ON THE LOGICAL FOUNDATIONS
OF CURRICULUM DESIGN

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ABSTRACT

The thesis begins with a distinction drawn between the notion of an administrative discipline and what I call a logical discipline. A logical discipline is one which is differentiated from other such disciplines by reference to its logical peculiarities, but to say this much still leaves the concept unclear. The concept of a logical discipline is found embedded in discussions on curriculum design, especially when the role of a variant of the "Discipline Principle" is in question, for example, variants such as "Teach only the disciplines", or "Design the curriculum around the various distinct disciplines", etc.. It is not the notion of an administrative discipline that is usually referred to in such contexts. A need is shown for a precising definition of "logical discipline" before such discussions become significant. In the precising definition which is offered, I propose that logical disciplines differ one from another insofar as the type of statement-networks they embody differ. The differences between the various types of statement-networks are explicated by referring to the types of relations holding between statements in the networks (expressing the way in which the concepts involved are related), and the resultant differences in the types of inferences they may be made from one statement to another. In the second section of the
thesis an analysis of several administrative disciplines is undertaken in order to discover the logical disciplines embodied in them. There is no claim that the administrative disciplines analysed give rise to an exhaustive list of the logical disciplines now in use. The administrative disciplines analysed represent the broadest possible cross-section of disciplined studies with which the writer has had some experience. In the third section an attempt is made to articulate as clearly as possible the particular features of those logical disciplines arrived at by the analyses of the previous section; for example, the features of what is termed the value-judgement discipline are set out, and those of the axiomatic discipline.

Finally, an educational aim is recommended which justifies an important place for the teaching of the logical disciplines in a secondary school curriculum. This aim emphasizes the value of students being given the means with which to recognise problems of different types, i.e., where different logical disciplines are appropriate to the articulation and to the solving of them. It stresses the importance of ensuring that students have the 'tools' with which to approach problems not met with in the classroom. In a sense, it is a recommendation for a 'non-sterile' concept of education.
The thesis concludes with some recommendations for further research on this issue, especially in the analysis of administrative disciplines not dealt with in this thesis.
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My thanks are due to W.P. for frequent encouragement, and also for help in typing and retyping a thesis where references conspired to appear too late on a page to be footnoted.
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SECTION I.

The Need for a Precising Definition of "Logical Discipline".

I intend, within this thesis, to clarify the notion of a "logical discipline", and to set out in as much detail as possible the characteristics of as many existing logical disciplines as I can. I shall then offer a justification for teaching the structures of logical disciplines.

I wish to draw a distinction between two usages of the term "discipline":

a) to refer to an area of study falling under the charge of some administrative department in, for example, a university. I shall use the term "administrative disciplines" to refer to areas of study differentiated one from another by reference to customary administrative departments alone.

b) to refer to a type of study differentiated from other types by reference to certain logical peculiarities. I shall use the term "logical disciplines" to refer to types of study differentiated in such a way.

In current literature on the role of "the disciplines" in curriculum design, there is not infrequently an oscillation between the two usages of the term as though, in fact, the administrative-discipline classification and the logical-
discipline classification are one and the same. One paper in which this oscillation occurs is "The Disciplines and the Curriculum" by Jane R. Martin. Whilst, in fact, being concerned with disciplines in sense (b), the examples Martin gives of such disciplines are not shown to be differentiated one from another by reference to their logical peculiarities. The examples given—physics, economics and geology—are indeed usual administrative disciplines, but this is not sufficient to show that they are also logical disciplines. To know that something is a logical discipline, one must know why and how it is to be distinguished from any other discipline. Martin speaks rather vaguely of disciplines as sets of theories, laws and principles, but this does not help us to differentiate one logical discipline from another.

The reader is informed that a discipline has "some


2See especially Martin, pp. 68-69.

3Martin, p. 68.

4Martin, referring to the 'view' of the earlier part of the paper, writes: "In general, when we view the disciplines as sets of theories, laws, principles, and the like arrived at through inquiry, . . . .", p. 72.

5Hereafter, "discipline" is to be understood as a shorthand for "logical discipline". "Administrative discipline" will always be unabbreviated.
realm of study"⁶, but it is not claimed that disciplines differ one from another insofar as the "realms" they deal with differ one from another. (A great deal of work would be needed in any case to make this criterion intelligible.) It is stated that a discipline has "some vocabulary"; it is not stated that there are some special differences between the vocabulary of one discipline and that of any other. ("Special differences" would have to be given intelligible content.) It is stated that a discipline has "some methods", but, ignoring for the present the extreme vagueness of "methods", it is not claimed that a difference in kinds of "methods" provides a criterion for differentiating disciplines. Now until one understands why something is considered to be a discipline, and more especially why it is considered to be differentiated from any other discipline, I do not consider that one has any right to claim to "know a good deal about theoretical disciplines."⁷

The layman would understand the concept of a "discipline" if he understood why something is to be considered a discipline, and how (i.e., in what respects) it is differentiated from any other discipline. Of course, if it were typical for the layman to have such an understanding,

⁶Martin, p. 68.
⁷Martin, p. 68.
then there would be little need for this thesis. It is precisely because the usage of "discipline" (in sense (b)) is so vague, it is precisely because the 'ordinary person' does not have the conceptual 'tools' to differentiate one discipline from another on logical grounds, that the work involved in the writing of the thesis is worthwhile and long overdue. The problem I am concerned with is the role of "the disciplines" in decision-making about curricula. This is not a simple question; on the contrary, it is a very sophisticated albeit a very necessary question for an educationalist. There is no reason to assume a priori that ordinary language concepts are sufficiently clear and sufficiently sensitive to relevant logical distinctions to serve in giving an answer to this question. Unless it is clear what is meant by the term "(logical) disciplines", the question cannot be asked intelligibly. Precising definitions play an important part in providing the conceptual 'tools' with which to tackle such a problem, and this is how the vagueness of the sense (b) usage of "discipline" will be overcome.

Neither stipulative nor lexical definitions can serve to reduce the vagueness of a term. A vague term is one for which borderline cases may arise, such that it cannot be determined whether the term should be applied to them or not. Ordinary usage cannot be appealed to for a decision, because ordinary usage is not sufficiently clear on the matter—if it were, the term would not be vague. To reach a decision, then, ordinary usage must
be transcended; a definition capable of helping decide borderline cases must go beyond what is merely lexical. Such a definition may be called a precising definition. 

To make the question "Should the disciplines be taught in (secondary) school" intelligible, I shall work towards a precising definition of "discipline" (i.e., of usage (b) given on p. 1 above).

A Precising Definition of the term "(Logical) Discipline".

In this sub-section I shall give a precising definition of "discipline" used in sense (b) (p. 1 above), i.e., "to refer to an area of study differentiated from other such areas by reference to certain logical properties." One attempt to illuminate this concept of a "discipline" involves analysing the different disciplines according to the different forms of knowledge they embody. Martin refers to "the disciplines of knowledge" in her paper, but does not pursue the idea. Hirst, in his paper "Liberal Education and the Nature of Knowledge"10, makes a more worthwhile

\[8\text{Irving M. Copi, Introduction to Logic, 3rd ed. (New York, 1968), pp. 100-101.}\]

\[9\text{Martin, p. 68.}\]

attempt to base the different disciplines on different "forms of knowledge". Although the paper is very helpful in suggesting possible approaches to the articulation of the precising definition of "discipline", I think that one must look for a less obvious but less restrictive starting-place for the definition than the concept of a "form of knowledge".

I shall hint at the kind of limitation of candidates for the title of "discipline" following upon our basing the notion of a "discipline" itself on that of a "form of knowledge". Ayer articulates the necessary and jointly sufficient conditions for knowing as:

i) "that what one is said to know be true";

ii) "that one be sure of it";

iii) "that one should have the right to be sure."\(^{11}\)

Condition (ii) is sometimes written by other philosophers as: "that one believe it." Ayer is concerned here with "knowing that" as opposed to "knowing how". Thus, one would know that p, where p is a proposition if, and only if, i) p is true, ii) one is sure that p, and iii) one has the right to be sure that p. "Proposition" means "that which is expressed by a meaningful sentence and which is true or false."

Now, if I introduce the term "statement" to mean "that which is expressed by a meaningful assertorically used sentence",

then, for the early logical positivists, this term would be equivalent to "proposition", for the only meaningful assertorically used sentences acknowledged were either tautologous or, via the verification principle, empirically verifiable. I do not accept the theory of meaning underlying this restricted use of "meaningful assertorically used sentences" (although there is not room here to enter into my reasons), for I hold that there are several kinds of meaningful assertorically used sentences which are neither tautologous nor empirically verifiable. That is to say, I hold that propositions are a subclass of statements. A proposition is, by definition, true or false, and to say that a statement is true or false is to imply that it is the sort of statement to which the law of the Excluded Middle applies (i.e., -(p.p) ). The "objectivity" of truth is displayed in our not countenancing a statement's being "true for you, but not for me." It is not both true and false for to speak in this way is to ignore, in particular, the full force of the ontological grounding of an empirically true statement. (A statement may, of course, be believed to be true by you and not by me, but this is a different matter--we are introducing an intentional concept.) Although the law of the Excluded Middle does apply to propositions, there are some statements to which it does not apply. For this reason some statements, although meaningful
and open to a certain kind of justification, are not true or false. When this is so, it is inappropriate to speak of knowing them since condition (i) of "knowing that" (that what one is said to know be true) cannot be met. An example of this kind of statement is that of a value-judgement. The possibility of being able to know that typical statements are true before something is allowed to qualify as a discipline is very much more restrictive than it might seem to Martin and to Hirst. This being so, there are three possibilities open to us: 1) to exclude "ethical reasoning" from the class of disciplined studies because of its reliance on value-judgements (and perhaps other types of "reasoning" too would have to be excluded upon close inspection); or 2) to build up a new epistemological basis for the conditions for claiming to "know that" (which would necessitate a new analysis of the notions of "true" and "false", as opposed to "justified" and "unjustified", and a new view of the ontological grounding of some types of "true" statements); or 3) to cease to speak of the "disciplines of knowledge" and to work towards a more useful albeit more complex explanation of "discipline". I have chosen this last approach as being the most fruitful.

Although, for the reasons given above, I shall not refer to disciplines as being based on distinct "forms of knowledge", I have nevertheless found it very worthwhile to
examine the specific criteria Hirst suggests by which one discipline may be differentiated from another.\textsuperscript{12} The precising definition of "discipline" that I give is based on an examination of Hirst's suggested criteria.

He gives four criteria by which different forms of knowledge may be distinguished.\textsuperscript{13}

i) They each involve certain central concepts that are peculiar in character to the form.

ii) In a given form of knowledge these and other concepts that denote, if perhaps in a very complex way, certain aspects of experience, form a network of possible relationships in which experience can be understood. As a result the form has a distinctive logical structure.

iii) The form has expressions or statements that in some way or other are testable against experience in accordance with particular criteria that are peculiar to the form.

iv) The forms have developed particular techniques and skills for exploring experience and testing their distinctive expressions.

\textsuperscript{12}Hirst's criteria are suggested as useful in distinguishing forms of knowledge (see the four criteria reported on this page). Since, however, I shall not make use of the concept of a "form of knowledge", I am examining their possible usefulness in relation to the precising definition of "(logical) discipline".

\textsuperscript{13}See Hirst, pp. 128-129.
I wish to make a few comments on these criteria. Criterion (ii) contains the germ of a key-concept of this thesis, viz, "network of possible relationships", but it is not clear how Hirst wishes this concept to relate to that of a "distinctive logical structure". To be explicit, I shall propose that (logical) disciplines differ one from another insofar as the types of statement-networks they embody differ. The differences between various types of statement-networks I shall explicate by referring to the types of relations holding between statements in the networks (expressing the ways in which the concepts involved are related), and the resultant difference in the type of inferences that may be made from one statement to another.

Upon reflection upon the notions of type differences in networks and type differences in the inferences between statements in the networks, criteria (ii) and (iii) lose much if not all of their distinctness. In testing for the truth (or, alternately, for the objective acceptability) of a statement, we often make use of other related statements. Where this is so, the types of relations holding between those other statements and the particular statement in question will significantly affect the way in which we reason within the discipline in question. Consider the difference between establishing an a priori statement which is logically deducible from a set of other statements, and establishing
a statistical statement which is inferable with a certain degree of sureness (i.e., with specified odds as to the chances of the statement's being true) from a set of other statements: there is not a clear distinction here between saying that the networks in which they figure are different in type and in saying that the criteria used to establish their truth (or, acceptability) are different in type.

The relationship between criteria (iv) and (ii) is ambiguous depending on the level at which one is speaking of a difference in techniques. We can make the difference in technique trivial if we contrast chemistry experiments in which test-tubes are used with others in which they are not used. A difference of technique at this level does not necessarily suggest a difference in the discipline involved. Were there a correlation between a difference in techniques at such a level and a difference in disciplines (and I doubt that such a correlation will be found), then the correlation would be purely contingent. On the other hand, if we speak of a difference in technique in that one experimental design involves the use of a "control" group (for example, in many behavioural psychology experiments) whereas some other experimental design involves no such group (for example, in many simple experiments in chemistry), it may well be argued that the difference here is logically dependent on the different disciplines involved, i.e., that the types of
inferences that may be made in the one discipline necessitate the introduction of a control group whereas the types of inferences that may be made in the other discipline (where we have the possibility of universal laws) are such that no comparative technique is required. The difficulty is, however, that although some differences in technique seem obviously to be logically dependent on the differences in the disciplines involved, and although we could think of some differences in technique which would seem obviously to have no relation (or at most a contingent connection) with the differences in the disciplines involved, there are other levels of differences in techniques where it is not clear whether they are logically or contingently connected with differences in the disciplines involved. Consider, for example, the differences in technique involved in observing an ape in a cage move to a box, which has been specially placed, and stand on it to reach food dangling high above him, as opposed to observing an ape in his natural environment utilising some object in a similar manner. The first requires that the observer first arrange the setting (i.e., place the box in the desired part of the cage and hang up the food, etc.). The second requires no such arranging. But do such differences in technique imply that different disciplines are involved, that different types of statement-networks are appropriate? If someone claims that the element of 'control'
in the first case implies that a different "type of reasoning", a different statement-network, is appropriate, does this also imply that if, in the second case, the observer had placed some suitable object near the ape, then this act would be sufficient to change the discipline appropriate to the investigation? "Levels of differences in techniques" do not come already clearly distinguished, and the task of deciding which levels of differences in techniques are logically connected with the logical peculiarities of the different disciplines involved is quite daunting. I am not claiming that Hirst's criterion (iv) is not a feasible starting-place, but I think that criterion (ii) involves less inherent complexity. I shall focus on criterion (ii) rather than on criterion (iv).

I shall now explain why I shall focus on criterion (ii) rather than on (i) so that my choice of priority shall be seen to be not entirely arbitrary. There is no one way to characterize concepts. 1) One way to do so would be to speak of their being either empirical concepts or moral concepts (although I do not recall having heard the expression "analytic concept" to refer to a concept which was typical of an axiomatic system--whatever this may mean). What the differences are between these types of concepts I shall not state since I consider this way of classifying concepts to be over-simple. 2) Or one may speak of the
following categories: concepts defined via necessary and sufficient conditions, or concepts defined via "clusters" of criteria. 3) Or one may categorize concepts according to whether they are based on lexical or stipulative definitions, etc., where the focus is more on the purpose of the use of the concept. I do not find it logically sound to choose at random any one way of classifying concepts in order to make sense of Hirst's notion of concepts being "peculiar in character to the form" (or "discipline", for my purposes). This is not to say, of course, that in analysing differences using criterion (ii) I might not find differences in the types of concepts involved being a relevant factor of the differences in the types of networks. Rather it is to say that by analysing from the differences in the types of networks, I allow for my finding that here some problems in methodology are understood once it is appreciated that one is dealing with "cluster" concepts, while there some problems are understood once it is appreciated that one is dealing with intentional concepts, where the notions of a "cluster" concept and of an intentional concept do not come from one and the same way of classifying concepts. This point of analysing towards concepts rather than from them is an important point of methodology; since there are so many ways of classifying concepts, I shall not decide a priori on the special explanatory value of one.
After reading SECTION II it will be seen that more than one way of classifying concepts enter as explanatory factors of some methodological peculiarity or other (for example, the fact that one is often dealing with "cluster" concepts in the 'inexact' sciences, and the fact that one is often dealing with intentional concepts in some types of historical explanation produce distinctive features), and I have for this reason not chosen differences in the type of concepts involved as a distinguishing criterion for the differences between one discipline and another. Thus the priority is on criterion (ii) as I have explicitly phrased it, viz, disciplines differ one from another insofar as the types of statement-networks they embody differ.

I shall now set out as clearly as possible the important features of the concept of a "discipline" (i.e., the important features of the precising definition). This will help avoid unnecessary confusion as to the status and meaning of what will be, in any case, a sufficiently complex concept.

i) The Implication of Using a Precising Definition.

I have attempted in the following explanation of the term "discipline" to capture what little is embedded in the sense (b) usage of "discipline" given on p. 1 above. I have, of course, gone beyond what was given in order that
questions as to whether or not the "disciplines" should be taught attain a clear meaning. It should be noted, however, that I have decided upon one rather complex criterion by which one discipline is to be differentiated from another. Another philosopher of education may choose to differentiate disciplines according to the type of key-concepts found in each area (although I have given reasons why this is a logically perilous choice of criterion). Insofar as we both have captured the minimal content of usage (b), there is no sense in which one of the two differingprecising definitions is "correct" and the other "incorrect". A way of classifying the "disciplines" other than the one I have decided upon may be just as useful and just as logically sound.

ii) The Minimal Content of the Definition which is to be Precised.

The minimal content which is to be precised is usage (b) as given on p. 1 above, viz, the use of "discipline" to refer to a type of study differentiated from other types by reference to certain logical peculiarities.

iii) The Disciplines are Not A Priori Decidable.

The disciplines, as I present them, were discovered by an investigation into how we in fact reason about different
issues. That the presented disciplines are in use is a contingent truth. It follows that they may be refined or modified at any time, that some may even be rejected as no longer useful, and that some others may come into use. Bearing this in mind, there is a need for constant reappraisal of the disciplines being taught to ensure that they are, in fact, the disciplines now in use in systematic and advanced level investigation.

iv) The Distinction between a Discipline and a Disciplined Study.

A discipline is a type of statement-network. I shall speak of a disciplined study as any study which is systematic and open to justification and which embodies one or more of the disciplines articulated. Many administrative disciplines will be seen, upon close examination, to be disciplined studies and embodying, in fact, more than one discipline.

v) The Notion of "Different Types of Methodologies".

Many disciplined studies not only embody one or more disciplines but also necessitate some ability with certain practical skills—the sort of skills that fall more aptly under the heading of "knowing how to . . ." than "knowing that . . .". Within this thesis I am not concerned with the
claim that one must be able to perform such skills before having an "understanding" of the area in question. This claim I consider to be extremely dubious, since it seems that one may have a very rich understanding of the nature of physical inquiry even though one is in some way manually handicapped. However, I shall not debate this point. The difference in type of methodology that I am concerned with is not related to skills of the "knowing how . . . " kind. The concept of difference in type of methodology I am using is that of difference in the type of reasoning appropriate to the problem in question. I shall clarify this notion further.

vi) Different Types of Reasoning.

[See pp. 10-11 above.]

The notion of a "discipline" I am tying with the notion of a type of statement network, and the use of different types of statement networks is what I mean when speaking of different types of reasoning. Statements are justified in different kinds of ways and with different degrees of sureness. A body of statements forming a theory of some kind are related to one another, i.e., some may be deducible from some others, or some may be inferable with a specified degree of sureness from some others (as with a statistical statement), etc.. There is a resultant difference in the
types of inferences that may be made within a statement network of one type as opposed to a statement network of another type. The types of relations holding between statements of a theory of a special kind will guide the way in which we 'reason' about issues covered by the theory. The nearest I can come to a short indication of the way in which I am using the term "discipline" is to say that a discipline is based on an inference pattern of a special type. (The notion will become clearer on reading SECTION III.)
In this section, an analysis of several administrative disciplines is undertaken in order to discover the logical disciplines they embody.

Mathematics, Pure and Applied

The notion of an axiomatic system is intimately connected with the logical notion of validity. An argument is said to be valid if and only if the conclusion(s) follows from the premises, or, if the premises were true, the conclusion(s) would have to be true also. Thus to say that an argument is valid is not to say that the premises are in fact true. Within an axiomatic system we typically have:

a) primitive terms, including logical constants and punctuation marks;

undefined terms which will be used in the definitions of other non-primitive terms; their admittance into the system is necessary in order to avoid an infinite regress in the defining of the terms of the system; (in other areas this infinite regress is sometimes avoided by recourse to ostensive definitions);

b) formation rules;

rules stipulating what is to count as a well-formed expression (or formula) within the system;
c) axioms;

some set of well-formed expressions which are not asserted to be true when viewed in isolation, but rather are assumed or decided upon as would be a convention;

d) rules of inference and substitution rules;

rules licensing operations on well-formed formulae;

e) definitions;

those definitions we wish to introduce (making use of the primitive, undefined terms).

These are sufficient to distinguish one axiomatic system from another, i.e., to completely specify it. There will also be:

f) theorems;

the set of all well-formed formulae which are derivable from the axioms by using the rules of inference, substitution, definition and form given for that system (thus a theorem in one axiomatic system need not be a theorem in some other axiomatic system).

Thus to say of a well-formed formula that it is a theorem of a certain system is to say only that given the axioms and the rules of inference, then this formula is derivable from them.

The terms within an axiomatic system (as opposed to an axiomatic system in application) have meaning only insofar as they are defined ultimately by using the primitive terms of the system (or are themselves such primitive terms). There
is thus a sense in which the non-primitive terms are
divorced from meaning in the usual sense, and this is
basically because the notion of empirical reference has no
place within the system. This is an important feature of an
axiomatic system since it allows one system to be interpreted
in various ways.

Another important feature of such a system is that it
must be internally consistent (since a contradictory system
would permit the derivation of any proposition whatsoever.)
It is this notion of consistency rather than of empirical
truth that serves as the key to an understanding of the
nature of pure mathematics. Now it is true that finding or
constructing a realization of an axiomatic system (i.e., some
physical model exhibiting analogous relations between its
elements as between the elements on an interpretation of the
system) guarantees the consistency of that system, but it is
not true that all consistent systems must have some
realization. Euclid's triangle is different from that of
Riemann, and each of these is different from that of
Lobachevsky. The construction of non-Euclidean systems of
gometry has done much to clarify the notion of consistency
within a mathematical system.

The arithmetic of natural numbers has been axiomatized
by Peano, with the following five axioms:
1. 0 is a number.
2. The successor of any number is a number.
3. No two numbers have the same successor.
4. 0 is not the successor of any number.
5. If \( P \) is a property such that a) 0 has the property, \( P \), and b) if a number \( 'n' \) has \( P \), then the successor of \( 'n' \) has \( P \), then every number has \( P \).

The primitive terms would be "0", "number" and "successor", and we should need to introduce definitions of "+" and ".":

D1. Addition ("+"): (1) \( x + 0 = x \)
     (2) \( x + y' = (x + y)' \)

D2. Multiplication ("."): (1) \( x . 0 = 0 \)
     (2) \( x . y' = (x . y) + x \)

It is important to note that insofar as "0" is a primitive term, it is uninterpreted and could be interpreted to mean what in ordinary language would be denoted by, for example, "100". The system would still be consistent, and theorems such as "2 + 2 = 4" would still be derivable, although they would then be interpreted to mean something other than what is meant by "2 + 2 = 4" in ordinary language. The proposition has, so to speak, only a meaning intrinsic to the system, i.e., it has no meaning which would give it a referential use outside of the system. These, then, are the characteristics of propositions within pure mathematics:
1) they are either axioms within a system of mathematics, or definitions, or theorems derivable via the rules of inference of that system from the axioms of that system;

2) they are uninterpreted insofar as they rest ultimately on the primitive, undefined terms of the system;

3) they may or may not have meanings and use referential to some types of situation outside of the system when they are interpreted, i.e., there may or may not be a realization of the system.

We must now examine the notion of interpretation in order to see how we arrive at propositions of applied mathematics. In an axiomatic system a certain structure is exhibited, a certain schema within which certain relations hold. It is possible to have several interpretations which must be isomorphic to one another based on one uninterpreted axiomatic system.

There may seem at first to be two kinds of interpretations of the number concepts of arithmetic:

i) interpretations involving definitions which are not of existential definition;

ii) interpretations involving definitions which are a kind of existential definition.

Now I suspect that this distinction, although specious, will not function at all as a distinguishing tool without further refinement. Consider Russell's attempt to interpret the
terms of Peano's system in terms of logical constants\textsuperscript{14}.

Is this not the sort of interpretation most likely to be of type (i)? He adds an explicit definition: two is the class of all classes that have two members. The definition is not circular since the expression "have two members" can be defined without treating the "two" contained in the expression as a unit concept. Ultimately the use of "logical constants" referred to is the following:

The class of f-things has two members.
\[ \exists x \exists y (fx.fy.x \neq y. (z)(fz \geq [(z=x) v (z=y)]) ) \]

The primitive terms of the system -- "successor", "0" and "number" -- may be defined in the following way:

- a) the successor of a number 'n' is the class of all classes that arise when to a class with n members exactly one element is added. (We are speaking here of logical addition, where the logical sum of two classes is the class of those things that are members of one or the other.)

- b) "0" is the number of the null class (i.e., of the class of f-things where \(-\exists x fx\)).

- c) natural numbers are those numbers satisfying Peano's axiom 5 (the principle of mathematical induction).

Now it is well known that these interpretations, while turning axioms 1, 2, 4 and 5 into tautologies, give rise to difficulties in the case of axiom 3, viz, "No two numbers have the same successor." Suppose there were but a finite

number of concrete individuals in the universe, let us say 'N' individuals. Then there would be no class having N+1 members, and consequently the number N+1 (i.e., the class of all classes having N+1 members) would be the null class, as would also N+2. Thus N+1 would equal N+2. Thus the number N (a non-empty class) would have N+1 as its successor and, since N+1=N+2, would also have N+2 as its successor. Also N+2 is the successor of N+1. Thus we should have two numbers with the same successor. To avoid this type of counter-instance to axiom 3, Russell introduces the axiom of infinity, viz, "There are infinitely many concrete individuals in the universe." Now this 'axiom' is existential in character and, insofar as axioms are presupposed and not asserted, insofar as they are akin to conventions, it is very questionable, to say the least, whether this existential proposition may legitimately function as an axiom. The existential nature of the 'axiom' follows from what we may now realise to be the extensional nature of the definitions of the number concepts. Insofar as they are extensional and thus involve what I shall call an existential commitment, it seems odd that Russell is reputed to have defined the number concepts in terms of purely logical apparatus, although, of course, there is a sense in which the core of the definitions is in terms of logical constants.
But is even this true? It will be remembered that the basis of this use of logical constants is:

$$\exists x \exists y (fx.fy.x \neq y. (z) \{fz \in [(z=x)v(z=y)]\})$$

It is my contention that some type of ostensive definition is ultimately required if we are to break out of the "circle of language" in order to give the number concepts some referential significance (to situations in the 'real world'). And yet we appear to have the basis for defining number concepts in terms of logical constants. I think I can best express my uneasiness at this description of Russell's definitions by trying to show the reliance on identifiable and reidentifiable particulars in all interpretations of number concepts, whether these particulars be the logical constants "x" and "y" as in Russell's definitions or concrete individuals as, for example, in definitions of numbers via counting procedures. For, surely, insofar as "x" and "y" are distinguishable symbols on the page, we have a set of (two) distinct individuals; and insofar as any seeming-other individual, "z", is not permitted a distinct individual status (i.e., is the same as "x", or is the same as "y"), do we not in fact have a kind of ostensive definition (which is a kind of extensional definition)? But let us consider a way of defining number concepts which is clearly ostensive, and then see just what are the differences,
if any, between this and Russell's approach.

An example of this would be to define number concepts in terms of counting procedures using the following principles of counting\textsuperscript{15}:

(C1) To count the objects of a collection or aggregate K is to establish a one-to-one correspondence between the objects of K and a set N of numbers (or numerals), such that;

(C2) N includes 1; [but not 0, since, for example, in Peano's system as usually set out, 0 is a number] ---my addition.

(C3) there is at most one number in N whose immediate successor is not in N;

(C4) the number of objects in K is the number mentioned in (C3) if it exists; otherwise the number of objects in K is infinite;

(C5) the one-to-one correspondence may be carried out (i) by actually attaching one number (or numeral) to each object; or (ii) by forming partial non-overlapping correspondences of type (i), as when we count by twos or fives or hundreds; or (iii) by specifying a rule for actually attaching numerals to as many objects as we please. [I am concerned here with method (i)]--my addition.

Thus a specific number 'N' could be defined as the number of objects obtained using these principles where N is the one whose immediate successor is not in the set of numbers (C3). This does serve to interpret the number concepts

\textsuperscript{15}These principles were taken from: Hector-Neri Castaneda, "Arithmetic and Reality", from Paul Benacerraf and Hilary Putnam, eds., Philosophy of Mathematics (Englewood Cliffs, New Jersey, 1964), p.414.
specifically since it prevents the previously uninterpreted symbol "1", for example, being understood to mean what "100" or "1000" etc. mean in ordinary language. That is to say, by this method we define the number concepts to mean what they mean in ordinary language (this claim again relies on the notion of an identifiable and reidentifiable particular). With this type of definition the mathematical proposition "2 + 3 = 5" would be 'spelled out' to mean "If I count the objects of set A and arrive at 2, and if I count the objects of set B and arrive at 3, then, given that A and B are non-overlapping sets, if I count the objects of set A and of set B together, I shall arrive at 5." Such a proposition, involving in the definition of its contained number concepts explicit reference to counting processes, is a posteriori.

But, then, are propositions such as "2 + 3 = 5" in Russell's interpretation of Peano's system also empirical? I wish to say that they are a priori, but how is this compatible with my suggestion that some kind of ostensive definition cannot be avoided even in Russell's interpretation? Now I suspect that Pap is attempting to solve basically the same paradox when he writes:

It is true that in order to formulate the latter proposition [a mathematical proposition using
Russell's interpretation via logical constants] one has to count variables, but the proposition does not mention any counting procedure.¹⁶

I think the basic difference is that where explicit reference is made to counting procedures in the definitions of number concepts, then explicit allowance is made for human errors.

I conclude that a proposition in applied mathematics has the following minimal characteristic:

1) that the constituent terms be interpreted.

Propositions within applied arithmetic may be either a posteriori or a priori depending on whether or not the number concepts are defined with explicit reference to counting procedures. But the Russellian kind of interpretation is, of course, not possible for those branches of mathematics which are not offshoots of arithmetic, for example, topology, geometry, abstract algebra.

The primitives of the various branches of abstract algebra have no specific "customary meaning"; and if geometry in its customary interpretation is thought of as a theory of the structure of physical space, then its primitives have to be construed as referring to certain types of physical entities, and the question of the truth of a geometrical theory in this interpretation turns into an empirical problem.¹⁷


This last is in many ways analogous to applied arithmetic where the number concepts have been defined via an explicit reference to counting procedures.

(Deductive Reasoning Outside of an Explicitly Articulated Axiomatic System)

The deducing of a conclusion from a set of premises outside of an explicitly articulated axiomatic system may be assimilated to the interpreted axiomatic type. The premises are analogous to interpreted axioms, and the conclusion is analogous to a theorem of the same interpreted system. In deductive reasoning outside of an articulated axiomatic system, we may deduce the conclusion from the set of premises by a series of elementary argument forms which we know to be valid. These elementary argument forms are used as rules of inference such that conclusions drawn in accordance with them are validly deduced from the premises; for example, such argument forms as Modus Ponens, Modus Tollens, Hypothetical Syllogism, Disjunctive Syllogism, etc..18 Certain rules of replacement may be added to these argument forms, and these rules of replacement allow the substitution of one expression for another logically.

18See Copi, pp. 249-250 where nine, commonly used argument forms are set out.
equivalent expression in certain specified instances (for example, the rule of Commutation, of Association, or Distribution, etc.). The elementary argument forms together with the commonly used rules of replacement constitute all of the rules of inference commonly used in deductive reasoning. (Of course, a person may use them without being able to 'label' them.) As Copi points out, the use of argument forms and rules of replacement in this way "accords fairly well with ordinary methods of argumentation." Thus ordinary deductive reasoning (i.e., deductive reasoning outside of an explicitly articulated axiomatic system) is reducible to the interpreted axiomatic type.

History

Some historians aim, within their investigations, to produce generalizations; where this is so, their investigations are similar in many respects to those of other social scientists. On the other hand, other historians claim that history is sui generis in that it does not employ the methods of the sciences. It will be useful to draw a distinction between nomothetic and ideographic studies. The

19See Copi, pp. 254-255 where ten, commonly used rules of replacement are set out.

20Copi, p. 247.
former "seek to establish abstract general laws for indefinitely repeatable events and processes", and the latter "aim to understand the unique and the non-recurrent." The physical sciences are nomothetic in character, but it is not the aim of some historians to seek to establish general laws. Insofar as I am considering history as apart from the social sciences, I wish to make it clear that I shall focus only on the kind of historical inquiry which is ideographic in nature.

What does it mean to try to understand particular events in their "uniqueness"? Events are not found in the same way as are material objects, they do not already 'exist to be found' in any strict sense. It is misleading to speak of the set of events unless one gives "the" set of events a context. For example, a geologist may speak of "the" set of events between two dates in the context of his work, just as an archeologist may speak of "the" set of events between the same two dates in the context of his work, and we may expect the 'events' to be different, or, less misleadingly, we may expect the event-descriptions to be different. The event-descriptions chosen for study by an investigator will depend upon his purposes. Ignoring the fact that phenomena do not come ready-labelled may give rise to the view that there is

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one series of event-descriptions which has a special priority (and that other event-descriptions on some different level, and made for some different purpose, must be in some way inferior). More typically, however, a certain purpose (or set of purposes) is claimed to have priority (for example, to produce generalizations which have a very high prediction reliability), and this, in turn, gives rise to a claimed priority for the event-descriptions associated with this purpose (for example, the event-descriptions of a physical science). It is to be appreciated that this type of move rests ultimately on a value judgement and that, even if subscribed to, in no way entails that other types of study, relying on other levels of descriptions, cannot be coherently and systematically pursued. (Of course, to say that the claimed priority of a study rests on a value judgement is not to say that the study itself rests on value judgements.) Now it is often claimed that to "understand an event" is to know why it occurred, i.e., to be able to give a causal explanation of the event, and that to do this one must classify it as an event of a certain kind such that it falls under the scope of some general law about that kind of event. For this reason it is sometimes suggested that to speak of understanding events in their full particularity, of focussing on their uniqueness, is a doomed project. Let us consider this objection.
To speak of an event as being an event of a certain kind is to say no more than that some features (perhaps just one) have been found in other events, or, as I prefer to phrase it, some features mentioned in the event-description are mentioned in some other event-descriptions. This is so in the event-descriptions of the physical sciences just as it is in those of other studies. What is special to the descriptions within the physical sciences is:

i) the high degree of selectivity of features;

ii) the high level of description (to be explained).

There is a quite stringent set of 'rules' as to what features are relevant to an event-description within the physical sciences. Yet, I do not mean to suggest by this that these 'rules' are not empirically based. In running a simple experiment such as testing the reaction of magnesium when placed on water, the colour of the experimenter's jacket would be deemed an irrelevant feature of the event, or, better, it would 'fall outside' the event-description. This is to say, it has been found to have no bearing on this kind of experiment. The two assumptions which seem not to be empirically based (within the physical sciences) are that time and place are irrelevant; i.e., if two experiments with the same design but conducted in different locations (but note, not different qualitatively) or at different times yield different results, it is deemed to be so because
of some non-parallel factor other than the different location or the different time. Even when no factor presents itself, this is held to be so. The irrelevancy of the actual time and the actual location (as explained above) to event-descriptions within the physical sciences is thus untested and functions as a methodological presupposition. I suggest that the possibility of such 'rules of relevancy' is connected with the level of description found within the physical sciences (i.e., (i) and (ii) above are interconnected). I shall clarify this notion of level of description. First, I do not intend anything as naive as that the "level of description" found within a study correlates with the physical size of the objects dealt with in that study (especially since some studies do not straightforwardly "deal with physical objects"), nor shall I focus on the length of time of a typical event under investigation (if there are events under investigation). Rather I intend the notion of a level of description to correlate with the stringency of the definitions of the key-concepts of the study, and thus also with the stringency of the relations between statements containing these concepts. Definitions in terms of necessary and sufficient conditions are not as common as traditional philosophy suggests. This type of definition would qualify as a stringent type of definition in that the criteria for testing whether or not
something is a thing of a certain kind are explicit and finite. The question, "Is this figure a Euclidean triangle?" is precise in the sense that it can be translated into the question, "Is this figure (a) a closed figure (b) with three straight sides (c) lying in a plane?" This type of definition seems to appear typically in the mathematical sciences and in the more classical parts of the physical sciences. It may be noted that there is a connection between the stringency of definitions and the stringency of the rules of relevancy for features of event-descriptions. This should not be surprising since most rules of relevancy are empirically based (i.e., they are generalizations based on repeated experiments or observation-instances), and stringent definitions of the concepts found in the event-description help the experimenter considerably in setting up an experiment to be qualitatively the same as a previous one. (For example, if we define "magnesium" and "water" in terms of necessary and sufficient conditions, it helps considerably in the recognising of an experiment as a repeat of an earlier one, i.e., it helps us to recognise the two instantiations of the concepts involved--the concepts being unambiguous.) In those sciences (or parts thereof) where this stringency of the rules of relevancy for features of event-descriptions is found, I shall speak of their high level of description since statements on a high level of generality may be
generated within them.

But many definitions are not of this kind. Often we are dealing with a "cluster" concept. Consider, for example, the term "horse". There is not a finite list of features something must have before we shall call it a horse, although there are typical features, for example, having four legs, having a tail, having a mane, etc.. But the concept is not stringent in the way in which the concept of a Euclidean triangle is, since if we found a creature in many respects like an ordinary horse except that it was orange, there is no hard and fast rule as to whether or not it shall be deemed a horse. This is to say it is not obvious whether the statement "The horse is orange" is analytically false or synthetic. In fact, with this kind of definition the whole distinction between analytic and synthetic statements becomes rather artificial and, if anything, tends to promote rather than dispel confusion. Usually, if there is a "cluster" of the more obvious criteria for something's being a horse satisfied, then it will be deemed to be a horse. The relative imprecision is obvious. Historians make use of many such concepts, for example, "war", "revolution". There is a relative imprecision in ruling which features are relevant in deeming something to be a revolution. This is one reason why historians may give different event-descriptions of "the same event", and this may occur even
when they have the same purpose in mind. (Another obvious, but philosophically uninteresting, reason would be if their purposes were different.) The event-descriptions will include everything that the historian considers relevant for his purpose, but there is difficulty in establishing any stringent rules of relevancy for any one kind of event-description (for example, revolution-description); which is to say, the notions of repeatability and of generalization are problematic here. Where this is so, I shall speak of a low level of description.

But it is not the aim of the historians I am concerned with to produce empirical generalizations with a high prediction value. It may be true that the lack of any precise rules of relevancy for features which are to be mentioned in event-descriptions in history results in a difficulty in describing the event under investigation as an instance of this or that type of event. It may be true that the concern not to omit any feature which might be relevant gives rise to long event-descriptions that begin to sound more like narrations of stories than descriptions of individual events. And, of course, it is true that the more features which are specified in the event-description, the more unlikely it is (contingently) that there has or will be another event which is alike in all the respects mentioned, or, in other words, the more likely it is that you are giving
a unique event-description. But if we grant this, how can the historian defend himself against the objection that the concept of explaining the event, of understanding the event, breaks down?

It is important to realise that causal explanation of the sort which is held to be universally applicable to events of a certain type is not the only kind of explanation available. We also make use of teleological and statistical or probability explanations, and it is these, I suggest, on which the historian tends to rely.

Let us look at the notion of a teleological explanation. First, let it be noted that to give a teleological explanation is not to preclude the possibility of giving some sort of causal explanation; they are not mutually exclusive. Which type of explanation is given by an investigator will depend upon his interests and purposes. The historian is typically concerned with human actions, events in the social life of man. To give a teleological explanation of a human action is to give the reason for which it was done; it is sometimes characterized as being "forward-looking", whereas a typical causal explanation is characterized as being "backward-looking". It is in giving teleological explanations that the "rationality assumption" has a place. We have ideas as to what would be the 'rational thing to do' in a given set of circumstances; and to avoid the easy assumption
that this makes the explanation totally subjective, it is important to bear in mind that the ideas we have on what a man in the circumstances would do need not involve prescribing the ends towards which he is moving. Were this unavoidable, there would indeed be a strong case for holding teleological explanations to be subjective, since to prescribe the goals for the man would necessarily draw into the heart of the explanation our own moral code. More typically, however, the historian is to some extent at least aware of the person's long term goals. More typically the question is: "What would be the reasonable thing to do in those circumstances, given that this and this were his goals?" This kind of question may be answered even though the goals may be totally unacceptable to the investigator.

In one respect giving an explanation of human action is very unlike giving a universally applicable causal explanation: to say that someone did something in a certain set of circumstances is not to say that if the circumstances arose again, he would act in a similar manner. (He may, on some occasions, ignore his long-term goals and act on whim; or his goals may change.) This fact has been variously described as "Man is a free agent", "Man is not determined", etc.. The point is that it is not a methodological presupposition that in sets of similar circumstances people will always choose to act in the same way, that they will
always act **consistently** (although the more consistently they act, the more **rational** they are deemed to be), whereas there is an analogous methodological principle with some branches of the physical sciences. This is not to say, of course, that people cannot act consistently, but rather that generalizations about what people do in certain types of situations (where we are speaking of what would typically be described as a "voluntary action" or a "deliberate action") are **statistical** in nature. They are **tendency** statements, not **universal** statements. Thus some sort of generalization is used within historical explanation, but it will be remembered that it is not the **aim** of the historian under consideration to produce such probability statements.

Thus the historian seeks not only to state what has happened, but also to understand **why** it happened. This distinction between what happened and **why** it happened is far less clear-cut within history than within the physical sciences; and this is because the demand for an "accurate account of what actually happened", given that the key-concepts are of the sort they are, i.e., given the level of event-descriptions within the discipline, is not a straightforward demand. I have argued that the rules of relevancy for features mentioned in event-descriptions in history are necessarily vague (because of the abundance of "cluster" concepts used in the study). There cannot be generated,
therefore, universal generalizations of the sort found in classical parts of the physical sciences, for, without precise rules of relevancy, the notion of repeating an observation--i.e., of observing results of the same kind in situations of the same kind--does not gain a foothold. This, in turn, is because no unambiguous sense can be given to "situations of the same kind". The historian's focussing on events in all their unique particularity is thus a natural consequence of the level of description available within the study. Statements on a high level of generality may not be produced where there is this absence of precise rules of relevancy and where there is a reliance on teleological explanation.

Social Sciences

The social sciences are frequently regarded as being inferior to the physical sciences, these latter being taken as the paradigm of a 'real' science. I wish to examine some of the grounds for this objection. Let us compare the social sciences with the physical sciences with respect to the following:

1) repeatability of observations;

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2) verifiability of hypotheses;

3) predictability of future events.

As has been pointed out in the passage on History, there is a connection between the possibility of verifying an hypothesis (i.e., of producing some sort of generalization) and the possibility of repeating a set of observations, whether these observations take place in a controlled or an uncontrolled environment. I have argued also that there is a connection between the possibility of repeating the observations and the precision of the concepts used in the event-descriptions of the type in question. Also, the possibility of building up what I have called rules of relevancy for features to be mentioned in event-descriptions of this type is related to the possibility of testing hypotheses. These latter two connections may be understood if we bear in mind that in order to 'repeat' a set of observations, we must know that the situation we are observing is the same in the relevant respects as the one(s) we have observed previously; i.e., we must know it to be of the same type. It seems to be generally agreed that the concepts mentioned in event-descriptions of the social sciences are typically imprecise in that their extensions are not unambiguous. This relative lack of rules of relevancy (compared with the physical sciences) and its resultant difficulty in verifying an hypothesis has been
variously articulated. A particularly illuminating description is found in Olaf Helmer and Nicholas Rescher, "Exact vs. Inexact Sciences: A More Instructive Dichotomy?":

For the examples we have been considering show that in assessing the probability of an hypothesis H—typically a description of a future event—we are in many instances required to rely not merely upon some specific and explicit evidence E, but also on a vast body of potentially relevant background knowledge K, which is in general not only vague in its extent (and therefore indefinite in content) but also deficient in explicit articulation. In many practical applications, particularly in the inexact sciences, not even that part of K which is suitably relevant to H can be assumed to be explicitly articulated, or even articulable. . . . For such an indefinite K, we cannot expect \( dc(H, E \& K) \) to be determinable or even defined. [where \( dc(H, E \& K) = \) the degree of confirmation of H on the basis of E and K.]

Observations within the social sciences often occur in an uncontrolled environment, i.e., it is not uncommon to test out hypotheses via field studies and survey techniques rather than by controlled laboratory experiments. The reasons for this may be ethical, or uncertainty as to how to design an experiment (this latter is again connected with the not unusual lack of rules of relevancy). I shall try to 'spell out' why a relative lack of control may give rise to quite a degree of reserve on the part of the social scientist as to the significance of the conclusion(s) of a set of observations. My example is taken from the realm of behavioural psychology. We must draw a distinction between

\(^{23}\)Krimerman, p. 192.
a manipulated independent variable and a normative independent variable. The traditional experimental set-up is one in which the experimenter manipulates one or more independent variables and measures one or more (usually one) dependent variable, while controlling other factors. Suppose we wish to know whether the temperature of the room affects the rate of working out simple multiplication problems. The temperature of the room(s) would be under the experimenter's control, and he would 'assign' subjects randomly to Temp₁ and Temp₂ (or more) in order to compare the results of the Tm₁-subjects with those of the Tm₂-subjects. In such an experiment the temperature of the room(s) would be a manipulated independent variable. The dependent variable in such a case would be some measurement of the rate of calculation of the different groups of subjects. But suppose we wish to know whether gender affects the rate of calculation. This is not under the experimenter's control and is, therefore, not a manipulated independent variable. He cannot put subjects randomly into the male category or the female category; he must take them as he finds them and classify them according to what they already are. The gender of the subject is a normative independent variable. (This may be thought by many to be an odd use of the word "normative" and rather misleading. Nevertheless, this is the term used in several standard psychology text
books.) So are many other ascriptions concerning the subjects, their habits, their background history normative variables. It may be seen that in testing for correlations in demographic studies we are typically dealing with normative independent variables. It is clear that the modern psychologist has far more reservations about correlations in the latter type of case (where we have normative independent variables) than when we have manipulated independent variables. Let us suppose that in some schools there is pressure on the boys to specialize in "the sciences" and on the girls to specialize in "the arts". Then if our subjects in the correlating-gender-with-rate-of-calculation experiment were drawn from these schools, we might well find that being male is correlated with a quicker rate of calculation--and this would be misleading. A normative variable is one that tells the experimenter something about the subject (or his history), something which is the case, something which may have been the case for many years, something which during that time may have had certain consequences of which both the subject and experimenter are unaware, etc.. As has already been pointed out, there are many cases in which a normative variable could be manipulated, but it would not be ethical or convenient to do so. Here the psychologist may try at least two further approaches: a) he may design an experiment with animals in
which the variable is manipulated (the problems of using this type of experiment as a model deserves attention), and b) he may investigate further with human subjects trying to isolate different factors, etc., by making comparative studies, (for example, by testing in other schools where, although there may be other uncontrolled factors, there is not the pressure on the boys to specialize in "the sciences" and on the girls to specialize in "the arts"). Nevertheless, the psychologist is right in having reservations about his normative variable experiments, since the probability theory underlying many of the psychology statistics presupposes that the contrastive groups of subjects were chosen at random from some larger population of which they were all members.

I wish now to look at the notion of predictability of events within the social sciences. Again, my focus will be on behavioural psychology. I wish to suggest that statistical techniques are not always used with the aim of predicting future events; in particular, I wish to argue that some statistics function at a pre-hypothesis level, i.e., they have a definite role in the logic of hypothesis-discovery. The techniques of the "inexact" sciences (which largely, but not exclusively, correlate with the "social sciences") are dependent in their methodology on their use of statistical methods, and the behavioural sciences and other social sciences dealing with people as people and not merely as
bodies are distinctive in that the abundance of normative variables gives rise to a pre-eminence for comparative studies as opposed to laboratory experiments.

A distinction should first be drawn between statistical generalization and what is usually called direct-statistical inference. Statistical generalization occurs if, on observing a certain frequency (F) of a certain property (P) in a sample of things/people of a certain kind (K), we conclude that the frequency of the property (P) in the whole class of K-things/people is also F. F is a measure of the correlation between the property, P, and the property/set of properties, K. The frequency, F, will be more than 0 and less than 1. Direct statistical inference is the converse inference. From a statement giving the frequency of a property in a whole class we infer a statement about the frequency of the property in a sample of the class. Again, the frequency will be more than 0 and less than 1. The frequency would be 0 if none of the class had the property, and it would be 1 if all of the class had the property. In either case, inferring from these frequency-extremes to the frequency of a sample would be a straightforward piece of deductive reasoning, which may be assimilated, as has been shown above\(^2\), to the interpreted axiomatic type of statement-

\(^2\)See pp. 31-32 above.
network. A great deal of the research undertaken in the social sciences is an attempt to produce statistical generalizations by observing a sample of the class for which a generalization is desired. Let us look at the use of statistics for this purpose within behavioural psychology.

The use of psychological statistics frequently, if not always, relies on a frequency interpretation of the concept of probability. To arrive at the frequency of an event, or rather, of a type of event, (For example, a coin's landing face upwards after being tossed), we make a fraction by placing the number of times events of this sort have occurred over the number of times that this sort of event could have occurred. There is a sense, then, in which we may say that the frequency of a type of event E summarizes the results of the tests we have performed in which E could have occurred. But do we have any right to use this frequency as a predictive measure?

It is not difficult to appreciate that the frequency of an event (to be understood as type of event) may differ according to when in our series of tests we stop to calculate it. Perhaps six times out of the first ten the coin lands face upwards, but perhaps after one hundred tests, we find that the event has occurred fifty-five times. Then the frequency of the event will be: \( F(E) = \frac{55}{100} = 0.55 \)
Now suppose someone asks for the frequency of $E$. We may see that there is very little point in our giving just a fraction, for these fractions may differ from one stage in the series of tests to a later stage. But what will the inquirer do with the extra piece of information (i.e., the number of tests)? The frequency of the event in question (i.e., of a coin's landing face upwards after tossing) differs according to the number of tests performed, but more important than this, the frequencies differ in a systematic way. It is not that $F(E)$ after 10 tests is 0.6, and after 1000 tests is, say, 0.53, and after 100,000 is back to 0.6 again. This does not occur (a contingent fact). Rather the frequencies tend to move in one direction, and in this case, they move towards 0.5. Thus in telling the inquirer how many tests we have performed for the frequency given we are, in a sense, telling him how stable the figure is becoming. The experimenter is, of course, not giving reasons why the frequencies should form this type of number series, but since they tend to, the number of tests performed is a piece of data that may not be ignored. Perhaps we may see how one comes to use the concept of a mathematical limit in the conclusion of a statistical generalization where the class is infinite (for example, the class of coin-tossing events).

In tossing a coin, we expect an equal chance of its
falling 'heads' as of its falling 'tails', for coins are (or should be) of the same density throughout, and the actual edge slopes neither to one side nor to the other (if it did, then whenever the coin landed on its edge, it would tend to fall towards one side rather than the other).

Let us say that we have the hypothesis that the probability of the event in question (of its falling 'heads') is one half, i.e., \( P(E) = 0.5 \). This hypothesis should be testable.

We toss the coin once. But if we speak of \( P(E) = 0.5 \), then this will not do. We cannot verify that each of two types of events is equiprobable if we have made only one test.

Now if we make two tests, shall we consider the hypothesis to be verified only if we have the result: Head-Tail (i.e., H T), or T H? But if we run four tests in the coin tossing experiment we shall allow any of the following possible outcomes as confirming the hypothesis that \( P(E) = 0.5 \):

\[
\begin{align*}
a) & \ H \ T \ T \ T \\
b) & \ H \ T \ H \ T \\
c) & \ T \ H \ T \ H \\
d) & \ T \ H \ H \ T \\
e) & \ H \ H \ T \ T \\
f) & \ T \ T \ H \ H \\
\end{align*}
\]

Consider (e) and (f); both these runs of results seem to support the hypothesis, and yet both contain runs which would not have encouraged the experimenter had he stopped after two tests. It is not difficult to see that after eight tests there could be instances in which we had 4H and 4T, and
yet should have had little support for the equiprobability hypothesis had we ended our tests after the fourth. And this will be so for any number of tests. But here is the important point: the probability of getting the N results (half of which are Hs and half of which are Ts) as a straight run of Hs followed by a straight run of Ts decreases as the number N increases.

I wish now to see what is built into this notion of a series of an 'infinite number' of tests. Consider again the hypothesis that in the case of coin tossing, the probability of the coin's landing 'heads', i.e., of an H-type event occurring, is 0.5. We find by experimentation that the "margin of error" (this phrase is explained in the next sentence) for 100 tosses of the coin is about 15, that the corresponding margin of error for 1000 tosses is 50, and that the corresponding margin of error for 10,000 tosses is 150. There is no very simple way of explaining the meaning of "margin of error": to say that the margins of error correspond is to say that it has repeatedly been found that the percentage of a set of n-sets-of-100-tests in which the H-T split falls within (35H-65T) and (65H-35T) is equal to the percentage of a set of n-sets-of-1000-tests in which the H-T split falls within (450H-550T) and (550H-450T), which in turn is equal to the percentage of a set of n-sets-of-10,000-tests in which the H-T split falls within
(4,850H-5,150T) and (5,150H-4,850T). It may be noted that, although the actual margins of error increase in size (from 15-50-150), their size relative to the number of tests in a set (i.e., 100, 1000, 10,000 respectively) decreases; these percentages decrease from 15% to 5% to 1 1/2%.

We may now construct a graph of these values in the following way:

![Graph of margins of error relative to 'N' in sets-of-N-tests (in percentages).]

Margins of error
(15 for sets-of-100-tests;
50 for sets-of-1000-tests;
150 for sets-of-10,000-tests.)

This gives the decreases of percentages for sets of up to 10,000 tests. Even if we have tested in sets-of-10,000-tests, we are not denying that the sets-of-N-tests could have been larger. Nor are we claiming that after the sets-of-N-tests (where 'N' is the number we choose to settle
for) the graph will 'level off', for why should it? And this I think is some of what is built into the notion of an infinite series of tests or sets-of-tests: if we arrive at a graph like the one above, we do not expect it to change its direction after sets-of-10,000-tests, or after sets-of-1,000,000-tests, for then we should have to answer the question why this change occurred at this point, not before and not later; that is to say, we should feel obliged to give some explanation as to why the sets-of-$N$-tests were crucial (for one value of 'N'). It must be borne in mind that the actual experimental conditions would be the same throughout; no matter how large the 'N' in the sets-of-$N$-tests under examination, the actual experimental design is to be the same as for any other value of 'N'. Failing to find a fault of operation (i.e., in his executing of the experiment), the experimenter would be at a loss to explain a change of direction when dealing with sets-of-$N$-tests (this problem seems to have been met by modern physicists by rejecting the assumption that such a change must have a cause, although such a last resort has been bitterly fought against by some, more traditional, theorists in physics). He may hypothesise some hitherto absent interference factor, or he may insist that, although he was unable after rigorous examination to discover a fault of operation, there must nevertheless have been some such fault. But the
traditionally based scientist will not both rule out the possibility of an interference factor and insist that the experiments were carried out in the same way as those for those sets-of-tests of different values. In the case of the graph given above, he would expect the graph to be asymptotic with the x-axis. It is because of the traditional assumption that the graph will not change direction that the data expressed by the curve as it is in the diagram may be said to contain some kind of predictive content. The predictions, however, are not of a high level of precision, they are not of a high level of reliability (being statistical in nature).

Just as the psychologist's notion of probability allows for a straight run of 'heads' (even though the probability of 'heads' is 0.5) a certain percentage of the time, so also does he allow that one may arrive at a "significant" correlation of two variables in a sample of elements under examination even though, were he to examine the whole population, these variables would be found to be not correlated.

The diagram on p. 57 below represents a population, the elements of which may be classified as: a) either 'a' or 'b', but not both, and b) either 'A' or 'B', but not both. These classifications must be exhaustive for many of the statistics to be useable, i.e., every element must be either
'a' or 'b', and either 'A' or 'B'.

There are in fact six of each possible type in the population, but within the sample there are 6(aA), 1(bA), 1(aB) and 0(bB). This might lead to a correlation of 'a' and 'A' even though, ex hypothesi, we know that they are not correlated. To say that a test shows that two variables are correlated at the five per cent level is to say: if one takes a random sample from a population in which the
variables are not correlated, one will nevertheless arrive at this 'correlation' five per cent of the time; i.e., the probability of this 'correlation' occurring "by chance" is 0.05, it will occur once in twenty times. It is customary within psychology to settle for the five per cent level of significance in the testing of an initial hypothesis.

I wish now to examine the notion of a "lead statistic" by examining the $x^2$ (chi square) statistic. By so doing, the role of some statistics in the logic of hypothesis-discovery will be illuminated. When using this statistic we must define a population such that the population is exhaustively partitioned. That is to say, each element must be assignable to one and only one of the variable-subsets (I shall speak of "categories").

An example: Suppose we have a sample of adults (i.e., 21 years old and over) consisting of 65 men and 35 women, and suppose we wish to know whether gender is correlated with height. First we must make the hypothesis precise. We define "tall" as "6 feet and over", and "short" as "under 6 feet". The null hypothesis, $H_0$, becomes: "There is no significant difference between [the proportion of tall men to short men] and [the proportion of tall women to short women] in the population in question." That is, $H_0$ is: $TM = TW$. Since the $H_0$ is that gender is not correlated with height, we do not speak of a sample of 65M and 35W,
but a sample of \((65+35)\) people, i.e., of 100 people. We then observe the number of tall people and the number of short people, regardless of gender, and note that there are, say, 16 tall people and 84 short people, i.e., \(TP = 16 = 4\). On the \(H_0\) we should expect:

a) the number of \(TM = 65 \times \frac{4}{25}\)

b) the number of \(SM = 65 \times \frac{21}{25}\) The expected frequencies.

c) the number of \(TW = 35 \times \frac{4}{25}\)

d) the number of \(SW = 35 \times \frac{21}{25}\).

We have also the observed frequencies which are, say:

a) the number of \(TM = 15\)

b) the number of \(SM = 50\)

c) the number of \(TW = 1\)

d) the number of \(SW = 34\).

This is all the information necessary for constructing the \(X^2\) cell table. In this case we should construct a cell table with two categories of one type (i.e., the men and women categories of the type--adult) and two categories of a second type (i.e., the tall and short categories of the type--height). I shall not go into the calculations, but rather say that the \(X^2\) statistic is a measure of the differences between the expected frequencies on the null hypothesis and the observed frequencies (to show how unlikely it is that the null hypothesis holds).

At a preliminary stage of investigation we may have
reason to believe from casual observation that one variable is correlated with another (for example, gender with height), but often the experimenter does not know how strongly the variables are correlated. The $H_0$ is, in a sense, precise since it states that the two variables are not correlated. The $\chi^2$ statistic has been constructed so that its value is zero if the $H_0$ holds, i.e., if all the pairs of observed and expected frequencies show no difference. The considerations for working with $H_0$ rather than with the corresponding positive hypothesis support my claim that the $\chi^2$ statistic is a "lead statistic"--to say that we have falsified the hypothesis that the two variables are not significantly correlated is a lead statement, it says little in itself but suggests that further investigation here would be worthwhile.

Ultimately we are attempting to make certain events, phenomena or actions intelligible. I shall make two claims about this statistic (i.e., $\chi^2$):

I) that there are to the trained observer or experimenter certain systematic questions to be asked and certain procedures to be adopted in formulating the $H_0$ underlying the cell table eventually constructed;

II) that a "significant" correlation does not give us a causal relation in the traditional sense, but rather
that $x^2$ is a "lead" statistic and is typically used
to help arrive at a more theoretically grounded hypothesis.

Let us suppose that a psychologist meets with an event or
a series of events which he finds puzzling. This might
occur in a laboratory situation, while working as a school
psychologist, in a psychological survey situation, etc..

To say that he finds the event puzzling is to say that the
event does not 'fit in' with what he already knows about
events in this area with this type of subject.

Suppose we have a simple T-maze (i.e., "T" shaped)
with a food pellet at one end of the crossbar, in which we
place rats a) one hour after being fed, and b) five hours
after being fed, and we find that the rats in situation (b)
learn the maze quicker than the rats in situation (a).

Suppose also that a psychology student is brought into the
laboratory, but is not informed that one set of trials was
made after one hour of food deprivation while the other set
of trials was made after five hours of food deprivation.

He is asked to construct an hypothesis on the verification of
which the results become more 'intelligible'. He is allowed
to observe the rats as closely as he wishes and he may check
them in any way he wishes. Now in attempting to make some
occurrence intelligible we are trying to 'fit it in' with
something that is already known. I think it is clear that
in the situation described above there is more to the task than just standing and watching the two sets of rats make trial runs. There are some systematic moves to be made, and insofar as they are systematic a person with some training in experimental laboratory conditions is in a better position to solve the problem than the experimentally naive person.

These moves that I refer to in a typical experimental-psychology situation seem to be best expressed in question form:

a) Is there any difference in the apparatus used for the two groups of subjects? (Adapt the question for demographic investigations: Is there any difference in the questionnaire, the form of the question-set, etc.?)

b) Are there any differences between the two groups of subjects? (Adaptation: Is there some factor not covered in the survey or test which is common to some subjects in the population under investigation but not to all?)

Both these questions are "lead" questions. That is to say, they are questions suggesting certain investigations that could be made, but they are not questions that, even when investigated, will typically give the investigator a very full explanation of the events or results in question.
Rather they serve to lead to further hypotheses.

The student meets with two different results in what seem at first glance to be the same conditions. Is he right in supposing that the conditions are the same? The two questions above enable him to ask this question in more helpful terms (i.e., in a way that suggests things to be checked). In response to question (a), the student checks that there is the same kind of T-maze for both groups of rats, that the food pellets in both cases are the same both in content and in size, that the maze itself remains in the same part of the laboratory for both series of trials, and that there is no noise factors or smell factors present during one set of trials and not during the other.

Attention is then directed to question (b). Let us assume that the two groups of rats are taken from the same litter, then what differences will he look for? It is in answering question (b) especially that the training of the student will be displayed. He will not first check lengths of whiskers or lengths of tails for differences between the two groups, but he may well check perceptual factors. He may ask to see records of their background to check, for example, that both groups of rats are accustomed to the sort of food given in the pellets, etc. The notion of 'relevancy' at work here is connected with the theoretical background in psychology which the student has. He wishes
to check relevant features, but there is no finalized list of such features for this problem. Rather it seems to work this way: the psychology student knows which differences between the two groups of rats would not be 'dead-end' differences (differences in tail-lengths would be a 'dead-end' difference—he would not know what to do with it). He knows which differences would give him good leads, i.e., he knows what sorts of differences in subjects in learning situations 'tie in' with theories in the fields of learning-psychology and perception-psychology, etc.. This is, of course, why the trained observer is in a better position than the naive observer. He knows what sorts of things to look for.

Let us assume that he draws a blank on the questions specifically mentioned so far. He is trying to explain the results by relating some feature(s) of the event-description to some theoretical body of propositions already well established, and since the experiment is a learning-situation experiment, he draws on the theories of learning with which he is familiar. He attempts to bring into the event-description the concepts found within classical (Pavlovian) conditioning, but the concepts of stimulus and response as typically found in classical conditioning experiments do not seem to apply. Prominent in the experiment is the idea of the food pellets' being rewards for making a successful
maze-run, and this will lead him to a consideration of operant conditioning theory. He knows that the lever-pressing activities of a rat in a lever-box decrease as the rat 'loses interest' in the food pellets as rewards. From here it is a short step to drawing up a cell table for the laboratory experiment to correlate hours of food deprivation with the time taken to learn the maze—for a difference in the hours of food deprivation is the most obvious cause of a difference in the rats' 'interest' in the food pellets.

I shall now look at the second claim (given on pp. 60-61 above), that the $\chi^2$ statistic itself functions as a "lead" statistic. The Humean notion of causality in which a causal law of the form "X causes Y" is built up from observing $(X_1 \text{ then } Y_1), (X_2 \text{ then } Y_2), (X_3 \text{ then } Y_3), \ldots$, does not illuminate the notion of a "law" as found in modern behavioural psychology. The widespread use of statistical methods has brought with it its own notions of support for an hypothesis. It is especially in demographic studies (where we study 'existent populations' rather than construct populations for our purpose in a laboratory situation) that statistical methods are prominent; and it is here also that the psychologist makes much use of the distinction between correlation and what he calls a "causal" connection. The $\chi^2$ statistic alone gives support for mere correlation, but this is not to say that the investigation need end here.
To look back at the psychology student example, the correlation between hours of food deprivation and the rate of maze learning becomes more intelligible in the light of psychological motivational theory. The correlation statistic in this example acts as a lead statistic in that the experimenter/student is free to use other things as rewards (for example, water, or a female rat for a male rat, etc.) in order to test for the aptness of the concepts used in his event-description (for example, he may test for the aptness of his choice of "reward" in preference to "food pellets", or "food pellets of such-and-such weight"). He is testing out the rules of relevancy he has used in giving the event-description so that he may relate the concepts employed to those found within motivational theory. It is when our hypotheses become theoretically-grounded that we have the possibility of one hypothesis giving rise to more than one set of predicted observations. Then we have the possibility of cross-checking the hypothesis via these other predictions, and successful cross-checking in this way will be an important consideration in the retaining of a theory. The theory which is rich in implications which are then verified is the well established theory.

I am suggesting that statistical techniques especially within the behavioural sciences have special features, and that the mere absence of universal laws does not mean that
we cannot come to understand the type of reasoning involved; there are sets of rules to be followed in reasoning here just as there are in the classical physical sciences, but the rules are different. I have tried to show also that the fact that some statistics (for example, $x^2$) have a very limited prediction value and explanatory value in no way diminishes their overall value since they may well function as lead statistics. I have, in fact, tried to articulate some of the logical features of statistical inquiry, especially as they relate to the "verifiability of hypotheses" and "predictability of future events" (two aspects of the social sciences mentioned on p. 44 above as being worthy of investigation).

Moral Reasoning

There has been a lot of discussion as to whether or not moral statements are imperative in nature, the underlying question being just how directly they function as a guide to action. This is not a problem I am concerned with in this section, and for this reason I shall draw a distinction between what I shall call a value-judgement, by which I refer to statements in the indicative mood expressing a judgement of value, for example, "Truth-telling is desirable", and what I shall call a moral rule, by which I refer to statements in the imperative mood recommending a course of
action. Depending on how directly the reader assumes "moral statements" to be relevant to a choice of action, he may find it helpful to focus on one or other of the above kinds (of moral statements). However, I wish to point out that the comments made in this passage apply equally to pieces of reasoning embodying value-judgements as to pieces of reasoning embodying moral rules (as I have explained these terms). This is to say, networks of statements of the value-judgement kind and networks of statements of the moral rule kind are not different in type in the sense that the types of inferences that may be made from one group of statements to another are analogous—the types of rules appealed to in deriving some new statement are the same.

Neither a value-judgement nor a moral rule may be supported by an appeal to empirical evidence alone. The arguments to show this are well-known and I do not intend to list them here. Basically, the claim is that no amount of empirical data about a choice-situation will establish either deductively or inductively which of the possible courses of action is the most desirable or which is the "one to do". This is not to say that empirical data are irrelevant to the recommending of a course of action, but rather that one will not arrive at a recommendation from empirical data alone. (Insofar as a person accepts a purely emotive interpretation of ethical judgements—}
sort of theory found in A. J. Ayer's *Language, Truth, and Logic*—empirical data may suffice. But this is no paradox for in such a case it amounts to asking, not for reasons why I disapprove of so-and-so, but rather for causes of my feeling of disgust. The notion of recommending loses most, if not all, of its force: any imperatives become incapable of justification in the normal sense of the term, i.e., by an appeal to principles, and insofar as they are based solely on one's feelings of liking and disliking, they may be inconsistent from day to day, from hour to hour—and this cannot be construed as a fault; a man cannot be reproved because he likes muffins every other hour—and there is little point in exhorting him to be consistent.)

But if value-judgements and moral rules are not supported solely by empirical evidence, how are they justified? I shall contrast the type of reasoning found within ethics with the type of reasoning found within an axiomatic system—the disanalogies are illuminating.

Within a system of moral reasoning the following peculiarities may be found:

i) there can be only one 'axiom';

ii) the 'axiom' is not a self-evident truth (in the sense that its denial is self-contradictory);

iii) the derived moral principles require the support of some empirical generalizations.

(These claims I shall defend.)

The type of model I shall try to explicate may function either with value-judgements or with moral rules (as I have explained these terms), i.e., the 'axiom' or first principle may be either of the form: "X is intrinsically valuable/X is desirable in itself", or of the form: "Do Y in all choice-situations." The above mentioned peculiarities will be found in both kinds of systems.

Now the axioms of a system must be consistent with one another, since from inconsistent axioms anything at all may be generated. But where a moral system contains as first principles two value-judgements or two moral rules, it is always possible in principle to construct a choice-situation in which the two would conflict. It must be noted that there would be no formal inconsistency between, for example,

i) the greatest happiness of the greatest number is intrinsically valuable;

and ii) truth-telling is intrinsically valuable.

Nevertheless, we may speak of their being putatively inconsistent in an extended use of the notion of consistency since they may recommend conflicting courses of action. Bearing in mind that ultimately the moral system is to guide
our decisions, we may appreciate the need for a decision-principle in the case of such a conflict, which principle will, in fact, function as "the" first principle. This point applies where a person has two or more value systems dealing with two or more seemingly "unrelated" areas of human action--this is to say no more than that the areas are usually non-conflicting. A priority of one of the systems must be decided upon if the overall priority is to be unambiguous.

But this first principle is not in itself self-evident. To say that "The intrinsically valuable is intrinsically valuable" is to say something which is analytically true, i.e., self-evident; but such a statement, although analogous to typical axioms in symbolic logic systems (for example, Axiom 1 of System PM = (pvp)vp; Axiom 2 = qvp(pvq), etc.), could not function as the first principle. The use of "axiom" too is thus an extended use, and it is for this reason that I have spoken not of the axiom, but of the 'axiom', or, more informatively, first principle. The 'axiom' or first principle, then, must in an important sense have content. This is so since we are not building the system in order to establish more elaborate, but nonetheless self-evident, truths, but in order to establish more specific value-judgements or moral rules.

Let us now examine how we move from the first
principle to the more specific value-judgements or moral rules. Here again there are important differences between moral systems and strictly axiomatic systems. Let us take for our first principle the moral rule "Pursue the greatest happiness of the greatest number." Now this does not logically imply more specific moral rules such as "Tell the truth" or "Do not steal." That is to say, these more specific moral rules are not derivable from the statement "Pursue the greatest happiness of the greatest number." Rather they rely in their derivation on intervening statements which are established empirically. These are generalizations of the form—"Doing Y tends to produce X." The relation found within these statements is that of means to an end, and not of logical implication. We thus have:

(FP) (First principle):
"Pursue/do X in all choice-situations."

(EG) (Empirical generalization):
"Doing Y tends to produce X."

(DMR) (Derived moral rule):
"Do Y."

Now it may be seen that although the first principle may not be qualified in any way (cf. Kant's "categorical imperative" in his Groundwork of the Metaphysic of Morals\(^2\)), the derived moral rule may be qualified and, indeed, should be.

It is possible that the derived moral rule may in some choice-situations not be applicable, and this is understood if we examine the nature of the intervening empirical generalization. A typical means-end generalization used in ethical reasoning is a tendency statement. Were the generalizations universal in their applicability, then the derived moral rule, "Do Y", would be universally applicable also and would stand in need of no refinement. However, since the means-end generalization, "Doing Y tends to produce X", is a tendency statement, the derived moral rule is best refined by adding the ceteris paribus condition. This is something of a blanket clause, but it serves its primary function very well--namely, to draw the attention of the decision-maker to the possibility of the derived moral rule being inapplicable in any particular choice-situation.

(DMR) "Do Y, ceteris paribus."

It may be noted here that one may use empirical generalizations of the form: "Doing Z tends to prevent X", from which may be derived the moral rule: "Do not do Z, ceteris paribus." These are prohibitive moral rules. A stronger version of an empirical generalization giving rise to a prohibitive moral rule is: "Doing Z tends to produce the antithesis of X", but I have suggested only a minimal interpretation of the kind of generalization used in
deriving prohibitive moral rules.

One may also make use of empirical generalizations of the form: "Doing W tends not to produce X" and, given that this generalization gives as complete information as possible about the effects of doing W relevant to the production of X (i.e., given that it is not true to say that "Doing W tends to prevent X"), then this empirical generalization may be used to establish the derived moral rule: "One may do W, ceteris paribus." This may seem odd at first glance since the empirical generalization called upon seems to place the doing of W in an unfavourable light, but this is not so. It merely fails to place it in a favourable light, and given that this generalization is the most complete possible, then it, in fact, establishes the tendency of doing W to be irrelevant to the production or hindrance of X. Such a derived moral rule is permissive.

It may be seen that a derived moral rule, D₁MR, may itself, via another empirical generalization, lead to some other more specific moral rule, D₂MR, which, in turn, may lead to some other yet more specific moral rule, D₃MR, and so on indefinitely.

I wish now to examine the criteria used in decision-making between two or more DMR's, bearing in mind the differences between the DMR's of the three different categories, i.e., the recommendatory (Rc₁, Rc₂, ...), the
prohibitive \((Pr_1, Pr_2, \ldots)\), and the permissive \((Pe_1, Pe_2, \ldots)\) DMR's.

a) Decision between two or more recommendatory rules.

There are two distinct criteria here, corresponding to the distinction between stakes and odds in a betting situation:

i) Of the possible Rc's, choose the one most likely to produce the desired result. (This criterion is always applicable, no matter what the first principle is.)

ii) Of the possible Rc's, choose the one whose result, if produced, would be the most desirable. (This is not always applicable. It is applicable typically in cases where the first principle recommends the pursuit of X where X in some way admits of quantitative differentiation; for example, "Pursue the happiness of others." There could be in such a case Rc_1, Rc_2 and Rc_3, where Rc_2 would produce far more happiness than the other two.)

Where either criterion (i) or criterion (ii) could be used, the choice-maker must use a second-order rule in the meta-language of the system stipulating which of the two criteria is to be used in which kind of situation.

b) Decision between two or more prohibitive rules.

(N.B., the minimum specification of an empirical
generalization used in the derivation of a prohibitive rule is that the course of action in question tends to prevent X, where X is the end recommended in the first principle.)

1) Insofar as the courses of action mentioned in the Pr's tend to produce the antithesis of X (i.e., where the Pr's are derived via the strongest type of empirical generalization giving rise to rules of this kind), they may be graded in a fashion analogous to that of deciding between two or more recommendatory rules:

i) Of the possible Pr's, choose the one least likely to produce the antithesis of X. (This criterion is always applicable.)

ii) Of the possible Pr's, choose the one whose result, if produced, would be the least damaging. (This is not always applicable. It is applicable typically in cases where the antithesis of X in some way admits of quantitative differentiation. Cf. criterion (ii) for deciding between two or more recommendatory rules [p. 75 above].)

Where either criterion (i) or criterion (ii) could be used, a second-order rule must be used stipulating which of the two criteria is to be used in which kind of situation.
2) Insofar as the courses of action mentioned in the Pr's tend merely to prevent X (i.e., where the Pr's are derived via the weakest type of empirical generalization giving rise to rules of this kind), a decision may be made by an appeal to the following criterion:

i) Of the possible Pr's, choose the one least likely to prevent X.

c) Decision between two or more permissive rules.

Insofar as the decision is a moral decision, there are no criteria to use in deciding between two or more Pe's. They are all equally permissible.

d) Inter-category decisions.

Having suggested criteria for deciding upon one of several possibilities within one category of rules, I shall now suggest criteria for choice-situations involving choosing one rule from possible rules of more than one category. We may have any of the following choice-situations:

i) Rc or Pr or Pe : then choose Rc.

ii) Rc or Pr : then choose Rc.

iii) Pr or Pe : then choose Pe.

iv) Rc or Pe : then choose Rc.

Similar criteria could be suggested mutatis mutandis for a system couched in terms of value-judgements instead of
in terms of moral rules.

I shall conclude with a few comments on the ceteris paribus condition of the DMR's. I have remarked that the intervening generalizations involved in the derivation of moral rules are typically tendency statements, for example, "Truth-telling tends to produce the greatest happiness of the greatest number" (where the greatest happiness of the greatest number is the end recommended in the first principle). The DMR in this case would be, "Tell the truth, ceteris paribus." Under which conditions, then, do we not apply a DMR?

A) When it is not a possible course of action. This is not included within the ceteris paribus condition, rather it is a pre-condition of the decision to apply or not apply the DMR. Were the course of action mentioned in the DMR not a possible one in the circumstances, then it would make no sense to ask whether or not the DMR should be applied.

The conditions under which a DMR is not applicable and which are included within the ceteris paribus condition are the following:

B.1) for Rc's (of the form, "Do Y, ceteris paribus."): i) when in the circumstances doing Y would not produce X, i.e., in circumstances which would give rise to a counter-instance to the tendency
statement;
ii) when, although doing Y would produce X, there is some other possible course of action which, in the circumstances, would produce more X (this is possible only when X is in some way quantitatively differentiable).

B.2) for Pr's (of the form, "Do not do Z, ceteris paribus."):  

a) where doing Z tends merely to prevent X: 
   i) in circumstances which would give rise to a counter-instance to the tendency statement.

b) where doing Z tends to produce the antithesis of X:  
   i) in circumstances which would give rise to a counter-instance to the weaker corresponding tendency statement, viz, "Doing Z tends to prevent X."

   ii) when, although doing Z would produce the antithesis of X, the other possible courses of action would produce more of the antithesis of X (choosing the "lesser of two evils"). (This is possible only when the antithesis of X is in some way quantitatively differentiable.)

B.3) for Pe's (of the form, "You may do W, ceteris paribus."):  

i) in circumstances which would give rise to a counter-instance to the tendency statement. (In such circumstances doing \( W \) would not be irrelevant to the production or prevention of \( X \); it would either help to produce or help to prevent \( X \). N.B., I have given the condition under which a Pe is not applicable; to say that a Pe of the form "You may do \( W \)" is not applicable is not to say that one must not do \( W \). It may be that such a Pr is applicable, or it may be that an Rc of the form "Do \( W \)" is applicable.)

B.4) for all categories of DMR's:

i) where precedence should be given to a DMR of another category—perhaps because of considerations such as are mentioned in (B.1), (B.2) and (B.3) above. (See also the criteria for inter-category decisions on p. 77 above.)

In making a few final remarks on the counter-instances to the tendency statements, I shall look only at (B.1)(i) (see pp. 78-79 above). The comments apply mutatis mutandis to (B.2)(a.i), (B.2)(b.i) and to (B.3)(i). Consider an example of an Rc—"Tell the truth, ceteris paribus." If the first principle were "Pursue the greatest happiness of the greatest number", then one instance in which the DMR "Tell
the truth" would not be applicable would be when questioned by an insane person as to the whereabouts of an innocent man whom the lunatic intended to kill. Now we could add this clause to the DMR, and in doing so we should have added a specific example of a counter-instance to the tendency statement, "Truth-telling tends to produce the greatest happiness of the greatest number"; but there is no limit to the number of possible examples, even though some or most of them would be very unlikely ever to occur. We may try to characterize the circumstances in which the DMR "Tell the truth" is applicable: "When a person asks for information, then tell the truth", but obviously this is but a guide-line. As shown above, there are many possible cases in which it would not be the right thing to do under the circumstances and given the first principle. Although we may refine the DMR, we may expect never to arrive at a complete characterization of the sorts of circumstances in which the DMR is applicable, since there is in principle no feature of the situation which may not be relevant to the question of whether or not the DMR is applicable. To refer back to the above example, even such a feature as the colour of the questioner's suit would become relevant if the questioner wore a blue suit, and if we knew that the escaped lunatic wore a blue suit. This is to say, in attempting to specify the type of situation in which the DMR is applicable,
there are no hard and fast rules of relevancy for which features are to be included in the event-description. Nevertheless, this should not be thought to imply that a decision as to whether or not to apply a certain DMR is a haphazard affair since, ultimately, the decision will rest on an appeal to the first principle and to empirically supported means-end statements.

**Biology**

**Taxonomy**

The area of biology known as "taxonomy" is concerned with the classification of organisms into various kinds. Experimentation within taxonomy appears to have two roles:

i) **Experimentation before the system of taxa names is decided upon.**

Experimentation here is especially important where a taxonomic system is to be based on a study, not of phenotypic properties, but rather of genotypic properties, i.e., where experiments are undertaken to see what genetic variations there are between temporarily labelled kinds of organisms, and to determine the genetic inter-relationships between them in order to arrive at some useful conception of the evolutionary units involved. Also, where ecological or breeding relationships are to be used as the basis of a taxonomic system--and such relationship-properties have
played an increasingly important role in the drawing up of a taxonomic system in recent years--experimentation to bring to light what are the ecological relationships or breeding relationships of temporarily labelled kinds of organisms will greatly assist the biologist in deciding upon a convenient system of taxa names.

ii) Experimentation after the system of taxa names has been decided upon.

When the system of taxa names has been decided upon, experimentation may be undertaken to test for the presence of some property--whether a phenotypic property or some less obvious property, for example, a genetic property, or an ecological or breeding relationship property--denoted by some predicate in the definition of some taxon name.

Consider now the role of general laws within a typical scientific theory, i.e., those laws which are empirical generalizations. Such generalizations represent hypotheses which have been tested and which are open to revision in the light of further empirical evidence. What empirical generalizations can be built up within taxonomic theory?

Before the system of taxa names has been decided upon, certain general hypotheses may be tested, for example, correlating chromosome morphology with certain chromosome 'behaviour' (where the taxonomic system is to be based on genotypic properties of the organisms), or correlating
certain phenotypic properties of some plants/animals with
certain ecological relationship properties, etc. Such
general hypotheses (when tested) at this stage of the study
provide basic information about some chosen set of
properties of plants/animals (where, possibly, the plants/
animals are grouped into temporarily labelled kinds) which
will be helpful in suggesting various ways of drawing up a
convenient taxonomic system. These general hypotheses give
rise to empirical laws which are open to revision in the
light of further experience. It is important to note,
however, that if such an empirical law is refined or
rejected after further investigation, this does not entail
that the taxonomic system drawn up in the light of that
empirical law is in some way "incorrect". It may well be
less convenient to use (for example, in that the difference
between two taxa is more difficult to test for than first
appeared), but the system of taxa names itself, whether
convenient or inconvenient to use, is a system of
conventions. (I shall say more of this below.)

After the system of taxa names has been decided upon,
the hypotheses that are testable with reference to this
theory of classification are hypotheses of identification.
Such hypotheses are not general hypotheses but particular
hypotheses, i.e., they make reference to a particular
plant/animal or to a particular group of plants/animals.
Such particular, identificatory hypotheses, although open to revision or rejection in the light of further investigation, do not provide empirical generalizations; they do not give rise to correlational laws whether of statistical correlation or of a more stringent causal correlation.

Classification is a systematic procedure and, of course, a highly skilled one. It is, in fact, a disciplined study and, I suspect, one in which only one discipline is involved. A classificatory theory ensures that classification is systematic in that it provides classificatory rules. These, upon inspection, are definitional rules. They are rules to be followed as opposed to laws which have been empirically tested. Ultimately, they are conventional in character. There may well be reasons for the adoption of some classificatory rules rather than others, for example, perhaps the presence of some definitional properties is much easier to test for than would be the presence of some other properties, or perhaps some expansions or refinements are in order in the event of some new and unforeseen plant/animal being discovered:

... very few properties are needed to distinguish modern man from any other known species. However, if a species of ape were to begin to develop along the same lines as man, acquiring comparable properties, the definition of Homo sapiens would
have to be expanded to exclude this new form if _Homo sapiens_ is to be kept minimally monophyletic.\textsuperscript{27}

Empirical facts may well play a part in the choosing of new classificatory rules, for example, in establishing which properties are easier to test for; or in the refining or expanding of existing classificatory rules, for example, as in the above quotation, in the distinguishing of some species from some new form of organism: "Even if a taxonomist wanted to, he could not supply these distinguishing properties in advance."\textsuperscript{28} Insofar as there are such reasons for giving one classificatory rule rather than another, reasons which on occasion may involve some empirical investigation, the classificatory rules are not completely arbitrary. It should be noted, however, that to say this is not to deny their basic conventional nature. They are not propositions, i.e., they are not true or false. They represent decisions to classify plants and animals in certain ways rather than in others.

I mentioned above that taxonomy seems to embody one discipline; it embodies deductive reasoning which, as has been shown\textsuperscript{29}, is to say that it embodies the interpreted

\textsuperscript{27}David L. Hull, "The Effect of Essentialism on Taxonomy--Two Thousand Years of Stasis (1)", _The British Journal for the Philosophy of Science_, v. XV, February 1965, p. 326.

\textsuperscript{28}Hull, p. 326.

\textsuperscript{29}See above, pp. 31-32.
axiomatic discipline. The reasoning involved in classifying a plant or an animal is of the form:

i) This particular plant/animal has the properties $p_1, p_2, \ldots, p_n$.

ii) All plants/animals having the properties $p_1, p_2, \ldots, p_n$ belong to the taxon $T$.

iii) Therefore, this particular plant/animal belongs to the taxon $T$.

There is one very important point to appreciate about this argument form common to classificatory inquiries. The second premise is a proposition, i.e., it has the property of being true or false. At first glance this may seem inconsistent with my having spoken of the classificatory rules as not being propositions, but of being conventions, i.e., akin to decisions; for surely, it may be objected, the second premise is a classificatory rule? In fact, there is no inconsistency here and it can be shown quite simply. Premise (ii) is not a classificatory rule, but a proposition supported by an appeal to the corresponding classificatory rule. This distinction has a well-known analogy within symbolic logic and explaining this analogous distinction will best clarify the distinction we are concerned with here.

Consider the symbolic system PM$^{30}$. Within this calculus we

$^{30}$From Alfred North Whitehead and Bertrand Russell, Principia Mathematica.
have the definition (i) $p \supset q \equiv df -(p \cdot q)$. Consider now the move from step (a) to step (b) below (the sort of move that might be made in the deductive proof of some theorem of the system):

a) $(p \supset q) \equiv (p \supset q) \text{ (by the Law of Identity + the Rule of Substitution)}$

b) $(p \supset q) \equiv -(p \cdot q) \text{ (by the Definition of } \supset \text{ + the Rule of Substitution).}$

It is important to distinguish (i) from (b) since it has a quite different logical status. In symbolic logic this difference in status is made explicit by the use of different equivalence signs. Thus "$(p \supset q) = df -(p \cdot q)$" is neither true nor false, being within the system PM a stipulative definition, whereas "$(p \supset q) \equiv -(p \cdot q)$" is necessarily true since it is arrived at by an appeal to the stipulated definition. In non-symbolic contexts such as the one under consideration, such an explicit indicator of logical status is rarely found. The same sentence may be used in more than one way. One and the same sentence may have a different logical status depending on the usage involved. A classificatory rule is analogous to the symbolic expression (i) in that it is ultimately conventional. The second premise of the model argument (on p. 87 above) is analogous to the symbolic expression (b) in that it is necessarily true and is justified by an appeal to the corresponding classificatory rule.
Functional Statements

It is sometimes claimed that within biology a mode of analysis is required which is different in kind from that of the physical sciences. It is claimed that the 'teleological' explanations appropriate to this study are not found within the physical sciences. First, I shall assume along with Nagel that 'teleological' or functional statements in biology "normally neither assert nor presuppose in the materials under discussion either manifest or latent purposes, aims, objectives, or goals."31 This is to say, deliberate goals, etc., are not assumed in the explanation of biological phenomena. Since I reserve the label "teleological explanation" for explanation where deliberate goals and aims are under discussion (as in the study of history, for example), I shall speak in the context of biology, where non-conscious purposes or functions are under discussion, of "functional explanation". The important question which presents itself is whether or not there is something special to functional explanation such that a functional statement cannot be reduced to a non-functional statement. Ruse, in his paper, "Functional Statements in Biology"32, reports Nagel as having analysed the functional

31Nagel, p. 402.
The function of chlorophyll in plants is to enable plants to perform photosynthesis into the following two non-functional statements:

1) Chlorophyll is necessary for the performance of photosynthesis in plants.

2) Plants are goal-directed, that is to say, they are capable of persisting towards some end, despite fluctuations and changes in their environment.

Statement (1) may seem a dubious claim, but Nagel himself points out that it is logically possible that plants might manufacture starch without needing chlorophyll. He is asserting merely that "there appears to be no evidence whatever that in view of the limited capacities green plants possess as a consequence of their actual mode of organization, these organisms can live without chlorophyll." To avoid the role of "necessary" in statement (1) being misunderstood, Ruse changes the statement to:

1') Plants perform photosynthesis by using chlorophyll.

The point of the article is that Ruse is accusing Nagel of having confused the two concepts of "adaptation" and

\[ \text{\cite{Nagel, p. 403.}} \]

\[ \text{\cite{Ruse, p. 87.}} \]

\[ \text{\cite{Nagel, p. 404.}} \]
"adaptability", and that statement (2), which states that plants are adaptable (i.e., capable of responding to changes in the environment), is not part of the analysis of the original statement. For any functional statement of the form, "The function of x in z is to do y", Ruse offers the following analysis:

i) z does y by using x.  [cf. statement (1')]  

ii) y is an adaptation (where "we can understand by 'adaptation' either a characteristic which contributes to the reproductive fitness of its possessor, or a characteristic which contributes to the reproductive fitness of an organism of the same species as the possessor."36)

Ruse argues for (ii) by examining the statement, "The function of long hair on dogs is to harbour fleas", and by building up a story such that fleabites provide immunity from some insect parasite. Were this the case, he argues, the statement, "The function of long hair on dogs is to harbour fleas", would be no longer strange, for the harbouring of fleas would then play an important role in the survival and reproduction of dogs. More generally, the 'story' shows that a statement of the form, "The function of x in z is to do y", implies that y is the sort of thing

36Ruse, p. 92.
which aids the survival and reproduction of z. This is the implication which is stated in statement (ii).

Now this appears to be an adequate analysis of a functional statement into non-functional form, and I do not think I can improve on it. There seems to be nothing irreducible about the notion of purpose or function as found in the context of biology. The ease with which a statement of the form, "The function of x in z is to do y", is translated into a non-functional form points out that no intentional concepts are involved, and that to speak of "purpose" or "goal", rather than of "function", is to speak of them in their non-deliberate, non-intentional senses.

In fairness to Nagel, it should be pointed out that upon close inspection he does provide statement (ii) used in the analysis of a functional form statement, although no mention of this is made in Ruse's paper. Speaking of the statement, "The function of chlorophyll in plants is to enable plants to perform photosynthesis", Nagel writes:

This statement accounts for the presence of chlorophyll . . . in plants . . . . It does so by declaring that, when a plant is provided with water, carbon dioxide, and sunlight . . . . it manufactures starch . . . only if the plant contains chlorophyll. The statement usually carries with it the additional tacit assumption that without starch the plant cannot continue its characteristic activities, such as growth and reproduction . . . .

[My emphasis]

37Nagel, p. 403.
It can be said, I think, that Ruse makes explicit the analysis of a biological functional statement into a non-functional form.

... some biologists maintain that the distinctive character of biological explanation appears in physiological inquiries, in which the functions of organs and vital processes are under investigation, even though most biologists are quite prepared to admit that no special categories of explanation are required in morphology or the study of structural traits.\textsuperscript{38}

In the physiological account of the eye, for example, a biologist "specifies the activities in which its various parts can or do participate, and the role these parts play in vision."\textsuperscript{39} Now we have seen that a functional statement of the form, "The function of x in z is to do y", may be reduced to the two non-functional statements (i) and (ii) given on p. 91 above. This is to say that in physiological inquiry where the function of some organ or vital process is under investigation, the biologist may formulate and test out two hypotheses of the forms (i) and (ii). When they have been well established, then he has well established the truth of the corresponding hypothesis of the form, "The function of x in z is to do y." Given that the functional statement is materially equivalent to the two non-functional

\textsuperscript{38}Nagel, pp. 424-425.

\textsuperscript{39}Nagel, p. 425.
statements of the forms (i) and (ii), then we know from the definition of "material equivalence" that, given this equivalence, we can, if presented with statements of the forms (i) and (ii), deduce the corresponding functional statement. The type of reasoning involved here is not special to physiology. Given the above analysis of a functional statement, only the interpreted axiomatic discipline is involved.

**Emergent laws**

Emergentist biologists deny that one can acquire an adequate understanding of an organism by examining its parts in isolation. They claim that certain properties appear only at certain levels of organization ("emergent" properties), and that no matter how diligently one examines the parts individually, i.e., outside of that organization, one will not find evidence of such emergent properties in so doing. Some levels of organization, for example, appear to have a degree of adaptability allowing self-maintenance when environmental changes would otherwise tend to disintegrate the organization. This is claimed to be an organizational feature, i.e., a feature not evidenced by the parts in isolation. This kind of feature may be thought of as an organic feature in that a kind of organic unity is displayed. Such an emergent law (describing some organizational feature), it is claimed, "cannot be confirmed
indirectly, by deduction from more general laws, before direct confirming evidence is at hand."  

According to the definition of an emergent law\(^4\) at least a necessary condition (but perhaps likewise a sufficient condition) of the emergent character of a law of the form "If \(C_1, \ldots, C_n\), then \(R\)" (where the antecedent refers to a set of interacting components, and the consequent to a result of this interaction) is that instances of \(R\) would have to be observed before the law could be known with some probability.  

I do not find in Pap’s articulation of Broad’s concept of an emergent law sufficient emphasis on the biological conception of a "level of organization" and so I shall offer a slightly different definition of "biological, emergent law". At a certain level of organization, \(O\), let us suppose that some organic feature, \(F\), appears. What does it mean to say that \(F\) is a biological, emergent property?  
The emergent law, "The organic feature, \(F\), is manifested at the level of organization, \(O\)\(^5\), cannot be deduced from any combination of laws discovered by observation of lower levels of organization.  

It may be seen from this explication of "emergent property"

\(^{40}\)Pap, p. 366. Pap is referring to Broad’s notion of an emergent law (an "ultimate" law, in Broad’s terminology). See C. D. Broad, The Mind and Its Place in Nature (Paterson, New Jersey, 1960), ch. II.  

\(^{41}\)Pap is referring again to Broad’s notion of an emergent law. See especially the "silver-chloride example", Broad, pp. 64-68.  

\(^{42}\)Pap, p. 366.
(and thus, contextually, of "emergent law") that the biologist must be wary of assuming dogmatically that some feature is an emergent feature. It seems possible that an advancement in physico-chemical laws (the sorts of laws usually used in reference to the parts not exhibiting any organic unity) might cause a biologist to review his belief that a certain feature is an emergent property. It is very easy to assume that laws about certain organic features (for example, such features as "growth" and "self-reproduction") are in principle not reducible to physico-chemical laws. Setting aside my preconceptions, I can find no sound argument for this, although I have encountered plausible arguments against the view.\(^4\) (Of course, a mechanist thesis of the form, "All laws about organic features must be reducible to physico-chemical laws", would be equally dogmatic.)\(^4\)

A biological, emergent law, as I have articulated it, is a law which will not appear as the conclusion of a deductive argument, the premises of which are laws based on observations of lower levels of organization than the one mentioned in the emergent law. This peculiarity in the

\(^4\)See Nagel, pp. 433-435.

\(^4\)It should be noted that I am commenting only on the logical peculiarity of a biological, emergent law. I do not feel competent to weigh the empirical evidence for and against the need for emergent laws within biology.
establishing of an emergent law does not warrant the assumption that statement-networks in which one does appear are distinctive in their inference-patterns. Broad, speaking of a "trans-ordinal law"--his own special version of an emergent law--writes:

A trans-ordinal law is as good a law as any other; and, once it has been discovered, it can be used like any other to suggest experiments, to make predictions, and to give us practical control over external objects.45

Thus one may predict from a biological, emergent law of the form, "The organic feature, F, is manifested at the level of organization, 0", the appearance of F in some particular instance. This is to say, it is possible for emergent laws to appear in statement-networks which involve deductive reasoning, i.e., which embody the interpreted axiomatic discipline.

45Broad, p. 79.
Within this section I shall articulate as clearly as possible the distinctive features of the disciplines (i.e., the types of statement-networks) arrived at by the analyses of the various administrative disciplines mentioned in SECTION II. Where I consider it helpful to do so, a model argument exhibiting some of the features of the network type will be given. Most of the explanations of the features mentioned here will be found in the previous section and I have attempted to avoid repetition of these explanations. The following list of disciplines is by no means claimed to be exhaustive of the disciplines now in use. There may well be other basic types of statement-networks. Neither is it claimed that the disciplined studies mentioned as embodying the various types of statement-networks are the only disciplined studies in which the type of network in question is found.

But first of all, before giving the features distinctive of each discipline, I shall offer a final and, I think, more helpful clarification of the notion of a discipline, i.e., of a type of statement-network.

Final Clarification of the Notion of a "Discipline".
In speaking of different types of statement-networks
I am concerned with the notion of different types of ways of grounding a statement. Let us suppose that we wish to establish the truth of a statement, S, for example, "Food deprivation in rats up to a certain optimum level increases their rate of learning mazes if food pellets are used as rewards." There is in principle no limit to the number of tests we may perform in support of this particular statement. We may run experiments with rats in mazes; then the greater the number of experiments (producing the expected results), the more confident we shall be that the statement is true. But we may also look at the concepts used in the statement, for example, "food deprivation", "learning", "reward", and examine other statements in which they occur. We may then hope to find some kind of relation between some of these other statements and the statement, S. Perhaps we may find some statements that are already well-established and which are on a higher level of generality than S, i.e., which are more fundamental to that particular theoretical network, in which case it is possible that we may sometimes infer with some special degree of sureness (depending on the type of grounding involved) the statement, S, from them. Or we may construct statements on a lower level of generality, in which case it is possible that we may sometimes infer with some special degree of sureness these statements from the statement, S; in this case these
constructed statements will probably (at this stage) have the character of 'hypotheses', or 'proto-hypotheses', to be used in the discovery and support of a more theoretically significant statement, viz, S. If these 'hypotheses', when tested, find support, then they give a kind of (non-deductive) support for the statement, S.\(^\text{46}\)

We may expect that the greater the body of statements to which the statement, S, is in some way related, where these other statements are already well established,

i) the greater the degree of confidence we have in accepting S as true (or, objectively acceptable) or in rejecting S as false (or, objectively unacceptable) since, via its connections with these other statements, there will be other observations or tests which will become relevant to the establishing of S;

and ii) the greater the degree of intelligibility S has in the sense that we are better able to explain the truth (or, acceptability) or falsity (or, unacceptability) of S than if there was no such body of statements since, via its connections with these other statements, we are more able to see the statement in context; we are better able to discourse

\(^{46}\)Cf. Pap's more restricted concept of "conversion of deduction"; Pap, p. 142.
about the subject-matter of the statement, and the statement loses its 'strangeness' in that we see how it 'fits in' with what we already know (or, accept).

Thus we may see that a statement's belonging to a theoretical network both: i) adds support for its truth or falsity (or, acceptability or unacceptability); and ii) allows for a better explanation for it.

This notion of a theoretical network has sometimes been used in the context of the physical and the social sciences. Insofar as I am concerned with the explanations and support of statements via bodies of related statements, insofar as I am concerned with the types of relations to be found within such networks and the resultant differences in the types of inferences that may be made from one statement or a set of statements to another, no matter what the area of study, I shall extend the notion to cover networks of statements found within other areas.

**Axiomatic Discipline**

i) Uninterpreted

Disciplined studies embodying the discipline: Symbolic Logic.

This is the paradigm of "deductive" reasoning. The 'statements' (an extended use of the term to cover the well-formed formulae of symbolic logic) have meaning only insofar as their constituents are defined ultimately in
terms of the undefined primitive terms. 'Statements' of the system are either axioms or definitions, or theorems derived from the axioms via the rules of inference. Given explicit and finite rules of inference for any one system, and given that the theorems do not have meaning in any normal sense (i.e., they have no referential use to 'states of affairs'), then the acceptability of the theorems is logically guaranteed. They are, after all, but the results of certain transformations upon symbolic expressions where one is already given explicit authorization to make these transformations. The relations between a theorem and the theoretical body of 'statements' is thus of the strongest kind, it is logically implied by them. Also, because of the finiteness and explicitness of the rules of inference and of the axioms, this seems to be the only type of network in which the notion of a complete (i.e., final) explanation has any sense.

Summary of features of an uninterpreted axiomatic network:

1) The theorems of the system are logically implied by the theoretical body of 'statements'; i.e., their acceptability is logically guaranteed.

2) One can give a clear sense to the notion of a complete and final explanation in the sense that the theorems are not subject to continual reassessment given the axioms and the rules of inference.
ii) Interpretation

Disciplined studies embodying the discipline: Mathematics, Biology.

(The logicist interpretation of number concepts in arithmetic is rather special in that an attempt is made to give the concepts a referential use by defining them in terms of logical constants. My comments on this attempt are found in the passage on Mathematics, Pure and Applied [see pp. 24-30 above]; I do not intend to focus here on this problematic case.)

We interpret an axiomatic system so that the statements within the system have a referential use. The types of relations between statements within the system are still the same as in the case of an uninterpreted system, but the system as a whole may be appraised. One may give specific interpretations to the primitive terms to see what theorems are produced via the specified rules of inference; these theorems in many cases may then be tested against experience. Failure to find support for the theorems when empirically checked may result in the rejection of the system for the purpose in mind, or in the refinement of the axioms or in the re-interpretation of the terms within them. This last may in fact function to restrict the types of situations in which the system is applicable (consider the restrictions imposed upon the Euclidean geometrical system.
in order to allow for peculiarities of 'spatial' properties and distances found at a microscopic level). Also, it may be appreciated that in refining the axioms of a system one is not in some way finding fault with the system; one is, strictly speaking, creating a new axiomatic system, for the fault was not within the system but in the system's being of no use for a particular purpose. Such refinements and restrictions do not affect the type of relations between statements within the system; they do not affect the types of inferences that may be made within the system; they are assessments of the usefulness of the system. This being so, the network is not different in type (in the way I have explicated the notion of different types of statement-networks) from that of an uninterpreted axiomatic system.

Summary of features of an interpreted axiomatic network:
As for an uninterpreted axiomatic network (see p. 102 above).

Moral Discipline
Disciplined studies embodying the discipline: Ethics.

One may overestimate the strength of the relations between statements found within a moral system by focussing solely on the nature of the first principle. The first principle itself is not supported in the same way as are the DMR's; it is needed in order to support the DMR's. In this respect it is analogous to an axiom. It is assumed, rather
than asserted. It is true that one is in some sense committed to the first principle of one's own moral system in a way in which one is not committed to those of the systems of others (unless they have the same first principle), but the logical status of the first principle is not affected by this—it is still a non self-evident statement which is assumed in very much the same manner as an axiom. Rather this commitment is a feature of our assessment of the system as a whole—we have decided to use the system to guide our decisions.

Now the similarity (in the above mentioned respect) between the first principle of a moral system and an axiom can mislead one into thinking that the relations between statements within the moral system are stronger than they are, i.e., one may think that they too are like those found within an axiomatic system. But one must bear in mind the constant intervention of empirical statements, i.e., of means-end generalizations.

Model argument:

(FP) (First principle): "Pursue/do X in all choice-situations."

(EG) (Empirical generalization): "Doing Y tends to produce X."

(DMR) (Derived moral rule): "Do Y, ceteris paribus."

Because of this intervention, the DMR's are less than
logically guaranteed. Since the empirical statements are subject to continual reassessment and possible rejection, so too are the DMR's resting upon them. For the same reason the notion of giving a complete (i.e., final) explanation here has no sense. It is true, however, that the 'rules of derivation' used to produce new DMR's from the first principle and used to produce one DMR from among several possible DMR's, are explicit to a high degree (see the passage on Moral Reasoning above, especially pp. 71-77), but again they are not as explicit as those found within an axiomatic system (since, for example, one of the more general rules will be to the effect--"Make use of statements giving means to the end mentioned in the first principle", and this rule does not refer us to a finite and finalized set of statements).

Summary of features of a moral network:

1) The first principle is analogous to an axiom in that it is not supported as are those principles derived from it, i.e., it is assumed, rather than asserted.

2) The first principle is disanalogous to an axiom in that it must, in an important sense, have content. This content will be to the effect that something is intrinsically desirable/desirable in itself or that some course of action should be undertaken in all choice-situations.
3) The DMR's are less than logically guaranteed and, because of the intervention of empirical statements, are subject to continual reassessment.

4) Thus the notion of giving a complete and final explanation for some DMR has no sense.

(On Teleological Explanation)
[This is not a presentation of another discipline.]

I shall here argue that the moral network is the type of network which is the key to understanding teleological explanation. In giving a teleological explanation we are giving the reasons why a person performs a certain action or makes a certain decision. It may be noted here that we do not have the methodological presupposition that people always act consistently, i.e., always with an eye on one first principle, although an explanation of a person's actions is, of course, much easier to give if he does act fairly consistently. To the extent that one is aware of a person's long-term goals, and to the extent that one is aware of relevant means-end generalizations, one may set about giving an account of why a person performs some particular action or set of actions. Thus a statement of the form, "He does X for such-and-such a reason", finds its best support against a background of statements about the person's professed goals and statements about the various
means by which to reach these goals. If there are no statements relating to the person's professed aims, some estimation may nevertheless be made by viewing his actions to see whether they tend in some direction. It is obvious, I think, that the type of relation between the body of statements (about the person's aims and the means-end generalizations) and the statement under investigation (that he performs a particular action/set of actions for such-and-such a reason) is far less tight than the analogous relation found within an axiomatic system (cf. feature (3) in the Summary of features of a moral network, p. 107 above). We have far less right to be sure of our conclusion. There is no limit to the body of statements to which the statement in question may relate. There is no limit to the number of statements which may help to support and explain the statement—not only because of the intervention of the empirical means-end generalizations, but also because, even when one has within the body of statements some relating to the person's professed goals, it is possible that one may later acquire information strongly suggesting that the person was lying or joking when he divulged his 'goals'.

It is important to note that in giving an account of why a person performs a certain action, one person may be considered in some sense to be in a special position—namely, the person whose action we are considering. (Whether or not
we shall allow that his claims about his reasons for doing something have the status of knowledge claims--some verificationalists, for example, the later Wittgenstein⁴⁷, almost certainly would not allow them this status--does not materially affect the point that there is some sort of asymmetry at work here between his claims about his reasons and the claims of others about his reasons. Were we to ask him why he does so-and-so, he would not have to make the same kinds of moves as should we. (Cf. the moves one makes in order to decide whether or not some person other than oneself is feeling pain with cases where the question is whether oneself is in pain.)

I am suggesting that the moral network type is used in two very different ways:

i) First person use
Here it is used in order to decide which of several actions I should perform. Here only one moral system is referred to, and it is distinguished from all others in that I am in some sense committed to its first principle.

ii) Third person use
Here it is used in order to explain why a person

other than myself performs a particular action/set of actions, i.e., it is used in giving a teleological explanation. Here we must be prepared to make use of different systems in explaining the actions of different people, and we may or may not find that one of the systems we make use of is the one we are committed to.

I shall now present diagrammatically how this use of the moral network type fits into the giving of a teleological explanation. (I think this is by far the clearest fashion of presenting this point.)

I am thus suggesting that the network embedded in a teleological explanation is not different in type from the moral network type already presented. For this reason I am also suggesting that the moral discipline is found within the following disciplined studies: History, Sociology and Law.
Pre-condition for giving a teleological explanation: one needs to establish that the person in question is acting rationally. The question, "Is he acting rationally?" may be 'spelled out' into questions about his intelligence (for example, "Is he sufficiently intelligent to plan actions?"), any relevant physical health aspects (for example, "Is he under the influence of drugs?", "Is he suffering from brain damage or a brain tumour?"), about his age (for example, "Is he no more than a child?"), etc.. There is no finite and finalized set of questions here, although some are obvious.

Teleological Explanation

a) **Empirical checks on the person's veracity**--applicable only where the person in question gives or there is some record of his having given in some form or other some statement(s) as to his goals and values. These checks are necessary insofar as statements relating to his professed goals are found within the body of statements used to support the particular statement under investigation (see pp. 107-108 above).

b) **Third person use of the moral network type**

Used in order to explain why (i.e., for what reason) a person other than myself performs a particular action/set of actions.

First person use of the moral network type

Used in order to decide which action of a set of possible actions I should perform.
I am focusing within this thesis on statistical probability rather than on logical probability. We know that there is a concept of probability other than the frequency interpretation since, if we are faced with the choice of betting that "The next person we meet will have black hair" or betting that "The next person we meet will have either black hair or red hair", we shall bet on the latter statement (even when we have no relevant empirical evidence). This statement is a priori more probable than the former; it covers more "possible states of affairs". To say that one statement is a priori more probable than another is to say that it is more probable relative to no empirical evidence (i.e., in the absence of any relevant empirical evidence). Although my focus is on statistical probability, a close examination of the statistical network involved in statistical generalization has revealed that some mention of a priori probabilities is unavoidable.\^8

\^8I shall make use of the traditional, Laplacian concept of logical probability based on the notion of equally specific possible outcomes, rather than on recent refinements of the concept, for example, Carnap's use of the notion of a "structure-description" rather than a "state-description" as the basis for an interpretation of logical probability. To use Carnap's work would involve introducing an unbearable amount of further detail without any compensating gain since
I am concerned with the network type involved in statistical generalization (see p. 49 above). Model arguments have been devised in order to exhibit some of the peculiarities of the statistical discipline.

Model argument (A):

1) In a sample of N members of the class, K, R members have the property, P; i.e., the frequency of P in the sample is the fraction, \( \frac{R}{N} \).

2) The sample is a random sample of members of the class, K.

3) Therefore, the frequency of P in the (whole) class, K, is \( \frac{R}{N} \).

This is a very general model for statistical generalization. Obviously the greater the size of the sample in question, the greater our confidence in the conclusion. The reason for this increase in confidence finds an articulation in Bernoulli's theorem. The gist of the theorem is that of all the frequencies of occurrence of some type of event of probability, p, possible in a sample series of occasions, the most probable is that frequency equalling p. (This statement is, in fact, analytic--it helps to clarify the frequency interpretation of the notion of probability.)

The theorem asserts that "a close approximation of the relative frequency to the probability becomes more and more probable as \( n \) [the number of elements in the sample] increases."\(^4^9\)

It is important to note, however, that the notion of having full confidence about the conclusion (in the same way as we have full confidence about the theorems of an axiomatic system in that they are logically guaranteed) is out of place here. No matter how large a sample we test, it still remains a sample:

We might say that probability statements are about open classes in the sense that no matter how many observed members the class has at any given time, it is always logically possible that it have still other—so far unobserved—members. This is logically possible in the sense that it is not excluded by the meaning of the predicate by which the class is determined. Clearly, openness in this sense is compatible with finitude.\(^5^0\)

Let us now look at line (2) of the model. To say that a sample is random is to say that there is as much chance of any one member of the class involved being included in the sample as there is of any other member being included. Suppose we have an urn containing four balls, two of which are white and two of which are black; i.e., we have the balls

\(^{4^9}\)pap, p. 183.

\(^{5^0}\)pap, 190.
Suppose also that we extract random samples of two balls to test for the frequency of W-balls. To say that the sampling is random is to say that each ball has an equal chance of being extracted; i.e., the following possible outcomes of the sampling are equally probable:

1) $W_1W_2$
2) $W_1B_1$
3) $W_1B_2$
4) $W_2B_1$
5) $W_2B_2$
6) $B_1B_2$

This notion of randomness is thus closely connected with the notion of a priori probability, for we may say that the above possible outcomes, being equally specific, are equally probable. (The outcomes, "Both W", and "One W and one B", for example, are not equally specific in that there are more ways in which the second outcome may appear.)

Insofar as we are concerned with the frequency of W-balls, the specific ball appearing in a sample is irrelevant to our investigation. We are testing for the frequency of a type of member. Given that the above combinations of two balls are equi-probable, we may expect within a set of six samplings (where each sample has two balls):

1 case in which the frequency of W-balls $= \frac{2}{2} = 1$;
1 case in which the frequency of W-balls $= \frac{0}{2} = 0$;
4 cases in which the frequency of W-balls $= \frac{1}{2}$.

Ex hypothesi we know that the frequency of W-balls in the
finite 'class' in question is $\frac{2}{4} = \frac{1}{2}$. We may note, then, that even though this is so, other frequencies occur every now and then as a result of random sampling. In generalizing from the frequency of a property in a sample to the frequency of the property in the class [as in model (A)], one may expect to find our generalization rejected later a certain percentage of the time. In a sense, it is built into the notion of the statistical discipline found within statistical generalization that it will, a certain percentage of the time, fail us.

The conclusion of the model argument is a general statement; we have generalized from premise (1). It has a predictive content in that it states more than is contained in premise (1), and insofar as it has a definite predictive content, it is open to rejection in the light of further observations. This is to say, it is possible that new arguments may be constructed along the lines of model (A) where (in the new arguments) the sample is larger than the sample mentioned in the original argument; where this occurs, precedence is given to the conclusion based on the larger sampling. Because of our greater confidence in the conclusion the larger the sample tested, and because of the "openness" of the class involved, the frequency for the (whole) class is often identified with the limit towards which the frequencies tend as the samples become larger and
larger (see p. 51 above). (This mathematical notion of a limit is dependent on there being an infinite series of numbers; it makes explicit the openness of the class.)

I shall now present a model argument which is, in fact, no more than a proto-version of model (C):

Model argument (B):

1) There is a correlation between the variables, V and W, in a sample of the class, K.

2) The sample is a random sample of members of the class, K.

3) Therefore, there is a correlation between the variables, V and W, in the (whole) class, K.

For the modern behavioural psychologist a statement of the form, "There is a correlation between V and W", is distinct from a statement of the form, "There is a causal connection between V and W" (where we are using the traditional notion of causality). To say that V is correlated with W is not to say that when we find V, we shall always find W. The psychologist tends to make use of the notion of "correlation" specifically where there is less than one hundred per cent 'correlation'. This being so, there is a problem in constructing a precise hypothesis to test for premise (1), for the premise, as it stands, does not state the degree of correlation between V and W. It is for this reason that use is made of a null hypothesis--that there is no correlation between V and W--for any degree of correlation will falsify this hypothesis, i.e., the hypothesis is easy to test (see
In order to arrive at a generalization of the sort given in model (B), an argument along the lines of the following model is, in fact, used:

Model argument (C):

1) On the assumption that there is no correlation between the two variables, V and W, (the "null hypothesis"), we expect to find them occurring together by chance in a certain number, \( n_1 \), of a sample of N subjects in the class, K (i.e., \( n_1/N \) is the expected frequency of VW-subjects within the sample).

2) The observed frequency of VW-subjects in the sample is \( n_2/N \).

3) The difference between \( n_1/N \) and \( n_2/N \) (i.e., between the expected and observed frequencies of VW-subjects in the sample) is given by the measure, Y.

4) The chances of Y's occurring as a result of random sampling are, say, 19 to 1 (this is the "5% level of significance" commonly tested for in behavioural psychology).

5) Therefore, the general statement that there is no correlation between the variables, V and W, in members of the class, K (the "null hypothesis"), has only a 1 in 20 chance of being true. [Via model (A).]

(Compare line (4) of the model with the example of the balls in the urn [pp. 115-116 above], where we expect 'unrepresentative' frequencies of W-balls to appear in the samples some percentage of the time.) Thus one establishes the general statement (3) of model (B) by showing how unlikely it is that its negation is true. To say that "-p" is unlikely to be true is, of course, to say that "p" is likely to be true. This move allows for a degree of support for the generalization of model (B) without the experimenter's
having any preconceptions as to the degree of correlation involved. Upon close consideration I suggest, therefore, that arguments along the lines of model (C) are not different in type from model (A) and that they embody, in fact, a disguised version of the statistical generalization network.

Summary of features of a statistical network:

(It must be remembered that I am concerned with statistical generalization where inferences are made from statements giving the frequency of a property/correlation of properties for a sample of a class to a statement giving the same frequency of the property/correlation of properties for the whole class.)

1) The larger the sample mentioned in the body of statements from which the generalization is inferred, the stronger the support for the inferred statement.

2) But the body of statements from which the generalization is inferred is never able to guarantee the inferred statement.

3) The inferred generalization has a predictive content such that it is always open to revision in the light of further observations. (This 'ties in' with the openness of the class involved.)

4) There is a sense in which it is built into the notion of the statistical network exhibited in model (A) that it
will, a certain percentage of the time, fail us. (This is based, as I have shown, on the use of the notion of random sampling.)
SECTION IV.

In this final, short section an educational aim is recommended which justifies an important place for the teaching of the logical disciplines in secondary school, and some suggestions are made for further research in the subject area of the thesis.

An Educational Aim

Martin, in discussing a variation of the Discipline Principle, viz, "Only the disciplines should be taught", is concerned about the lack of "relevance" of teaching the disciplines to the solving of everyday, practical problems.51 She speaks of the Discipline Principle functioning together with the Structure Principle so that advocates of the Discipline Principle insist that the structure of each discipline be taught first and foremost, and, furthermore, that the structure be taught from the standpoint of the practitioner of the discipline.52 Thus we read:

Given a conflict of interests--to illuminate some vital everyday sort of topic or to illuminate the methods and structure of the disciplines--the

51 See Martin, "The Disciplines and the Curriculum."
52 See Martin, p. 75.
outcome is never in doubt: the methods of the disciplines have priority.$^53$

Now to be critical of the priorities mentioned in the above quotation (as Martin is) is to be educationally misguided, and is based on a confused understanding of the priorities.

Let us look at the former of these two claims. In his everyday experience a person will meet with different types of problems which must be tackled in different kinds of ways. In terms of my own conceptual apparatus, to say that two problems are of different types is to say that different disciplines (as I use this term) or different groups of disciplines are called into play in the solving of them. Martin speaks of Physics as a discipline.$^54$

Now for a person to understand that a problem he is now faced with is a physics problem (and therefore to be approached using the concepts, methods and principles of that 'discipline'), it just is not enough to be able to say: I know that this is a physics problem because it was mentioned in a physics course at secondary school. This is to define the term "physics problem" extensionally, i.e., as one of those problems labelled "physics problem" at school. This is a disastrously sterile way of grounding

$^53$ Martin, p. 77.

$^54$ Martin, p. 68.
differences between types of problems. In itself, it does not allow of differentiating types of problems when the person has not previously met with the problems under consideration. It is surely desirable that a person should be able to tackle problems of different types even though he may no longer be under the guidance of secondary school teachers. But unless he has been provided with the necessary 'tools' for this purpose (foremost among which are certain logical and conceptual apparatuses), such progressive problem solving cannot be expected. To understand that a certain problem is a physics problem, he must have some kind of characterization of that 'discipline'; he must have some understanding of the methods and type of reasoning involved that make it a 'discipline' on its own.

I am concerned to ensure that students, upon leaving secondary school, have:

a) the necessary conceptual tools for understanding what type of problem/question he is faced with—in those cases where he has not met with the problem/question in a neatly labelled secondary school course (i.e., he should be able to understand which disciplines are involved in solving the problem);

b) the means for understanding how this problem should, therefore, be approached (which methods, which types of reasoning are relevant).
To ignore the "structure of the disciplines", therefore, I hold to be fundamentally misguided.

I claim above that Martin is also confused in her understanding of the priorities mentioned in the quotation on p. 121 above. I shall now explain what is meant by this. Teaching students the structural differences between the disciplines, their different types of reasoning, their different logical methodologies, is one very important educational aim. I have mentioned why I consider it to be so important—I refer to the need to be able to recognise and set about solving hitherto unknown problems of differing logical types. I have not, as a philosopher of education, said how these differences are to be taught. This is an empirical question. Now, insofar as the reason behind the aim is what it is, one has not achieved the aim if students cannot recognise and understand how to approach problems of differing types which they do in fact meet with. The "conflict" mentioned in the quotation above is far less obvious than Martin realises. It may very well be that one may teach the differences between the disciplines via specific topics and problems; but somewhere in a secondary school instruction, students need to have explained why and how one problem is different in type from another.
Suggestions for Further Research

There are two obvious areas for further investigation:

a) Analyses of further administrative disciplines.

There are many administrative disciplines not analysed in the thesis, and an examination of them may bring to light new disciplines which are found to be embodied within them. For example, there may well be other types of statistical networks than that found in statistical generalization. This area of investigation falls within the province of philosophy of education.

b) Investigation into how the disciplines should be taught.

We may ask, for example, whether the teaching of the disciplines should be "topic orientated", or whether the structure of the disciplines should be taught in some more general form, if the disciplines are to be learned as efficiently as possible. (Ultimately, of course, we should wish the general features of the different disciplines to be explained to the students.) We should also wish to know at what age students are able to learn and handle the conceptual and logical apparatuses necessary for an understanding of the different disciplines. Such questions are straightforwardly empirical; they do not fall within the province of philosophy of education, although they are relevant to a realization of the educational aim recommended.
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