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# WOODGER ON GENETICS
# A CRITICAL EVALUATION

by

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SUMMARY

A critical analysis of Woodger's work on formal logic in biology, especially genetics, reveals that the claim for the value of such methods in genetics is misplaced.

For a number of years J. H. WOODGER has argued that biologists should read and borrow from the work of formal logicians. (WOODGER, 1937, 1939, 1952, 1959) We find, moreover, that a number of thinkers seem to agree with WOODGER that a biology refined with a stiff dose of formal logic (after the manner of WOODGER himself) is the best kind of biology, if one is to have any kind of systemized biological theory beyond the random jottings of natural historians. Thus, for example, the biological chapter of H. KYBURG's book, *The Philosophy of Science: A Formal Approach,* is devoted to an exposition and discussion of a formalization and derivation by WOODGER of MENDEL's first law of genetics. KYBURG's chapter has an impeccable precedent since no less a person than RUDOLF CARNAP in his *Introduction to Symbolic Logic and its Applications* also developed an axiom system for a piece of biology after the fashion of WOODGER. D. WIGGINS in the introduction to his widely acclaimed monograph, *Identity and Spatio-Temporal Continuity,* bemoans the fact that there seems to be no recent work "... worthy to succeed the seminal writings of J. H. WOODGER ..." (WIGGINS, 1967, p. viii) In a very recent book on the philosophy of biology, M. A. SIMON (1971) tells us that if biology is to stand on its own two feet, then it must be axiomatized, and he tells us also that: "The only portion of biology that has thus far been completely axiomatized is classical genetics, which J. H. WOODGER has done using the formal apparatus of WHITEHEAD and RUSSELL's *Principia Mathematica.*" (SIMON, 1971, p. 26) And finally we can mention C. G. HEMPEL (1958) who tells us that formally scientific theories can be considered as axiomatic systems; but, for HEMPEL, in biology only WOODGER's work seems to fit this scientific ideal.

What is perhaps more interesting than this honouring of WOODGER's work is the fact that even a philosopher who has criticized WOODGER fiercely seems to view biology in part through Woodgerian spectacles. J. J. C. SMART at one point attacked an early work by WOODGER[1]; but in his more recent writings we find that SMART is still affected by WOODGER in the sense that he (SMART) seems to think that WOODGER's approach to biology is the definitive formal approach to biology. SMART writes:

> Both biologists and philosophers have frequently wondered why biology does not seem to have the precision and close-knit theories which we find in physics and chemistry. Sometimes they hope that in future biology will be brought into such a precise and unified form. Partly for this reason J. H. WOODGER has tried to axiomatise genetics. However, there has been a very odd look about such attempts to treat a biological discipline on the model of a close-knit physical theory. (SMART, 1963, 50)

Unfortunately, partly because he seems to think WOODGER's way to be the only way to produce formal biological systems, SMART concludes that he doubts the existence of any axiomatic biological theories at all.

> Writers who have tried to axiomatise biological theories seem to me to be barking up the same gum tree as would a man who tried to produce the first, second, and third laws of electronics or of bridge building . . . The writers who have tried to axiomatise biology . . . have wrongly thought of biology . . . as a science of much the same logical character as physics, just as chemistry is. I shall try to show that the important analogy is not between biology and the physical sciences but between biology and the technologies, such as electronics. (SMART, 1963, 52)

Given this identification of WOODGER's work by some thinkers, with what is significant and worthy of attention in formal biology (or what would be significant and worthy of attention if anything were significant and worthy of attention in formal biology), it seems worthwhile to attempt a critical analysis of the results of the labours of WOODGER. This task I shall attempt in this paper. More particularly, I shall discuss WOODGER's work on genetics (thus avoiding any overlap with SMART who criticized WOODGER's cytological endeavours, and that only incidentally as a general attack on formal methods in science). It will, simply, be my claim that there is little if anything of value in WOODGER's work, and that therefore the time has now come to draw a decent veil over a biological dead-end.

## 1. WOODGER *on genetics*

WOODGER's major attack on the problems of genetics is to be found in his book *Biology and Language* (1952) and, more recently, in a paper

[1]) See SMART (1953).

"Studies in the Foundation of Genetics" (1959). In the book, WOODGER does not attempt to axiomatise genetics; but he deals with some of the problems which would be encountered in such an attempt. In the paper, WOODGER actually supplies a piece of formalism—one which ends with the derivation of MENDEL's first law. This might not seem like a very great achievement, but the claims that WOODGER makes for what he is doing are, to say the least, not unduly modest, for at the end of the major biological analysis of the book he writes:

> If I have seemed too fussy over details, or if I have confined myself to topics which geneticists have long left behind (as my ignorance compels me to do), I would remind you that we are laying the foundations of a new science of genetical methodology and we must not grudge the effort needed to make them secure. (WOODGER, 1952, p. 200)

Perhaps the thing which strikes most forcefully on the reader new to WOODGER's work is the extent to which his practice and results are so strongly influenced by, what is best described as, a stringent empiricism. One of WOODGER's overriding forces seems to be the desire to express genetical truths by straying as little as possible from the path of plain, unadulterated, observation statements. If one can avoid talk of cells in favour of talk of short or tall pea-plants, then one should. If one can avoid talk of chromosomes in favour of talk of cells, then one should. Above all, if one can avoid talk of genes, then one most definitely should. Moreover, WOODGER makes no attempt to hide his empiricism—indeed, he is quite explicit about his underlying philosophy. In his paper, he introduces the notion of "epistemic priority," and about this he writes as follows:

> A theory in natural science is like an iceberg—most of it is out of sight, and the relation of epistemic priority holds between a statement A and a statement B when A speaks about those parts of the iceberg which are out of water and B about those parts which are out of sight; or A speaks about parts which are only a little below the surface and B about parts which are deeper. In other words: A is less theoretical, less hypothetical, assumes less than B. (WOODGER, 1959, p. 412)

He then goes on to say:

> If what you want to say can be expressed just as well by a statement A as by a statement B then, if A is epistemically prior to B, it will (if no other considerations are involved) be better to use A. In what follows I shall try to formulate all the statements concerned in the highest available epistemic priority. Statements concerning parents and offspring only are epistemically prior to statements which also speak about gametes and zygotes; and statements about gametes and zygotes are epistemically prior to statements which speak also about the parts of gametes and zygotes. The further we go from the epistemically prior inductive hypotheses the more we are taking for granted and the greater the possibility of error. The following discussion of Mendel's first law will be in terms of parents, offspring, gametes, zygotes and environments. (WOODGER, 1959, p. 413)

In order to give the reader a flavour of WOODGER's approach, let me discuss briefly his treatment of the concept of "genotype" in his book, with an even briefer digression to show his method of argument in his paper. Most biologists when they refer to an organism's "genotype" refer, in some sense, to the genetic make-up of the organism. They refer to the units of heredity of the organism, those things which make the organism what it is—tall, short, red, yellow, man, wolf. Thus, for example, the geneticist TH. DOBZHANSKY writes: "The sum total of genes of an individual or a population constitutes the genotype." (DOBZHANSKY, 1951, p. 20). WOODGER, however, means by "genotype" something very different. He means a specified set of organisms (usually called "phenotypes" by biologists) which all behave the same way in breeding tests. Thus his concept is, in his terminology, very much ahead of the normal concept in epistemic priority.

Now, informally speaking, the way in which one would treat such a (Woodgerian) notion of genotype is not that difficult to see. Suppose, for example, one were considering pea-plants and one wanted to introduce the notion of plants which had yellow seeds and which, when bred to each other, gave yellow-seeded offspring (for all future generations). Normally, one would speak of such plants having genotypes "homozygous" for the gene for yellow seed colour, and by this, one would mean that each somatic cell of the plants carries a pair of identical genes (for yellow seed colour). WOODGER would also speak of such plants as being "homozygous" for yellow colour, but for him this would mean (and be defined purely in terms of) actual breeding results. If a plant always has yellow-seeded descendants, it is homozygous for yellow-seededness—otherwise not.

WOODGER's formal treatment of the concept of "genotype" is (to my mind) more complex; but it is worthwhile for us to try to follow it through. We start with the following semantical rule:

'$\mathbf{F}_{X,\,Y,\,Z}\,(W_1,\,W_2)$' denotes the set of all offspring $x$ such that for some $u$ and some $v$, $u \leftarrow W_1$ and is a parent of $x$, $v \leftarrow W_2$ and is a parent of $x$, and $u$ has developed in an environment belonging to the set $X$, $v$ in an environment belonging to the set $Y$ and $x$ in an environment belonging to $Z$.

Roughly speaking, this functor gives us the offspring of $W_1$'s mated to $W_2$'s. Obviously, normally the parents and offspring will develop in the same environment, and so for simplicity when $X = Y = Z$ we can write:

$$\mathbf{F}_X\,(W_1,\,W_2) = \mathbf{F}_{X,X,X}\,(W_1,\,W_2)$$

Applying the functor twice, for the second generation of offspring (*i.e.* for the grandchildren of the original $W$'s) we can write:

$$\mathbf{F}^2{}_X\,(W_1,\,W_2) = \mathbf{F}_X\,(W_1,\,W_2)\,\mathbf{F}_X\,(W_1,\,W_2))$$

And also, with the help of our mating functor, we can formulate statements like this:

$$\mathbf{F}_X (Y, Z) \subset W$$

Where this is to be understood as saying that every member of the set $\mathbf{F}_X (Y, Z)$ is a member of the set $W$.

Now, let $Y$ be the set of all garden peas with yellow cotyledons. We shall write '$K\mathbf{q},_X$ (Z)' (or '$Z$ satisfies the condition $K\mathbf{q},_X$') as an abbreviation for

'$Z \subset \mathbf{Y}$ and $\mathbf{F}_X (Z, Z) \subset \mathbf{Y}$ and $\mathbf{F}^2_X (Z, Z) \neq \Lambda$ and $\mathbf{F}^2_X (Z, Z) \subset$ Y.'

(This condition captures the idea that both first and second generation descendants of yellow-seeded plants, bred to each other, are yellow-seeded plants).

We next write '$\phi(\mathbf{Y},X)$' as an abbreviation for 'there is a $U$ such that $K_{\mathbf{Y},X}(U)$ and for all $Z$ and $W$, if $K_{\mathbf{Y},X}(Z)$ and $K_{\mathbf{Y},X}(W)$ then $K_{\mathbf{Y},X}(Z \cup W)$'. With the help of these abbreviations we now define '$\mathbf{H}(\mathbf{Y},X)$' or 'the homozygous genotype of $\mathbf{Y}$ with respect to the environmental set $X$' as follows:

$\mathbf{H}(\mathbf{Y},X)$ is the Boolean sum of all sets $Z$ such that $K_{\mathbf{Y},X}(Z)$ provided that $\phi(\mathbf{Y},X)$. If $\phi(\mathbf{Y},X)$ does not hold then $\mathbf{H}(\mathbf{Y},X) = \Lambda$.

Next we proceed to *maximize* $X$ by defining the environmental set $\mathbf{E}(\mathbf{Y})$ as follows:

[D. 13] $\mathbf{E}(\mathbf{Y})$ is the Boolean sum of all sets $X$ such that $\phi(\mathbf{Y},X)$. Finally, we define the homozygous genotype $\mathbf{H}(\mathbf{Y})$ of $\mathbf{Y}$, as follows:

[D. 14] $\mathbf{H}(\mathbf{Y}) = \mathbf{H}(\mathbf{Y}, \mathbf{E}(\mathbf{Y}))$.

(WOODGER, 1952, p. 108. '[D. 13]' and '[D. 14]' are the numbers of the definitions in the book).

And this concept, it will be realized, in no way makes reference to that theoretical, unobservable entity, the gene.

Heading up to and going away from the concept of the "genotype" in *Biology and Language*, WOODGER works (up) through a number of "levels". A new level, with its attendant concepts and hypotheses, is introduced only when a problem arrives and proves insoluble at all lower levels. WOODGER starts obviously, with observation records—for example, MENDEL's records of plants in his garden at Brno. Next one has zero level statements—these include statements involving the concept just discussed in detail, the genotype. They go beyond mere observation records because they refer to potentially infinite classes (for example, to speak of plants "homozygous" for yellow-seededness is to speak of all such plants, past, present, and future). Then one has first level statements introducing concepts about "gametes", their "union" (or "fusion"), the product of such a union, a "zygote", and so on.

In his derivation of MENDEL's first law in his paper, WOODGER starts from axioms which are at this first level and, through them, attempts to explain phenomena describable by statements at the zero level. Thus in the paper he offers (amongst others) the following primitives:

(i) '$u\mathbf{F}x$' for '$u$ is a gamete which fuses with another gamete to form the zygote (fertilized egg) $x$'
(ii) '**dlz** $xyz$' for '$x$ is a zygote which develops in the environment $y$ into the life $z$'
(iii) '$u$ *gam* $z$' for '$u$ is a gamete produced by the life $z$'

Then we get a number of definitions, following which we get the assumptions needed "in order to derive the characteristic Mendelian mating descriptions". For example, we have one assumption which translates out as the claim that the set of male gametes produced by lives developing from zygotes formed by the union of a gamete belonging to class $\alpha$ and a gamete belonging to class $\beta$ contain half of type $\alpha$ and half of type $\beta$, and the same holds also for females.

And then, from hypotheses like these, we can derive (zero level) statements. For example, it can be shown that 3/4 of the offspring of hybrids will have dominant type phenotypes, and 1/4 will have recessive type phenotypes.

In *Biology and Language* WOODGER does not stop for lengthy deductions, but pushes on up through second, third, and fourth levels. Second level hypotheses introduce us to parts of cells, in particular, nuclei and cytoplasms. Third level hypotheses introduce us to parts of nuclei, in particular, chromosomes. Then finally, at the fourth level we encounter parts of chromosomes. These are invoked to explain problems involving the breaking and linkage of chromosomes, and they are called (or, at least, some such parts are called) "genes". But notice how it is only when there seems to be no other option that these genes are introduced. Problems soluble by hypotheses at lower-levels do not call for, and are not given, explanations in terms of hypotheses using concepts drawn from the highest level. In this way, the directive that one should maximize epistemic priority influences all of WOODGER's work.

## 2. *Criticism*

Sketchy though this discussion of WOODGER's ideas has been, let us now turn to the problem of providing a critical evaluation. In order to begin, let us ask ourselves a fairly basic question—why should one develop and accept a new scientific theory or a new way of doing science, more specifically, why should one accept a Woodgerian-type genetics? I suppose the obvious answer is that acceptance is demanded because the new science or new genetics will lead to fresh discoveries and solve old problems. Accept, for example, Darwinian evolutionary theory, and the problem of adaptation is a problem no more (or, at least, is much less of a problem). The reason for eyes and arms, breasts and teeth, is no longer hidden. However, it seems hard to see how a case could be made for WOODGER's work based on this kind of discovery. As he himself admits, at best WOODGER arrives at things which everyone has long known, like MENDEL's first law. (This is an advance on some of WOODGER's earlier excursions into axiomatic biology. In *The Technique of Theory Construction* (1939), at great effort WOODGER proves that no cell arises both by division and fusion. Admittedly this earlier work is supposed only to illustrate the virtues of the axiomatic method; but, as SMART (1953) asks with glee, what virtue is there in a 40 page proof of that conclusion?)

Nevertheless, one might want to argue (against SMART) that the discoveries WOODGER's work leads to are of a more subtle nature than discoveries found by ploughing straight ahead. These discoveries are discoveries of things and problems of which we were formerly only half consciously aware, but when we are made fully consciously aware of them, we see as being very important. In fairness, it does seem to be in this kind of sense that WOODGER really thinks his method leads to discovery. At least, it is in this sense that he makes one of the strongest pitches for his endeavour. At the beginning of his paper he writes:

> Modern genetics owes its origin to the genius of MENDEL, who first introduced the basic ideas and experimental procedures which have been so successful. But it is time to inquire how far the Mendelian hypotheses may now be having an inhibiting effect by restricting research to those lines which conform to the basic assumptions of MENDEL. It may be profitable to inquire into those assumptions in order to consider what may happen if we search for regions in which they do not hold. The view is here taken that the primary aim of natural science is discovery. Theories are important only in so far as they promote discovery by suggesting new lines of research, or in so far as they impose an order upon discoveries already made. (WOODGER, 1959, p. 408)

Thus, WOODGER seems to argue, his genetics will let us break from the shackles of the past, and through this break, new unsuspected problems will arise, leading in turn to hope of significant biological discovery.

But what kinds of assumptions are geneticists making from which WOODGER would liberate them? At the end of his derivation of MENDEL's first law, WOODGER mentions two. Let us take them in turn. First, we get the notion of randomness.

The above analysis has shown the central role which is played by the hypotheses of random union of the gametes and of random development in obtaining the Mendelian ratios . . . These do not receive the attention they deserve in genetical books. Sometimes they are not even mentioned. This is particularly true of the hypothesis of random development. (WOODGER, 1959, p. 427)

I must confess that this complaint of WOODGER puzzles me a little. I am not quite sure how one could present and understand MENDEL's first law without talking about the random way in which the genes (or gene carriers) are transmitted. That of any pair of genes carried by the parent, one and only one will be randomly chosen for transmission seems to be the essence of the law. Moreover, as far as WOODGER's complaint about random development is concerned, this, if anything, seems to back-fire on his programme. WOODGER is supposed to be showing us things which Mendelian geneticists have missed; but WOODGER himself admits that none other than the earliest Mendelian of them all, GREGOR MENDEL, brought up the very point about random development which WOODGER claims to unearth. MENDEL wrote:

A perfect agreement in the numerical relations was, however, not to be expected, since in each fertilization, even in normal cases, some egg cells remain undeveloped and subsequently die, and many even of the well-formed seeds fail to germinate when sown. (Quoted by WOODGER, 1959, p. 427)

Furthermore, modern geneticists seem no less aware of this point. For example, several times in his important work *Genetics and the Origin of Species*, TH. DOBZHANSKY (talking of fruit flies) explicitly acknowledges that if one does not have random development, the proportions of the different types "should be different among the eggs and among adult flies." (DOBZHANSKY, 1951, p. 117)

The second thing WOODGER mentions is the question of the environment.

. . . perhaps the most striking feature of the Mendelian systems is the fact that only one class of environments is involved and is usually not even mentioned. Some interesting discoveries may await the investigation of multi-environmental systems. (WOODGER, 1959, p. 427)

Such interesting discoveries may indeed lie around the corner; but, it is hardly the case that conventional geneticists are ignorant of the effects of the environment (on development). Consider the following discussion which follows the introduction of the concepts of phenotype and genotype in a recent elementary textbook.

It is important to realise that an adult animal is the result of the interaction during development of the genes and the environment. If MENDEL's tall pea plants had been grown under poor conditions while the short ones had been grown in the very best environment, the phenotypic appearance of the two could have been very similar. In conducting experiments on heredity it is therefore of paramount importance that, when comparing two or more types, they should be reared under identical conditions. It is this requirement that makes the study of human heredity so difficult. (GEORGE, 1964, p. 46)

Nor is it the case that there is something perverse in the admitted fact that geneticists, particularly population geneticists, tend to ignore the environment in their calculations. The problems of genetics are so complex that, so far, they have just had to make simplifying assumptions, particularly about the environment. But what scientist does not make simplifications?

Finally, for all that he says about the importance of the environment (both in his paper and elsewhere), it is hardly as if WOODGER makes a significant advance in bringing environmental considerations within the structure of genetics. He mentions the environment, and then just lets it drop. For example, in *Biology and Language*, after introducing the functor $\mathbf{F}_{X,\,Y,\,Z}\,(W_1,\,W_2)$ which mentions the three environments $X$, $Y$, and $Z$, as we have seen WOODGER immediately collapses the three environments into one, defining

$$\mathbf{F}_X\,(W_1,\,W_2) = \mathbf{F}_{X,X,X}\,(W_1,\,W_2)$$

and, from then on, he works almost exclusively with this simpler functor. Thus, I would suggest that WOODGER's claims about the value of his approach for genetical discoveries is, on the case he makes, not proven.

What other reasons could one have for embracing a Woodgerian approach? MARY WILLIAMS suggests that one of the chief virtues of an axiomatic approach is that it leads to a unification of scientific ideas. She writes that "NEWTON's impact was not due to his discovery of previously unsuspected laws . . . but was due to his synthesis, from the chaotic mass of known or suggested principles of a coherent deductive theory . . ." (WILLIAMS, 1970, p. 345) But, if anything, WOODGER seems to take us away from a unification. Instead of explaining all cases of heritable transmission in terms of the one set of entities, genes, (as most biologists do), WOODGER explains different phenomena in different ways. Some things are explained in terms of gametes, some in terms of chromosomes, and only some in terms of genes. I see no synthesis here—in fact, the opposite is the case.

WOODGER himself suggests that his approach will be valuable for readers of general treatises and elementary textbooks on genetics. (WOODGER, 1952, p. 178-80) He thinks that in such works too much emphasis is placed on diagrams, and he thinks also that a formal approach will eliminate the ambiguities and difficulties so irritating to the reader unversed in genetic knowledge. Again, I must confess I find myself unconvinced. My experience is that few things frighten the average student more than a string of symbols. Even KYBURG feels called upon to say, when presenting the reader with definitions that take nearly twenty inches of small printed type, that "it is therefore important to struggle through their complexity and to arrive at a clear understanding of what they say." (KYBURG, 1968, p. 287) Of course, complexity is a fairly subjective matter, but if, in addition to KYBURG, such a biological expert as G. G. SIMPSON confesses to finding WOODGER's approach uphill, I doubt the attractiveness of the approach to beginners. (SIMPSON, 1961, p. 22) Moreover, for all that he says about diagrams, both WOODGER and his followers revert to them at times[2]). For whose benefit are these intended to be, if not for the learner?

KYBURG, at the beginning of his book, suggests that a Woodgerian approach might be needed to avoid talk of vital forces and other such outmoded ideas. He writes:

> Thus it might be maintained that although in modern scientific biology no concept of a vital force is required, and although no such concept is presupposed by the biological system as it stands, yet there are probability considerations that render the existence of such a force more probable than not. I shall not maintain this—I don't think it is true—but the point here is that either to establish or to refute such a view requires the biological system in question to be laid out in enough detail (and that the relevant sense of probability be closely enough defined) so that a fair degree of uniformity of opinion can be expected concerning whether or not, in point of fact, the biological facts cited do lend probalistic force to the claim that a vital force exists. (KYBURG, 1968, p. 6)

Unfortunately, KYBURG does not tell us who might argue in this way, or how they might so argue. Hence, it would seem that since, in fact, modern biologists are not troubled by visions of vital forces, there is little point in disturbing their serenity, if all one is going to prove is that biologists are right in not being troubled by visions of vital forces.

The only other possible reason that I can see for adopting WOODGER's approach—although this I suspect may in WOODGER's mind be the main reason—is that in some important sense his approach is philosophically superior (and this in turn would possibly lead to a scientific superiority). Is it not better, one might feel, to be an empiricist and to avoid talk of so-called "theoretical entities" wherever and whenever possible? If talk of gametes will do, then avoid talk of genes; but, if possible, talk neither of gametes nor of genes; but of observable characters.

[2]) See, for example, WOODGER, (1939), p. 33; and WILLIAMS, (1970), p. 369.

Thus, as one of WOODGER's followers (but, in the long run, strongest critics) remarks, WOODGER defines genotypes "without resorting to those somewhat mysterious concepts"—*i.e.* genes[3]). Hence, it might be claimed, it is in his avoidance where possible of non-empirical concepts that WOODGER's real importance lies.

There are several reasons why this kind of empiricism seems as unwanted as it is in fact unrealized in modern science. In the first place, although WOODGER might find there to be something ontologically distasteful about things like genes, despite what he and his group say, genes seem in themselves to be no more mysterious than things almost inevitably invoked by the highly successful physical sciences. Why then single out genetics for a radical empiricism, when physicists talk (happily) of molecules, atoms, electrons, and other, far more esoteric things? Is the gene so very mysterious compared to, say, an electron with all of its peculiar properties pertaining to its dualistic wave-particle nature? (One might object that these electron-properties are not so very peculiar; but this objection only serves to underline my point about the non-peculiarity of genes.) In any case, as we have seen, in *Biology and Language* WOODGER does finally have to invoke the gene. If it is legitimate to do it eventually, why should one not do it at an earlier stage? Are we to understand that only when we can do without it no longer that the gene loses its mystery?

In the second place, although for philosophical reasons one might like an extreme empiricism, there are good scientific reasons for not taking too strong a stand. R. B. BRAITHWAITE (following F. RAMSEY) has shown how theoretical concepts have what one might call a certain "openness" about them—they have, as it were, hidden sides which can be used for explanations in fields unexplored (and perhaps even unthought of) when the concepts were first introduced. (BRAITHWAITE, 1953) To insist on an explicit definition in terms of observables before any concept can enter into theories might well be to cut off hope of future development. More specifically, in the case we are considering, one can only heave a sigh of relief that geneticists did not try to eliminate talk of genes. By the fact that biologists brought the concept of the gene out into the open, by the fact that they worried about its nature, and above all, by the fact that they wondered what it in turn is made of (*i.e.* they thought in the very opposite direction to that to which WOODGER

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3) PRZELECKI, 1964, p. 316. Later, she makes hay of the fact that WOODGER's concept of genotype has no application in the absence of breeding tests—this is extremely worrisome for an empiricist like WOODGER.

would direct us) have come about some of biology's most important advances. I think here particularly of molecular genetics, which has replaced the old classical gene concept with the DNA molecule. Had biologists taken WOODGER's advice, it seems hard to see how molecular biology would ever have started. WOODGER would have us, where possible, replace higher-level hypotheses by lower-level hypotheses. But in fact, the greatest modern biological advances have come through an even greater flight from what WOODGER calls "epistemic priority."

Thirdly and finally against WOODGER's position I would point out that, as several recent writers have noted, WOODGER's old fashioned empiricism based on a fairly clear line between what is observable and what is theoretical carries a host of unsolved (and perhaps insoluble) difficulties. (ACHINSTEIN, 1968; SPECTOR, 1966) Quite apart from the recent charges that there is no such thing as unadorned observation—everything is in some sense theoretical (KUHN, 1962)—some of the things at WOODGER's lowest levels seem far less observable (and hence farther from naked empiricism) than some of the things at his higher levels. For example many chromosomes can be seen with the aid of a light microscope. On the other hand, many phenotypes (of phages for instance) can, at best, be seen only with the aid of an electron microscope. Of course, this kind of inversion does not hold for the same organisms in both cases; but one does begin to wonder why, for example, a geneticist working with large organisms might be admonished not (if at all possible) to use concepts referring to entities discernable only through certain instruments (*e.g.* the electron microscope), whereas presumably, the geneticist working with small organisms could use results achieved with such instruments without qualms. Or would WOODGER exhort all geneticists to turn to the study of elephants and whales?

In short, there seems no good reason for accepting WOODGER's proposals for genetics, and I hope the reader will now agree with me that the enthusiasm of those thinkers mentioned at the beginning of this paper, is misplaced.

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