; ecology, emerging from natural history as a

[10] H. S. Jennings, "Biology and Experimentation," Science 64(1926): 101.

[11] W. S. Lazarus-Barlow, "Vivisection," Encyclopedia Britannica (14th ed.), vol. XXIII, p. 227-228.

[12] L. C. Dunn, A Short History of Genetics: 1864-1939, (New York: McGraw-Hill, 1968), p. ix-xiii, 148-157.

concern for the 'economics of nature' and focusing on the interrelations of organisms and organismal groups[13]; biochemistry, emerging from the separation of physiological chemistry from physiology proper and the application of new physical and chemical techniques (e.g. crystallization of protein, X-ray diffraction)[14]; and animal behavior, emerging from the comparative animal psychology of the medical schools with a rejection of their physiological focus and methodology.[15]

The Recognition of Supra-empirical Order Concerns

The rapid changeover in methodological approach of which Wilson spoke as well as the delimiting of new, independent areas of study began a rapid increase in biological information in the 1920's and 1930's. In order to house this information and promote channels of communication for these increasingly specialized areas of interest, new journals were begun focusing on one or another empirical domain as well as new university departments promoting singularly fruitful areas of study. In the face of this, the goal of a unitary view of living nature was becoming less and less a realizable goal. Some biologists were troubled by the artificiality and inaccessibility of the fractionated

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[13] Charles Elton, "Ecology, Animal," Encyclopedia Britannica (14th ed.), vol. VIII, p. 915-924.

[14] J. C. Drummond, "Biochemistry," Encyclopedia Britannica (14th ed.), vol. VI, p. 589-593.

[15] C. Lloyd Morgan, "Animal Behaviour," Encyclopedia Britannica (14th ed.), vol. I, p. 960-963.

knowledge of biology. Other biologists confused the Author of nature with the editor of Nature![16] Within this climate some biologists began speaking of the need for a 'general biology', one which would provide a unified picture of the biological world. Responses to this problem were as varied as the biologists themselves.

Several common characteristics of the issue seem evident however. First it seems clear that the discussion of this problem was not a popular one in the mainstream of biological discussion. While it was discussed indirectly, no major forums were provided for it in the major journals. Moreover, non-empirical discussions on the questions of mechanism-vitalism, holism, emergent evolutionism, hormic and mnemic theories of life, organicism and psychobiology were all deemed "philosophical" in their concern and thus of little interest to the working empirical biologist. The solutions or general perspective which these discussions posed were obscure, biased, and at best speculative--they did not share in the empirical motive.[17] Secondly, the participants and contributions to the problem of overcoming the specialized separatism in biology were made from one empirical domain or another; and due to domain-specific interest, the terminology

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[16] I believe this witticism is attributed to the philosopher C. D. Broad.

[17] This matter is the concern of two contemporaneous studies: R. L. Wheeler, Vitalism, (London: H. F. and G. Witherby, 1939); W. MacDougall, The Riddle of Life: A Survey of Theories, (London: Methuen, 1938).

and perspectives were of little interest or were conceptually inaccessible to biologists of other empirical domains.

One of the most telling statements was made by J. Arthur Thomson (1861-1933) in his article "Biology" in the Encyclopedia Britannica,

> Biology may be used, then, in a comprehensive way to include all the special sciences that deal with different parts of the biosphere; but the stricter usage, which began with Treviranus and Lamarck, is in reference to the study of the larger or deeper questions that apply to all sorts of living creatures. In this sense the biologist inquires into the nature of the particular kind of activity that we call life.[18]

Thomson speaks here first of empirical biology and then of a more general concern, one at least 'theoretical' but perhaps 'philosophical', one which would use the findings of all the special sciences to discover the most general characteristics of life, in a word a general theoretical biology. Furthermore, it seems to me that Thomson was intimating that the modern biological enterprise was neglecting its original goal.[19]

The same motive and goal was behind the writings of other biologists, discussions which penetrated to the heart of the problem only to fall to the seduction of framing their own perspectivally-biased

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[18] J. Arthur Thomson, "Biology," Encyclopedia Britannica (14th ed.), vol. III, p. 602-609.

[19] An interesting discussion of this period along the lines of the "birthright of biology" is E. Sinnott, "The Cell and the Problems of Organization," Science 89(1939):41-46.

theoretical biologies. Two interesting examples of this are E. S. Russell (1887-1954) and J. S. Haldane (1860-1936). Russell, a fisheries zoologist and student of animal behavior, in his 1924 book The Study of Living Things[20] saw the problem as the partiality of the methodological points of view of biology (e.g. morphological, physiological). After explicating the character of these points of view, Russell developed that perspective which would provide a "balanced" picture of life. He showed that his perspective, the "functional" viewpoint was based in the common-sense attitude toward life. Pervading his discussion however was his insistence on the recognition of "concrete biological facts," especially the "striving after ends" or "goal directedness" of organic activity. Russell then took the task of framing a theoretical biology onto his own shoulders, claiming the general utility of his concrete fact developed from his functional viewpoint.

One of Russell's critics argued that his position was untenable because he used psychological concepts at the sub-organismal level--an error as great as the reduction of the organism to physico-chemical mechanisms in the mechanistic interpretation.[21]

John Scott Haldane went a step further in the problem and for the most part resisted the temptation of single-handedly framing a

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[20] E. S. Russell, The Study of Living Things, (London: Metheun, 1924).

[21] J.B.S. Haldane, review of The Study of Living Things, by E. S. Russell, in Nature 115(1925):218-219.

theoretical biology. In his most comprehensive statement, The Sciences and Philosophy[22], Haldane reflected on his nearly half-century of research in physiology realizing that he still found the mechanistic and vitalistic interpretations of life equally inadequate. A third interpretation was required and through his early interest in philosophy Haldane saw the working out of this interpretation in some sense "philosophical."[23] For Haldane nature is presented to experience in principally three modes, the mechanical, the living, and the personal. These are studied according to certain incommensurate modes of interpretation, which correspond to the special sciences--physical, biological and psychological or humanistic. Thus for Haldane, "what we perceive is a matter of interpretation and [interpretation] depends upon the axioms or working hypotheses which we adopt as consistent with our observations."[24] Thus the development

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[22] J. S. Haldane, The Sciences and Philosophy, (London: Hodder and Stroughton, 1929).

[23] In the preface of his book Haldane states, "I was a medical student at the time [1890's], ...my chief interest was philosophy, and I had already seen that the mechanistic biology, which was everywhere in the ascendent, was as radically unsound as vitalistic biology. We said so in our essay [in Essays in Philosphical Criticism, ed. by A. Seth, 1884] but could give only general reasons for our conclusion that the real axioms of biology are neither mechanistic nor vitalistic. The truth was that in matters of detail many of the available data were so vague and unsatisfactory that mechanistic interpretations of them, though certainly not the most characteristic data, were at least plausible [i.e. a third interpretation was actually what was needed], p. v-vi.

[24] Haldane, Sciences and Philosophy, p. 362.

of a third interpretation would involve both a revision of the standing interpretations (since these are the only ones given to us) and the gaining of a handle on the problem of the variability of interpretation. To the first task Haldane contributed two revisions of physical theory of special relevance to biology.[25] As for the second task Haldane insisted that "from a philosophical standpoint it is of the utmost importance to face and thoroughly discuss the difficulty [of interpretation], ..."[26] Thus in the face of the fractionation of the biological enterprise and the general intolerance for any non-empirical discussion in the main, Haldane was making a plea for the emergence of a platform or order of discussion to deal with theoretical-philosophical concerns.

This brief survey indicates that during the early decades of the 20th century there was a growing concern over the absence of a unifying theoretical perspective in biology and a striving, by some concerned biologists for the recognition of supra-empirical order

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[25] J. S. Haldane, The Theory of Heat Engines, (London: Oliver and Boyd, 1930), a discussion of the thermodynamic conceptions of Carnot and Kelvin as well as a discussion of internal combustion and reciprocating steam engines both used in a discussion of muscles as heat engines; Gases and Liquids: A Contribution to Molecular Physics, (London: Oliver and Boyd, 1928), discusses gases and liquids according to the interpretation of molecular dynamics and then considers the utility of thermodynamics and steam engine dynamics for biological conceptualization.

[26] Haldane, Sciences and Philosophy, p. 183.

concerns. But the attainment of such a perspective necessitated, as a precondition, the establishment of justified and independent supra-empirical orders of discussion, ultimately of communal status. Seminal in this development was the work of J. H. Woodger (1894-). In this paper I will trace in depth the emergence of supra-empirical order concerns in the thought of this individual biologist and then look at his attempts to found justified and independent supra-empirical orders of discussion for these theoretical and philosophical aspects of the biological enterprise. In my conclusion I will discuss the question of the communal status of such orders of discussion and in an epilogue look briefly at Woodger's logical work.

Contemporary philosophy of science with its emphasis on the logic of scientific activity no doubt understands the differences among the various orders of the scientific enterprise. Indeed it can articulate these differences with great sophistication if not logical rigor. But to see the substantive change in a scientist's thinking about his working empirical domain to concerns more theoretical and philosophical, to see the context in which an orderly change in his use of abstraction occurs and is reflectively utilized by him, seems to me to be a more meaningful goal.

II. J. H. WOODGER AND THE EMERGENCE OF SUPRA-EMPIRICAL ORDERS OF DISCUSSION

Woodger: The Man and Scientist

Early Years and Education

Joseph Henry Woodger was born May 2, 1894 and spent his early life in the Norfolk County of Eastern England. Here, in the fishing village of Great Yarmouth his family had lived for generations, and here the young Woodger spent his early years.[27] The surrounding countryside, known as the "broads," was an expanse of flatlands criss-crossed with lakes, rivers and land bridges and teeming with a wide variety of land, fresh and saltwater flora and fauna.[28] In this environment Woodger gained a lifelong enthusiasm for the natural biological world.

Woodger's early education took place at the centuries-old Felsted Public School in Essex. His interest in biology was encouraged here and upon graduation he enrolled at University College in London to study zoology under J. P. Hill (1873-1954), then one of the foremost

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[27] Biographical information about J. H. Woodger is from J. R. Gregg and F.T.C. Harris, eds., Form and Strategy in Science: Studies Presented to Joseph Henry Woodger on the Occasion of His Seventieth Birthday, (Dordrecht, The Netherlands: D. Reidel, 1964).

[28] H. Hadfield, ed., The Shell Guide to England, (New York: American Heritage Press, 1970), p. 403-404.

zoologists in England. At this time Hill was sponsoring a special honors degree in zoology which emphasized vertebrate embryology and involved coursework, an extensive study of original papers and materials, as well as a laboratory research problem in the senior year.[29] Woodger worked with Hill on a problem in avian embryology and graduated with honors. He won the College Prize in Zoology as well as the Derby Scholarship, the only money available for graduate research students at this time. With the outbreak of World War I in the fall of 1914, Woodger's hopes of continuing his research were shattered.

Woodger left University College and volunteered in the spring of 1915. He was commissioned in the Norfolk regiment but served only a short time in the European war before being sent to Mesopotamia (now Iraq) where he remained until his demobilization in 1919. While in Mesopotamia Woodger learned a number of important things that would influence his career decisions when he returned to England. In the period prior to Woodger's assignment to Mesopotamia, the British expedition there was proving to be a disaster both politically and in loss of human life. The goal had been to regain control of the city of Baghdad from the Turkish forces. The first battle, the battle of Kut-el-Amara, resulted when General Townshend's Anglo-Indian corp

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[29] D.M.S. Watson, "James Peter Hill, 1873-1954," Biographical Memoirs (Royal Society of London) 1(1955):110.

was besieged by Turkish forces, routed from its march on Baghdad and forced to withdraw to the city of Kut. The Turks continued their assault, blocking all relief forces and supplies. Operations initiated by the Indian government to bring troops to General Townshend's aid resulted in the loss of 6,000 men in the seasonal flood of that area. This was seen as unforgivably bad scheduling on the part of the Indian government. Facing mass starvation and disease, General Townshend surrendered to the Turks in the spring of 1916. This situation was reversed in 1918 with the battle of Shumran Bend, where troops under General Mande (Woodger among them) forced the retreat of the Turks from Kut-el-Amara as well as Baghdad.[30] This was Woodger's first lesson in the value of human life and suffering from the viewpoint of politics and government; life-long cynicism was his reaction to it.

Woodger had two other, more positive, experiences while stationed in Mesopotamia. First, while working at the Central Laboratory in Amara, he gained experience in protozoology while studying house flies as carriers of disease-transmitting parasites. He described his findings in a paper published in the Annals of Applied Biology.[31] Woodger's work at the Central Laboratory was largely research oriented, his superiors encouraging him to follow his own ideas.

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[30] T. Harbottle, Dictionary of Battles, (New York: Stein and Day, 1971), p. 151-152.

[31] J. H. Woodger, "Notes on a Cestode Occurring in the Haemocoeles of Houseflies in Mesopotamia," Annals of Applied Biology 7(1921): 345-351.

This freedom no doubt contributed to his interest in invertebrates, hitherto overshadowed by the focus on vertebrates in his formal education. Secondly, Woodger made the acquaintance of Dr. Ian Suttie (1889-1935), a fellow officer who later became a psychiatrist known essentially for his criticisms of the Freudian theories and methodology.[32] Woodger attributed to Suttie the arousal of his own interest in scientific methodology as well as the history and philosophy of science.[33] Thus in the spring of 1919 Woodger returned to England with a new biological focus on invertebrates and a wider perspective on the scientific enterprise. And the period from 1919 to 1922 saw a development in Woodger's interest in these directions.

Cytological Research

Upon his return to University College in 1919 Woodger was appointed Assistant in Zoology and Comparative Anatomy. At this time the Senior Assistant was J. Bronte Gatenby (1892-   ) who in a meteoric career was only a few years away from a full professorship at the age of 29.[34] While Woodger was in military service Gatenby had begun his research career in cell morphology and embryology. In

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[32] Ian Suttie's major work was The Origins of Love and Hate, (London: Kegan Paul, 1935).

[33] J. H. Woodger, Biology and Language, (Cambridge: Cambridge University Press, 1952), p. 302.

[34] "Gatenby, J. B.," Who's Who in British Science, 1953.

1917 Gatenby began publishing a series of investigations under the title of "The Cytoplasmic Inclusions of Germ Cells."[35] Woodger first collaborated with Gatenby on a 1920 paper concerned with some methodological problems in cytology[36] and then in 1921 on the ninth installment in Gatenby's series.[37]

At this time by far the most oustanding "student of the cell" was the American Edmund B. Wilson[38] who worked at the University of Chicago. In 1896 Wilson published the first edition of his monumental The Cell in Development and Inheritance[39], a comprehensive survey of research on cells. In 1900 the second and in 1925 the third edition appeared, the size expanding from 371 to 1232 pages: the study of the cell, through its affiliations with anatomy, histology, embryology, general physiology and genetics had undergone a veritable explosion in information. The two major lines of study seemed to be first the

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[35] A series of papers published in the Quarterly Review of Microscopical Science (1917-1921) concerned with the cytoplasmic inclusions of the germ cells of various invertebrates with special emphasis on the range and effectiveness of the cytological techniques involved. For full listing see the Bibliography.

[36] J. B. Gatenby and J. H. Woodger, "On the Relationship between the Formation of Yolk and the Mitochondria and Golgi Apparatus during Oogenesis," Journal of the Royal Microscopical Society, March 1920, p. 129-156.

[37] J. B. Gatenby and J. H. Woodger, "The Cytoplasmic Inclusions of the Germ Cells. Part IX. On the Origin of the Golgi Apparatus on the Middle-piece of the Riper Sperm of Cavia, and the Development of the Acrosome," Quarterly Journal of Microscopal Science, 164(1921):265-288.

[38] See footnote 3, p. 2.
[39] See footnote 4, p. 2.

identification of the "formed-bodies" of the cell (i.e. the identification of the persisting structures and/or the tracing of them through their changes in form and location) and secondly the identification of their unique micro-chemical characteristics. There was a third important factor, the finding of an agreeable nomenclature for these newly revealed basic cell parts. Wilson faced all these dimensions head on in the succeeding editions of his book.[40]

Gatenby entered the rapidly expanding field of cytology centering his attention on gametogenesis and the golgi bodies (also called golgi apparatus and "dictysomes"), one of the newly postulated formed-bodies of the cytosome and one seemingly involved in the process of cell reproduction.[41] Through observations on the golgi bodies of insects, porifera, molluscs, amphibia and mammals, Gatenby studied the origin and development of germ cells. From his papers it appears that Gatenby was concerned equally with the suitability of his study materials, the quality of fixation and staining, the accuracy of observations on these materials, and the interpretation of these observations with respect to the total process of gametogenesis. He sought above all, to identify basic criteria for recognizing the form and location of golgi bodies against the variable background of their polymorphism within a single cell and among different kinds of cells and species of

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[40] Wilson, The Cell (3rd ed.), p. 1-18.

[41] For a discussion of the work of this period on the golgi apparatus see Wilson, The Cell (3rd ed.), p. 48-53, 714-715.

of organisms. In each of his papers he devoted great attention to technique and made numerous suggestions of a trouble-shooting nature. Several of his papers were presented as guides for rendering all of the intracellular structures distinctly visible.[42]

Woodger's role in these collaborations was largely that of a technician. In their first collaboration the major intent was to pin down an as yet little known "deutoplasmic sphere" (a particular, inert, non-living granule of the cytoplasm). While the common usage of the term "deutoplasmic" was in reference to apparently fundamental granular formations of the cytoplasm of eggs, Gatenby and Woodger demonstrated through painstaking chemical technique that these yolk granules were in reality an aggregate composed of the already well-known mitochondria and golgi bodies.[43] The purpose here was two-fold: to limit the multiplication ad infinitum of so-called "fundamental" intracellular parts and to demonstrate the power of chemical technique in revealing and identifying them. The authors then continued in a similar vein to discuss yolk formation in other classes of organisms in the form of charts suggesting the developmental pathways of intracellular components during the maturation of the oocyte in these

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[42] For example see J. B. Gatenby, "The Cytoplasmic Inclusions of Germ Cells. Part VII. Modern Cytological Technique," Quarterly Journal of Microscopal Science 64(1920):267-301.

[43] Gatenby and Woodger, "Cytoplasmic Inclusions, Part VII," 1920, p. 144-145.

organisms.[44] Also a chart was given of the techniques to be used by others wishing to repeat these observations and the character of the structures they should expect to observe.[45]

Their second collaboration, part IX in Gatenby's series on cytoplasmic inclusions of germ cells, occurred shortly before Gatenby vacated the Senior Assistant position at University College and Woodger assumed it. The research was concerned with the golgi apparatus of ripe cavian sperm cells. Here the focus was on spermatogenesis rather than oogenesis and the concern was for the mode of origin of a developmental membrane that seemed to have all the microchemical characteristics of golgi elements but whose identity was in question. As mentioned above the two major strategies in cytology were (1) microscopic observation leading to visual identification of cell parts and (2) microchemical analysis of those parts to ascertain their unique chemical characteristics. Here the line of reasoning was from the latter to the former, in the spirit of clarification. Through serial observations of developing spermatocytes, the authors were able to note discrepancies in the size, color and location of the microchemically identified "golgi bodies."[46] While the authors could not prove

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[44] Ibid., p. 147-149.

[45] Ibid., p. 153.

[46] Gatenby and Woodger, "Cytoplasmic Inclusions, Pt. IX," 1921, p. 274-279.

conclusively that this membrane was not of the golgi body kind, they were able to present a clarified picture of the development of the "middle-piece" of sperm cells and their surrounding membrane.[47] Again the power of adhering strictly to observation and technique was revealed.

After these papers the collaboration between Woodger and Gatenby ended. Yet the short association had a large influence on Woodger. He had been confronted with the orientation of a critical empirical biologist: critical concern in the choice and handling of study materials; belief in the power of technique when correctly understood and applied; a sense of judgment about the accuracy of observation and a fundamental cautiousness in the speculations founded on empirical results. From Gatenby's intracellular organizational schemes Woodger was also confronted with the methodological, perhaps natural, order required of the objects of study and their nomenclature for successful investigations in an empirical domain such as cytology. Woodger's 1925 paper, a report of some research which he carried out alone indicates that he had learned from this experience.[48]

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[47] Ibid., p. 284-287.

[48] J. H. Woodger, "Observations on the Origin of the Germ Cells of the Fowl (Gallus domesticus), Studied by Means of Their Golgi Bodies," Quarterly Journal of Microscopal Science, 69(1925):445-462.

Woodger's general research problem in the 1925 paper was concerned with the development of the primitive germ cells in fowl. He wished to trace their development back further toward their origin in the embryo than had previously been attempted. Earlier studies had been complicated by the problem of the developmental polymorphism of these cells. Woodger's approach was to use the presence of characteristically-shaped golgi elements as markers for tracing the cells through their developmental changes and for distinguishing them from other cells in the embryo.[49] Woodger's choice of materials was probably guided by both his previous experience in avian embryology as well as the problematic state of knowledge about the origin of germ cells in fowl. His belief in the power of technique is evidenced by his use of the golgi elements as tracers, while his care in the accuracy of observation is revealed both in his statement of his many failures in preparing his study materials and in his caution at pointing out what might be "optical effects" or observations which in his judgment were "indefinite" in what they revealed.[50] In generalizing from his observations Woodger clearly distinguished between "facts" derived from the best possible observations he could make and "strong belief" on his part, giving his bases for inference and citing the results of other workers in support or at variance.[51]

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[49] Ibid., p. 446.

[50] Ibid., p. 447-448.

[51] Ibid., p. 460-461.

Thus it would seem that Woodger had absorbed from his association with Gatenby an appreciation for the orientation of a critical empirical biologist and moreover had assumed that "posture" in his own research efforts. Given this appreciation it is not difficult to understand why in his career as en educator Woodger emphasized a solid grounding in the principles at work in successful biological work beyond the empirical methods and techniques. Here is Woodger's dawning realization of supra-empirical order concerns.

First Book: Elementary Morphology and Physiology for Medical Students

The post-war period saw not only Woodger's first serious research efforts but also important changes in the schools of the University of London: changes made in order to update the departmental structure to accommodate the expansions in the sciences. In Woodger's own University College the decision was made to move the study of histology from physiology into an enlarged department of anatomy. The new department included a Chair of Embryology and Histology to which Woodger's teacher, J. P. Hill was appointed in 1921.[52] At the same time the Middlesex Hospital Medical School wishing to update its medical curriculum and in hoping to become a

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[52] See Watson's biographical memoir of J. P. Hill, footnote 29, p. 14.

school of the University of London, expanded its offerings from chiefly anatomy and physiology to chemistry, physics, and biology as well. In this process the teaching of biology was removed from the Department of Physiology and made into a separate department with a university-funded readership. Woodger was appointed to this position in the summer of 1922.[53]

In this capacity Woodger taught the new biology course as well as lecturing and supervising the laboratory work in the histology course. Here too he continued his research combining his earlier work in avian embryology with his research interests in cytology.[54] Woodger's students were now principally medical students and he soon became involved in the general question of the best content of a medical curriculum. Against the widespread view of the unimportance of the early years of medical training, Woodger insisted that a solid grounding in biology should preface any medical practice. He surveyed the commonly used elementary biological texts to see if they at least contributed to a "sufficient grounding in the scientific principles of the [medical] art to be able to respond in a scientific spirit to the new advances that the coming generation is likely to witness."[55]

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[53] H. C. Thomson, The Story of the Middlesex Hospital Medical School: 1835-1935, (London: Murray, 1935), p. 109-111, 119.

[54] This research resulted in Woodger's 1925 paper discussed on p. 22.

[55] J. H. Woodger, Elementary Morphology and Physiology for Medical Students: A Guide for the First Year and A Stepping Stone to the Second, (London: Humphrey-Milford, 1924), p. v.

Finding no text that did so he set out to write one, and the result was his Elementary Morphology and Physiology for Medical Students, published in 1924.

This was Woodger's first comprehensive statement of his view of the biological enterprise, standing in transition between his early empirical work and his later work in theoretical and philosophical biology.

While Woodger's task was to provide medical students with an adequate account of the biological enterprise relevant to the concerns of their education, the "adequacy" Woodger desired would require him to frame a balanced picture of the interrelated empirical, theoretical, and philosophical aspects of biology when these were not yet clearly distinguished in his own mind (or the minds of others for that matter). The evaluation of Woodger's attempt must be in two ways: (1) as a reflection of his training and experience in empirical biology as well as from his reading in the philosophy of science of his day[56], and (2) as background for his critical study of biology, Biological

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[56] Woodger's discussion of the philosophy of science is based upon A. D. Ritchie, Scientific Method: An Inquiry into the Character and Validity of Natural Laws, (London: Kegan Paul, Trench, Trubner, 1923). A. D. Ritchie (1891-1967) was a biochemist at Cambridge who early in his career began writing in the history and philosophy of science. He developed his own view from the writings of B. Russell, A. N. Whitehead, C. D. Broad, and G. E. Moore; he discusses the data of scientific knowledge; the methods of the generalization of that data and the statement and validity of hypotheses and laws that arise from those generalizations. This book may well have given Woodger one of his earliest exposures to the views of A. N. Whitehead.

Principles, where supra-empirical orders are differentiated and pursued. Thus while we study Woodger's approach in Elementary Morphology and Physiology it is these two factors that we must keep in mind.

From the beginning of the book Woodger emphasizes the importance of biology students having an understanding of the method of science and he criticizes the standard texts of elementary biology and zoology for not providing instruction in it. Woodger contends that the study of the method of science early in the medical curriculum would help students acquire a "sharpness of observation, critical ability in interpretation, and the breadth of view demanded of those who would and should see their immediate problems in relation to biological phenomena as a whole."[57] In this regard Woodger saw the elementary texts heaping on descriptions of anatomical observations without providing the student with grounds for their interpretation, grounds which could provide a rational basis upon which knowledge of the human body might be built. Woodger then admitted that his selection of material for his book was made with an eye toward upholding this distinction between observation and interpretation as well as for emphasizing the interdependence of form and function, essential to the understanding of biological processes.[58]

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[57] Woodger, Elementary Morphology and Physiology, p. v.

[58] Ibid., p. vi-vii.

Following the table of contents Woodger placed a forward to the student filled with canons on "practical work and method of study."

> The first step in the pursuit of any science is the observation of facts: in biological science, of facts about animals and plants. The second step consists in the arrangement and interpretation of these facts.

Observations should be made accurately and recorded simply--preferably by drawing and the student should be careful not to "slur over the difficulty when he finds that his specimen does not conform to type," for to do so is to shut "his mind more and more to the possibility of fresh discovery and weaken his ability to form an independent judgment."[59] With respect to the interpretation of observations the student "should strive to connect his observations into an orderly whole, so that no fact is merely retained in his mind by an arbitrary effort of memory, but is connected by reasoned links to something else he has learnt in the subject."[60] Woodger then makes the distinction between drawings--which are observationally-based, and diagrams--which are interpretationally-based, being an attempt to simplify the complexity of nature.

> All science consists in seeking to diagrammatize nature, to express diverse and particular facts in the most general and most comprehensive way, and so to enable the mind to deal with them in a way that would be impossible if it were compelled only to treat of particulars separately. A diagram is therefore the expression of a generalization and its 'truth'

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[59] Ibid., p. xi.

[60] Ibid., p. xii.

> will depend on the truth of the conception it represents. This is frequently very difficult to determine and is often a matter of opinion. The beginner is warned therefore not to confuse a diagram with a drawing, but to regard it as an aid to understanding.[61]

These passages demonstrate clearly that Elementary Morphology and Physiology emerges from Woodger's previous experience in empirical biology as well as from his fundamental concern for scientific methodology. In fact it is apparent that Woodger had here instituted some of the very elements of the critical biologist's orientation that he learned from J. Bronte Gatenby: the distinction between observation and interpretation, driving from the basic caution in speculating (i.e. making interpretations) from empirical results; his sense of judgment about the accuracy of observations becoming a set of canons for the beginning student to follow so that he might learn to observe clearly and make rational decisions about those observations. From Woodger's comments about drawings and diagrams the beginnings of a line of reasoning can be seen: from individual observations as "diverse and particular facts" to the "diagrammatic" unification of them, constituting a comprehensive generalization whose truth is dependent upon the truth of the conception it represents. While his reasoning lacks clarity, its thrust is to deal with the diversity of observational facts from some encompassing, more abstract order of

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[61] Ibid., p. xii-xiii.

discussion. And Woodger develops this line of reasoning to a greater extent in his epilogue on the method of science.[62]

The format and scope of the 15 chapters presenting empirical biology reveal Woodger's overall aim.[63] The introductory chapter discusses the relationship between biology and medicine through historical example. Chapter II sets the focus on 'animal organization' and in a general overview gives the manner by which a multi-faceted picture of it will be developed in the chapters to follow. Woodger then for the sake of example presents the principal structural features of a frog and the function of these parts to give the student an understanding of what a frog is, how its parts are put together, what functions they fulfill, and the nomenclature biologists use in describing these aspects. Having presented this initial "interpretation," the chapters following develop a more detailed account of the same and other "grades" of organization and the relationship between these grades. Thus beyond

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[62] See p. 39-43 below for a discussion of this epilogue.

[63] Specifically the following chapters: I, Introduction; II, Animal Organization. The Functions and Their Organs; III, The Tissues; IV, The Cells; V, Animals Without Cellular Tissues. The Protozoa; VI, Animals With Cellular Tissues. Metazoa; VII, Animals With Three Fundamental Cellular Layers. Flatworms; VIII, Animals With aCavity in the Mesoderm. Collomates; IX, The Primitive Chordata; X, The Cartilaginous Fishes; XI, The Transition from Water to Air; XII, Closer Adaptation to a Terrestrial Life; XIII, The Racial History of the Mammals; XIV, The Coming into Being of the Individual; XV, Comparative Physiology.

the focus on organization the chapter format develops progressively through the "grades," "types" and "plans" of organization.

Chapters III-VIII develop the idea of grades of organization for the "lower" invertebrates. Woodger discusses the nature and types of tissues which make up organisms (Ch III) and then looks at the organization of tissue as cells, discussing cell multiplication and the nature of intracellular organization--protoplasm and its colloidal constituents (Ch IV). He then distinguishes the protozoa from the metazoa as different types of organization, non-cellular and cellular respectively and discusses the unique structure, behavior and mode of life of each type (Ch V and VI). Moving to the higher grades Woodger considers the flatworms (Ch VII) with three fundamental cell layers and the life characteristics of this grade. He then considers the coelomates (Ch VIII) which arise from a variation in the tri-layer plan by a cavity being formed from the mesodermal layer. This brings the discussion to a consideration of the emergence of chordates from variations in the plan of coelomates (Ch IX).

Beginning with the structure and development of the most primitive chordates, Woodger considers the next higher grade, the cartilaginous fishes (Ch X) and there presents a "ground-plan" of vertebrate organization from which the higher vertebrates would be discussed. Chapter XI considers the amphibia and reptiles as involved in the transition of vertebrates from a water to air environment and then discusses mammals and biologist's classification of them (Ch XII). The next three chapters interpret the past chapters from three different perspectives, Ch XIII, the racial history of mammals or their phylogeny; Ch XIV, mammalian ontogeny and the theories put forth to explain it (e.g. preformation vs epigenesis); and Ch XV, a physiological interpretation of the vital functions and their manifestation through the various grades of organization.

These chapters as a whole are ordered both according to Woodger's idea of grades of organization and according to the overarching doctrine of evolution. I want to make two main points here. According to the first ordering scheme each organism manifests an organizational "plan." Collectively organisms manifesting the same plan of organization constitute a "type" of organization which in the evolutionary picture have undergone simple-to-complex changes resulting in different "grades" of organization of life.[64] The distinction between type, plan and grade of organization is important since the theme of organization is a central one in Woodger's later discussions on theoretical biology. Secondly, the scope manifested by these chapters was that required to cover the variety of grades of organization emerging from the process of evolution and ordered according to the doctrine of

[64] This view of evolution was the standing one among vertebrate embryologists of Woodger's student days. It may be seen to be a development of the idea of basic "plans" of the 19th century comparative anatomists but revised to accommodate the new knowledge from the embryological studies of the late 19th and early 20th centuries. (cf. Wilson, E. B., "Aims and Method of Study in Natural History," Science 13(1901):14-23.)

evolution. Woodger sees biological organization as a key concept involved in the understanding of the evolutionary process and therefore constitutes a "handle" on the diversity of living phenomena.

Following the empirical section, Woodger discusses the study of animal behavior. Woodger's intention in his chapter on animal behavior (Ch XVII) seems to have been twofold. First he desired to set the focus for considering "behavior" and then frame a definition applicable through the grades of biological organization. In this regard he states,

> We began our discussion of animal organization in Chapter II with the behaviour of animals, and to this subject we must return, for in this one supreme function all the others are included and in terms of it alone can they be interpreted. The several parts of animals to whose origin and functions so much of this book has been devoted exist only for the purpose of establishing and maintaining the animal as a whole in its environment.

And against the pervasive evolutionary backdrop:

> Now just as we found an interesting elaboration in the organs themselves as we ascend the animal scale, so we find an increasing elaboration of behavior, that is to say we find an increasing fullness and perfection of the organism's relation to its environment.[65]

Woodger points to consciousness as the most important factor in human behavior and gives as the biologist's task the inquiry into the characteristics of conscious life and how consciousness is related to other biological phenomena. Thus for Woodger the investigation of

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[65] Woodger, Elementary Morphology and Physiology, p. 480.

consciousness must be preceded by investigation of simpler behavior at the lower grades of organization. In this vein he defines behavior as the "action of the organism as a whole on its environment,"[66] and then proceeds to present some of the studies of lower behavior (e.g. H. S. Jennings' Behavior of the Lower Organisms, 1906). Secondly, having through a discussion of studies of the behavior of higher organisms (e.g. C. L. Morgan's Animal Behaviour, 1900) reached 'consciousness', Woodger desired to present the subject of consciousness in a broader treatment than the usual in biology proper. The problem in Woodger's estimation is not whether consciousness is or should be a separate category of existence or whether the postulation of the entity called 'mind' is justified, but rather the "ownership" of the problem of consciousness itself: Does it belong to biology or psychology or physiology or to the study of animal behavior? In Woodger's estimation the subject of consciousness constitutes a separate province of study, the proper concern of a science of psychology. The difficulty in understanding this Woodger attributes to the general ignorance concerning the nature and method of science in general which involves an abhorrence of philosophy[67]; the denial of

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[66] Ibid., p. 480.

[67] "Unfortunately philosophy is too often held in disrepute by students of physical science who believe it to be incapable of contributing to the solution of their problems. While this is no doubt true as far as the internal problems of the several branches of science are concerned, when problems involve the interrelationships between the sciences we cannot neglect the critical inquiries of

consciousness in the standing mechanistic explanation[68] and the adequacy of the neural explanation of consciousness.[69] In the end Woodger decides,

> At the root of these controversies lies the fact, often overlooked, that the concepts of matter and energy are only fabrications of the mind to enable us to interpret physical phenomena. In so far as they do this they are useful and necessary, for they enable us to describe in general terms a host of diverse happenings in the physical world. It must none the less be remembered, when we borrow them for the purposes of biology, that there is nothing 'absolute' about them, that they are only hypothetical man-made objects and are constantly undergoing change in the minds of physicists, especially at the present day.[70]

Woodger insists that discussion about various explanations of problematic phenomena are of a different order than either "philosophy" or "science" proper. What is needed is to bring the critical spirit and methods of philosophy into science. Realizing the unlikelihood of this

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philosophy. This neglect is very largely responsible for the intolerance which at the present day often vitiates discussion of the psycho-physical problem" (Ibid., p. 486).

68 "The plausible 'explanations' of those who would avoid the difficulties of consciousness by denying consciousness must therefore be the products of an incomplete view of nature and of our powers of comprehending nature and this applies to many of the mechanistic interpretations of biological phenomena. Such explanations are often the result of a purely analytical method and constitute abstractions from the facts presented by living organisms" (Ibid., p. 487).

69 "If the neural explanation of consciousness is nothing more than an act of faith by those who have an innate abhorrence of non-material entities, or who, because they cannot conceive of anything which is not matter and energy, must therefore turn their faces away from all phenomena which are not describable in those terms" (Ibid., p. 490).

70 Ibid., p. 490.

given the low opinion of philosophy held by most scientists, Woodger would have to carry out their combination within his own person.

It is in Chapter XVI, "The Interpretation of Biology," that Woodger first explicitly explores the province of what he would later call "speculative" (theoretical) biology and the examination of which is the concern of a "critical" (philosophical) biology along with its relation to empirical biology and its methodology. In retrospect this is what I believe was "in use" in his chapter on animal behavior discussed above. The distinction between these two orders of discussion while not yet explicit for Woodger clearly separates observation and interpretation but speaks interchangeably of "critical" and "speculative" concerns. Also there is an embryonic contribution to theoretical biology in the general interpretive framework Woodger presents and utility of which he demonstrates in a "critical" examination of the doctrine of evolution.

This interpretive framework stems from Woodger's recognition of the "threefold aspect of life," <u>form</u>, <u>function</u> and <u>environment</u>. The interrelation of these aspects leads to three questions: What is the nature of the form of living organisms, what is the manner in which form's functioning is achieved, and what is the nature of the relation between function and environment. In answering the last of these questions the problem of purpose is raised. In Woodger's thinking the problematic nature of purposive behavior is due to its inadequacy as a scientific explanation:

> A 'scientific' explanation is one which describes a phenomenon in terms of other phenomena which are simpler and of more general application. An explanation of a function in terms of the purpose or end it fulfills is called a teleological one and is clearly useless for the purposes of science because it introduces no known more general terms, it carries us no further for it takes us out of the realm of science altogether, it invokes final causes with which science has nothing to do. A teleological explanation may be a true one, but science cannot accept it until it has explored all other possible lines of inquiry, and consequently many attempts have been made to find other ways of describing biological phenomena than those of a teleological nature.[71]

In one sense the history of biology has been the search for these "other ways of describing biological phenomena" and Woodger sees the age-old controversy between mechanism and vitalism as disagreement on modes of explanation suitable in science. He also sees any empirical biologist's sympathy in this controversy as a function of the particular empirical domain he works in. Thus, for example, an analytical domain like physiology sees the whole organism as an aggregate of part-functions and utilizes a mechanistic mode of explanation; animal behaviorists, viewing the organism as a whole, tend toward more vitalistic modes of explanation. Woodger also sees the preformation-epigenesis debate as a mechanism-vitalism kind of argument within embryology. That the mechanists retain the upper

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[71] Ibid., p. 459.

hand Woodger attributes to the fact that mechanistic explanation does not breach the fundamental belief in the continuity of nature, where "the notion of continuity supposes that living matter has been evolved by the elaboration of inorganic material, and that once formed in this way the properties of matter and energy were sufficient to produce from it all the living forms we find at the present day."[72] This provides the theoretical grounds for Woodger's examination of the evolution doctrine.

Woodger's discussion of the evolutionary doctrine focuses on the evidence put forth in its support, evidence both direct and indirect. The indirect sources of evidence are provided by comparative anatomy and embryology, the former having revealed the existence of a small number of fundamental ground-plans upon which animals are built and that these types of organization through phylogenetic groupings reveal a simple-to-complex "grading" in organization; the latter presenting detailed pictures of the development of individuals and revealing that in some way ontogeny recapitulates phylogeny which further suggests characteristic "modes" of development. The direct evidence was provided by paleontology though gaps in its fossil record make discussion of transitional forms problematic. Having presented the evidence, Woodger makes the point that regardless how vast an amount

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[72] Ibid., p. 461.

of evidence is put forth to substantiate a process of evolution, a truly explanatory theory of evolution would need also a modus operandi for the evolutionary process. Darwin was the first to do this and thus was able to account for the evidence of comparative anatomy, embryology and paleontology. His explanation was a mechanical one, resting partly on observed fact and partly on hypothesis and inference. The adequacy of the modus operandi, the selection hypothesis, Woodger explains as follows:

> This hypothesis involves all three aspects of the threefold relation of form, function, and environment. Selection is the result of the operation of environment in the widest sense of the term, but of itself it can do nothing. The organism supplies the raw materials for selection to work upon in the shape of variations of either form or function. It is the special merit of Darwin's theory that it took into account all three aspects of the problem, as all theories of the organism must do.[73]

The rest of this chapter is spent bringing Darwin's theory up to date with a discussion of the experimental study of variation and heredity (G. Mendel, T. H. Morgan, and others) though Woodger's interest extends little beyond merely presenting the facts. Woodger's discussion of the doctrine of evolution is interesting because it demonstrates the connection in his thinking between the evidential base of interpretation and interpretive frameworks. Also it shows him trying to understand what is involved in being "theoretical," in deciding

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[73] Ibid., p. 465.

on the proper relationship between observation and interpretation, the judgment of the most adequate interpretive framework, and the role of the factor of internal consistency.

To some extent in the chapters dealing with empirical biology and much more so in the last two chapters, Woodger sees at the base of most problems in science a counterproductive ignorance about the nature and method of science. Thus his placement of the discussion of this topic in an epilogue mistakenly suggests its status in the book as an afterthought or apology. Rather it should be considered as an all-pervading, foundational concern for Woodger, related to the preceding chapters as Aristotle's Metaphysics is to his Physics.

A biologist first and last, Woodger sees the basic nature of science as a style of rational behavior of human organisms acting on their environment. He distinguishes science from other "modes of approaching nature" by its method, for science and method are synonymous. For Woodger "science is analytical and communicable from one person to another through the logical machinery of the normal human mind." By "scientific method" he means "the application of the rules of logic to the investigation of nature" and it "consists in the proper use of inference applied to the observed facts of existence."[74] In this regard observation is seen to be the process by which the raw

[74] Ibid., p. 493.

materials of science pass through the sense organs to the "factories of the mind." The mind's processing involves several kinds of inference, principally analogical, inductive, and deductive. In science these methods of inference are used according to certain rules which have been reached through a process of historical development. From comments on the ancient Greeks with their mathematico-deductive method of inquiry Woodger moves to the 17th century to discuss Francis Bacon's "empirical method" which had as its purpose "to observe and record the ways in which phenomena occurred together and, when the observations were as complete as possible, to endeavor to make inductive inferences of universal truths from them." While for Bacon experimentation was a means only for gaining new information about nature, for Isaac Newton and his "mixed method," "experiment became not merely an avenue for chance discovery but a powerful instrument for testing the truth of deductions and thus for <u>confirming</u> the validity of the inductive principles upon which they were based."[75] The applicability of these methods differs among the sciences, physics profiting from the use of the mixed method while biology and psychology rely on the empirical method. Woodger then gives examples from the history of biology to exemplify the role of method in biological investigation. Of these, his examination of the

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[75] <u>Ibid.</u>, p. 495.

doctrine of natural selection is most revealing of both his critical approach in analyzing biological matters as well as the power of logical analysis for bringing order to vaguely related observations and their interpretation. The following is a lengthy quote, the best developed example of Woodger's early analytical prowess:

> The application of scientific method may be further illustrated by the following analysis of the doctrine of natural selection, a much more difficult and complicated example. The selection hypothesis is based on the observations: 1. That many species have become extinct. 2. That existing forms produce a surplus of offspring over and above what is required to preserve their numbers. 3. That the offspring resemble their parents in a general way. 4. But that they vary about a mean. 5. That this mean is between that of the parents and that of the species. 6. That domesticated species have been changed by artificial selection. The hypothesis is further based on two assumptions: 1. That the factor which determines which of the offspring (cf. 2 above) will survive is 'fitness' or adaptation. 2. That the variations in successive generations may progress indefinitely from the specific parental mean and establish a new mean. Of these assumptions the second is in the nature of an induction. It is the crucial link in the argument and the one to which the experimental test must be applied. We saw in Chapter XVI [The Interpretation of Biology] some attempts at such experimental verification in the researches of Johannsen on beans. It will be recalled that this observer was unable to show any progressive shifting of the mean of variations, but that these results cannot be taken as conclusive because the experiments, being made under greatly simplified conditions, do not admit of conclusions of a comprehensive nature being drawn from them. The first assumption is almost axiomatic. Extinction, the opposite of survival, is due to external or internal causes. External or environmental causes must be assumed in the long run to affect both the 'fit' and 'unfit' equally. But the internal factor, based on observation 4 above, will determine a higher percentage survival in some members of the species than others; and since the total number of the population in a given region is limited (observation 2), the proportion of those members possessing favourable

> variations will increase until the ancestral type is extinguished. To say that the fittest survive is to state a self-evident truth, for survival is the only criterion we have of fitness.
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> The great interest and importance of the selection hypothesis lies, not in any proof or demonstration it gives to evolution, but in the fact that it gave form and direction to biological thought. From being mere vague belief, lying outside the range of scientific method, the theory of evolution was defined in such a way that its implications could be worked out and subjected to experiment, and it thus became a directive influence in research. This example illustrates, therefore, the immense power of a great inductive generalization.[76]

Having presented his case for the importance of familiarity with the scientific method, Woodger proceeds to discuss the limitations and working assumptions of its use. With basically a pragmatic argument Woodger portrays science as working its way progressively to an understanding of its own perceptual foundations but at its present stage it must accept this "unanalyzed ground at the bottom," supporting the super-structure of scientific activity. He then makes what seems to be a basic plea for a separate 'philosophy of biology' which is of central significance and I quote the passages in full:

> In the biological sciences especially the analytical nature of scientific method must always be borne in mind, for organisms possess a wholeness which is lost in analysis, and it cannot be assumed that we can re-create the whole by synthesizing the parts. For the solution of its immediate problems science must neglect much that does not appear to be relevant, but in imposing restrictions upon itself it must not forget them and draw conclusions embracing the whole universe when its self-imposed limitations have confined it to a part. Such warnings would hardly seem necessary were it not that we are still under the influence of the unjustifiable over-confidence of the second-half of the last century: a period

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[76] Ibid., p. 498-499.

> when the slow developments of scientific method of preceding ages enabled undreamt-of progress to be made in the physical sciences; when the warnings of the critical philosophy went unheeded; when mechanical explanations were rampant and were applied by analogy from things to which they were legitimately applicable to those to which they were most manifestly only applicable by a neglect of the most obvious truth; and when, in short, science was exalted to a position to which its humble origin and its limited method do not entitle it.
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> The first two decades of the present century have not witnessed the fulfillment of the extravagant hopes of the last. The fundamental concepts of physical science upon which they rested have themselves undergone extraordinary changes and are still in a state of such flux that it is difficult for those outside the immediate sphere of physical research to follow them. The phenomena of life and of mind, which were supposed to be soon reducible to those physical concepts, still retain their secrets inviolate, and we are in possession of no facts which would justify any such belief in their impending solution as was indulged in by our immediate predecessors.[77]

Woodger finishes this epilogue with a discussion of the lessons which those "immediate predecessors," and the history of science in general, can teach scientists about the workings of science.[78]

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[77] Ibid., p. 500-501.

[78] If Woodger's epilogue is indeed his nascent attempt at philosophy of biology, how does it fare as philosophy? As an order of discussion it secures itself in the history of philosophy and science, discussing the subject of scientific method from that basis and relying heavily on current interests in the philosophy of science of his day (see footnote 56, p. 25). His discussion expresses principally the need for biology to be a fully autonomous enterprise, in its philosophical as well as scientific aspects. While Woodger criticizes destructively he offers no hint of what might replace the standing theoretical and philosophical framework of biology. It should be remembered that Elementary Morphology and Physiology was a biological textbook, not a philosophical treatise. Woodger had here neither a platform to suit him nor the philosophical insight for a constructive philosophical contribution.

The reception of Elementary Morphology and Physiology was very positive. The review in Lancet praised it for its originality of content and presentation as well as for the quality of its illustrations.[79] It was to be highly recommended as long as it was supplemented with other more mundane texts to "fill out" the students' grounding in biology. Its review in Science Progress was very positive, again complementing it on its originality and fine line drawings.[80] Nature, however, did not review the book though it regularly reviewed both medical books and elementary biology texts.

In concluding this section I will return to the initial problem of interpreting Woodger's "task" before looking at the whole of the book as a development from Woodger's earlier experience in empirical biology and as background for his Biological Principles.

It will be recalled that Woodger's task in Elementary Morphology and Physiology was to frame a balanced picture of the interrelated empirical, theoretical, and philosophical aspects of biology even while they were not yet clearly separate in his understanding of them and certainly not separate in his understanding of the biological enterprise. The difference between a discussion in and a discussion on a particular

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[79] Anonymous, review of Elementary Morphology and Physiology for Medical Students, by J. H. Woodger, in Lancet 208(1925):978.

[80] Anonymous ["E. A. F. "] , review of Elementary Morphology and Physiology for Medical Students, by J. H. Woodger, in Science Progress 19(1925):704-705.

aspect of biology needs to be recognized since they are discussions of different orders. Thus, for example, much of Woodger's chapter on interpretation in biology (Ch XVI) was a discussion in (and thus a contribution to) the discursive order[81] of theoretical biology. His critical examination of the doctrine of natural selection in this chapter and in the epilogue, however, was a discussion on theoretical biology and thus a discussion in (and contribution to) the discursive order of the philosophy of biology. Furthermore while these two types of discussion are of different orders, the "higher" order encompasses not just a description of the "lower" order but also deals in a broader scope with the relation between that lower order and its subject matter (e.g. what criteria are best for judging the adequacy of one interpretation of a set of data over another?). Thus the discursive order of philosophical biology discusses the theoretical domain and the empirical domain, attempting to solve problems in the interpretation of observations arising from questions of methodology. This is exemplified especially in the chapter on animal behavior (Ch XVII) and also in some sense constitutes the order of concern of the book as a whole.

Concerning the effects of Woodger's collaboration with J. Brönte Gatenby it was answered that he was certainly presented with the orientation of a critical empirical biologist and these aspects, pervaded

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[81] The terms 'order of discussion' and 'discursive order' are synonymous and I will use them interchangeably as style recommends.

by a critical spirit, were seen to be at work in Woodger's own research efforts. And their role in Elementary Morphology and Physiology is readily apparent. The book is pervaded with a concern for scientific methodology, a concern in its more technical aspects of high premium for cytologists searching for fundamental intracellular structures. Woodger's initial and ever-present distinction between observation and interpretation follows from this concern for method and is the basis for which he considers the problem of explanation (Ch XVI) and the kinds of inference involved in the interpretation of empirical evidence (Epilogue). His presentation of the empirical aspect of biology (Ch I-XV) is centered on a major morphological "interpretation," the existence of typical "plans" of animal organization which can be ordered according to phylogenic considerations as "grades" of biological organization--from cell to organism. Thus organization, which was the watchword of cytologists, had become in Woodger's "task" a handle on the diversity of the contents of empirical biology against the changing background of the evolution of life.

With these last concerns Woodger truly leaves the role of concerned empirical biologist and situates himself in a more abstract order of discussion, a discussion on the interpretation of empirical evidence and thus a discussion in theoretical biology made from the discursive order of the philosophy of biology. He contributes to the theoretical order when he postulates what he considers to be a minimally-adequate interpretive framework (form, function, and environment) and uses it to discuss the adequacy of the doctrine of natural selection. In his epilogue he developed both his understanding of the empirical and theoretical aspects of biological science. Beginning with the use of conceptions of scientific method taken from the philosophy of science of his day, he moved to identify any unique uses of that method in biology and expressed doubts about its wholesale utility there. In this transition Woodger bridged the gap between philosophy and science to delimit the order of discussion required of a "philosophy of biology." It is from this discursive order that Woodger speaks when he examines the logic of the doctrine of natural selection.

Given these developments in Woodger's thinking about the biological enterprise, Elementary Morphology and Physiology may be seen as a nascent exercise in the discussion of biology from three different orders, three different though interrelated discursive orders. This book stands then as the "space" where Woodger localized all his major concerns about the biological enterprise and as such it constitutes the backbone of his critical study, Biological Principles.

Woodger: Biologist in Transition

A Term Leave: Vienna and Hans Przibram

Not all of the influences on Woodger's thinking came from biology, for he was aware and becoming actively concerned with the professional philosophy of his day. By understanding his choices in philosophers and philosophies we can better understand both his own orientation as a philosopher of biology and the role of philosophy in his continuing task of framing a balanced picture of the biological enterprise.

Following the publication of Elementary Morphology and Physiology Woodger continued in his teaching and research efforts. In the spring of 1926 he was given a term leave to study under Hans Przibram (1874- ?) in Vienna. Upon his arrival at Przibram's vivarium[82] it was decided that he would work on transplantation in annelids. But it was early spring; and as the ground was still frozen the proper specie of worm could not be collected and Woodger found himself with a great deal of free time. He spent this time taking part in Przibram's intra-departmental discussions on the methodological problems raised by the new experimental biology. To understand what influence these discussions might have had on Woodger, three factors need to be considered. First of all, by far the most exciting biology being done at this time was the experimental embryology (Entwicklungsmechanik) begun by Wilhelm Roux (1850-1924) where the major problem areas

[82] Przibram was at this time Professor of Experimental Zoology at the University of Vienna; Director of the Experimental Biology Station; and the Administrator of the "Vivarium," a laboratory where animals are maintained in environments simulating their natural ones (footnote 1, p. 1).

were transplantation and regeneration.[83] Second, Hans Przibram was educated and worked within this new tradition, having published on these problems since 1900.[84] Third, among the more philosophically-oriented German-speaking biologists there was concern about the heterogeneous nature of biological knowledge.[85] It was felt that a critical sorting of fundamental concepts was needed to promote a strictness of thought equal to the strictness of investigation required in the new biology.

Przibram was no stranger to these discussions against "Begriffs-romantik" in biology, in fact his major work was clearly a contribution to it.[86] His Experimental-Zoologie (seven volumes, 1906-1930)

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[83]For a discussion of this period of Roux's Entwicklungsmechanik, see F. B. Churchill, "From Machine Theory to Entelechy: Two Studies in Developmental Teleology," Journal of the History of Biology 2(1969):165-185.

[84]For bibliographic information on Przibram see Enciclopedia Universal Ilustrada, 1922 (Europeo-Americana), s.v. "Przibram, Juan." Later than 1922, see his periodic contributions (under the title "Experimental Studies on Regeneration") in Wilhelm Roux's Archiv fur Entwicklungsmechanik Der Organismen (Berlin: Julius Springer, 1894-present).

[85]Julius Schaxel (1887- ?), a marine zoologist and theoretical biologist at the University of Jena; defined the major problems of biological knowledge that came to be known as the movement against 'Bigriffs-romantik' in his Grunzuge der Theorienbildung in der Biologie (Jena, 1919) and then provided a platform for their discussion in a series entitled Abhandlungen zur Theoretischen Biologie (Berlin, 1919-1930). (See Enciclopedia Universal Ilustrada, 1922 (Europeo-Americana), s.v. "Schaxel, Julio.")

[86]e.g. Przibram cites Schaxel in his 1925 article (see p. 1, footnote 1 above).

attempted to present both a comprehensive treatise on the major aspects of experimental zoology as well as an openly speculative discussion of its theoretical foundations and methodology.[87] Reviews of these volumes in Nature commented repeatedly on the broadness of their treatments of their subject matter, discussing "large and difficult problems" in an "objective and undogmatic" way.[88] Thus Przibram as a biologist was a comprehensivist who recognized concerns of supra-empirical order involved in many biological problems. The influence Przibram might have had on Woodger can be easily guessed. Here was an eminent empirical biologist who was perceptive enough to see the need for other orders of discussion but yet in working through those concerns was undogmatic and logical. Woodger at once realized

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[87] Przibram's Experimental-Zoologie was published in seven volumes from 1906 to 1930 by Franz Deutike in Leipzig and Vienna. Only the first volume was translated into English and only the first, second, fourth and sixth were reviewed by Nature. The volumes are Embryogenese (developmental mechanics), vol. 1(1907); Regeneration, vol. 2(1909); Phylogenese, vol. 3(1913); Vitalitat (theoretical models for interpreting empirical data, e.g. the crystal analogy), vol. 4(1913); Funktion (physiology), vol. 5(19?); Zoonomie (a survey of experiments and theories in development and heredity), vol. 6(1928); and Zootechniken (on the methodology of experimental zoology), vol. 7(1930).

[88] [J. Arthur Thomson], review of Experimental-Zoologie Bd. I Embryogenese, by Hans Przibram, in Nature 77(1908):529; F. H. A. Marshall, review of Experimental-Zoologie Bd. II Regeneration, by Hans Przibram, in Nature 81(1909):61; Anonymous, review of Experimental-Zoologie Bd. IV Vitalitat, by Hans Przibram, in Nature 94(1914):193-195; and E. W. MacBride, review of Experimental-Zoologie Bd. VI Zoonomie by Hans Przibram, in Nature 126(1930):639-643.

that there were fundamental unanalyzed assumptions in the theories in circulation amongst biologists and that his training had not equipped him, nor was it likely to have equipped anyone else, to examine or identify these assumptions. It is likely that in this atmosphere, unlike the empirically-dominated, philosophically-intolerant atmosphere of English biology, Woodger may have overcome any self-doubts as to the propriety and utility of openly importing the philosophical spirit and methods into that biological enterprise with which he was finding increasing fault. In any case upon his return to England later that year he began a study of philosophy that would aid him in an analysis of biological theory.

Philosophical Study: Early 20th Century English Philosophy

English philosophy at the turn of this century was dominated by a neo-Hegelian philosophy of absolute idealism.[89] Foremost among the idealists was F. H. Bradley (1846-1924) who was attempting to refute the extreme sensationalist philosophy of J. S. Mill through the development of an idealistic metaphysics. Bradley, like Hegel, promoted the view that reality is spiritual "through and through" and in the face of this if we try to think through the implications of any fact,

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[89] T. Robischon, "New Realism," Encyclopedia of Philosophy (1965), vol. 5, p. 185-489.

such as a fact of natural science, we are ultimately forced to conclude that a single, immediate unity of consciousness is "behind it all." The fundamental doctrine underlying the whole structure of Bradley's philosophy is his doctrine of internal relations which holds that everything in the world is constituted by its relations with other things. In the face of this theory reality must be an inseparable, interrelated whole where analysis can only be an act of falsification.[90] For the younger philosophers, more sympathetic to natural science and mathematics, Bradley's philosophy was unbearably confining. The death blow to the doctrine of internal relations was dealt in 1903 by two Cambridge philosophers, Bertrand Russell (1872-1968) and G. E. Moore (1873-1958); in the process a new realist school of philosophy was initiated and eventually developed a comprehensive philosophical platform.[91]

This "New Realism" as it came to be known was based in a faith in the common sense of "native realism." While philosophers realized that a complete refutation of idealism was not possible, they strove to free philosophical discussion from the confines of absolute idealism. Thus while the New Realism was based in common sense a

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[90] Anthony Quinton, "British Philosophy," Encyclopedia of Philosophy (1965), vol. 1, p. 386-396. Also useful in this section, G. T. Warnock, English Philosophy Since 1900, (London: Oxford University Press, 1958).

[91] Warnock, English Philosophy, p. 1-11.

strong emphasis was placed on explicating the logic of natural language in which the individual, immediate (and in this sense "real") experiences were articulated.[92] Moore's "Refutation of Idealism" was a painstaking analysis of the idealist philosophy in which he revealed subtle but logically-contradictory ambiguities in the concepts and reasoning of the idealists.[93] While Moore was resisting the limits placed on common sense reality by absolute idealism, it was his analytical method that was seen to be important and powerful. Russell in his Principles of Mathematics brought the idealist doctrine of internal relations under its heaviest criticism for Russell realized that only if that doctrine were false could mathematics be possible.[94] In its place Russell argued for a theory of external relations according to which relations have a reality beyond the terms (or things) they relate and are not essential in the definition of those terms. This led Russell to a "logical atomism" whereby the experience of "things" was replaced by immediate "facts" of experience, complex facts being analyzable into simpler facts. The communication of these facts in language Russell termed "atomic propositions" while conjunctions of these constituted "molecular

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[92] Ibid., p. 12-42.

[93] G. E. Moore, "The Refutation of Idealism," Mind 12(1903), 433-453.

[94] B. Russell, The Principles of Mathematics (Cambridge: Cambridge University Press, 1903).

propositions," propositions no longer of immediate experience. It can be seen that in his logical atomism Russell was positing both an epistemology and a logic of language.[95]

The doctrines of internal and external relations were central to the philosophy of the early 20th century.[96] While the position of these doctrines is basic to any philosophy, their status is difficult to assess. On the surface they seem to involve the way things are known, i.e. the knowing of "properties"; they are used in a logico-mathematical way by Russell (e.g. the logical concept of "identity") and in either case reflect certain metaphysical assumptions such as that reality is either atomistic or holistic. The four basic concepts involved are "property," "relation," "internal" and "external." Common sense tells us that if some properties are taken away from a thing it would not be the same thing it was, though this would not be said for all its properties. When considering a multiplicity of things the question of "relation" is raised. "Relational properties" are those properties that involve the reference of one thing to another or to others. If this relational reference is essentially involved in the constitution of a thing, then the relation is said to be "internal"; if the reference is "accidental" (non-essential) the relation is said to be

[95] John Passmore, A Hundred Years of Philosophy (2nd ed., London: Duckworth, 1972), p. 210-220.

[96] R. M. Rorty, "Relations, Internal and External," Encyclopedia of Philosophy (1965), vol 7, p. 125-132.

"external." Conflict arises when the extent of this referring is assessed: If all of a thing's essential properties are relational, then all relations are internal, only the doctrine of internal relations is true and analysis is logically impossible. If none of the constitutive properties of a thing are relational, then no relations are internal. This latter doctrine of external relations, in the hands of Russell, shifted from being an idealist's question of the possibility of knowledge to the realist's analysis of naming and predicating--more amenable to the powerful logical technique he and A. N. Whitehead (1861-1947) were developing. Woodger assimilated these emphases through his reading of Russell 's early writings, but as Russell's views moved more towards open phenomenalism Woodger's sympathy waned. Two other Cambridge philosophers, Alfred North Whitehead and C. D. Broad (1887-1958), captured Woodger's attention.

By far the most important philosopher for Woodger was A. N. Whitehead.[97] Whitehead's multifaceted career involved important contributions to mathematics, logic, philosophy of science and metaphysics. Woodger steeped himself in Whitehead's developing thought, absorbing it and developing its implications for biology. Whitehead began his career as a mathematician at Cambridge interested in theoretical physics. In a memoir to the Royal Society entitled "On

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[97]Discussion on Whitehead based principally on: Dorothy M. Emmet, "Whitehead, Alfred North," Encyclopedia of Philsophy (1965), vol 8, p. 290-296.

Mathematical Concepts of the Material World" Whitehead articulated his first statement of disapproval with physical science and its philosophy of nature.[98] In this paper he explicated the logical structure of the Newtonian world view and attempted to frame new concepts of "other worlds." His general interest at this time was in the abstract structures of science and their relation to the physical world, i.e. the world of immediate experience. His general method of thinking was that of extensive generalization. Whitehead soon came to the conclusion that scientists work with an inadequate logical apparatus, preferring to reduce all multirelational situations to dyadic ones for the sake of mental manipulation and thus arriving at grossly oversimplified concepts of the world.[99] Whitehead's unrest was held in abeyance while he worked with Russell on Principa Mathematica (three volumes, 1910-13). Then, instead of writing an intended fourth volume on the logical foundations of geometry he abandoned this project and began his philosophical writing. His initial concern was again the adequacy of the "neat, tidy exact world of science" in the task of articulating the world of "fragmentary individual experiences"

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[98] A. N. Whitehead, "On Mathematical Concepts of the Material World," Philosophical Transactions of the Royal Society of London, Series A, 205(1906), 465-525.

[99] W. Mays, "The Relevance of 'On Mathematical Concepts of the Material World'," in Ivor Le Clerc, ed., The Relevance of Whitehead (London: George Allen and Unwin, 1961), p. 255-262.

and after finding the Russellian scheme inadequate, broke away from the British tradition altogether. His major writings of this period and those writings through which Woodger first became acquainted with Whitehead's thought were An Enquiry into the Principles of Natural Knowledge (1919) in which Whitehead presents a new philosophy of physical science to meet the demands of the revolution going on in physics; The Concept of Nature (1920), a more general discussion of the principles which unify scientific knowledge; and Principles of Relativity (1922) in which Whitehead shows that Einstein's theory of relativity is deducible from his (Whitehead's) own natural philosophy.

While Whitehead in these works develops the first concepts that Woodger saw applicable to biology, he also develops the doctrine of internal relations arguing that natural knowledge is exclusively concerned with "relatedness." While rejecting Russell's view of perception and the exclusivity of external relatedness, Whitehead qualified his belief in fundamental internal relatedness by reasoning that perception is always from within nature and always of the systems of events that make up nature. Thus while we never experience the whole of nature simultaneously, our experience is nevertheless of events which constitute nature. And Whitehead's theory of perception is a biological one, the perceivor being a natural organism reacting to the world around him. Herein lies the germ of his "philosophy of organism" and perhaps a justification for biological interest in his philosophy. Whitehead, like Russell, wanted to save mathematical concepts from the label of intellectual fiction. In this regard he developed a "method of extensive abstraction" by which the commonly known geometrical terms are derived from immediate experience (e.g., "instants" are defined as a class of sets of durations with certain special extensive relations to one another).

Whitehead's multifaceted critique of modern science and his alternative philosophy of nature found their first full expression in his Science and the Modern World (1926). Two things are accomplished in this book.[100] First, Whitehead presents his interpretation of the history of science and traces its lines of reasoning and "intellectual habits." His major criticism in this history is the "bifurcation of nature" which has come about through the Galileo-Locke distinction between a world of immediate experience (e.g. colors, sounds, scents) and the world of "scientific entities" (e.g. mass , quanta , electrons); his basic corrective demand is that science must correlate whatever is known *without* making any references to the fact that it *is* known.

Secondly, in this book Whitehead is constructing in piecemeal fashion his basically seamless "philosophy of organism," a general view of the unities of nature which function with non-instantaneous

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[100] R. Palter, "Science and its History in the Philosophy of Whitehead," in W. Reese, ed., Process and Divinity (LaSalle: Open Court, 1964), p. 51-78.

spatio-temporal extension or in short a philosophy of process. Whitehead's major work in philosophy, Process and Reality: An Essay in Cosmology (1929), develops a scheme of the most general features of experience through to a most encompassing description, beginning with a theory of perception and ending with a discussion of God and His relationship with the universe. One of this essay's strongest claims is that it is coherent and self-consistent.[101]

C. D. Broad,[102] although he never developed his own "system" of philosophy, did become a prominent philosopher by explaining and developing the philosophical discussions of others with a clarity and succinctness unequaled in his day. Broad's writings, devoted to problems of mutual concern to science and philosophy (e.g. scientific concepts and their perceptual bases, phenomenalism, mechanistic explanation, induction and probability), reflect two outstanding characteristics which Woodger came to prize highly. First, Broad's self-concept as a philosopher was as a "clarifier" rather than a contributor and his intensive conceptual analyses manifest an equal emphasis on the critical and speculative functions of philosophy in the service of biology. In his later writings he supplemented his conceptual analyses with the rigorous logical techniques of Russell and Whitehead. Secondly, all of Broad's writings reveal the thinking of a highly

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[101] Emmet, "Whitehead," p. 294.

[102] Passmore, A Hundred Years of Philosophy, p. 347-350.

spirited philosopher behind them, and this critical spirit became second nature to Woodger.

What philosophical orientation might Woodger have gotten from Russell, Whitehead and Broad for he certainly studied them all. From Russell perhaps he perceived a general emphasis on logic and language as the realist base for understanding an epistemologically-neutral world where external relations prevailed; from Broad, a critical spirit and an appreciation for the clarifying power of conceptual analysis. From Whitehead, perhaps he obtained one critique of modern scientific thought, a view of internal relations in tension with Russell's views, and an alternative philosophical foundation (based on process) better suited to the needs of a biological science where mechanism, vitalism, and materialism were providing inadquate logical and conceptual apparatuses and methodologies. This study, conclusions of which surfaced periodically in his reviews of current biological writings,[103] constituted the philosophical spadwork for his forthcoming study of the biological enterprise.

While Woodger's essay reviews are of little help in telling us what other authors he was reading, they do reflect an almost brazen spirit in his criticism of philosophical-naivety of the average working biologist. It is obvious too where his sympathies lie. On the one

[103] For a complete list of the essay reviews of this period in Science Progress, see the bibliography below.

hand, in reviewing The Study of Living Things (1924) by E. S. Russell, Woodger underscored Russell's critique of the analytical method in biology with one of his own on the disciplinary level, giving justification for independent sub-disciplines in biology with freedom of choice in methodological questions. On the other hand in reviewing Living Organisms (1924) by E. S. Goodrich (1868-1946), Woodger elevates the inconsistencies in the argument of a single chapter to question the coherence of the work as a whole. Furthermore, Woodger had little patience with pretenders to the title of "philosopher of biology." In his review of Logic and Law in Biology (1972) by P. Chalmer Mitchell (1864-1945), he carried out a sustained polemic against the use of "philosophical canons of scientific method" as the ultimate judge of the adequacy and consistency of empirical biology. Also in this review, entitled "Science and Metaphysics in Biology," Woodger employs the arsenal of weapons he had borrowed from the concurrent "new realist" philosophy.

Mitchell's use of T. H. Huxley's "canons" represented to Woodger the right idea but the wrong approach. In general Woodger saw most scientists as philosophically ignorant and if at all knowledgeable, then most likely it was of the old idealism and not the new realism or process philosophy. Thus Mitchell was systematically applying an outdated mode of interpretation, one steeped in a materialism which the new physics had long since abandoned.

Woodger saw Mitchell, like most scientists, promoting a brand of native realism "scientific common sense," that was basically fiction and largely responsible for the current problems in the theories of evolution and abiogenesis. This naive view constituted, even with its opposing viewpoint, only two of a large number of possible modes of thought. Biology, instead of digging up outdated maxims or bickering over which of two views is best, needs to develop its own mode of thinking about its unique objects of study. General ignorance too about the nature of human knowing are responsible for the persistent adherence to absolutes and mutually exclusive interpretations, both regressions to the old idealism.

Mitchell himself, pretender to the title of "philosopher of biology," becomes self-contradictory at the hands of Woodger's analytic prowess. Because Mitchell had not made his own philosophical assumptions explicit nor explored their implications, he could not for long remain consistent--abusing the very "canons" he was upholding. In Woodger's view, philosophical schemes which are not thusly "self-consistent" write their own death sentence; schemes such as Mitchell's that pretend philosophical neutrality by claiming a phenomenalist stance are all subject to the fallacy of reductio ad absurdum and reasoning by infinite regress. Woodger sums his final reaction to Mitchell's viewpoint with a discussion which explicates the wide scope of concerns looming beneath the surface of Mitchell's own discussion.

> Dr. Mitchell is not satisfied that materialism has furnished us with a fruitful methodology; he wants to believe that the whole world consists of, and can be exhaustively interpreted as, a collection of little bits of stuff pushing each other about--a notion derived, as Professor Bridgman says, from everyday experience. It is one to be used intelligently but not to be adhered to with all the tenacity of original sin. Every such notion has its "field," but there are many fields, even in what is called nature, and to suppose that we already possess one mode of thinking which covers them all, and to scorn all contrary opinions, seems to be the height of "anthropomorphic vanity." When we consider how brief has been the history of human thought, and how much briefer still the life of natural science; when we remember that the latter is only one part of the whole field of intellectual inquiry; when, further, we reflect that our experience has been limited to only one part of the vast scale of magnitudes, and only one part of the universe during a brief period of its existence; when we recall, too, that during that period our thought has been dominated by but a few fundamental notions derived, for the most part, from the still more limited field of daily life; then, it seems to be the height of folly to suppose that any one of our constructive schemes, however comprehensive and carefully elaborated, or however well rooted in our experience, can be even approximately true and exhaustive of what we know, still less that it entitles us to suppose that there are no further surprises in store for us.
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> The most hopeful suggestion seems to be, then, that there are many "fields" and that whilst we are indebted to a few men of genius for finding the right concepts for some of them, it would be better in biology not to persist in stretching our old ideas beyond their original fields until they become so thin as barely to conceal the ignorance they cover. It is rarely a question: can a given concept be used in a particular field, but, are we justified in asserting it to be the <u>only</u> one that can be profitably? Thus if we change our attitude towards scientific concepts, we see that there is need, not for competition among them, but cooperation, room for mutual supplementation but not for mutual exclusion.[104]

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[104] J. H. Woodger, review of <u>Logic and Law in Biology</u>, by P. Chalmer Mitchell, in <u>Science Progress</u> 22(1927):318-319.

These paragraphs sum up a period of transition in Woodger's thinking. Woodger learned that the problems he saw in the biological enterprise were deep-seated historical ones common to science in general; that his early concern with "scientific method" had a philosophical counterpart in questions about the nature of knowledge (epistemology) and that this latter concern might clarify the nature of the former; and that as pernicious as theoretical order conflicts seemed to be, they were merely the result of bad intellectual habits. Any ultimate fatalism could be dispelled with renewed effort to think more creatively than ever before. This last point should be remembered, for even when Woodger is being his most heartless critic, he ultimately is striving for a conceptual freedom beyond the confines of scientific convention.

Woodger: The Philosopher of Biology

A. N. Whitehead--Revisited

Woodger owed a profound debt to the thought of A. N. Whitehead. In fact it seems that it was the reading of that philosopher's developing critique of science and philosophy of nature that allowed Woodger finally to make the leap from the concerns of his first to those of his second book: the change from non-essential discussions on theoretical and philosophical problems in empirical biology to the recognition of necessary, justified and interdependent orders of discussion "higher" than that of the empirical. Elementary Morphology and Physiology was previously described as a nascent exercise in the discussion of biology from three different orders and nascent because in that book Woodger used the established categories of 'empirical biology', 'biological theory' and 'philosophy of science' to identify those orders. Through his reading of Whitehead he was able to see these orders of discussion as discourse about organisms or the study of organisms in varying degrees of abstraction. Moreover, with Whitehead's philosophy of nature and its founding theory of perception Woodger could begin to consider the unique epistemological nature of the subject matter of biology and begin to discover "modes of abstraction" beyond those employed in the physical sciences for its adequate characterization.

Woodger's task was therefore both critical and revisionary. If the products of biological observation are in some sense a function of the mode of abstraction employed, then the discussion of the interpretation of a particular set of data should be separate from a discussion of the adequacy of the various possible interpretations of that data--with respect to the rest of its empirical domain and to biology as a whole. That Whitehead's view of nature and knowledge required Woodger to deal with biology in this way is a basic assertion of my discussion and one which I will develop in my discussion of Biological Principles. One obvious implication of this assertion is that in order to understand Woodger's second book, one must understand the basic ideas of Whitehead's philosophy.[105]

In previous discussion of Whitehead it was pointed out that his general interest was in the abstract, theoretical structures used by science to explain the physical world, albeit as a grossly simplified version of the world of immediate experience.[106] In his philosophical writings Whitehead desired to rectify the situation, to develop constructs that truly derived from immediate experience. Since Whitehead believed science must correlate whatever is known without making reference to it as known, it was also necessary to raise the issue of the futility of distinguishing between appearance and reality or for that matter between primary and secondary qualities. Contrary to the Humean analysis of the world of immediate experience as atomistic

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[105] Woodger devotes some great deal of space in Biological Principles presenting and explaining Whitehead's complex ideas and showing the revisions they begin to require of the study of living organisms. Even so, at least one important commentary on the book has ignored Whitehead altogether and has, I think, distorted Woodger's intention and accomplishments in the book. Morton Beckner in The Biological Way of Thought (New York: Columbia University Press, 1959) interprets Woodger's book wholly as a plea for "biological explanations." Through an analysis of biological explanation via Hempel's logical model of explanation, Beckner is able to "correct" Woodger's presentation and show the "truth" of the matter--that "biological explanation" does not differ essentially from physical explanation. I believe Beckner's focus on the logic of explanation has blinded him to deeper-seated problems with which Woodger was concerned--e.g. the epistemic foundations of biological knowledge, and the communal language used for different orders of biological concern.

[106] See p. 56 above.

impressions or sensations and also contrary to Russell's insistence on the external relatedness of all nature, Whitehead believed that our primary experience is of "something going on" with spatio-temporal "spread." This he called the "passage of nature" which is constituted of what he calls "events"--spatio-temporal happenings that extend over one another. Thus the building of a house is a slice in the passage of nature, an event extending over the raising of its sides and converg- ing by an abstractive "route of approximation" to the event which is here and now: the hammering of <u>this</u> nail. The events themselves display recurrent patterns called "objects" which stand as persisting potentials for actualization. An important distinction among such objects is their uniformity or non-uniformity, where a uniform object is one located in an event throughout a duration of time and thus characterizes any slice of that duration (e.g. a bar of iron is still a bar of iron no matter how short the perceptual duration) while the non-uniform kind of object requires a minimal span of time in order to manifest itself at all (e.g. a molecule, which cannot exist in lesser time than that required by the periodicity of its atomic constituents). Woodger saw this distinction as basic for differentiating organic from inorganic events.[107]

With Whitehead's emphasis on the 'passage of nature' and his characterization of primary experience, three major fields of study

[107] Emmet, "Whitehead," p. 292.

were opened for scientific investigation: 1) a study of the diversification of nature into events which in their continuous encompassment provide for the fundamental character of passage in nature; 2) a study of the diversification of nature into objects whose selective actualization of events gives natural objects their persistent qualities and thus their identity; and 3) an examination of the external relationships of events--as, for example, how they come to include each other--in order to derive from their experience in perception the abstract concepts we use to conceptualize them and their relationships.[108] For Whitehead science should be concerned as much with the permanence of nature as with its passage and because these are inextricably linked they are the root problems of scientific understanding. How after all can the characters of events endure while the events themselves pass?[109]

While for Whitehead the question of a reality unknowable or hidden behind appearance is meaningless, he does recognize that perceptual experience abstracts from the web of events certain persisting or recurring features and constitutes 'physical objects' from them. This is an unavoidable fact of perception, but Whitehead says that we must "seek simplicity but distrust it": Science abstracts from

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[108] N. Lawrence, Whitehead's Philosophical Development (Berkeley: University of California Press, 1956), p. 127-139.

[109] Ibid., p. 130-132.

the fundamental passage of nature certain features with which it constitutes an enduring picture, a picture composed of 'things' of differing degrees of 'thinghood'. Science becomes dogma when it takes one particular "mode" of abstraction as being the true one while in fact there are a multiplicity of possible modes of abstraction. The standing modes have become dogmatic habits of thought. They abstract principally spatial or temporal aspects and arrive at conceptualizations that are functions of these modes.[110] For Woodger this raised the question of the adequacy of the standing modes of abstraction for dealing with the study of biological "objects" in their non-uniformity and differentiating development; this became a pervasive concern in Biological Principles.

2. Second Book: Biological Principles: A Critical Study

If there were a formula to state briefly the basis of Woodger's thinking in Biological Principles[111] it would be that the most encompassing concern, "critical biology," is essentially the investigation of the epistemological foundations of biological knowledge, and this latter concern is itself essentially the conceptual analysis of fundamental biological notions. For Woodger, taking Whitehead's philosophy quite

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[110] Ibid., p. 133-140.

[111] J. H. Woodger, Biological Principles: A Critical Study (London: Routledge and Kegan Paul, 1929).

seriously, these are the preliminary tasks required for putting biological science on adequate epistemological foundations. Interspersed in these tasks are his revisionary efforts, for example alternative concepts of 'organism', 'development', and 'organization'--required by his rejection of the mechanistic-materialistic view of nature and his conversion to Whitehead's philosophy of organism. The tasks of critical biology correspond to the dual character of biology: First, biology as a cognitive activity toward nature is an epistemological concern which is the subject of Part I (The Data of Natural Science and The Principles of Systematization of Scientific Knowledge) and secondly, biology as a body of natural knowledge is the concern of conceptual analysis which constitutes Part II (The Problems of Biological Knowledge). Woodger develops this dicotomous concern in his General Introduction as the standpoint from which the longstanding "biological antitheses" can most adequately be articulated and resolved.

According to Woodger biology has gone the way of all developing sciences, sub-dividing into special branches characterized by a divergence of methods and outlooks.[112] Unlike the other (i.e. physical) sciences, however, biology has no unifying generalizations to knit the findings of its branches into a single whole. Biology is thus cleaved by fundamental arguments or "antitheses" preventing the

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[112]The following is a discussion of the "General Introduction," Ibid., p. 11-24.

harmonization of its general theoretical results. A true resolution of these antitheses cannot issue from the viewpoint of any one special branch but must in some sense be communal. Woodger's solution is the generality of his inquiry: What is it that biologists do ? What is biology? Biology is a science that investigates animals and plants. If biology is a science, what is meant by "science"? That term is usually the name given to a certain activity towards nature or sometimes to a particular "theory" about nature, especially naturalism. But this definition says nothing of the sense of science as a body of knowledge. Then perhaps a more adequate definition would be that science is a "systematized body of propositions about some subject matter or other, enshrined in books or in the minds of individual men."[113] This line of reasoning leads Woodger to consider that the biological antitheses may be problems not in empirical biology but in biology _as_ knowledge.

> Now the possibility suggests itself that the roots of those biological antitheses do not lie wholly in the nature of the _organism_ and are not, therefore, to be studied as part of the subject-matter of biology but that they arise partly out of the nature of _biology_ itself as knowledge. This further suggests that by the study of biological thought from this point of view we might throw some light upon the nature of biological controversies.[114]

Biologists thus need to understand the nature of biology as knowledge which itself requires an understanding of the nature of

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[113] _Ibid._, p. 13.

[114] _Ibid._, p. 14.

knowledge in general--subjects which Woodger discusses in Part II and Part I respectively. In this introductory discussion Woodger has focused on the nature of biology as a science and as a body of scientific knowledge. He was thus applying in an initial way the analytic blade and showing biology to be a set of propositions and an activity toward nature involving a particular epistemological base--concerns more Russellian than Whiteheadian at this point.

Woodger's treatment of the nature of knowledge in general and the character of scientific or "natural" knowledge specifically has four parts. Beginning with a critique of the standing theory of knowledge at work in science--"phenomenalism,"[115] he frames an alternative theory in which the special problems of scientific knowledge are comprehended and then analyzes the various scientific uses of the notions of 'substance' and 'causation' from this revised epistemological standpoint. He concludes Part I with a discussion of the less explicit principles of systematization at work in science, "demands," "postulates" and "subjective factors" which to a great extent are prejudicial canons of habits of thought.

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[115] "Phenomenalism" refers in this period to the pre-"direct realism" theory of knowledge which held that all that can be known are the "images" or "phenomena" of things. This particular version was the one reacted to in the late 19th century and that prompted the new realism of the early 20th century, W. Tatarkiewicz, Twentieth Century Philosophy: 1900-1950 (Belmont: Wadsworth Publishing, 1973).

Why a critique of phenomenalism?[116] Woodger devoted attention to it because it violated beliefs he held concerning the relationship between science and philosophy and the "lessons" scientists should have learned from the history of science. Some aspects of the doctrine of phenomenalism date back at least as far as the 17th century bifurcation of nature into primary and secondary qualities. In the late 19th century as the Newtonian world view was coming under increasing anti-materialistic criticism (e.g. the question of the adequacy of perceptual concepts at infraperceptual levels of reality), phenomenalism rose to replace the untenable materialistic theory of knowledge in science invalidating the imaginative role of science and retaining calculation as the primary task of science. The goal of science should be the mathematical articulation of the formal aspects of the world, putting forth elegant but simple and economic conceptual schemes to subsume the phenomena given in perception. Thus some phenomenalists believed that all that is "real" is what is given in perception and others differentiated between appearance and hidden reality while in either view science had nothing to say about "reality." "Reality" is the subject-matter of metaphysics. Besides placing these strictures on the working of science, phenomenalism accepted the Cartesian dualism which perpetuated what many biologists held

[116] The following discussion on the history of phenomenalism is based on Woodger's review of Experimental Embryology, by G. de Beer, in Science Progress 21(1926):697-703.

to be an untenable separation of the living body and mental faculties of the organism--a separation which aggravated the conflicts among the various sub-branches of biology over the utility of psychological concepts.

The dean of phenomenalists was Ernst Mach, and Woodger's criticisms were principally toward his version of the doctrine.[117] According to Woodger, Mach's fundamental error was that while he used the existence of the external world as a working assumption he could not account for it and denied its complete demonstrability. Thus Mach begged the question by developing his doctrine by dogmatic assertion and gliding over his many contradictions, for example the use of spatial and causal notions at points in human knowing where they are not yet available as reflective knowledge and thus should not be used to conceptualize. But what troubled Woodger most seemed to be the tacit adherence and discussion of phenomenalism by scientists not philosophically knowledgeable enough to know what they were supporting. This kind of adherence based in ignorance was in Woodger's thinking the source of fundamental confusion in science, of long and meaningless debates, as well as a general waste of time and preoccupation. It illustrated the ascent of a merely practical motive to a pseudo-philosophical defense of doing science in the face of

[117] Woodger, Biological Principles, p. 85-129 (discussion of Chapter I).

potential labeling as metaphysical speculation.[118] Having put forth his argument against phenomenalism Woodger developed his alternative theory of knowledge, relying heavily on Whitehead and the New Realist platform of epistemic neutrality.

The first requirement of such a theory of knowledge is that it be 'self-consistent', not presupposing that which it later denies.[119] Such a doctrine about knowledge must also be able to account for itself through an acknowledged margin of "unarticulable" or prereflective conceptions. Historically, epistemic systems have demanded an explicit statement of their sources and Woodger through Whitehead understood this to be impossible. Epistemic doctrines have also been troubled by the use of the term 'real', often complicated by the changing use of that term by physicists. Woodger suggests it should be used to mean "anything which may be said to exist."[120] This can be seen to be epistemically neutral (subject and object are of equal status) by considering that one cannot be sensually aware of that which does not exist.

All that is 'real' in this sense may be called the "world of being" which may be inventoried as composed of principally three "realms," 1) the primary realm of events, known through the objects

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[118] Ibid., p. 126-128.

[119] Ibid., p. 130 (discussion of Chapter II).

[120] Ibid., p. 133.

of consciousness or intuition (i.e. sense objects, such as the color red of a definite shade here and now); 2) the secondary realm or the realm of "things" of daily life and common sense knowledge (i.e. known as perceptual objects; and 3) the realm of scientific (and philosophical) knowledge.[121] The relationship between these realms is that the knowledge of the second is derived from knowledge of the first and the third from the second. Knowledge does not begin in the primary realm; that realm is merely given and we do not contribute to it but select from it according to our interest or standpoint. Rather our knowledge begins in the realm of common sense experience. Then via critical reflection involving simplifcation and abstraction we arrive at the primary realm which after extensions and corrections constitutes the realm of scientific knowledge. The perceptual process through which knowledge is gained, for Woodger, is not simply sensing but involves thinking or knowing. To phenomenalists who argue over the priority of 'sensation', sense objects, and physical objects, he says "we do not sense the object. We sense the primary realm but we perceive the object. We know the object but we do not know the primary realm save by means of the object."[122] Thus perception is seen as an interpretive process.

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[121] Ibid., p. 133-135.

[122] Ibid., p. 137.

From a consideration of the world of being, its constitutent realms, and the nature of perception Woodger next involved the epistemology of Whitehead in a discussion of this "interpretive process." The primary realm as given is a complex. In order to be grasped it must be analyzed--some repetition must be recognized. Such "patterns" or "things" form "centres round which the recurrent features of the primary realm crystallize while the non-recurrent features pass unnoticed."[123] Thus the interpretations of perception are the "objects" of common sense knowledge standing as the <u>concepts</u> by which the primary realm is known. But it must be recognized, says Woodger, that perception is a very primitive mode of knowing--limited in scope and dependent upon the level of interpretation <u>even while</u> perceptual objects arise from the same primary realm.[124] This intrinsic potential for variable interpretation must be seen to extend to the notion of "properties" of objects as well since these arise from the observation of the relations amongst those objects.

If this variability <u>is</u> fundamental, can anything of certainty be said of the primary realm itself? For Woodger this question arises from the general misconception of the meaning of 'object' and the problem of its reification in our habits of thought. As observers we expect objects to be constant while their appearances may vary. This

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[123] <u>Ibid.</u>, p. 138.

[124] <u>Ibid.</u>, p. 138-139.

comes from thinking that what we are looking at is a thing with inherent qualities rather than a region of the primary realm to which various qualities (in both the primary and secondary classical sense) are complexly related. In this latter view the constancy of properties is dependent not solely on the object of perception but on the constancy of the circumstances of perception. These circumstances may include other regions beyond that of the immediate perceptual event and the body of the percipient event himself; but at any rate perceptual events are always given within a wider encompassing volume, the boundaries of which are set by the interest or viewpoint of the perceivor and are not necessarily coincident with nature's. At base the objects of the interpretive process must be seen to be as much creations of intellectual activity as discoveries about what is actually the case with nature.[125]

With this discussion on the source of perceptual objects Woodger had developed Whitehead's notion of 'object'. With respect to perception, perceptual objects are products of thought and do not belong to the primary realm. Perceptual knowledge being limited in its scope requires that scientific knowledge still be based in experience and exemplified in a manner in some sense recognizable to common sense imagining. But the scope of scientific knowledge may be wider because its objects involve more abstract exemplifications. Still, at most the

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[125] Ibid., p. 140-147.

primary realm may be said to "uphold" or "tolerate" the scientific interpretation, though in a less direct way than is apparent.[126]

Essentially then, Woodger's alternative to phenomenalism involved a recognition of perception as an interpretive process, interpreting the given primary realm and involving abstractive simplification. "Levels" of interpretation result from the use of different modes of abstraction and the products of interpretation may be simply the perceptual objects of common sense knowledge or more abstract objects such as scientific concepts.[127] In any case the common sense notion of "object" being a variable of level of interpretation (and mode of abstraction) needs revising for its use in scientific thinking as does the classical notion of the perceptual event.[128] Woodger continued this revision through an analysis of the uses of the categories of 'substance' and 'causation' in common sense and scientific thinking.

At the outset Woodger states his conclusions about the use of the notions of substance and cause in modern science: they are used uncritically and stand as manifestations of one fundamental mode of thought, namely the search for permanences in nature.[129] After reviewing the meanings attributed to these notions by classical philosophers Woodger concludes that the category of substance is

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[126] Ibid., p. 147-149.

[127] Ibid., p. 155-158.

[128] Ibid., p. 167-168.

[129] Ibid., p. 170-201 (discussion of Chapter III).

"wrapped-up" with that of permanence while the notion of causation, with that of change.[130] These are the pervasive elements of experience, exemplified in solid things, the pushing-pulling they undergo and the repeatibility of that kind of change. For modern science the permanence or "solidity" of nature extends from the all pervasive doctrines of conservation of matter and energy to permanences of lesser degrees such as more or less permanent "things"--a crystal, an organismal type (substance in Aristotle's secondary sense) or an inert gas (enduring substance lacking spatial boundary); and more or less permanent "processes"--the fall of a stone, the beating of a heart, or the disintegration of a radioactive substance.[131] It would seem so far that permanence or "thinghood" is a function of spatial boundary and of some localized independence. From this everyday notion of thinghood science derives its notions of 'state' (e.g. when a thing retains a particular qualitative characterization without change) and the notion of 'property' (e.g. when the state of a thing changes and begins to manifest a recognizable type of relationship with other things in its environment). Thus when a certain thing undergoes change we predicate properties on it according to what class of things it belongs to and whether or not in its relation to other things it manifests a recognizable type of change.[132] In making decisions

[130] Ibid., p. 175.

[131] Ibid., p. 175-178.

[132] Ibid., p. 178-179.

about "permanences" and their manifestation our thinking is guided by two general features: substance, the foothold of knowledge whereby some persistence or regularity is individuated from its environment and causation, whereby the 'permanence' of nature under our attention exhibits some contingency as say a dependence on other things in its changing. Woodger concludes that our habit of thought is to look for permanences of things and permanence of process.[133] These involve, however, differing "modes of characterization."

In order to understand the epistemic status of such modes of characterization we need to reconsider the nature of the perceptual situation. Such a situation involves 1) an awareness that 'something is going on' in space-time separation from our bodies; 2) awareness of sensa (colors, shapes and sizes) related to the 'something going on'; 3) knowing this something as or in terms of 4) something else--usually as a solid perceptual object which can be abstractively simplified into sensa.[134] Now it is our habit, Woodger reemphasizes, in the perception of an event of the primary realm to attribute properties to the perceptual object rather than to the event which is merely known as the perceptual object. This habit has led us to the recognition of a type of persistence, the persistence of the mode of characterization

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[133] Ibid., p. 179.

[134] Ibid., p. 180.

of an event.[135] With respect to change this habit had led us to the recognition of types of change of characterization of which there appear to be two: the differentiation of the spatial parts of an event and the differentiation of the temporal parts of an event. This led Woodger to a categorization of the "solid things" of nature.

A. Those which are homogeneous as regards to intrinsic[136] temporal characterization
   1. Those with contingent spatial characterization, e.g. a stone
   2. Those with intrinsic spatial characterization, e.g. a crystal

B. Those which are differentially characterized in their temporal extension
   1. rhythmically, e.g. heart
   2. serially, e.g. individual organism[137]

This characterization allows Woodger to make important distinctions between the lower inorganic orders of nature and the organic: A chemical substance has no intrinsic temporal extension whereas an organism is intrinsically differentiated in its temporal extension. This is important because it raises the basic difference between the two modes of characterization, that is the instantaneous spatial mode and the temporal mode, which requires a certain minimal duration to

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[135] Ibid., p. 180-181; in this and further discussions of "mode of characterization" understanding will be aided by considering the character of an event through time slices of its duration. In this way the changes in type of characterization may be grasped.

[136] 'Intrinsic' here does not refer to absolute independence but that the special peculiarities of the thing under attention are not directly correlated with its surroundings.

[137] Ibid., p. 183.

manifest itself at all. Thus while a crystal may be crushed and its intrinsic spatial characterization altered, the alteration was external and contingent. Organisms, however, and especially in their development, change not only their intrinsic spatial characterization but do so with some great independence from their external environment.[138]

But not all change is intrinsic, indeed the general notion of causation arises from extrinsic change. As was mentioned above causation involves the recognition of a repeatable pattern of change. Of course no process is strictly repeatable, but what is meant is the reenactment of a certain group of events and relations. Thus "what persists and is repeated is the <u>character</u> of a complex of events, but the events themselves pass irrevocably."[139]

This gives the method of causal analysis in science a problematic nature. Take for instance the study of starch formation in a plant.[140] In our investigation we attend to a four dimensional "slab" of nature's passage of which the plant is a part, an event with duration. We attend to a part of that event, a leaf, and more closely yet to the starch formation process in that leaf. The study of this process is historical, referring to the time slice when the measurement was made and not necessarily to the enduring plant; but we

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[138] Ibid., p. 184-185.

[139] Ibid., p. 186.

[140] The following example, Ibid., p. 187-188.

assume that starch formation continues. Our study reveals three groups of conditions--essential, accidental, and "those beyond our knowledge or control." We find that the contributing constituents of starch formation are sunlight, carbon dioxide, water and a certain temperature; plant cells with chloroplasts containing chlorophyll; starch and oxygen. Some of these "things" change, some remain the same and some disappear. Chlorophyll is found to be essential. Thus a causal analysis of this process consists in finding the simultaneously present conditions which are essential. The search for 'cause' and 'effect' in this example, says Woodger, would merely emphasize the most outstanding features of a basically continuous process. In reality the situation is more complex. In the concrete situation what is "available" is a duration the earlier slices of which contain a plant in a certain spatial relation with the perceptible stuff called water, air, etc. In a later slice the spatial relations are altered. In a still later slice the change called 'starch formation' characterizes certain temporal parts of the event which is known as the plant in question--and this "event" only about a certain period of its life history and only some of its spatial parts, its leaves.[141] Our habit is to treat such a

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[141]This discussion adapted from an example about the study of the effects of adrenalin on the heart (Ibid., p. 189)--equally applicable to the study of starch production in plant leaf because comments are of the methodological order.

partial analysis as adequate and the regularities observed as constants, any departures being due to "interferences." Furthermore, the recognition of successive marked features of events leads us to speak of "chains" of causation but this is a result of concentration on the serial or temporal aspect of the event at the expense of its spatial or simultaneous aspects.

The relation between substance and causation can now be understood.[142] Given the spatial and temporal modes of characterizing events, the observer arrives at an "individual thing" by the persistence of spatial characterization before him, with or without temporal homogeneity, while in the absence of temporal homogeneity the observer arrives at an object-like permanence from the constancy of <u>mode</u> of change before him (rhythmical, serial, etc.). Thus in the observation of causal regularities two situations can arise. On the one hand <u>different</u> complexes of events can be known as the <u>same</u> through a sameness of type of serial change in their mutual relations; or on the other hand, if the constituent events are not parts of a "substantial unity," the manifestation of their mode of characterization is contingent upon their coming into appropriate space-time relations as a recognizable character (e.g. periodic contiguity). These situations manifest for Woodger two kinds of causation, "immanent" and "transeunt" respectively. This allows him to schematize the

[142]<u>Ibid.</u>, p. 190-191.

"permanences of nature" in their substantial and causal features:

1. [Permanences] Of spatial characterization without intrinsic change
   a) Things or objects (i.e. known as 'objects')
      i) perceptual
      ii) purely conceptual
2. [Permanences] Of modes of change of characterization (i.e., temporal differentiation)
   b) Relating to one thing (immanent causal laws)
   c) Involving two or more things (transeunt causal laws)
3. [Permanences of] Relations[143]

Some important implications arise from this treatment. Woodger claims that the standing notion of causation is based on transeunt processes alone, making other kinds of causation difficult to characterize as well as obscuring the complementary function of immanent causal description. Also, the common sense notion of 'thinghood' perpetuates an illusion of constancy about nature even though its substantiality is merely a function of length of duration and intrinsic freedom from contingency of its character. Moreover when considering the relationships among 'things' from the standpoint of the persistence of one thing, all other things become the essential or accidental <u>conditions</u> of its persistence.[144]

This discussion has revealed the more or less explicit 'principles of systematization' at work in scientific thinking as far as Woodger is concerned. He concludes his discussion of the nature of

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[143] <u>Ibid.</u>, p. 192.

[144] <u>Ibid.</u>, p. 195-201.

knowledge with a consideration of the less explicit factors at work in the gathering, selecting, and ordering of natural scientific knowledge.

Woodger's discussion of the subjective dimension of scientific thinking[145] is in fact a collation of those factors which are "capable of 'furnishing a motive for research', are in some sense a priori, [and] are liable to be used blindly and uncritically. . . ."[146] He groups these factors under three headings, "demands," "postulates" and "subjective factors" and while he gives reasons for separating them, they all stand as "habits of thought" differing perhaps in their justifications. Demands and postulates do seem to be closer to the surface of recognized usage and perpetuation by the working scientist:

I. Demands of metaphysical status
   a) Desire for monistic interpretations
   b) Demand for continuity
   c) Refusal of arbitrary breaks
   d) Attempt to reduce all natural science to physics
   e) Demand for persistence and identity
II. Demands of methodological intent[147]
   a) Demand for simplicity and economy in explanation
   b) Occam's razor
III. Desire for atomistic interpretations (outcome of analytic mode of thought)
IV. Demand for verification

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[145] Ibid., p. 202-280 (discussion of Chapter IV).

[146] Ibid., p. 203.

[147] (a) and (b) in this section seem identical. Perhaps some difference in their meaning is revealed by Woodger's statement, "We experience intense satisfaction on reducing a muddle into an orderly and consistent scheme, and the maximum achievement with the minimum of means is part at least of what is aimed at by good 'style' in all branches of art as well as in proving mathematical theorems. . .," Ibid., p. 208.

V. Demand for stuff (thinking based in common sense experience)
VI. Demands clustering around notion of causation
   a) Demand for predictability, univocal determination, universality, 'necessity', and 'uniformity of nature'
   b) Postulate of the validity of inductive generalization[148]

By 'subjective factors' Woodger is referring to habits of scientific thinking the rationality of which is questionable. These he places in three main groups, 1) conservative tendencies or "groove thinking" which covers the "simian laziness" of persisting to interpret new knowledge in terms of the old rather than seeking new, more adequate modes of interpretation; 2) the blindness of success which allows the success of past tradition to dictate the future directions of endeavors; and 3) the triad of dogmatism-intolerance-myopia which promotes specializing selectivity while ignoring its consequent artificial fragmentation of the study of nature (ergo the possible framing of a holistic picture).[149]

By explicating the various implicit aspects of scientific thinking Woodger was not arguing their invalidity. Rather he feared their power as unacknowledged and yet operational modes of thought not necessarily commensurate with the task of understanding the complexities of nature. His point is simply that "Our orthodox traditional way thinking in biological science is not an expression of a preestablished harmony between thought and things, but a mode of thought which has

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[148] Ibid., p. 203-221.

[149] Ibid., p. 222-228.

grown up in adaptation to a certain sphere of experience over a certain limited scale of magnitudes."[150]

Having developed his philosophical framework in Part I, Woodger uses it in Part II to explicate the major problems of biological knowledge, the "antitheses" or theoretical arguments which cleave biology throughout the scope of its investigations. It is important to emphasize here that these antitheses are not problems in empirical biology directly; their pervasive influence is because they are of a more encompassing, theoretical, order. Standing and alternative modes of thought and abstraction being used uncritically or inconsistently yield conflicting interpretations of biological data regardless of the empirical domain of their origin. Woodger does not explain his label of these theoretical problems as being 'antitheses'. It is possible that since Woodger mentions the writings of Kant that he was referring to the thesis-antithesis of that philosopher's antinomies.[151] It is more probable, however, that his intimation at dialectic is a purely logical use: systematic reasoning with conflicting or contradictory ideas, with an eye to resolving the conflict. This view would at least be consistent with Woodger's "analytic" approach.

[150] Ibid., p. 204.

[151] An interesting and relevant contemporary discussion here is M. Grene, "Reducibility: Another Side Issue?" in Interpretation of Life and Mind, ed. by M. Grene (New York: Humanities Press, 1973), p. 14-37.

In Part II Woodger discusses six antitheses--mechanism and vitalism; structure and function; organism and environment; preformation and epigenesis; teleology and causation; and the mind-body problem. Between the discussion of the first and second is a chapter entitled "The Theory of Biological Explanation." Its placement and content are significant because of his conclusions about the first antithesis, mechanism and vitalism. Mechanists and Vitalists disagree over accpetable bases for explanation and both contribute to the confusion by their lack of conceptual clarity. Thus a chapter was needed to discuss first the nature of scientific explanation in general and secondly the need for and development of a biological "mode" of explanation commensurate with the subject matter of biology and with biology as natural scientific knowledge. After this had been accomplished the other five antitheses could be treated in both a critical and revisionary manner.

I believe it is safe to say that the full meaning of the mechanism-vitalism controversy of the early part of this century remains to this day a moot issue. Some contemporary historians of biology say its affective dimension is still with us,[152] others say that it has always been a pseudoproblem.[153] If there has been any consensus about the

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[152] E.g. H. Hein, "Mechanism and Vitalism as Meta-Theoretical Committments," Philosophical Forum 1(1968):185-205.

[153] E.g. M. O. Beckner, "Mechanism in Biology," Encyclopedia of Philosophy (1965), vol 5, p. 250-252.

conflict--either now or when it was happening--it is that the whole was ruled by confusion and obscurity. Woodger, in his day and from his vantage point, saw the conflict concentrated in mainly three camps: the "toughminded" mechanists, the "tenderminded" vitalists, and a middle group of 'antimechanists' who shared the anti-mechanistic beliefs of the vitalists but not their basic tenets.[154] Assuming that mechanists and vitalists alike were studying the same subject matter, that is, one empirical domain or another, Woodger reasoned that the disagreement was over three different factors. The first factor involved what was to be considered the "correct" interpretation of what was 'given' in experience, the "metaphysical" problem of life and not really a scientific problem at all. The second factor involved what was to be the proper methodological standpoint both for the study of life and as the proper basis for its explanation. And the third factor was a general failure to distinguish between the empirical and theoretical standpoints. These problems are not about organisms but about explanations, and these are logical problems and not biological ones. One problem is a disagreement about the standing methodological viewpoint, mechanism, which contends that mechanical explanation is the only one which is admissable in science. Another is the conflicting motives behind the methodological and theoretical viewpoints,

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[154] Woodger, Biological Principles, p. 229-272 (discussion of Chapter V).

the former motivated by a desire for immediate heuristic success which mechanism continued to insure while the latter was concerned not with the success of a particular methodology but rather if it was true that this was the only possible one for biology as a science: Is life wholly amenable to the mechanistic interpretation?[155]

From the various discussions of the mechanism-vitalism question Woodger arrived at some conclusions about the sources of confusion and the various dimensions of the question. One problem was the prejudicial equation of science with the amount of mathematics in it and Woodger attempted to clarify the situation by pointing out that "Physics is scientific not because it admits of mathematical treatment but because it possesses 'clear relevant initial concepts'."[156] Thus the present controversy blatantly illustrated the consequence of not reasoning from such 'clear relevant initial concepts'. For those vitalists insisting that organisms are not machines, or likewise those mechanists insisting that they are, Woodger argued that this kind of question lends itself only to speculation and not scientific solution. Rather a more fruitful line of attack for scientific biology would be to consider "the methodological question of whether organisms are, must, or can be treatable from the scientific standpoint exclusively as

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[155] Ibid., p. 229-232.

[156] Ibid., p. 235.

machines or mechanisms, and if so in what sense."[157] It seemed to Woodger that 'mechanical explanation' was being used in four different ways, as an interpretation in terms of classical mechanics; as an explanation using the concepts of physics and chemistry, as an interpretation on the analogy of a machine in the common sense; or as a methodological postulate asserting a basic orderliness of its subject matter. None of these uses follow consistently the physico-mathematical meanings of mechanism. Thus Woodger concluded that biologists did not know what they meant when they spoke of mechanistic explanation.[158]

Ultimately Woodger attributed the problem of mechanism-vitalism to the workings of tenacious yet counter-productive subjective factors--lack of clarity in concepts used; lack of appreciation for the fruits of critical, abstract thinking; dogmatic views of the various interest groups; and a fundamental lack of appreciation for the difference in, yet interdependence of the empirical and theoretical aspects of biology.[159] In fact Woodger could state the problem in an historical way, these factors having been perpetuated through the generations of scientists.

> We put the emphasis on the wrong place when we take our ambitious constructive schemes too seriously as a foundation to which everything must conform, when in truth they

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[157] Ibid., p. 238.

[158] Ibid., p. 259-260.

[159] Ibid., p. 270.

> are only superstructures always liable to revision when increasing knowledge demands it. In biology our constructive theories have in many cases been based on an intensive study of some limited field by one gifted and successful investigator who has then proceeded to generalize his results over the rest of the organic world. . . . But there has never been much effort made to widen the intellectual horizon of the biologist still further and challenge even the basal assumptions which this branch of natural science has shared with others.[160]

But if historically biologists have not explored other modes of interpretation and explanation, can it be stated categorically what any alternative--indeed "biological"--modes would need to include? Since explanatory bases are at least indirectly a function of one or another mode or level of interpretation, a development of the unique epistemic basis of levels of biological interpretation might result in truly biological modes of explanation. This Woodger attempted in his chapter on biological explanation.

The general aim of explanation is to "make clear the obscure, to render intelligible the unintelligible."[161] With this aim explanation can proceed by either "exhibiting the relation of what is to be explained to something else, or in diminishing its complexity by analysing it." Before considering these two "modes" of explanation the constituents of the explanatory situation need to be itemized. They include "1) the entities to be explained; 2) the ways in which they can

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[160] Ibid., p. 271-272.

[161] Ibid., p. 273-325 (discussion of Chapter VI).

be explained; 3) the relations holding between the entities to be explained and other entities of the same and other kinds; 4) the relations holding between the entities into which what is to be explained is analyzable."[162] It is important to remember that for Woodger, as for Whitehead, the relatedness of nature is a basic assertion. Both subscribe to the doctrine of the priority of internal rather than external relations and it is unacceptable to 'simplify' the situation by disregarding the possibility if not reality of non-contingent internal relatedness.

While there seem to be two modes of explanation they should not be thought independent but rather mutually complementary, each fruitful in particular explanatory situations. With his eye to biology, Woodger differentiates five modes of analysis relevant to "explaining" the organismal event and its spatial-temporal part-events. There is _perceptual_ analysis whereby characters are discriminated by visual analysis; _genetical_ analysis where relations are revealed in successive generations of organisms; _manual_ analysis which yields anatomical entities; _physiological_ analysis which yields the "mutual relations of the parts in the living organism considered as an event yielding a complex of mutually dependent processes,"[163] and _chemical_ analysis whereby the parts are revealed in the relation of chemical composition. The mode of explanation which he calls 'relating' is characterized by

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[162] _Ibid._, p. 273.

[163] _Ibid._, p. 274-275.

the kinds of relations encountered in the study of organisms, 'biological relations': the relation between an organism and its environment; the relation between one organism and other organisms; the relation holding between one part of an organism and other parts of the same organism; and the relation holding between the organism as a whole and a given part.[164] Together these modes of explanation attempt to deal not only with the products of analysis but also the complex relations between these various relata. As these relations and relata exhibit various determinate spatio-temporal structures, a theory of biological explanation must be clear about what it means by biological _organization_. Before treating the relationship between organization and explanation, the sources of generalizations used as explanation and the intelligibility of such explanations needs to be discussed.

Explanations are made with respect to an existing body of knowledge and therefore may be carried out either as an instance of some standing generalization or framed as appropriate to the instance being explained. Woodger calls the former case "borrowing," and he emphasizes the caution with which this must be done. Since generalizations signify the discovery of persistences of some kind, their _a priori_ extension is not justifiable. The problem of intelligibility arises when the hearer of the explanation is not familiar with the body

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[164] _Ibid._, p. 277.

of knowledge being referred to in the explanation. At the most basic level of scientific explanation this problem arises from the use of imperceptibles. Woodger raises Bertrand Russell's distinction between knowledge 'by acquaintance' and 'by description' to distinguish explanations of the ostensive kind from those involving imperceptibles. He warns that the confusing of the two may lead to reification of scientific concepts, such as confusing the hypothetical entity 'electron' with a perceptible lump of matter which violates their cognitive priority.[165] Beyond the intelligibility problem of "borrowed" generalizations, the persistences for which generalizations are posited are themselves functions of level or mode of interpretation and moreover these levels or modes of interpretation should now be seen to correlate directly with the modes of analysis and relating.[166]

At this point, in a very clear sense, we can speak of modes of levels of explanation correlative to modes/levels of interpretation and we recall the variability of the latter with respect to the mode of

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[165] Woodger reasons that we cannot posit imperceptibles as explanatory of perceptibles since the concepts and properties of the former were posited by the originally known macroscopic realm to deal with the imperceptible realm. Thus to do so is to submit to vicious circularity and promote the habit of thinking of "taking the provisional ultimates of science as 'more real' than the experience from which we started in order to reach them," Ibid., p. 331. He does not seem to appreciate the contemporary notion of 'model', obviously for epistemological reasons.

[166] Ibid., p. 278-283.

abstraction employed. The idea of organization in biology reduces, through the chemical mode of analysis, to the notion of chemical composition--at least from the standing mechanistic viewpoint.[167] But the mechanistic view is fallacious both in its view of the properties of matter as inhering qualities and also in its "belief that the point of view of one science is in some sense 'deeper' or 'truer' or 'nearer to reality' than another . . . [but rather] different sciences differ in their degree of abstractness."[168] Thus biologists must use the notion of 'chemical composition' with great caution recognizing that it is only one interpretation by one mode of analysis of one 'level' of organization. Woodger is insistent that biological organization is a level of organization above the physico-chemical, "For if the employment of chemical explanatory entities involves abstraction from entities of a higher level of organization than the chemical, then the use of chemical notions cannot be exhaustive for those higher levels."[169] Just because the chemist is using a mode of abstraction in which the biological levels of organization do not exist, this is no disproof of their existence.

Woodger's arrival in this chapter at the concept of 'level of organization' completes his use and development of Whitehead's

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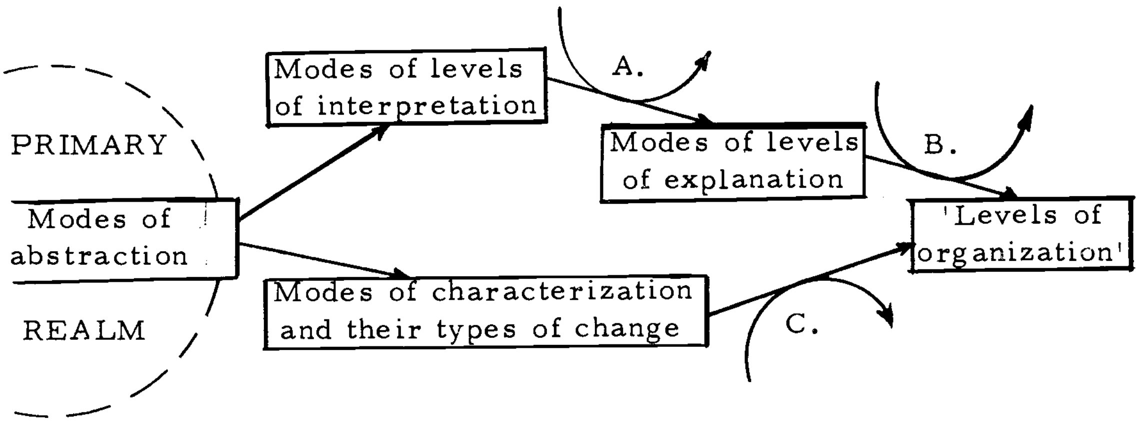
[167] Ibid., p. 283-286.

[168] Ibid., p. 286.

[169] Ibid., p. 286.

epistemology and philosophy of nature. The development can be diagrammed as follows:



A. Modes of analyzing and relating
B. Problems of organization
C. Whitehead's doctrines of 'event' and 'object'

Having discussed the general nature of explanation Woodger could develop the special relationship between biological organization and explanation. But first he needed to insure the distinction between perceptual and conceptual 'objects' to avoid being misunderstood in his discussion of organization. While the concept of biological organization, especially as cellular structure, has its origins in 'chemical composition' both the concept of 'cell' and 'protoplasm' refer to organization but their status is unclear. Contrary to the opinions of some biologists 'protoplasm' is not a perceptual object occurring in nature but rather a hypothetical entity, an explanatory concept of high abstraction not to be reified. Likewise, 'cells' do not occur in nature for the name refers to a type or pattern or level of organization; there is no visual perceptible counterpart to the term 'cell' and to believe otherwise is to subscribe to a mode of abstraction that violates the process-event nature of life.[170] The concept of 'biological organization' refers to an individual enduring spatio-temporal entity with parts whose organization results from the mutual relations of its parts; if the parts themselves have parts and so on, 'levels' of organization are defined, leading to successive levels within hierarchies of levels where the spatial and temporal aspects throughout are of a manifold character.

These distinctions constitute the beginnings of a powerful conceptual vocabulary for Woodger (one whose precision he would later sharpen with the use of Russell's and Whitehead's symbolic logic). To appreciate its character and acuteness, consider Woodger's description of this group of 'cells'--a protozoan, a fertilized ovum, an unstriated muscle fiber and a spermatozoan:

> The first is a whole _organism_ characterized throughout its history by the cell-type of organization; the second is a temporal part of an organism which later is not characterized by the cell-type of organization throughout its history; the third is a _spatial_ part of an organism characterized by the cell-type of organization; and the fourth exhibits the cell-type organization but has been separated for the purposes of organismal reproduction.[171]

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[170] _Ibid._, p. 293-295.

[171] _Ibid._, p. 295-296.

This manner of looking at biological organization might help prevent biologists from confusing a part with a whole: "A so-called unicellular organism is clearly one in which the whole is characterized by the cell-type of organization, whereas a multicellular organism is one which has parts so characterized."[172] 'Levels' of organization are distinguished by the character of the constituent parts. If those parts are homogeneous, then there is but one level whereas if the parts are heterogeneous--themselves having parts--there are at least two levels. Successive sub-dividing leads to a hierarchy of successive levels which in higher organisms is manifest as the inorganic level; the level of cell-parts, the level of cells (non-cellular parts); and the level of cellular parts (i.e. parts of organisms whose parts have the cell-type of organization).[173]

To a large extent biological discussion of organization has been centered on its spatial aspects. Biologists, as the philosopher Henri Bergson (1859-1941) said, have not yet "taken time seriously" and Woodger could at this point raise an important implication. From Whitehead's distinction between 'uniform' and 'non-uniform' objects Woodger reasoned that since most biological 'objects' are non-uniform the recognition of minimal spaces and times for their

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[172] Ibid., p. 296.

[173] Ibid., p. 298-299.

manifestation needs to be taken seriously.[174] For example, the common methodological attitude is to separate objects from their histories--as in the anatomical study of a pickled heart, observationally and conceptually, even while our knowledge of the heart derives from its temporal extension (history) in minimal duration. Thus the assimilation of space and time was a necessary requisite for the full appreciation of biological organization and in this direction Woodger had several comments to make. In the first place the organism must be understood to be a four-dimensional continuum (i.e. process) a slice of which is a four-dimensional event. The organismal process as a perceptual object is known in time-slices and its mode of characterization _revealed_ is a function of the duration and frequency of observation. Our understanding of the organismal process comes from a serial reconstruction of the biological object's history. The most serious implication of this piece-meal and reconstructional method is that it fails to appreciate the _difference_ between the spatial and the temporal parts of the organismal event, principally that only in the temporal parts is the _character of the whole_ repeated. Moreover, "stages" are posited in the organismal process purely as a function of the most enduring time-slices during observation. The ultimate seat of confusion due to an inadequate appreciation of time is

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[174] _Ibid._, p. 299-301.

the concept of 'division'. From it arises both 'endurance' and 'elaboration' and both of these as processes in time and space. A revisional consideration of these concepts is required prior to an adequate conceptualization of 'development' in the organismal process.[175]

The basic notion of 'division' is taken to mean some kind of spatial repetition of parts. But this is unclear. Division is at base the repetition of a type of organization, a process by which organization becomes differentiated--in space as well as time. If the spatial organization is repeated in successive temporal parts, then a uniform persisting object is the result. But living organisms have the capability of repeating their spatial organization spatially resulting in what we know as two whole organisms from one or by incomplete division an increase in the 'part's parts'--the increasing of levels of organization within a single organism. The division process may be either "immanent," i.e. caused by external events, as in the fertilization of an ovum; or "intrinsic" as when budding-off of another organism of the same level of organization as the parent occurs. Also, the products of division may be "part-producing," as when in development there is incomplete division, or "whole-producing," where the products are either "equivalent" (e.g. protozoons) or "non-equivalent" (e.g.

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[175] Ibid., p. 301-303.

parthenogenetic egg). One of the most important temporal products of division is symmetry, the outcome of repetition in time (bilateral and radial when it is simultaneous repetition; spiral when it is successive repetition).[176]

The differentiation or elaboration of development can be seen to arise from division as a result of the coordination of its completeness, character, and immanence. Simple division, merely repeats the initial spatial organization in time and is the source of an organism's endurance . Differentiating division can bring about changes in both an organism's spatial and temporal organization. Elaboration may go on within an individual part or it may bring about two new parts from a prior one through division. With respect to the levels of organization, elaboration seems to have five modes: organic synthesis (from inorganic stuff); division (part-producing); histological elaboration (cell-part producing); intercellular elaboration (differential development); and syngamy, the fusion of cells forming a new whole.[177]

If elaboration is the process by which new parts (ergo levels of organization) come into being, some attention should be given to the notions of 'whole' and 'part' since they are conceptual categories and not perceptual objects. "A part, if it is a living part, i.e. one capable

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[176] Ibid., p. 303-306.

[177] Ibid., p. 307-308.

of division, cannot exist in nature in an inorganic environment. If it persists in such an environment, it is the product of a whole-producing division and therefore no longer is a part."[178] With this notion of 'part' Woodger could list a "hierarchy of parts" in nature.[179]

A. Non-biological parts (incapable of division)
   a) Inorganic: occurring as such apart from the organism, e.g. mineral salts
   b) Organic: normally existing only as parts of an organism, e.g. proteins, carbohydrates, enzymes, hormones
B. Biological parts
   a) capable of part-producing division
      (1) cell-parts: chromosomes, 'genes', etc.
      (2) many non-cellular parts (cells)
   b) not capable of division in higher organisms
      (1) some non-cellular parts (e.g. nerve cells)
      (2) cellular parts: organs, which consist of territories of cells having a determinate organization _inter se_ and in which more than one type of cell is discernible
      (3) organ-systems[180]

As for wholes, they are capable of independent existence which is to say they do not stand in an organic relation as part to a wider whole. Wholes have inorganic environments upon which they are contingent while parts within an organic whole are not so contingent--their relations being internal and their properties a function of their place in the organic hierarchy.[181]

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[178] _Ibid._, p. 308.

[179] That Woodger does not treat here of supra-organismal levels is due, I think, both to his cell-level bias and to the state of knowledge in his day concerning those levels.

[180] _Ibid._, p. 309.

[181] _Ibid._, p. 309-310.

This discussion of biological organization leads to a view of the living organism as an event whose characterization differs from one temporal part of its history to the next. It is analyzable into a hierarchy of parts in a hierarchy of relations both of which change as the organism differentiates spatio-temporally. Thus the organism exhibits an hierarchical mode of organization whose character has implications for biological explanation. In the first place the study of a particular member of a level of organization is usually carried out in isolation from its relations with other members of its level, focusing usually on the lower-level parts of that member. The most adequate study of such a member would include not only a study of the member in its relation to the other members of its level, but also those members in <ins>their</ins> various interrelationships as well. Furthermore, studies usually disregard the fact that parts have environments too, and to study them in isolation makes them "abstracta" not "relata." For Woodger the main implication of the existence of the hierarchical mode of organization is that given the intrinsic nature of scientific investigation (its modes of abstraction, levels of interpretation) each level must be studied at its own level, no investigation at one level able to replace investigations of levels higher up on the hierarchy. That this mode of organization most likely requires a unique mode of explanation is supported by the fact that this type of natural organization has evolved through time with new relations elaborated and no doubt new types of regularity which are not encompassed by the mechanistic mode of explanation.[182]

Purely biological explanations are possible and necessary; they would not be vitalistic simply because they were not mechanistic. The arrival at such explanations is dependent ultimately upon six major factors: the relation between the stage of development of a science and the perfection of its means of observation; the abstract character of the mechanical explanations; the intrinsic limitations of those explanations; the existence of hierarchies of levels of organization in living organisms; the nature of the hypothetical method; and the fact that modern biology regards organisms as the outcome of an evolutionary process. In Woodger's thinking, an understanding of these topics would lead to discovering the conditions of biological explanation and an understanding of the biological way of thinking.[183]

Woodger's theory of biological explanation reveals the methodological dimensions of biology, the study and explanation of biological organization, to be inextricable from the epistemic dimension--modes of abstraction yielding modes of characterization and levels of interpretation. The remaining five antitheses that he discusses arise from misunderstandings of some or all of these dimensions or from misunderstandings about the nature and limits of science.

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[182] Ibid., p. 311-316.

[183] Ibid., p. 317-325.

The antithesis between 'structure' and 'function' involves from Woodger's viewpoint, the question of the priority between structure and function: structure determining function or function determining structure. The artificial separation of anatomical studies from physiological ones manifested the institutionalization of this quarrel. The problem can be discounted as simply resulting from the separation of space and time in differing modes of abstraction: anatomy abstracting organisms from time by viewing them in pickled time-slice while physiology centering its attention on temporal processes and arranging structures in subordinate roles. By clarifying the meaning of 'function' and 'structure' as used by physiologists and anatomists, Woodger was able to put them back together as special limited views of the organismal four-dimensional process.[184]

The antithesis between organism and (its) environment also involves an intolerable abuse of abstraction although from the physico-chemical standpoint it is no antithesis at all. The fact, however, that organisms are inextricably related to their environments--both in their degrees of contingency to them at this instant and as in some sense products of interaction with them throughout their life histories--makes this a genuine and irremovable antithesis. Even though "the organism considered as an event exhibiting a certain mode of

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[184] Ibid., p. 326-330 (discussion of Chapter VII).

characterization which is one of serial change presents a sharp contrast in its regularities to the shifting inconstancy of many of the contemporaneous environmental events. . . ,"[185] Woodger could map out a typology of predictable consequences to be expected from removing organisms from their natural environments.[186]

The conflict between preformational and epigenetic viewpoints Woodger considered the most comprehensive theoretical problem in biology.[187] Involving overlapping concerns of many empirical domains, the antithesis was being perpetuated by differing conceptual and explanatory bases--embryological, physiological, phylogenetic and ontogenetic. Woodger presents his discussion as an exercise in "critical theoretical biology" and as such its intention is not to resolve the problem once and for all, but to clarify and disentangle the complex issues involved: To gain a perspective on the conflict and thus understand the dimensions of the situation engendering the conflict.[188]

It is not clear, however, what Woodger accomplished in this, his largest chapter. His discussion is dense and technical, his line of reasoning treacherous and largely of implied continuity. In some

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[185] Ibid., p. 332.

[186] Ibid., p. 331-333 (discussion of Chapter VIII).

[187] Ibid., p. 334-428 (discussion of Chapter IX).

[188] Ibid., p. 334-335.

sense it seems that Woodger was struggling to gain access to the perspective necessary for a 'general theoretical biology'. Only this could guide him through the web of level-specific, empirical domain discussions toward a basically trans-level view of developmental processes, speak neutrally at all levels of biological organization and develop concepts that could be trusted.

Ostensibly the antithesis between preformation and epigenesis is about the nature of the developmental process, the former seeing it as a process of increase of scale only while the latter as a process of progressive increase in complexity.[189] Woodger, the cautious but thorough philosopher of biology, suspected every concept and viewpoint of harboring implicit or systematic confusion and inconsistency. Grappling with the concept of 'development' he sought to revise it to a general (abstract) concept applicable at both the individual and racial (evolutionary) levels.[190] The basic problem in doing this was to find a way of accounting for 'differences' in the normal repetitive function of development. While 'elaboration' was certainly a source of difference, with respect to the race to which a genetic succession of individuals belong, it too is repeated. Genetics claims to provide both continuity as well as difference through its postulation of "immanent factors" and their mutation. Thus geneticists can speak of approximate

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[189] Ibid., p. 335.

[190] Ibid., p. 337.

repetition through a contingency of organisms or parts of organisms to their environments. But if development was to be a fully general, theoretically-adequate concept it would need to be accounted for not from some lowest level seat of 'immanent factors' but in some sense at each higher level as well.[191]

At least part of the problem was the standing fractionation of the study of development, dictated for example by the special interests, modes of abstraction and levels of interpretation of various sub-branches of biology. Woodger intimates that this scheme with its artificial emphases might be ignoring some of the most important dimensions of development in living phenomena. Basic conceptual conflicts, while indicative of differences in interpretation, too often are taken as disciplinary conflicts where the pride of one is pitted against that of the other. A closer look at this situation reveals the empirical domains of embryology and genetics deal with mainly individual development while physiology and phylogeny are concerned with evolutionary development. By characterizing each domain by the questions it asks, Woodger could raise points from his conceptual framework as those silent areas where important clues to the nature of development might be found.[192]

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[191] Ibid., p. 337-338.

[192] Ibid., p. 344-348.

After having discussed the facts of development and raising various considerations about their relevance to the process of development in nature, Woodger stepped back to ask the Whiteheadian questions, what "persists," what "changes" and what "becomes" in development? At the individual level, development involves the persistence of the cell-type of organization, presisting by spatial as well as temporal repetition; what changes or develops is the mode of organization of the organismal event, the type of organization of the whole changes--from the fertilized ovum (an organism) to the full differentiated, multi-level of organization adult organism; and what becomes is the organismal event because it is an event.[193] For racial or evolutionary development, it is the mode of characterization of an organic race that persists with respect to a given environment; what changes is the given historical period of a race in which a particular mode of characterization of individuals displays itself through a certain temporal part of the race's history.[194]

How far Woodger got towards a general concept of 'development' is difficult to say. He does broaden its scope for it to apply both to individuals and races. In the individual, development is "a serial process manifested as changes in the mode of characterization of the successive temporal parts of an event knowable as one enduring

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[193] Ibid., p. 341.

[194] Ibid., p. 422-424.

thing, i.e. one individual organism."[195] In races, development is "the name given to the serial changes exhibited by the mode of characterization of an event which is knowable as an enduring organism which is an individual member of a succession of such individuals in genetic continuity each individual of which exhibits approximately the same serial changes."[196]

In the end, however, Woodger was not satisfied with his treatment of the preformation-epigenesis antithesis for he returned to it again in a later writing focusing his attention on the concept of 'organism' and the relation between embryology and genetics.[197]

The antithesis of teleology and causation, like that of mechanism-vitalism, stems from conflicting bases of explanation.[198] Explanation involves the bringing into a relationship the occurrence to be explained and something else, while that "something else" in teleological explanation is the outcome of the occurrence rather than its antecedent events.[199] This implicit use of the notion of 'purpose' is often construed as "conscious purpose" and this is what makes

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[195] Ibid., p. 335.

[196] Ibid., p. 337.

[197] This is a central concern of Woodger's 1930-1931 'Concept of Organism' papers, see footnote 212, p. 121.

[198] Woodger, Biological Principles, p. 429-457 (discussion of Chapter X).

[199] Ibid., p. 432.

teleological explanation unscientific. If, however, purpose is construed as a function in the maintenance of the whole or the race, teleological explanation loses much of what is untenable although it still suffers from the same ills as most of explanation in science for example the artificial separation of space and time; forced categorization of activities as either teleological or ateleological; no allowance for differences in kinds of teleology (external and internal).[200]

The solution to this antithesis is simply to take a wider focus when looking at purpose in organic events, a focus including the organism in its constitution of part-events with respect to the environments (and its space-time parts) to which the organism's characterization "adapts" it. The concept of 'adaptation' is the concept of 'purpose' at the racial level. Teleological and causal explanatory bases come to blows over the seat of responses--whether they are "biological," that is internal, or merely external and thus question the deliberateness of such responses. The organism in its contingency to its environment makes various responses to the events of the environment. The causal view sees these responses as the effects of external "causes" while the teleological view recognizes an internally-based regularity by which it responds to irregular external contingencies. Consequently the teleological view sees adaptation as a

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[200] Ibid., p. 432-434.

process of perpetual change in response to environmental contingencies whereas the causal view sees adaptation "accomplished" through mechanisms in the organisms.[201] Woodger in summing up the problem extends its implications as indicative of types of theoretical biology.

> To sum up: the difficulty of avoiding "teleological" modes of expression in biology rests partly upon the fact that in an organism the parts constituting it are so related to one another and to environmental events that typically the organism endures. An organism is therefore capable of biological responses, as well as inorganic reactions. Evolution along progressive lines requires a gradual elaboration of biological responses issuing in greater and greater independence of environmental contingencies. In so far as a mechanism for such responses has already been elaborated they are susceptible of a causal analysis. But from such a point of view neither particular appropriate acts of response which do not belong to a routine in relation to an environmental routine, nor the first appearance of those embodied now in a routine can be treated at all. They are accidents. Only two types of theoretical biology have so far been devised, both involving using the analogy of a humanly constructed machine; (1) vitalism (with a mechanic), and (2) the 'machine theory' (without a mechanic). This provides no independent biological way of thinking, because machines presuppose organisms.[202]

Appended to this chapter on the antithesis between teleology and causation is a "Summary on Explanation in Biology" in which Woodger reiterates some of the points of his theory of biological explanation and discusses how they are corroborated or extended by his discussion on biological organization and the antitheses up to this

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[201] Ibid., p. 434-440.

[202] Ibid., p. 440-441.

point. He also offers here a classification of explanation based on the epistemological status of the relata involved:

a) Perceptual explanation, in which the relata are perceived entities (e.g. empirical laws)
b) Conceptual explanations, in which the relata are not perceived entities
    i) those accompanied by calculation (e.g. quantitative generalizations, predictions)
    ii) those not accompanied by calculation (e.g. abductive explanation)[203]

The last theoretical argument Woodger considers is the mind-body problem.[204] His discussion of this theoretical problem is at the philosophical level concerned with those areas of humanity and common sense from which biological theories abstract, the smallness of the range of experience involved in their abstractions, and the precarious position of man as the subject theorizing about himself as object. He admits at the onset that this problem is a conundrum; all he proposes to do is to explain "why for epistemological reasons, the relations between biology and psychology are totally different from the relations between biology and the physical sciences."[205] There is a duality of sources of human knowledge, sense experience which is "public" and introspective experience which is "private." Much confusion arises, says Woodger, from not honoring their ultimately distinct status, submitting to the "monistic urge" and making of them

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[204] Ibid., p. 458-476 (discussion of Chapter XI).

[205] Ibid., p. 459.

one scheme of knowledge. Essentially this is not a problem for scientific biology. It only seems to involve the psycho-physical empirical domains of biology because of the similarity of its terms to the classical statement of it. It is not amenable to empirical investigation, only metaphysical speculation.[206] (Woodger's discussion of the problem is not so brief as I am portraying it. But in the end his attitude is one of Wittgensteinian silence rather than confused avoidance.)

The final chapter of Biological Principles entitled "The Future of Biology" provides us at last with an explicit perspective on Woodger's own conception of what he has been doing in this book.[207] He remarks, "In this book we have been studying what is commonly called the philosophy of science with special regard to the problems of theoretical biology." We may wonder why he placed such a seemingly introductory statement at the end of his book and he helps us with his next statement, "I have avoided this expression [philosophy of science] because I have been trying to close rather than widen the breach which separates the natural and philosophical sciences."[208] We can understand his caution if we recall that his opening chapter was a critique of the views of some scientists-turned-philosopher and

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[206] Ibid., p. 475-476.

[207] Ibid., p. 477-488 (discussion of Chapter XI).

[208] Ibid., 477.

we can guess that he no doubt wanted to avoid his work being construed as a contribution to that genre of writing while still giving the impression that it was a work of interest to the working scientist.

In Woodger's thinking there are, or rather must be, three aspects to biology, an investigatory one concerned with "the discovery of biological phenomena"; a speculative one whose purpose is "to discover means for systematizing the knowledge furnished by investigation"; and a critical one which investigates methods of interpretation from the standpoint of logic and epistemology.[209] After acknowledging the areas of contemporary thought he has utilized for his discussion, Woodger gives his "requisites for biological progress." These include a critical sorting of biological concepts, more open-mindedness and appreciation of the relationship between the facts of investigation and their theoretical interpretation, a "biological way of thinking" commensurate to the facts of biology, a unity between the various branches of biological science--especially for the parallel, because interdependent, development of the three aspects of biology, and a revised education for biologists to encourage thinking in the three aspects.[210]

It seems that Woodger's three aspects--investigatory, speculative, and critical--correspond more or less to contemporary

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[209] Ibid., p. 477-479.

[210] Ibid., p. 486-488.

divisioning of the biological enterprise into its empirical, theoretical and philosophical dimensions. Thus with respect to orders of discussion, Biological Principles is a discussion in the philosophy of biology, a discussion on theoretical biology and with the latter involves discussions on empirical biology.

Woodger in Biological Principles was attempting for biology what Whitehead had attempted for the physical sciences in his Enquiry Concerning the Principles of Natural Knowledge: an analysis of the nature of scientific knowledge and a revision of the philosophy of nature framed from that knowledge. The first task Woodger approached by first explicating the implicit theory of knowledge at work in the biology of his day and then by developing a revised epistemology which involved making explicit the use of certain philosophical notions by scientists. In this task Woodger's theory of knowledge suffers from the same opacity as Whitehead's own version as well as the questionable practice of couching difficult conceptualizations in deceptively common terminology. Woodger approached his second task of revising the biological view of nature to be in accord with Whitehead's philosophy of nature by examining the major problems of biological knowledge and revealing the conflicting bases of interpretation in these problems. It became evident from this discussion that these bases often allied themselves with a philosophy of nature, such as materialism, incommensurate with the data of biological science.

Woodger's constructive efforts in Biological Principles, especially his development of the notion of hierarchical levels of organization, illustrate the beginnings of a philosophy of biological nature which in his view was commensurate with the epistemologically-revised data of biology.

The development of these tasks in Biological Principles seems on the surface to be overly long and polemical. The discussion of the antitheses seems an unnecessarily long exegesis of textbook statements. It might be said that the book overall suffers from its length. Woodger's goal in the book was to "introduce and expound a new method of approach to such philosophical questions--a method which had not yet received the care and attention among biologists it deserves."[211] After all it is the orientation to this critical method and the fruits of its use that make up the bulk of this book. The book establishes discursive orders that were secure for communal development, a fact that should put its length in proper perspective.

Woodger: From Orders of Discussion to the Biological Enterprise

> A discussion of embryological topics and their relations to genetics with our current conceptual and linguistic apparatus is like performing a modern surgical operation with a pair of nail scissors and a potato peeler.

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[211] Ibid., p. 6.

The 'Concept of Organism' Papers

Shortly after the publication of Biological Principles Woodger contributed a series of three papers to the American journal, the Quarterly Review of Biology.[212] These papers are interesting and important because they mark the mid-way point between Woodger's developed understanding of the need for a communal theoretical order of discussion in biology and the beginning of his future work in developing a technical language for such an order of discussion. On the surface these papers seem to be a development of concerns raised in Biological Principles, but through the increasing technicality of their argument several of Woodger's most important realizations emerge, realizations which become the platform for all his future work.

Woodger begins the first paper by developing one of his concluding points in Biological Principles, the need for a revision in the curriculum of biological education to provide instruction in all three aspects of the biological enterprise. He reiterates the differences in motives and standards behind observation and interpretation and reemphasizes that "interpretation" is guided by knowledge of the properties of knowledge itself. Even so Woodger is troubled by the

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[212] J. H. Woodger, "The 'Concept of Organism' and the Relation between Embryology and Genetics," Quarterly Review of Biology 5(1930):1-22 (Part I), 438-463 (Part II), 6(1931):178-207 (Part III).

fact that "everyone recognizes the desirability of knowing something about the physical instruments used in scientific investigation, but the importance of understanding the properties of the intellectual tools involved--concepts, propositions, principles of inference, 'working hypotheses', postulates, etc.--is much less clearly appreciated. And from the standpoint of the process of interpretation this may be a misfortune."[213] Thus in these papers Woodger began by reemphasizing the process of interpretation and the need for a revision in the concept of the biological enterprise. German biology, Woodger contends, had already faced this fact, a forum was provided by J. Schaxel's Abhandlungen zur theoretischen Biologie[214] and the task of revising the outdated biological concepts underway. Central to the concern of these "theoretical biologists" is the concept of 'organism' for with its status and tenability depends the ultimate autonomy of biology as a science. Woodger acknowledges his debt to these thinkers and proceeds with his anglicized contribution to their task.[215]

In the remainder of this paper and for the most part throughout the other papers Woodger was concerned with first analyzing the concept of 'organism' (i.e. showing how it involves other concepts

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[213] Woodger, 'Concept of Organism', Part I, p. 3.

[214] Abhandlungen zur theoretischen Biologie, Berlin, vol 1-30(1919-1931); superceded in 1939 by Abhandlungen zur exakten Biologie, Berlin.

[215] Woodger, 'Concept of Organism', Part I, p. 5-6.

such as 'organic whole', 'organic part', 'organic relation' and 'organic order') and then developing it and the notion of 'hierarchical order' through the symbolic apparatus and mathematical concepts of Russell's and Whitehead's Principia Mathematica.[216] Thus we begin to see more and more of Woodger's thinking embodied not in natural language but in symbolic formulas and relational schemes whose precision and freedom from ambiguity was guaranteed by definition. With this conceptual and articulational apparatus Woodger returned to the problems of preformation-epigenesis and the use of the causal postulate in biological reasoning and was able to clarify much of the confusion of their treatment in Biological Principles.[217] This success

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[216] A. N. Whitehead and B. Russell, Principia Mathematica, 3 vols. (Cambridge: Cambridge University Press, 1910-1913). Volume I (1910) was of special influence for Woodger since it set forth the system of symbolic notation and logical apparatus of the entire work as well as developed what Woodger considered biologically-relevant concepts--classes, relations, and a cardinal arithematic for "calculating" with them. Of the first volume Whitehead said, ". . .[it] is devoted to the initiation of non-numerical quasi-geometrical sciences, together with a technique for their elaboration" (quoted from the frontispiece of Woodger's Axiomatic Method in Biology (Cambridge: Cambridge University Press, 1937).

[217] Woodger, 'Concept of Organism', Part I, p. 17-21, Part II, p. 452-462, Part III, p. 186-207. The first paper reviews the basic problem of the preformational and epigenetic interpretations as they manifest areas of overlapping concern (and conceptual confusion) for genetics and embryology. The second paper is "devoted to a study of some of the postulates, assumptions, and order-systems (e.g. logic of division, spatial and genetic hierarchies) upon which all inference in genetical and embryological science depends" (Part III, p. 178). The third paper uses this logical apparatus to differentiate and interpret the problems of the developmental process and in so doing clarify the domains of concern of genetics and embryology.

convinced Woodger of the utility of the logistic method in the biological enterprise, a conviction which is the base for all his future work.

Another important and related realization for Woodger was his conception of theoretical biology as a communal task and an integral part of the biological enterprise. In his first paper he raised the point that although there is a surplus of theories in biology, there is no theoretical biology in the sense that there is a theoretical physics.[218] This he attributed first to empirical domination of biological education which emphasizes knowledge of the tools of discovery to the detriment of knowledge of the interpretation of those discoveries, in a word the absence of instruction in the "theoretical standpoint." Woodger spends some time illustrating the failings in the history of biology which can be attributed to this ignorance. Finally Woodger provides the *sine qua non* for theoretical biology when he states, "Only if and when a system of logical relations is discovered from which the empirical generalizations can be deductively developed and into which the biological concepts enter as values of the logical variables will anything approaching theoretical biology which is at all comparable with theoretical physics be possible."[219] Thus Woodger is subscribing at this point both to the logical symbolization of biological concepts and statements and to

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[218] *Ibid.*, Part I, p. 3.

[219] *Ibid.*, Part I, p. 5.

the view that the reasoning done with these statements should be strictly logical as well.[220]

From this logical focus Woodger developed his biology of three aspects into the ten "pursuits" of a "well-developed science."

1. "Empirical investigation" (the making and recording of actual particular observations under a certain experimentally altered or controlled set of conditions)
2. The extension of these observations with a view to discovering whether and how far they can be inductively generalized
3. The discovery of clear concepts to embody the inductive generalizations, and the construction of an unambiguous language in which to express them
4. The discernment of the type or types of logical order exemplified in a particular field of empirical investigation
5. The investigation of the logical properties of types of order as such, i.e. apart from their exemplifications
6. The construction of theories embodying the data yielded by (1) and (2), systematized by the aid of (4) with or without the help of hypothetical entities.

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[220] The influence of logical positivism on Woodger's thinking is not clear. While Woodger was studying the logic of Principia Mathematica and developing his "biological axiomatics," he became acquainted and was subsequently much influenced by the philosophers Karl Popper, Alfred Tarski and Rudolph Carnap. From these men, especially Carnap, Woodger absorbed the "logic of science" philosophical focus which followed so easily from interests in the new logic. While Woodger pursued the formalized syntactical approach in his study of biological knowledge, in his later writings (e.g. Biology and Language, 1952) he widened this focus to consider the semantic dimension of communal language use. However, at the same time he continued to have great sympathy and enthusiasm for the philosophy of Whitehead, a philosopher who in no way could be considered a logical positivist.

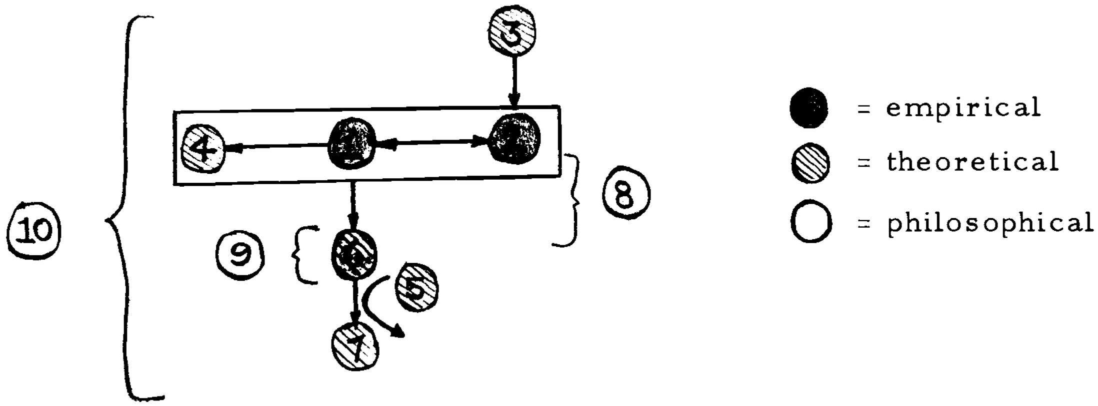
7. The deductive development of (6) with the help of (5), leading to renewed investigation under (1) with view to the "verification" of (6) and the checking of (2)

8. The systematic analysis of the postulates and assumptions underlying (1), (2) and (6)

9. The critical study of the ontological or "material" concepts employed in (6), e.g. 'space', 'time', 'thing', 'property', 'matter', etc.

10. The study of the problems which present themselves when the question of the "truth" and "objectivity" of scientific knowledge is raised[221]

If biology were a "well-developed science" then Woodger is saying that (1) and (2) should be the pursuits of its empirical aspect; (3) to (7) the pursuits of a "speculative" or theoretical aspect; and (8) to (10) the pursuits of a "critical" or philosophical aspect. A closer look at this list shows that there is a logical process implied by them, one which might better be visualized in the following way:



The central concern is the empirical aspect (pursuits 1 and 2) which is the subject matter of the theoretical aspect (pursuits 3 to 7), which

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[221]Woodger, 'Concept of Organism', Part II, p. 440.

together are the concern of the philosophical aspect (pursuits 3 to 10). What is important to notice here is that while the variously ordered aspects are in some sense "separate" ultimately their relationship is one of interdependence. Equally important to notice is that the theoretical pursuits begin with and their ultimate adequacy (i.e. their trueness to the biological viewpoint) rests on (3), the development of clear initial concepts and an unambiguous articulation language. The rest is either the logical investigation or development of conceptual propositions or their assumptional bases.

While Woodger could make various contributions to clarifying some of the many problems of biology as knowledge, at the end of his third paper he reiterates his pervasive aim of illustrating and urging the "general desirability of devoting some little attention to the linguistic and logical aspects of biology." Furthermore, "Our language, if it is to be adequate to deal with a given realm of fact, must have a logical structure of the same degree of multiplicity as that of the realm of fact itself."[222] This latter statement is perhaps the most significant in Woodger's thinking of this period: It marks the end of a long line of reasoning, the emergence of an orderly view of the biological enterprise, and the articulation of the task of developing a technical language to elucidate the theoretical and philosophical orders of

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[222] Ibid., Part III, p. 206-207.

concern of biology, a language which could be incorporated in the education of biologists as easily as that of mathematics in the education of physicists.

III. CONCLUSIONS

The Case for the Emergence of Theoretical and Philosophical Orders of Discussion in the Work of an Individual Biologist

An overall view of the transitions in Woodger's thinking about the biological enterprise shows the emergence of supra-empirical order concerns as well as his attempt to establish justified and independent orders of discussion for them. The awareness of a theoretical order of biological concern in Woodger's thinking was due ultimately to his emerging understanding of the biological enterprise as more than just "empirical" biology. Woodger's early education, like most biologists since the turn of this century, was in a specific empirical domain. Thus cytological research, especially problems in sub-cellular morphology, served to introduce him to working empirical biology. The transition in his conception of the full scope of biological science was initiated by his formal training in vertebrate anatomy and embryology. Then in the task he set for himself in the writing of Elementary Morphology and Physiology, he framed and further developed that conception to include questions of scientific method and interpretation peripheral to empirical biology. Woodger's realization of the dynamic nature of this scope was no doubt brought home to him in the rapidly expanding surveys of cytological research by E. B. Wilson.

In the period of philosophical self-instruction which followed the publication of his first book, Woodger began to question the adequacy of empirical biology as a collective label for the concerns of the whole biological enterprise. By the time he began his second book considerations of philosophical order forced him to drop that collective label forever. Indeed in Biological Principles empirical biology becomes the ground from which serious conflicts of theoretical order arise, a ground whose conceptual and explanatory apparatuses require explication and revision. The discussion in this book is one of supra-empirical order, partly a discussion of the nature of interpretation and partly a discussion of yet a higher order concerned with both the epistemic foundations of biological knowledge and the criteria of adequacy of theoretical interpretation. In his papers on "Concept of Organism" the "investigatory," "speculative" and "critical" aspects of biology--distinguished almost in retrospect in Biological Principles--become groups of "pursuits" in the logical machinery of biology as a "well-developed science." Woodger's earlier concern for "subjective factors" in these papers is minimal; this conforms to Woodger's new goal of developing a revised biological enterprise built on a balanced perspective of the orders of biological concern and their discussion, provided with a method and language with which biologists could approach the study of the living world.

The origins of Woodger's realization of the multiplicity of orders of concern within empirical biology are to be found in his understanding of the "orientation of the critical empirical biologist" discussed previously. The orders of concern Woodger later identified as "aspects" and "pursuits" of the biological enterprise in his second book and the "Concept of Organism" papers were collectively in use in the thinking of a critical empirical biologist. And it was Woodger's troubled realization that few biologists had this critical attitude. The constituting aspects of this "orientation" were 1) a critical concern in the choice and handling of study materials, 2) a belief in the power of technique when correctly understood and applied, 3) a sense of judgment about the accuracy of observations and 4) a fundamental caution in the speculations founded on empirical results. Thus this orientation involved not just canons of observation and methodology but also a critical attitude toward the goals of research and how far empirical data could take one toward those goals. From this arose the factor of the variability of interpretations which was of routine concern to cytologists as we have seen. Woodger carried an appreciation of this into the writing of his first book as his own demand that equally important to the presentation of the interpretations of empirical biology is a discussion of the <u>grounds</u> of these interpretations--their rational basis and part in the whole picture. This demand which in <u>Elementary Morphology and Physiology</u> stood as Woodger's basic concern in presenting general biology became in Biological Principles an order of discussion to which he contributed and about which he discussed. In securing this theoretical order of discussion Woodger undertook an epistemological analysis of biological objects and explanations and developed what he considered the more adequate base of Whitehead's philosophy. Then in contributing to the theoretical order of discussion, he focused on the concept of 'organization' as an epistemically-adequate handle on the diversity of biological phenomena. Thus the "grades" and "plans" of organization in Woodger's first book became in his second, "levels" and "types" or "orders" of organization.

While it was concerns of philosophical order that prompted Woodger to establish a theoretical order of discussion in Biological Principles, there does not seem to have emerged a philosophical order of discussion in his writings. There are several possible reasons for this. In Biological Principles the problem of the adequacy of interpretation was linked with such factors as modes of abstraction, implicit working assumptions and subjective factors. Woodger's discussion of these was for the most part an explication of the surface effects of their deep-seated or at least unacknowledged activity. In the 'Concept of Organism' papers Woodger discusses these dimensions to some extent but since his intent is revisional, their place and manner of prevention are not instituted in his model of the biological enterprise. Thus it seems to me that while these factors are certainly of the philosophical order of concern, they tend to arise and evaporate as the problems of approaching new empirical discoveries with standing or revised modes of interpretation arise and are communally resolved. In short, and for the most part, such subjective factors are the frustrating means and exasperating by-products of scientists working under too restricted a communal view of the nature of their enterprise, for example the biological enterprise as merely "empirical biology." Once the supra-empirical order concerns are recognized and developed within the communal biological enterprise, such secondary problems disappear or at least have less active roles.

Beyond these transitions in Woodger's awareness is his pervading intent of securing these orders for communal discussion. From his first book on, Woodger wished to see a revision in the curriculum of biological education in order to train biologists in the theoretical and philosophical dimensions of their enterprise. In fact, Elementary Morphology and Physiology as a textbook may be seen as Woodger's contribution to that revision. Woodger's self-concept as teacher is, perhaps, what allowed him to resist the temptation of system building as many of his similarly-concerned contemporaries did not, understanding that a truly adequate theoretical biology required a full biological perspective. Such a perspective, being at base communal, required a language for communication and in his 'Concept of Organism' papers Woodger's direction of future work was set to the task of establishing a technically-precise and unambiguous language with which to discuss the theoretical, and perhaps philosophical, problems of biology. When such a language was developed, it could serve biologists as mathematics serves physicists and could facilitate the institution of the theoretical order of discussion in the educational system of biology.

The Case for the Communal Status of These Orders of Discussion

While it is clear at this point that Woodger strived for at least a theoretical order of discussion to be given permanent place in the discourse of the biological enterprise, how can we be sure that such a need really existed? In a word, how well tuned was Woodger to the biological enterprise of his day? To answer that is to state the need for such orders of discussion being communal.

There appeared in 1926 in the annals of Nature a review of D. Noel Paton's (1850-1928) The Physiology of the Continuity of Life by J. S. Huxley (1887-1975).[223] Huxley's review elicited a critical letter to the editor of Nature by the author of the book[224] and so began an

[223] J. S. Huxley, review of The Physiology of the Continuity of Life, by D. Noel Paton, in Nature 118(1926):902-905.

[224] D. N. Paton, "Letter to the Editor," Nature 119(1927):159.

interchange in that column which lasted almost a year and involved 11 different people--philosophers as well as biologists. This set of interchanges which the editors entitled "Biological Fact and Theory" reflects with almost uncanny accuracy Woodger's critique of the biological enterprise of his day, as well as supporting his claim that communal orders of discussion on supra-empirical concerns were needed--especially in the curriculum of biological education.

Huxley's review began by heralding a new era of synthesis in biology for "At last we know the main outlines of the laws of most vital phenomena. . . ."[225] He continued in the vein that Paton's book was a synthesis of the broad field of reproductive biology. Huxley's criticism was essentially that the physiological point of view was of little value for interpreting the "fundamental facts of genetics." Paton's reply was a defense of his position since it agreed in its interpretation of the data of genetics with T. H. Morgan in his writings. Following Paton's letter was a counterreply by Huxley[226] in which he discusses at length what is the <u>correct</u> interpretation of genetical facts and rallying to his support the findings of other studies and then reasoning through again to his original conclusions. Throughout the rest of the interchange Huxley writes only twice again and Paton not at all.

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[225] Huxley, review of <u>The Physiology of the Continuity of Life</u>, p. 902.

[226] J. S. Huxley, "Letter to the Editor," <u>Nature</u> 119(1927):159-160.

E. W. MacBride (1866-1940), an embryologist and geneticist, who had supplied Huxley with those "other studies" communicated his disfavor at Huxley's interpretation of them,[227] insisting that physiological studies of inheritance were of equal validity as the genetic studies. Following MacBride's letter was one from a cytologist, Charles Walker[228] (? - ?) who criticized Huxley's dogmatic adherence to the chromosome theory of inheritance and suggested certain limitations in its utility for the broad-scoped evolutionary perspective. Also, for the first time in this interchange, the question of what was meant by the widely used term "character" was raised by Walker.[229] At this point in the interchange the first comments from a philosopher were received. The biologist and self-styled philosopher James Johnstone (1870-1932) developed Walker's doubts to a question of the meaning of <u>any</u> of the concepts used by geneticists and argued that no foundation of "fundamental genetic facts" had so far been erected.[230]

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[227] E. W. MacBride, "Letter to the Editor," <u>Nature</u> 119(1927):160-161.

[228] C. Walker, "Letter to the Editor," <u>Nature</u> 119(1927):16.

[229] Walker states, "It may be claimed that what I describe as characters are not characters in the sense intended by Prof. Huxley. In what, then, are they different except in degree? . . . But it also appears to me that we cannot place all these characters in the same category as regards their mode of inheritance" (p. 161).

[230] J. Johnstone, "Letter to the Editor," <u>Nature</u> 119(1927):319.

Johnstone's letter elicited the second reply from Huxley,[231] whose intention was to educate the good Professor Johnstone to the facts of the new science of genetics. Huxley begins with an enumeration of the "fundamental facts of genetics," 30 in all, and then makes reference to studies which are based on these and on which the progress of genetics is presently based. The tone of this letter seems to be unmistakedly one of unswerving commitment to the genetics of his day and its interpretations. Huxley's letter elicited a prosaic reply from Walker[232] with quotations from the Bible and the lyrics of an English opera. Again Walker criticized Huxley for his tendency toward "sweeping generalizations" and his "constant presentation of doubtful theories as proven facts"; as for Huxley's list of fundamentals, Walker insisted that they represented "not proven fact, but assumptions for which a varying amount of evidence is available."[233] Responding to the interchange in its present concern over 'fact', 'theory' and 'evidence' was the physiologist-biochemist-geneticist J. B. S. Haldane (1892-1964)[234]. Haldane began with an obscure distinction between techniques of heuristic motive and fundamental motive and that only the latter need be taken seriously. The rest of

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[231] J.S. Huxley, "Letter to the Editor," Nature 119(1927):350.

[232] C. Walker, "Letter to the Editor," Nature 119(1927):456.

[233] Ibid., p. 456b.

[234] J. B. S. Haldane, "Letter to the Editor," Nature 119(1927):456-457.

his letter was an apology for the primitive state of the "atomic theory in genetics"--barely beyond Dalton. Concurrent with Haldane's letter was one by Eric Ashby (1904-)[235] who expressed a middle ground position about the interchange to this point. His solution was one of "having patience," sympathetic to the heuristic motive of empirical research and suggesting its universal utility in science. Some questions, Ashby intimates, must wait until the synthesis of what analysis reveals before being answered.

The next two letters reiterated the general points of the last few letters. J. T. Cunningham (1859-1935), a zoologist,[236] questioned the habit of geneticists to "complacently assert that their discoveries explain all the important phenomena," of biology for example the large-scale morphological changes of evolution, while the Cambridge philosopher F. H. A. Marshall (1878-1949) gave a philosophical critique of the interchange to this point.[237]

> The discussion in _Nature_ under the above heading appears to me to be based in large measure upon failure to realize the character of scientific knowledge. Such misunderstanding is to be deprecated as being contrary to the interests of further progress in biology. I beg leave, therefore, to reiterate that scientific hypotheses, otherwise known as laws of Nature, are created by the mind of man for purposes of prediction and generalization. They do not represent absolute truth, and

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[235] Eric Ashby, "Letter to the Editor," _Nature_ 119(1927):563.

[236] J. T. Cunningham, "Letter to the Editor," _Nature_ 119(1927):563.

[237] F. H. A. Marshall, "Letter to the Editor," _Nature_ 119(1927):563.

> are always liable to be superceded by new hypotheses which are more widely embracing.[238]

Thus in Marshall's view the whole problem could be resolved in the beginning by the "proper appreciation" of the descriptive character of natural science and the limits of its domain.

The next reply was from Huxley who, saddened by the "introduction of personalities" into the interchange, chose to make one final statement. "The progress of biology in Great Britain is being retarded by the failure of specialists in its various branches to appreciate the bearing of work done in other fields than their own."[239] Since this was directed explicitly at Walker, the next reply was from him.[240] After his usual poetic opening, Walker reiterated his position on what had been said since his last reply: the <u>real</u> character of the "fundamental facts of genetics"; the role of theory in biology and the test of the "best" theory in biology as the one requiring the least in the way of assumptions; the problematic nature of the "Neo-Mendelian Chromosome Theory" and the need for geneticists to see their working hypotheses as useful conceptions and theories not laws and proven facts .

The last new contributor to this interchange, J. S. Dunkerly (? - ?), sought at this late date to temper the hypercritical and

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[238] Ibid., p. 563.

[239] J. S. Huxley, "Letter to the Editor," Nature 119(1927):639.

[240] C. Walker, "Letter to the Editor," Nature 119(1927):814-815.

hyperconservative attitudes of Walker and Huxley by suggesting a common-sense standpoint that the Mendelian theory was proving to be a "powerful weapon for the attack of biological problems."[241] To this Walker replied[242] with his most satirical quotations, badgering Dunkerly for totally misconstruing the substance of his letters and then Walker once again reiterated his position. Dunkerly replied[243] to this by reiterating the utilitarian viewpoint and ending with a lengthy quotation in German. So ended the interchange "Biological Fact and Theory."

This set of letters, this communal discussion of empirical and supra-empirical order concerns is as interesting in what it <u>shows</u> as in what it purports to say. On the surface it is a discussion about the proper interpretation of the facts of genetical studies with disagreement over which point of view is most valid and what explicit factors constituted the correct interpretation. But beneath the surface amidst the "introduction of personalities" are a host of features which are not the specific topic under discussion but yet are just as thoroughly involved: implicit adherence to either a materialistic or non-materialistic philosophy of nature; a confusion over, but only periodic questioning of, the meaning of concepts used regularly; a

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[241] J. S. Dunkerly, "Letter to the Editor," <u>Nature</u> 120(1927):12-13.
[242] C. Walker, "Letter to the Editor," <u>Nature</u> 120(1927):118-119.
[243] J. S. Dunkerly, "Letter to the Editor," <u>Nature</u> 120(1927):191.

pervading, almost blatant lack of understanding or appreciation for a "theoretical standpoint" independent from the empirical--resulting in redundancy, claims of misunderstanding or name-calling; a lack of fruitful interchange between philosophy and science, the former purporting to solve the problems of the latter instantly from a position of condescension, hostility, and privileged judgment.

These conflicting dimensions were perpetuated in this interchange by the general and pervasive intolerance for a blameless yet critical discussion of differences in interpretation. This intolerance Woodger epitomized quite well. It was composed of an abhorrence of speculative philosophy, of armchair philosophers "doing" science or framing cosmological schemes from bits and pieces of empirical data; a pervading empirical tenet that there must and can be only one true interpretation of the facts which is arrived at empirically; and a willingness to let subjective factors rule supra-empirical order discussion. Even so, this interchange seems to exhibit a general shift in the <u>order</u> of its concerns. Beginning with a discussion of the interpretation of particular empirical evidence, the concern shifts to the nature of interpretation itself--what are 'facts', 'theories', 'laws', 'proof'--and then amidst the barrage of subjective factors periodic concern for matters of a more philosophical order such as the nature of biology as a science and as a body of scientific knowledge. Perhaps the strongest support then for the relevance of Woodger's critique comes from the realization that throughout this interchange at no time was it suggested that a separate platform was needed for the discussion of concerns of a theoretical order even while the interchange "failed" for the very lack of such a platform. In the end Woodger would probably say, "Of course it failed, they had no language with which to communicate and none had been trained in the techniques of theorizing." His later writings were devoted to developing such a language as well as providing instruction in the logical techniques of theorizing.

Epilogue

In the preface to his 1937 book[244] Woodger expressed his aim there as "provid[ing] an exact and perfectly controllable language by means of which biological knowledge may be ordered." Such a goal was desirable

> . . .because if we have a perfect language we need not dispute, we need only calculate and experiment. From the standpoint of recent investigations into the nature of the exact sciences [e.g. Principia Mathematica] we can see the application of their methods to biology or any other branch of natural science consists in creating a scientifically perfect language in which calculation is possible--one in which we cannot deceive ourselves or others because nothing is concealed and only scientific ends are served.[245]

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[244] J. H. Woodger, The Axiomatic Method in Biology (Cambridge: Cambridge University Press, 1937).

[245] Ibid., p. vii-viii.

The book begins with a brief resume of the logic of Principia Mathematica and then develops an axiomatic system from ten undefined biological "signs": P (space-time part of); T (precedes in time); org (organized unities); U (gives rise to by one act of division or fusion); cell (cells); m (male gametes); f (female gametes); wh (whole organism); Env (is the environment of); and genet (genetic properties). Through the rest of the book Woodger explicitly redefines standing biological concepts in terms of these signs and by manipulating these formulae Woodger builds up more complex notions. This he does for discussions in genetics, embryology and taxonomy, aiming toward the logical structure of those disciplines. Equally important in this book, Woodger institutes the Whiteheadian, revisionary conceptual framework that he developed verbally in Biological Principles. This book thus demonstrates the utility of that revised conceptual framework as well as the utility of the axiomatic or "logistic" method for biology.

Reviews of Axiomatic Method in Biology were generally favorable though not surprisingly logicians spoke only of its logic while biologists spoke only of its biology.[246] Thus it seems that no reviewer was

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[246] E. g. review by a biologist: J. B. S. Haldane, review of The Axiomatic Method in Biology, by J. H. Woodger, in Nature 141(1938):265-266; review by a logician: F. B. Fitch, review of The Axiomatic Method in Biology by J. H. Woodger, in the Journal of Symbolic Logic 3(1938):42-43.

really certain what it was that Woodger had accomplished. Partly in response to this and for other reasons as well, in 1939 Woodger published a monograph entitled The Technique of Theory Construction.[247] It presented the same material of the 1937 book but in a less "terrifying" manner. Woodger used only essential symbolization and put equal emphasis on factors of biological methodology. It received at least one very favorable review by a biologically-oriented philosopher.[248]

In the years following the 1939 monograph Woodger began to de-emphasize the goal of formalization (i.e. the necessity of stating fully the logical syntax and semantics of a theory) and became more interested in what he called "general scientific methodology" or "criticism." Woodger's Tarner Lectures at Cambridge in 1949-1950[249] were devoted to explicating the method and concern of this "new science." Criticism while connected with biological problems deals with the subject matter of biology only indirectly. Instead of "talking biology" it is a "talking *about* biology." Woodger described it from two

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[247] J. H. Woodger, The Technique of Theory Construction, in International Encyclopedia of Unified Science, vol II, No. 5, ed. by Otto Neurath, Rudolph Carnap and Charles Morris (Chicago: University of Chicago Press, 1939).

[248] T. A. Goudge, "Science and Symbolic Logic," Scripta Mathematica 9(1943):69-80.

[249] J. H. Woodger, Biology and Language (Cambridge: Cambridge University Press, 1952).

points of view, first "as an activity connected with the pursuit of biology--one that is rendered necessary by the fact that biology is a growing subject, and its growth is not a simple matter of accretion, but involves frequent readjustment between theory and observation." Secondly, it is "an application to biology of a distinct branch of science which is itself much more general. For it is clear that questions about particular biological hypotheses and theories raise, or are themselves, instances of much wider questions such as: What sort of object is a hypothesis? Exactly what part do hypotheses play in natural science? . . . How do we decide whether one hypothesis is compatible with another?" Thus, "the investigation of these wider questions is the task of a distinct science which I will call general scientific methodology. What I have called criticism in biology can be viewed as an application of this science to biology and can be called biological methodology."[250] Woodger deals principally with the methodological problems of genetics but also discusses to some degree neurology and medical psychology. His treatment here while less logically rigorous is more analytically rigorous as it considers both semantic and syntactic habits of biology in its use of natural language.

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[250] Ibid., p. 4-5.

This seems to be the same concerns that have involved Woodger for a long time, but now he would give them a new name and a totally independent status. Again it is natural language which is the culprit and a symbolic, logical language is developed "to eliminate an enormous amount of unnecessary verbiage and to avoid the pitfalls to which the use of natural language exposes us." Thus throughout this book Woodger both "smokes out" the logical pitfalls of natural language in its use by biologists and develops a logical scheme for ordering the levels of biological statements (e.g. observational descriptions, hypothetical propositions) according to their scope and degree of abstraction.

How successful was Woodger in his goal of developing a technical language and method for a theoretical order of discussion in the biological enterprise? In the end, that is to say presently, we must admit that his success has not been great. The reasons for this are no doubt as complicated as the interweaving development of biology and philosophy.[251] We might attribute part of the reason to changes

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[251] Could this be due to the lack of an apparent "main task" or "central thrust" to Woodger's work? The festschrift devoted to him (see footnote 27, p. 13 above) contains papers on a wide variety of topics, philosophy of science, model theory, logical analysis, semantics, and analytic biology. One could construe this diversity as reflecting the many directions of Woodger's work. One might also construe it as indicating a lack of understanding of Woodger's overall task and thus celebrating areas which he investigated only in search of methods for his task.

in philosophical and scientific fashions, as in the falling out of fashion of the logical positivism with which Woodger associated himself in his later work. Also, while Woodger could demonstrate the utility of the logistic approach for biology, his discoveries were always couched in the logical symbolization comprehensible only to those initiated in the intricacies of mathematical logic. This indeed has been a continuing criticism of his work, at least by biologists who find logical symbolism a brick wall to comprehension. In his writings up to and including Biology and Language Woodger constantly reiterated the need for revisions in the training of biologists, it was now apparent what sort of "logical" revisions he had in mind. Thus biologists who found logic incomprehensible and even mathematics distasteful were not likely to be enthusiastic about making such changes in the biological curriculum. If Woodger had published only the results of his logical investigations and these in natural language, say his critics, then his work would no doubt have been more appreciated. Woodger's response would probably have been that this kind of comment merely reflects a gross ignorance of the advantages and the very spirit of the logistic approach. Thus in the end most biologists will not learn logic and Woodger will not violate the very premises of his work just for the sake of being comprehensible.

Perhaps it is the slowness of change in the biological enterprise, or the lure of physical theories and methods which keep biologists from the task of developing theories and methods consistent with and commensurate to the "biological way of thinking." If so, then Woodger is merely ahead of his time. But there is yet another, albeit more negative possibility. With regard to what was said above about the change in interests and intents in philosophy and science, Woodger's hope that others would build on the foundations he secured[252] is without basis since his task is no longer a viable one.[253] Which of these views one takes is I think contingent upon how transparent one finds logical formulations. Neither the slowness of communal change nor the claim of irrelevance, however, can dim the achievement of Woodger's insights. I think Edmund Sinnott expressed the best attitude toward this when he said,

> Let us never grow so pedantic that we shall frown on any brother who occasionally goes off the reservation of biological orthodoxy to refresh himself in other fields. He may bring back from his excursion a treasure which those who stay home can never find.[254]

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[252] Private communication, April 1972.

[253] This conclusion requires a "geographic" qualification. So far as contemporary Anglo-American philsophy of science is concerned, Woodger's task is no longer interesting or viable. To contemporary Polish analytic philosophers it is substantially more interesting and viable. Their focus has been one of narrowly focused, logical analyses of syntactical and semantical concerns (H. Skolimowski, Polish Analytical Philosophy (London: Humanities, 1967)).

[254] Sinnott, E., "The Cell and the Problem of Organization," Science 89(1939):44.

BIBLIOGRAPHY

Works of J. H. Woodger

1. Books

Elementary Morphology and Physiology for Medical Students. Oxford: Oxford University Press, 1924.

Biological Principles: A Critical Study. Routledge and Kegan Paul, 1929.

The Axiomatic Method in Biology. Cambridge: Cambridge University Press, 1937.

"The Technique of Theory Construction." International Encyclopedia of Unified Science, Vol II, No. 5, ed. by Neurath, O., Carnap, R., and Morris, C. Chicago: University of Chicago Press, 1939.

Biology and Language: An Introduction to the Methodology of the Biological Sciences Including Medicine. The Tarner Lectures for 1949-1950. Cambridge: Cambridge University Press, 1952.

Physics, Psychology and Medicine: A Methodological Essay. Cambridge: Cambridge University Press, 1956.

2. Translations

Von Bertalanffy, L. Modern Theories of Development: An Introduction to Theoretical Biology. Oxford: Oxford University Press, 1933.

Mainx, F. "Foundations of Biology." International Encyclopedia of Unified Science, Vol I, No. 9, ed. by Neurath, O., Carnap, R., and Morris, C. Chicago: University of Chicago Press, 1955.

Tarski, A. Logic, Semantics, and Metamathematics. Oxford: Oxford University Press, 1956.

3. Articles

"On the Relationship between the Formation of Yolk and the Mitochondria and Golgi Apparatus during Oogenesis." *Journal of the Royal Microscopical Society*, 1920, 129-156 (with J. B. Gatenby).

"Notes on a Cestode Occurring in the Haemocoele of Horseflies in Mesopotamia." *Annals of Applied Biology*, 7(1921), 345-351.

"On the Origin of the Golgi Apparatus on the Middle-Piece of the Ripe Sperm of Cavia." *Quarterly Journal of Microscopical Science*, 65(1921), 265-291.

"Observations on the Origin of the Germ-Cells of Fowl, Studied by Means of Their Golgi Bodies." *Quarterly Journal of Microscopical Science*, 69(1925), 445-462.

"Some Problems of Biological Methodology." *Proceedings of the Aristotelian Society*, 29(1929), 331-358.

"The 'Concept of Organism' and the Relation between Embryology and Genetics." *Quarterly Review of Biology*, 5(1930), I:1-22; II:438-463; and III:6(1931), 178-207.

"The Early Development of the Skull of the Rabbit." *Philosophical Transactions of the Royal Society*, Series B, 218(1930), 373-414 (with G. De Beer).

"Mr. Russell's Theory of Perception." *The Monist*, 40(1930), 621-636.

"Some Apparently Unavoidable Characteristics of Natural Scientific Theory." *Proceedings of the Aristotelian Society*, 32(1932), 95-120.

"A Simple Method of Testing Truth Functions." *Analysis*, 3(1936), 92-96 (with W. F. Floyd).

"The Formalization of a Psychological Theory." *Erkenntnis*, 7(1938), 195-198.

"The Origin of the Endoderm in the Sparrow." *Biomorphosis*, (1938) (with J. P. Hill).

"Remarks on Method and Technique in Theoretical Biology." Growth (Supplement), 1940, 97-99.

"Notes on the First Symposium on Development and Growth." Growth (Supplement), 1940, 101-111.

"On Biological Transformations." In Essays on Growth and Form, ed. by L. G. Clark. Oxford: Oxford University Press, 1945.

"Observations on the Present State of Embryology." Society for Experimental Embryology Symposium II, Growth, 1948.

"Toward an Inscriptional Semantics." Journal of Symbolic Logic, 16(1951), 191-203 (with R. M. Martin).

"Science without Properties." British Journal for the Philsophy of Science, 2(1952), 193-216.

"Problems arising from the Application of Mathematical Logic to Biology." In Applications Scientifiques de la logique mathematique (Actes du 2e Collogue Internationale de Logique Mathematique, Paris, 1952), 133-139.

"What do we mean by 'Inborn'?", British Journal for the Philosophy of Science, 3(1953), No. 12.

"From Biology to Mathematics." British Journal for the Philosophy of Science, 3(1953), 1-21.

"Proper Objects." Mind, 65(1956), 510-515.

"Formalization in Biology." Logique et Analyse 1(1958), 3-4.

"Studies in the Foundations of Genetics." In The Axiomatic Method, with Special Reference to Geometry and Physics, Berkeley, 1957-1958; Amsterdam, 1959.

"Biology and Physics." British Journal for the Philosophy of Science, 11(1960), 42, 89-100.

"Taxonomy and Evolution." La Nuova Critica, Series 3(1961), No. 12, 67-78.

"Biology and the Axiomatic Method." Annals of the New York Academy of Science, 96(1962), 1093-1104.

"Abstraction in Natural Science." In Logic, Methodology and the Philosophy of Science, Stanford: Stanford University Press, 1962.

"Theorems on Random Evolution." Bulletin of Mathematical Biophysics, 27(1965), 145-150.

"Many-Termed Relations in Biology." Acta Biotheoretica, 18(1968), 125-132.

4. Letters

"Biological Principles." Letter to the Editor Mind, 39(1930), 403-405.

"A Biologist Answers." Letter to the Editor Methodos, 2, no. 5 (1950), 31.

"Mental Health and the Basic Sciences." British Medical Journal, 1955, 419-420.

"Reply to Professor [J. B. S.] Haldane." British Journal for the Philosophy of Science. Notes and Comments VI, 1955, 245; Notes and Comments VII, 1956, 149-155.

"Time and the Nervous System." Lancet, 2(1958), 44.

5. Reviews

Review of The Study of Living Things, by E. S. Russell. Science Progress, 19(1924), 675-679.

Review of The Cell in Development and Inheritance, 3rd ed., by E. B. Wilson. Science Progress, 20(1925), 160.

Review of Living Organisms, by E. S. Goodrich. Science Progress, 20(1925), 326-337.

Review of *Experimental Embryology*, by G. De Beer. *Science Progress*, 21(1926), 692-703.

Review of *Animal Biology*, by J. B. S. Haldane and J. S. Huxley. *Science Progress* 22(1927), 336-339.

Review of *Elements of General Zoology*, by W. J. Dakin. *Science Progress*, 22(1927), 344-346.

Review of *Creation by Evolution*, ed. by F. Mason. *Science Progress*, 25(1929), 554-556.

Review of *The Sciences and Philosophy*, by J. S. Haldane. *Science Progress*, 26(1930), 522-534.

Review of *The Interpretation of Heredity and Development*, by E. S. Russell. *Science Progress*, 26(1941), 306-324.

Review of *The Genetical Theory of Natural Selection*, by R. A. Fisher. *Science Progress*, 27(1931), 721-722.

Review of *The Natural History of Mind*, by A. D. Ritchie. *Mind*, 45(1936), 399.

Review of *Modern Science and Its Philosophy*, by Phillip Frank. *British Journal for the Philosophy of Science*, 2(1951), 6, 168.

Review of *The Origins of Modern Science*, by Herbert Butterfield. *British Journal for the Philosophy of Science*, 1(1950), 4, 332.

Review of *Einfuhrung in die Logistik, vorzuglich in den Modalkalkul*, ed. by Oscar Becker. *British Journal for the Philosophy of Science*, 2(1951), 8, 337.

Review of *Essays on Logic and Language*, ed. by Anthony Flew. *British Journal for the Philosophy of Science*, 2(1951), 8, 338-342.

Review of *Lecons de logique algebrique*, by B. Curry. *British Journal for the Philosophy of Science*, 3(1952), 11, 293.

Review of *Aristotle's Syllogistic from the Standpoint of Modern Formal Logic*, by Jan Lukasiewicz. *British Journal for the Philosophy of Science*, 4(1953), 15, 251.

Review of Whitehead's Philosophical Development, by Nathaniel Lawrence. British Journal for the Philosophy of Science, 8(1958)), 32, 348.

Review of Einfuhrung in die symbolische Logik, by Rudolf Carnap. British Journal for the Philosophy of Science, 9(1958), 33, 70-72.

Review of The Basic Concepts of Mathematics, by Karl Menger. British Journal for the Philosophy of Science, 9(1958), 34, 172.

Review of Truth and Denotation, by R. M. Martin. British Journal for the Philosophy of Science, 19(1959), 157-159.

Review of Science and Persons, Personal Knowledge: Towards a Postcritical Philosophy, by Michael Polanyi. British Journal for the Philosophy of Science, 11(1960), 41, 65-71.

General Sources

Ashby, E. Letter to the Editor. Nature, 119(1927), 457.

Beckner, M. O. "Mechanism in Biology." Encyclopedia of Philosophy V(1965), 250-252.

Churchill, F. B. "From Machine Theory to Entelechy: Two Studies in Developmental Teleology." Journal of the History of Biology, 2(1969), 165-185.

Cunningham, J. T. Letter to the Editor. Nature, 119(1927), 563.

Drummond, J. C. "Biochemistry." Encyclopedia Britannica III (14th ed.), 589-593.

Dunkerly, J. S. Letter to the Editor. Nature, 120(1927), 12-13, 191.

Dunn, L. C. A Short History of Genetics: 1864-1939. New York: McGraw-Hill, 1968.

Emmet, D. M. "Whitehead, Alfred North." Encyclopedia of Philosophy VIII(1965), 290-296.

Enciclopedia Universal Ilustrada, 1922. s.v. "Przibram, Juan."

________________. s.v. "Schaxel, Julio."

Gatenby, J. Bronte. "The Cytoplasmic Inclusions of Germ Cells. I. Lepidoptera." _Quarterly Journal of Microscopal Science_, 62(1917), 407-463.

________________. "The Cytoplasmic Inclusions of Germ Cells. II. _Helix aspersa_." _Quart. J. Micro. Sci._, 62(1917), 555-611.

________________. "The Cytoplasmic Inclusions of Germ Cells. III. On Spermatogenesis of Some Pulmonates." _Quart. J. Micro. Sci._, 63(1918), 197-258.

________________. "The Cytoplasmic Inclusions of Germ Cells. IV. On the Dimorphic Spermatozoa of Paludina." _Quart. J. Micro. Sci._, 63(1919), 401-443.

________________. "The Cytoplasmic Inclusions of Germ Cells. V. On the Early Development of _Limaea stagnalis_, with Special Reference to the Golgi Apparatus and the Mitochondria." _Quart. J. Micro. Sci._, 64(1920), 445-491.

________________. "The Cytoplasmic Inclusions of Germ Cells. VI. On the Origin and Probable Constitution of the Germ Cell Determinant of _Apauteles_ and _Glomeratus_." _Quart. J. Micro. Sci._, 64(1920), 133-153.

________________. "The Cytoplasmic Inclusions of Germ Cells. VII. Modern Cytological Technique." _Quart. J. Micro. Sci._, 64(1920), 267-301.

________________. "The Cytoplasmic Inclusions of Germ Cells. VIII. _Grantia compressa_." _Journal of the Linnaen Society_ (1920).

Gatenby, J. Bronte and Woodger, J. H. "On the Relationship between the Formation of Yolk and the Mitochondria and Golgi Apparatus during Oogenesis." _Journal of the Royal Microscopical Society_, (June 1920), 129-156.

________________. "The Cytoplasmic Inclusions of Germ Cells. IX. On the Origin of the Golgi Apparatus on the Middle-piece of the Ripe Sperm of Cavia, and the Development of the Acrosome." _Quart. J. Micro. Sci._, 64(1921), 265-288.

Goudge, T. A. "Science and Symbolic Logic." Scripta Mathematica, 9(1943), 69-80.

Gregg, J. R. and Harris, F. T. C. Form and Strategy in Science: Studies Presented to Joseph Henry Woodger on the Occasion of His Seventieth Birthday. Dordrecht, The Netherlands: D. Reidel, 1964.

Hadfield, J., ed. The Shell Guide to England. New York: American Heritage Press, 1970.

Haldane, J. B. S. Review of The Study of Living Things by E. S. Russell. Nature, 115(1925), 218-219.

________________. Letter to the Editor. Nature, 119(1927), 456-457.

Haldane, J. S. Gases and Liquids: A Contribution to Molecular Physics. London: Oliver and Boyd, 1938.

________________. The Sciences and Philosophy. London: Hodder and Stoughton, 1929.

________________. The Theory of Heat Engines. London: Oliver and Boyd, 1930.

Harbottle, T. Dictionary of Battles. New York: Stein and Day, 1971.

Hein, H. "Mechanism and Vitalism as Meta-theoretical Commitments." Philosophical Forum, 2(1969), 185-205.

Hopkins, F. G. "Some Chemical Aspects of Life." Nature (Supplement), 129(1933), 381-394.

Huxley, J. S. Review of The Physiology of the Continuity of Life, by D. N. Paton. Nature, 118(1926), 902-905.

________________. Letter to the Editor. Nature, 119(1927), 159-160; 350; 639.

Jennings, H. S. "Biology and Experimentation." Science, 64(1926), 97-105.

Johnstone, J. Letter to the Editor. Nature, 119(1927), 319.

Lawrence, N. Whitehead's Philosophical Development. Berkeley: University of California Press, 1956.

Lazurus-Barlow, W. S. "Vivisection." Encyclopedia Britannica XXIII (14th ed.), 227-228.

_______________. Review of Experimental-Zoologie, Bd VI. Zoonomie, by Hans Przibram. Nature 126(1930), 639-643.

MacDougall, W. The Riddle of Life: A Survey of Theories. London: Methuen, 1938.

Marshall, F. H. A. Letter to the Editor. Nature, 119(1927), 563.

_______________. Review of Experimental-Zoologie, Bd. II. Regeneration, by Hanx Przibram. Nature 81(1909), 61.

Mays, W. "The Relevance of 'On Mathematical Concepts of the Material World'." In The Relevance of Whitehead, ed. by Le Clerc, I. London: George Allen and Unwin, 1961.

Morgan, C. Lloyd. "Animal Behaviour." Encyclopedia Britannica I (14th ed.), 960-963.

Morgan, T. H. "Edmund Beecher Wilson, 1856-1939." National Academy of Sciences, Biographical Memoires, 21(1941), 318-320; 335.

Palter, R. "Science and Its History in the Philosophy of Whitehead." In Process and Divinity, ed. by Reese, W. La Salle: Open Court, 1964.

Passmore, J. A Hundred Years of Philosophy. London: Duckworth, 2nd ed., 1966.

Paton, D. N. Letter to the Editor. Nature, 119(1927), 159.

Przibram, H. "Transplantation and Regeneration: Their Bearing on Developmental Mechanics (A Review of the Experiments and Conclusions of the Last Ten Years: 1915-1924)." Journal of Experimental Biology, 3(1925), 313-330.

Quinton, A. "British Philosophy." Encyclopedia of Philosophy I(1965), 386-396.

Review of Experimental-Zoologie, Bd. I. Embryogenesis, by Hans Przibram. Nature, 77(1908), 529.

Review of Elementary Morphology and Physiology for Medical Students, by J. H. Woodger. Lancet, 208(1925), 978.

Review of Elementary Morphology and Physiology for Medical Students, by J. H. Woodger. Science Progress, 19(1925), 704-705.

Review of Experimental Zoologie, Bd IV. Vitalitat, by Hans Przibram. Nature, 126(1930), 639-643.

Robischon, T. "New Realism." Encyclopedia of Philosophy V(1965), 485-489.

Rorty, R. M. "Relations, Internal and External." Encyclopedia of Philosophy VII(1965), 125-132.

Russell, E. S. The Study of Living Things. London: Methuen, 1924.

Singer, C. "History of Biology." Encyclopedia Britannica III (14th ed.), 617-618.

Skolimowski, H. Polish Analytical Philosophy. London: Humanities Press, 1967.

Tatarkiewicz, W. Twentieth Century Philosophy: 1900-1950. Belmont, California: Wadsworth Publishing Company, 1973.

Thomson, H. C. The Story of the Middlesex Hospital Medical School: 1835-1935. London: Murray, 1935.

Thomson, J. Arthur. "Biology." Encyclopedia Britannica III (14th ed.), 602-609.

Waddington, C. H. "Twenty-five Years of Biology." Discovery, 16(1935), 134-137.

Walker, C. Letter to the Editor. Nature, 119(1927), 161; 456; 814-815; 120(1927), 118-119.

Warnock, G. T. English Philosophy Since 1900. London: Oxford University Press, 1958.

Watson, D. M. S. "James Peter Hill, 1873-1954." Royal Society of London, Biographical Memoires, 1(1958), 101-115.

Wheeler, R. L. Vitalism: Its History and Validity. London: H. F. and G. Witherby, 1939.

Whitehead, A. N. and Russell, B. Principia Mathematica, 3 vols. Cambridge: Cambridge University Press, 1910-1913.

Wilson, E. B. "Aims and Methods of Study in Natural History." Science, 13(1901), 14-23.

________. "Some Recent Studies on Heredity." Harvey Lectures, 2(1906/07), 200-222.

________. The Cell in Development and Inheritance. New York: MacMillan, 1896 (1st ed.); 1900 (2nd ed.); 1925 (3rd ed.).

Woodger, J. H. Private Communication. April 29, 1972.