

The Formalization of Scientific Languages*

Part I

The Work of Woodger and Hull

Abstract: The extent to which scientific languages can be formalized is an important problem, especially if it is assumed that a theorem-proving machine will deal most effectively with formal systems. In Part I, the axiomatic attempts of Woodger in genetics and of Hull in the theory of rote learning are examined. In Parts II and III, to be published later, the more prominent efforts to formalize physical theory will be considered and a general study will be made of related questions.

General introduction to Parts I, II, and III

If scientific theories can be expressed in axiomatic languages, it is conceivable that machines can be fabricated to derive theorems within those languages. There are, however, many related problems. Among these, the extent to which scientific languages can be formalized is especially pertinent.

Clearly, any language which the machine can handle properly must satisfy demands of rigor and precision. Von Neumann and Morgenstern, early in their classic work upon game theory, raise the question why mathematics has not been more successful in economics. To begin with, they remark, economic problems have not been formulated clearly and "are often stated in such vague terms as to make mathematical treatment *a priori* appear hopeless because it is quite uncertain what the problems really are. There is no point in using exact methods where there is no clarity in the concepts and issues to which they are to be applied."¹

A probable requirement of rigor for any language suitable to the present purpose is that proofs admit of effective check. It is doubtful, however, that this prerequisite can be satisfied if the language in question is unformalized. As Church notes, the essential purpose in the formal construc-

tion of a logistic system is to obtain "an objective, external criterion for the validity of proofs and inferences."² Without formalization, we are subject to unforeseen difficulties and errors.³

If it can be assumed, then, that a theorem-proving machine will deal most appropriately with formal languages, the question is paramount to what extent scientific languages have, in fact, been formalized. Important, too, is the related question of the degree to which formalization can be expected in the future.

There is, of course, some confusion as to what constitutes a formal system. The fact that a theory has been set forth in axiomatic manner and that its individual problems can be stated in mathematical terms does not necessarily mean that it satisfies the various requirements of a formal system, as the latter are commonly understood by logicians. Number theory itself was not developed as a purely formal calculus until the latter half of the Nineteenth Century and the first years of the Twentieth.⁴

What, then, is a *formalized* language? Tarski, in a well-known, non-technical account of the matter, first of all describes an *exactly specified* language or system as one in which we have characterized unambiguously the class of those words and expressions to be considered *meaningful*. In addition, all words to be used without definition (*undefined* or *primitive* terms) are indicated. *Rules of definition*

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are provided for introducing new or *defined* terms, and explicit criteria are presented for distinguishing within the class of expressions those to be called *sentences*. Finally, "we must formulate the conditions under which a sentence of the language can be *asserted*. In particular, we must indicate all *axioms* (or *primitive sentences*), i. e., those sentences which we decide to assert without proof; and we must give the so-called *rules of inference* (or *rules of proof*) by means of which we can deduce new asserted sentences from other sentences which have been previously asserted. Axioms, as well as sentences deduced from them by means of rules of inference, are referred to as '*theorems*' or '*provable sentences*.' If in specifying the structure of a language we refer exclusively to the form of the expressions involved, the language is said to be *formalized*."⁵

Symbolic logic is the discipline most directly concerned with the derivation and study of formal systems, and especially with the use of such systems in the solution of problems. The task of formalizing a scientific theory can be viewed primarily as one of applied symbolic logic.

It must be noted, however, that there is much divergent opinion as to the scientific achievement and prospects of logic. Some writers, like Bridgman, have disparaged its usefulness almost with finality.⁶ Others, like Hull, have championed it, suggesting that it may eventually provide the solution to profound difficulties and problems.⁷ Still others, like Hempel, have adopted a more neutral, undecided attitude;⁸ so that, although the question has received widespread attention, there is much confusion and disagreement.

In the discussion which follows, we shall argue that less progress has been made in the actual formalization of scientific languages than is often thought. It will be maintained, however, that the lack of achievement usually rests upon *discernible* grounds. As a result, critical investigation of representative efforts can notably improve the possibility of success. We shall thus examine not only what is wrong with much of the work presently being done, but also shall consider how future work can be undertaken more profitably.

The discussion will be broken down into three chief parts. Part I, which follows immediately, will deal primarily with the work of Woodger and Hull. Both men have made significant attempts to formalize scientific languages. In Part II, to be published later, we shall examine some of the more prominent efforts to formalize physical theory. In Part III, we shall consider a number of related problems and sum up the argument.

Woodger

J. H. Woodger has published numerous papers and three books⁹ upon the formalization of theory, notably in biology. Like many methodologists, he looks to logic as a device for rendering scientific language internally sound. By formalization, he believes, we can better determine not only a theory's consequences but also the precise relationship of one theory to another.¹⁰

Woodger's argument is always provocative, but such assertions cannot be reasonably judged apart from specific

achievement. The immediate problem, then, is to examine Woodger's work critically.

Let us consider a fragmentary, but very typical, system *W*, reconstructed from a manuscript by Woodger.¹¹ Four extralogical expressions are taken as undefined, namely: '*P*', '*T*', '*cell*', and '*dl*'. The expression '*P*(*x*, *y*)' means that the thing *x* is a part of the thing *y*. '*T*(*x*, *y*)' means that the thing *x* is before the thing *y* in time. Woodger writes in explanation of the latter: "When we say . . . that *x* is before *y* in time, this might be understood to mean that *every* part of *x* precedes *y* in time, or that *x* ends at the moment when *y* begins . . . when either of these states of affairs holds for a given part *x* and *y* of things, we shall say that *xTy*."¹² '*x* $\in$ cell' means that *x* is a cell; and '*dl*(*x*, *y*, *z*)', that *x* develops in *y* into *z*. To understand the latter, we must first observe that the universe of discourse for *W* is "not restricted to purely spatial things but includes four-dimensional time-extended things."¹³ The thing *x* is taken as some complete initial part of *z*. The object *y* may be roughly described as the total causal entity operating upon *z*. If we imagine *z*, for example, as an elongated cigar stretching through four-dimensional space-time, one tip its starting moment and the other its stopping moment, *y* might be regarded as the remaining space-time having the same starting and stopping moments. Or, if we modify our notion of the universe somewhat, *y* might be construed as merely a larger, hollowed cigar totally encasing *z*, and so on. Such explanation of '*dl*' is, of course, vague; but there is no need to expand upon it for the moment.¹⁴

The logical machinery employed in *W* includes quantification theory, a theory of identity, a calculus of classes, and a calculus of relations. That machinery and the notational devices used are explained, with only minor deviation from what we shall employ, in Chapter II, *The Axiomatic Method in Biology*.¹⁵

With this brief account, we may now present some biconditionals and definitions. The words in parentheses, which follow in each case, provide verbal labels for the concepts in question.¹⁶

$$100^* \text{ } c(R, x, y) =_{\text{df}} (\exists u)[R(x, y) \cdot P(x, u) \cdot P(y, u) : (v) : P(v, u) \cdot \supset \cdot \vec{P}^*v \cap \vec{P}^*x \neq \Lambda \vee \vec{P}^*v \cap \vec{P}^*y \neq \Lambda]$$

(the event composed of *x* in *R* to *y*).

$$101^* \text{ } P_p =_{\text{df}} \hat{x}\hat{y}[P(x, y) \cdot x \neq y]$$

(proper part of).

$$102^* \text{ } \text{mom} =_{\text{df}} \hat{x}[T(x, x)]$$

(momentary).

$$103^* \text{ } C =_{\text{df}} T \dot{\cap} \tilde{T}$$

(contemporary with).

$$104^* \text{ } Sl =_{\text{df}} \hat{x}\hat{y}(x \in \text{mom} \cap \vec{P}^*y \cdot \vec{C}^*x \cap P^*y \subset \vec{P}^*x)$$

(time slice of).

$$105^* \text{ } A =_{\text{df}} \hat{x}\hat{y}\{T(x, y) \cdot E!(\exists u)[Sl(u, x) \cdot Sl(u, y)]\}$$

(adjoined to).

106* $B = {}_{\text{df}}SI \hat{\wedge} T$
(first slice).

107* $E = {}_{\text{df}}SI \hat{\wedge} \tilde{T}$
(last slice).

108* $D = {}_{\text{df}}\hat{x}\hat{y}[x \in \text{cell} \cdot y \in \text{cell} \cdot P_p(B^*x, E^*x)]$
(divides into).

109* $F = {}_{\text{df}}\hat{x}\hat{y}[x \in \text{cell} \cdot y \in \text{cell} \cdot P_p(E^*x, B^*y)]$
(fuses (with another cell) to form).

110* $\text{zygote} = {}_{\text{df}}\hat{x}\hat{y}[F - \hat{x}F^2 - \hat{y}F^2]$
(zygote).

111* $Ol(x, y) \equiv (\exists u, v, w) \cdot x = c(A, u, v) \cdot y = c(A, v, w)$
(x overlaps y).

112* $Ci(x, y) \equiv (\exists u) \cdot y = c(A, x, u)$
(x is contained initially in y).

113* $Cm(x, y) \equiv (\exists u, v) \cdot y = c[A, c(A, u, x), v]$
(x is contained medially in y).

114* $Ct(x, y) \equiv (\exists u) \cdot y = c(A, u, x)$
(x is contained terminally in y).

115* $U(x, y) = {}_{\text{df}}(u)[F(x, u) \cdot F(y, u) \cdot u \in \text{zygote}]$
(the zygote formed by union of x with y).

116* $dlz(x, y, z) \equiv dl(x, y, z) \cdot x \in \text{zygote}$
(zygote x develops in y into z).

117* $z(x) = {}_{\text{df}}(u)[(\exists v) \cdot dlz(u, v, x)]$
(the zygote of x).

118* $e(x) = {}_{\text{df}}(v)[(\exists u) \cdot dlz(u, v, x)]$
(the environment of x).

119* $g(x) = {}_{\text{df}}\hat{u}[u \in D^*F \cdot \vec{P}^*u \cap \vec{P}^*x \neq \Lambda \cdot z(x)D_{po}u]$
(the gametes (geneticist's) produced by x).

120* $Ps(x, y) \equiv z(x)D_{po} \mid Fz(y)$
(x is sexual parent of y).

It is significant, as we shall argue later, that Woodger early introduces so complicated a logical machinery.¹⁷ The techniques and symbolisms are largely reminiscent of Russell and Whitehead.¹⁸ In a simple applied functional calculus of first order¹⁹ for which the values of variables are restricted to spatial-temporal entities, we cannot completely reproduce the language W . Neither a calculus of classes nor a calculus of relations is available to us. It is simple to show, however, that a closely related language W' , apparently as useful as W in clarifying basic genetical concepts, can be constructed within the bounds of such a system.

Four predicates will constitute the undefined extralogical language of W' . ' Axy ' means that x is part of y ; ' Bxy ', that

x is before y in time, as explained for W ; ' Cx ', that x is a cell; and ' $Dxyz$ ', that x develops in y into z , also as explained for W . Using the customary devices of a first-order functional calculus, we proceed as follows:

200* $Exy \equiv Axy \cdot \neg Ayx$
(x is a proper part of y).

201* $Fxy \equiv Axy \cdot Ayx$
(x is the same as y).

202* $Gx \equiv Bxx$
(x is momentary).

203* $Hxy \equiv Bxy \cdot Byx$
(x is contemporary with y).

204* $Ixy \equiv Axy \cdot (z)(Hzx \cdot Azy \cdot \supset Azx)$
(x is a time slice of y).

205* $Jxy \equiv Ixy \cdot (z)(Izy \supset Bxz)$
(x is a beginning time slice of y).

206* $Kxy \equiv Ixy \cdot (z)(Izy \supset Bzx)$
(x is a final time slice of y).

207* $Lxy \equiv Cx \cdot Cy \cdot (\exists z)(\exists v)(Kzx \cdot Jvy \cdot Evz)$
(x divides into y).

208* $Mxy \equiv Cx \cdot Cy \cdot (\exists z)(\exists v)(Kzx \cdot Jvy \cdot Evz)$
(x fuses (with another cell) to form y).

209* $Nxy \equiv (\exists z)(Azx \cdot Azy)$
(x and y have a common part).

210* $Oxy \equiv Exy \cdot (z)(Gz \cdot Azy \cdot \neg Bxz \cdot \supset Azx)$
(x is an initial part of y).

211* $Pxy \equiv Exy \cdot (z)(Gz \cdot Azy \cdot \neg Bzx \cdot \supset Azx)$
(x is a terminal part of y).

212* $Qx \equiv (\exists z)Mzx \cdot (y)(z)(\neg Mxy \cdot Myx \supset \neg Mzy)$
(x is a zygote).

213* $Rxyz \equiv Myx \cdot Mzx \cdot (u)(Mux \supset Fuy \vee Fuz)$
(x is a zygote formed by a union of y and z).

214* $Sxyz \equiv Dxyz \cdot Qx$
(x is a zygote which develops in y into z).

215* $Txy \equiv (\exists z)Szxy$
(x is the environment of y).

216* $Uxy \equiv Cx \cdot Cy \cdot (\exists z)\{Oxz \cdot Pyz \cdot (u)[Cu \cdot Aus \cdot \neg Fuy \cdot \supset (\exists v)(Luv \cdot Avz) : Iuz \supset (\exists v)(Cv \cdot Avz \cdot Iuv)]\}$
(x is linked by cellular division to y).

$$217^* \forall xy \equiv (\exists z)Sxzy$$

(x is the zygote of y).

$$218^* Wxy \equiv . Nxy . (\exists z)Mxz . (\exists z)(Vzy . Uzx)$$

(x is a gamete (geneticist's) produced by y).

$$219^* Xxy \equiv (\exists z)(\exists v)(Wzx . Mzv . Vvy)$$

(x is a sexual parent of y).

$$220^* Yxyz \equiv . Xyx . Xzx . \supset Fyz . (u)(Xux \supset . Fuy \vee Fuz)$$

(x is an offspring of y and z).

We see from the above constructions that the earlier language W makes use of a somewhat complicated logical machinery. In a later section, we shall argue that initial economy in system-building is more than trivially important. Austerity may be said to be strategic. The chief criticism we wish to propose at present, however, is that Woodger has not sufficiently appreciated the need for an *unpresumptive point-of-departure*.

The fashionable objection against Woodger (and other similar writers) is a basically external one: namely, that he has not really *gotten anywhere*. He has not uncovered, it is said, new theoretical implications of interest to geneticists; nor has he laid bare previously unobserved links between genetics and other areas of theory. If, however, there is some justice in the fashionable objection, the primary concern is to discover *why* such is the case, why it is that such an effort as Woodger's has not *gone further*.

A tall building cannot be built upon sand. The choice of site is as important a matter of strategy as the variety of brick used. If we endeavor to formalize a scientific theory which presupposes in a vague or ambiguous way some other area of science, we may well contradict our purpose. We cannot render clear, explicit, and precise, that which presupposes what has not itself been already so rendered. We must seek, in other words, an *unpresumptive point-of-departure*.

The fundamental criticism of Woodger is that he has sought to formalize an area of theory too heavily dependent upon other areas of unformalized scientific theory. Consider, for example, the fragmentary system W . By 120*, we are able to define the notion that x is a sexual parent of y . It is almost universally true that the male gamete is smaller and more mobile than the female. Therefore, were we able to speak of relative size and mobility in W , we might well define the rather important ideas that x is male parent of y , and z , female parent. Unfortunately, however, the system W , as earlier presented, gives no obvious promise of such a development. The difficulty is that the basic language of a non-biological character with which we have to work is so meager and unexpanded that the purely biological construction itself is blocked.

Further, it is not entirely clear that the definitions already obtained are satisfactory. 106* asserts that a first time slice is one which comes before all others; 107*, that a last time slice is one which comes after all others. By 108*, we learn that, where cell x divides to form cell y , the first time slice of y is a proper part of the last time slice of x . Let us

imagine that we have two cells of the same organism undergoing mitosis at approximately the same time. We raise the question whether cell A 's last time slice came before, simultaneously with, or after B 's. Let us also suppose complete pictorial records are available of A 's and B 's almost side-by-side division to form new cells, so that no question arises as to whether, let us say, a pair of chromosomes from A or from B reached the opposite spindle first. It is reasonable to ask whether A or B ends first; but the answer to this question can be given only if we have arbitrarily selected some instantaneous event which occurs in cellular division as the moment of the last time slice of one cell and the first time slice of another. When this problem was raised by me in conversation with Professor Woodger, we were unable to determine any event, representable as instantaneous (with some degree of latitude, of course), which has unique occurrence in every instance of cellular division. It would seem, therefore, that, although a notion like 'cell' as presented in W is on the surface both familiar and definitive, it may actually involve latent complications and ambiguities which tend to destroy its capacity for clarifying other scientific concepts.

An even more serious difficulty arises, I believe, from Woodger's use of ' dl '. In his *Biological Principles*, Woodger emphasizes that changes in the foundations of physics are of concern to biology.²⁰ He attempts, nevertheless, to define the notion of environment, 118*, in terms of a primitive ' dl ', whose meaning patently must vary depending upon which principles of physics are accepted and rejected. Because such acceptance and rejection is for the most part implicit in W , the problem is not easily to be solved by a mere selecting of axioms for ' dl '. A given set of axioms for ' dl ' will probably be inadequate to differentiate significantly between one physical theory and another. Should it be adequate, however, the appropriate development of the physical language needed in W ought to be made early, in advance of the use of ' dl ' for the explication of genetical concepts. Let us suppose, for example, that we assume a classical field theory of mechanics. Then, commensurate with our earlier rough description of ' $dl(x, y, z)$ ', where $dl(x, y, z)$, the object y , insofar as mechanics is concerned, may suitably be taken as the enlarged, hollowed cigar also mentioned. If a different theory is assumed, however, in a universe where the local causal influence is not described by differential equations, y might be regarded as the remaining physical objects in the total area of space-time having the same starting and stopping moments as z . Or, if we assert that no influence is propagated faster than a certain speed, y might be taken as a kind of spatial-temporal cone. The first time slice of z would be a proper part of the cone's beginning, larger end. The last time slice of z would be identical with the cone's last time slice, and so on.

For all these reasons, although the constructions attempted by Woodger are both interesting and suggestive, one doubts that such attempts to formalize areas of theory heavily reliant upon other areas of unformalized theory can go far. Woodger's work is, of course, far more substantial than almost all similar efforts,²¹ the majority of which simply involve the sprinkling of a few logic symbols

here and there in familiar scientific statements. One does not much clarify, for example, the science of star gazing, if one rewrites "every revolving planet has two poles" as:

$$(x)\{x \in RP \supset (\exists y)(\exists z)[y \in PO \cdot z \in PO \cdot xHy \cdot xHz \cdot y \neq z \cdot (u)(u \in PO \cdot xHu \supset u = y \vee u = z)]\}.$$

Hull

The work of C. L. Hull (and his associates) has been notably subject to external criticism. In his most substantial logical publication, *Mathematico-Deductive Theory of Rote Learning*, a system is presented requiring sixteen extralogical primitives and eighteen postulates with corollaries. The portion of psychological theory embraced is admittedly rather small, indeed almost infinitesimal when compared with the broad areas of mathematics formalized from relatively meager beginnings. It is not surprising, therefore, that some critics have expressed such reservations as, for example, "pseudo-clarity," "too costly rigor," "too costly clarity," and so on.²²

It is easy to subject a formal system to a kind of eyebrow-raising criticism in which one cites apparently damaging statistics. As in the case of Woodger, it is of much greater interest to probe into the why-and-wherefore of those statistics. Hull is subject, I believe, to the same basic criticism raised against Woodger. He has chosen to formalize an area of theory heavily reliant upon other areas of unformalized theory. Since that objection was elaborated in such detail for Woodger, however, we shall devote our attention in this section to a different topic. The chief criticism we shall argue against Hull (and many of his disciples) is that he has sought to mold his formal construction too rigidly into a preconceived pattern. In so doing, he has endeavored to apply certain generally accepted, valuable methodological principles outside their appropriate spheres of usefulness. As a result, his actual formal construction has suffered.

Hull urges: (1) that "in any truly scientific system, all unobservables must be linked to one or more observables by unambiguous logical relationships;"²³ and (2) that "qualitative" postulates are not so satisfactory as "quantitative" ones.²⁴ Ultimate appeal to "unobservables" and the use of "qualitative" language are, then, to be avoided so far as possible.

In his most important application of logic to the theory of rote learning, however, Hull does not succeed in totally eliminating the unobservables from the language, which we shall call *H*. 'Stimulus trace', 'excitatory potential', 'inhibitory potential', and so forth, are all concepts falling within that category. He does indicate (perhaps motivated by Haldane's proposal that Woodger's *Axiomatic Method* should be rewritten, if possible, "in terms of observable phenomena only")²⁵ that much effort has been expended with that general purpose in mind.

Various remedies have, in fact, been propounded by others. Koch, for example (in remarks directed primarily toward Hull's earlier "miniature" systems,²⁶ but applicable to *H* also), suggests that by the use of Carnap's *reduction sentences*²⁷ such concepts can be defined *operationally* and

the general difficulty alleviated.²⁸ Unfortunately, the latter has never been accomplished.

But does Hull fully appreciate the problem? Einstein remarks, in reply to a comment by Bridgman: "In order to be able to consider a logical system as physical theory it is not necessary to demand that all of its assertions can be independently interpreted and 'tested' 'operationally'; *de facto* this has never yet been achieved by any theory and cannot at all be achieved. In order to be able to consider a theory as a *physical* theory it is necessary only that it implies empirically testable assertions in general. This formulation is insofar entirely unprecise, as 'testability' is a quality which refers not merely to the assertion itself but also to the co-ordination of concepts, contained in it, with experience."²⁹

Specifically, it is often asserted that the ideal scientific theory is one completely formalized, having only observables or operationally defined concepts as extralogical primitives. As indicated above in the remarks by Einstein, this is an ideal involving a basic extension to the essential function of a scientific theory, the latter being that it *imply* empirically testable assertions in general. If we endeavor to make the connection between our primitive concepts and the content of experience *too immediate*, as I believe Hull does, we may totally divorce from our construction those very fundamental, underlying statements and theoretical concepts which we normally expect any substantial theory to possess. It is, in fact, generally taken as a sign of progress in a scientific theory that its more primitive language does not deal directly with every-day objects and "middle-sized" facts. If one does literally attempt to start "in the middle," one cannot be expected to go far on only a small beginning; and the possibility of relating the specific area under advisement to other larger areas will be diminished.

Consider *H*. Of the sixteen extralogical primitives, the following are typical:

U1. *Syllable exposure (slex)*: A class of events each of which may be described as the stationary presence in the window of a memory machine of a syllable consisting of a vowel placed between consonants in a combination not used as a word by the subject. The syllable is supposed to be printed in such a way as to reflect clearly a characteristic pattern of light rays. The subject may or may not be present.

U1'. "a is a syllable exposure" will be expressed thus in symbolic logic: $a \in slex$.

U4. *Reaction (rn)*: The subject's act of speaking a syllable according to his previously acquired habits of speech.

U4'. "a is a reaction of b" will be expressed thus in symbolic logic: $a rn b$.

U6. *Stimulus trace (tr)*: The stimulus trace of a syllable presentation (explained in Def. 17) is a progressively changing activity within the subject's body corresponding uniquely to the syllable presentation in question. The beginning of the activity coincides with the beginning of the syllable presentation in question, and the end of the activity coincides with the end of the syllable-presentation cycle

(explained in Def. 21) in which the syllable presentation occurs.

U6'. "a is the stimulus trace of b" will be expressed thus in symbolic logic: $a \text{ tr } b$.

U16. *Ending time (nd)*: The time, in seconds, at which an event ends.

U16'. "x is the time at which a ends" will be expressed thus in symbolic logic: $x \text{ nd } a$.³⁰

It can be fairly said, I believe, that such primitives, typical of *H*, fall largely within what we might call roughly "experimental" language. Using such notions and those generated immediately out of them, we can, of course, make assertions that lead almost at once to experimental situations and problems. Postulate 1, for example, is as follows:

A. During all syllable-presentation cycles of the learning of any rote series, stimulus traces ($|s \text{ tr } s \dots s|$) extend from the beginning of every syllable presentation (*s*) through the remainder of the syllable-presentation cycle but no further.

$$A'. P \in \text{slpncy} . a \in C'P . \supset . E! \text{ tr}'a . \text{bg}'\text{tr}'a = \text{bg}'a . \\ \text{nd}'\text{tr}'a = \text{cylm}'P .^{31}$$

The immediacy of the language to experimental situations may well run counter, however, to deductive and definitional fertility. In addition, it may be difficult to link the language obtained with that of another area of theory in which the experimental situation is somewhat different. It is not surprising, therefore, as we noted earlier, that the language *H*, with sixteen extralogical primitives and eighteen postulates with corollaries, embraces so limited an area of theory.

Consider also Hull's remarks concerning quantitative and qualitative postulates. He writes: "Since it appears probable that everything which exists at all in nature exists in some amount, it would seem that the ultimate form of all scientific postulates should be quantitative . . . The great reason why qualitative postulates are so unsatisfactory is that they have so little deductive fertility . . . When the postulates can be written out in equations, or in words which readily generate equations, and especially when, in addition, the constants making up important portions of the equations are known from empirical determination, the rich store of powerful devices which mathematicians have invented, at once becomes available."³²

Postulate 13, typical of *H*, is as follows:

A. In the learning of any rote series, any inhibitory potential (I_n) resulting from any given massed practice decreases with the passage of time (*t*) following the termination of such massed practice, the rate of decrease at any time being proportional to the amount of inhibitory potential existent at that time, i. e.,

$$I_n(t) = I_n e^{-dt}$$

where *d* is a constant > 0 , *t* is the time elapsed since the termination of the massed practice in question with the exception of that part of such time which may be occupied by congruent massed practice, and

$$d\Delta^N I_n = b\Delta^N E_{n, n+1}.$$

$$A'. x = I_n(t)(P, T) . \equiv :: x \neq 0 : \equiv : t \geq 0 . (\exists v) . v = \\ t - \sum_{U \text{ INVL}(T, P, T)} u . x = R'(B'T, T) \times \Delta^{N'P} I_n \times e^{-dv} . C'T \subset \text{rtln}'P \\ \therefore d \times \Delta^{N'P} I_n > b \times \Delta E .^{33}$$

Clearly, when one's attention is restricted to those basic concepts out of which a language is generated, the distinction between "qualitative" and "quantitative" may well be blurred. A substantial part of the language of mathematics has been derived definitionally from such logical notions as the truth-functional connective 'neither-nor', the universal quantifier, class membership, class abstraction, and so on. It is not very meaningful, however, to characterize the latter as quantitative. Similarly, the axioms at the very foundations of mathematics (in such a system as Quine's *Mathematical Logic*,³⁴ for example) are not easily so characterized. Yet it is from these very notions and axioms that the numerous concepts and theorems of number and quantity, "that rich store of powerful devices which mathematicians have invented," are generated.

Even more elementary instances can be provided in which expressions apparently *quantitative* are defined in terms of expressions apparently *qualitative*, if it is, in fact, meaningful so to characterize the parts of a formal system at all. Consider, for example, the fragmentary system *H'*, which follows. Reading '*Axy*' as '*x* is part of *y*' and '*Bx*' as '*x* is a circular plane section', we employ the elementary machinery of polyadic quantification theory. The objects of our universe are merely non-vacuous, closed point sets of a three-dimensional Euclidean space.

$$300^* Cxy \equiv . Axy . Ayx$$

(*x* is the same as *y*).

$$301^* Dxy \equiv . Axy . - Ayx$$

(*x* is a proper part of *y*).

$$302^* Exy \equiv (\exists z)(Azx . Azy)$$

(*x* and *y* have a common part).

$$303^* Fxyz \equiv (u)(Aux \equiv . Auy . Auz)$$

(*x* is a total common part of *y* and *z*).

$$304^* Gx \equiv (y) - Dyx$$

(*x* is a point).

$$305^* Hx \equiv (\exists y)\{By . Ayx . (z)[Bz . Ayz . \supset Azx : Gz . \\ Azx . \supset (\exists u)(Bu . Ayu . Azu . Aux)]\}$$

(*x* is a plane).

$$306^* Ix \equiv (\exists y)(\exists z)(Hy . Hz . - Cyz . Fxyz)$$

(*x* is a straight line).

$$307^* Jx \equiv . -Gx . (y)(z)(Hy . Fzxy . \supset . Gz \vee Bz)$$

(*x* is a spherical solid).

308* $Kx \equiv, -Gx, (\exists y)(\exists z)(By, Iz, Fxyz)$
(x is a straight line segment, not a point).

309* $Lxyz \equiv, Hx, Hy, Jz, -Exy, (\exists u)(\exists v)(Gu, Gv, Fuxz, Fvyz)$
(parallel planes x and y are both tangent to spherical solid z).

310* $Mxy \equiv, Kx, Axy, (\exists z)(\exists u)(Lzuy, Exz, Exu)$
(x is a diameter of spherical solid y).

311* $Nxy \equiv (\exists z)(\exists u)(Lzux, Lzuy)$
(x and y are spherical solids of equal volume).

312* $Oxy \equiv (\exists z)(\exists u)(Mxz, Myu, Nzu)$
(x and y are line segments of equal length).

313* $Pxy \equiv, Kx, (\exists z)(Dzx, Ozy)$
(x is a longer line segment than y).

314* $Qxy \equiv, Jx, Axy, (z)(Jz, Azy, \supset -Dxz)$
(x is a spherical solid part of y and not a proper part of any other spherical solid of y).

315* $Rxy \equiv, Ky, Axy, (\exists z)(\exists u)\{Quz, Mxu, (v)[Gv, Avy, \supset (\exists w)(\exists x')(Qwz, Mx'w, Avx', Ax'y, (y')(z')(u')(Qy'z, Qz'z, -Cy'z', Fu'y'z', \supset Ny'z', Gu')]\}$
(line segment y can be broken up without remainder into segments equaling x , and x is one of the segments in question).

316* $Sxy \equiv (\exists z)(Rzx, Oyz)$
(x can be broken up without remainder into segments equalizing y in length).

317* $Txy \equiv (\exists z)(Sxz, Syz)$
(segments x and y are commensurable in length).

318* $Uxy \equiv, Kx, Ky, -Txy$
(segments x and y are incommensurable in length).

319* $Vxy \equiv (\exists z)(\exists u)[Rzx, Rux, -Czu, Ozy, (v)(Evx \supset, Evz \vee Evu)]$
(x is twice as long a segment as y).

320* $Wxy \equiv$ the reader may fill in at his pleasure
(x is 237/871 as long a segment as y).

Sample postulates of H' might be somewhat as follows:

P1. $(x)(y)[Axy \equiv (z)(Exz \supset Eyz)]$

P2. $(x)(y)(\exists z)(Axz, Ayz)$

P3. $(x)(y)[Bx, Dyx, \supset (\exists z)(Dyz, Dzx)]$

P4. $(x)(y)[Bx, By, -Axy, -Ayx, \supset (z)(Fzxy \supset -Bz)]$

The capacity of a formal language to generate problems which admit of numerical solution does not require that its postulates be "quantitative." The situation is akin to that mentioned earlier. In order for a physical theory to imply "empirically testable assertions in general," it is not necessary that its individual postulates be independently interpreted and tested operationally. Analogously, it is not requisite that the *postulates* of a theory be in the form of equations, in order for the theory itself to be of such a nature as to take advantage of existing symbolic techniques.

Once again, Hull is assuming for his construction an ideal which, from its very nature, can limit both the potential development and application of the system in question. It is important to have equations with which to work, but to take the latter as the rigid and perfect models for the postulates of a formal language is to be restrictive in a meaningless fashion. Hull has decided too much in advance that which the actual process of formalization can itself best settle—namely, which type of primitive concepts and theorems will yield the most satisfactory system.

Conclusion of Part I

The attempts of Woodger and Hull to formalize scientific languages are probably the most prominent which have been made outside of physical theory. The two projects are of great interest, but neither is very much developed. In each case, the inability of the author to go further rests upon discernible grounds. Both have undertaken to formalize areas of theory heavily dependent upon other areas of unformalized theory. Hull has also sought to mold his formal construction rather rigidly into a preconceived pattern.

In Part II, to be published at a later time, we shall examine some of the more notable efforts to formalize physical theory. If the criticisms advanced against Woodger and Hull are valid, special interest attaches to those efforts, since physics is the natural science which underlies all others.

In closing Part I, the author wishes to thank J. H. Woodger and W. V. Quine, with whom many of these problems have been discussed. P. Bernays, A. Cobham, H. Cole, E. Hutton, and R. Landauer have made helpful comments.

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8. Expressed in conversation with the author.
9. *The Axiomatic Method in Biology* (Cambridge, 1937); *The Technique of Theory Construction* (International Encyclopedia of Unified Science, Chicago, 1939) Vol. II, No. 5; *Biology and Language* (Cambridge, 1952).
10. See, for example, *The Axiomatic Method in Biology*, pp. 15-16, and *The Technique of Theory Construction*, pp. 1-4, and pp. 70-80. See also Hull on this matter, "The Conflicting Psychologies of Learning—a Way Out," *Psychological Review*, Vol. 42 (1935) pp. 495-97.
11. Privately communicated by Woodger. The system is a very neat précis of various elements in the three major works mentioned in reference 10.
12. *The Technique of Theory Construction*, pp. 32-33. The notational variation is without significance.
13. *Ibid.*, p. 32.
14. See *Biology and Language*, pp. 124-133.
15. *The Axiomatic Method in Biology*, pp. 18 ff.
16. The reader unacquainted with logic can omit the more technical expressions in this paper and still follow the general argument. The verbal labels after the various symbolic formulas specify the concepts under consideration.
17. See Woodger's comments in *Biology and Language*, p. 65 and p. x (Preface).
18. In *Principia Mathematica*.
19. A. Church, "Logic, Formal," *Dictionary*, p. 174.
20. *Biological Principles* (London, 1929), p. 1.
21. Such negative criticisms as we have advanced should not obscure the fact that Woodger has made a significant, pioneering contribution. He has done much to emphasize the need for formalized languages in science and has provided great stimulation to the field of applied logic.
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