CHAPTER VI

DEGREES OF TESTABILITY

THEORIES may be more, or less, severely testable; that is to say, more, or less, easily falsifiable. The degree of their testability is of significance for the selection of theories.

In this chapter, I shall compare the various degrees of testability or falsifiability of theories through comparing the classes of their potential falsifiers. This investigation is quite independent of the question whether or not it is possible to distinguish in an absolute sense between falsifiable and non-falsifiable theories. Indeed one might say of the present chapter that it 'relativizes' the requirement of falsifiability by showing falsifiability to be a matter of degree.

31. *A Programme and an Illustration.*

A theory is falsifiable, as we saw in section 23, if there exists at least one non-empty class of homotypic basic statements which are forbidden by it; that is, if the class of its potential falsifiers is not empty. If, as in section 23, we represent the class of all possible basic statements by a circular area, and the possible events by the radii of the circle, then we can say: At least *one* radius—or perhaps better, one narrow sector whose width may represent the fact that the event is to be 'observable'—must be incompatible with the theory and ruled out by it. One might then represent the potential falsifiers of various theories by sectors of various widths. And according to the greater and lesser width of the sectors ruled out by them, theories might then be said to have more, or fewer, potential falsifiers. (The question whether this 'more' or 'fewer' could be made at all precise will be left open for the moment.) It might then be said, further, that if the class of potential falsifiers of one theory is 'larger' than that of another, there will be

more opportunities for the first theory to be refuted by experience; thus compared with the second theory, the first theory may be said to be 'falsifiable in a higher degree'. This also means that the first theory *says more* about the world of experience than the second theory, for it rules out a larger class of basic statements. Although the class of permitted statements will thereby become smaller, this does not affect our argument; for we have seen that the theory does not assert anything about this class. Thus it can be said that the amount of empirical information conveyed by a theory, or its *empirical content*, increases with its degree of falsifiability.

Let us now imagine that we are given a theory, and that the sector representing the basic statements which it forbids becomes wider and wider. Ultimately the basic statements *not* forbidden by the theory will be represented by a narrow remaining sector. (If the theory is to be consistent, then some such sector must remain.) A theory like this would obviously be very easy to falsify, since it allows the empirical world only a narrow range of possibilities; for it rules out almost all conceivable, *i.e.* logically possible, events. It asserts so much about the world of experience, its empirical content is so great, that there is, as it were, little chance for it to escape falsification.

Now theoretical science aims, precisely, at obtaining theories which are easily falsifiable in this sense. It aims at restricting the range of permitted events to a minimum; and, if this can be done at all, to such a degree that any further restriction would lead to an actual empirical falsification of the theory. If we could be successful in obtaining a theory such as this, then this theory would describe 'our particular world' as precisely as a theory can; for it would single out the world of 'our experience' from the class of all logically possible worlds of experience with the greatest precision attainable by theoretical science. All the events or classes of occurrences which we actually encounter and observe, and only these, would be characterized as 'permitted'.*1

32. *How are Classes of Potential Falsifiers to be Compared?*

The classes of potential falsifiers are infinite classes. The intuitive

*1 For further remarks concerning the aims of science, see appendix *x, and section *15 of the *Postscript*, and my paper 'The Aim of Science', *Ratio* **1**, 1957, pp. 24–35.

'more' and 'fewer' which can be applied without special safeguards to finite classes cannot similarly be applied to infinite classes.

We cannot easily get round this difficulty; not even if, instead of the forbidden basic statements or *occurrences*, we consider, for the purpose of comparison, classes of forbidden *events*, in order to ascertain which of them contains 'more' forbidden events. For the number of events forbidden by an empirical theory is also infinite, as may be seen from the fact that the conjunction of a forbidden event with any other event (whether forbidden or not) is again a forbidden event.

I shall consider three ways of giving a precise meaning, even in the case of infinite classes, to the intuitive 'more' or 'fewer,' in order to find out whether any of them may be used for the purpose of comparing classes of forbidden events.

(1) The concept of the *cardinality* (*or power*) *of a class*. This concept cannot help us to solve our problem, since it can easily be shown that the classes of potential falsifiers have the same cardinal number for all theories.[1]

(2) *The concept of dimension.* The vague intuitive idea that a cube in some way contains more points than, say, a straight line can be clearly formulated in logically unexceptionable terms by the set-theoretical concept of dimension. This distinguishes classes or sets of points according to the wealth of the 'neighbourhood relations' between their elements: sets of higher dimension have more abundant neighbourhood relations. The concept of dimension which allows us to compare classes of 'higher' and 'lower' dimension, will be used here to tackle the problem of comparing degrees of testability. This is possible because basic statements, combined by conjunction with other basic statements, again yield basic statements which, however, are 'more highly composite' than their components; and this degree of composition of basic statements may be linked with the concept of dimension. However, it is not the composition of the forbidden events but that of the permitted ones which will have to be used. The reason is that the events forbidden by a theory can be of any degree of composition; on the other hand, some of the permitted statements are

[1] Tarski has proved that under certain assumptions every class of statements is denumerable (*cf. Monatshefte f. Mathem. u. Physik* **40**, 1933, p. 100, note 10). * The concept of measure is inapplicable for similar reasons (*i.e.* because the set of all statements of a language is denumerable).

permitted merely because of their form or, more precisely, because their degree of composition is too low to enable them to contradict the theory in question; and this fact can be used for comparing dimensions.*1

(3) The *subclass relation*. Let all elements of a class α be also elements of a class β, so that α is a subclass of β (in symbols: $\alpha \subset \beta$). Then either all elements of β are in their turn also elements of α—in which case the two classes are said to have the same extension, or to be identical—or there are elements of β which do not belong to α. In the latter case the elements of β which do not belong to α form 'the difference class' or the *complement* of α with respect to β, and α is a *proper subclass* of β. The subclass relation corresponds very well to the intuitive 'more' and 'fewer', but it suffers from the disadvantage that this relation can only be used to compare the two classes if one includes the other. If therefore two classes of potential falsifiers intersect, without one being included in the other, or if they have no common elements, then the degree of falsifiability of the corresponding theories cannot be compared with the help of the subclass relation: they are non-comparable with respect to this relation.

33. *Degrees of Falsifiability Compared by Means of the Subclass Relation.*

The following definitions are introduced provisionally, to be improved later in the course of our discussion of the dimensions of theories.*1

(1) A statement x is said to be 'falsifiable in a higher degree' or 'better testable' than a statement y, or in symbols: $Fsb(x) > Fsb(y)$, if and only if the class of potential falsifiers of x includes the class of the potential falsifiers of y as a *proper subclass*.

(2) If the classes of potential falsifiers of the two statements x and y are identical, then they have the same degree of falsifiability, i.e. $Fsb(x) = Fsb(y)$.

*1 The German term '*komplex*' has been translated here and in similar passages by '*composite*' rather than by '*complex*'. The reason is that it does *not* denote, as does the English 'complex', the opposite of 'simple'. The opposite of 'simple' ('*einfach*') is denoted, rather, by the German '*kompliziert*'. (*Cf.* the first paragraph of section 41 where '*kompliziert*' is translated by 'complex'.) In view of the fact that *degree of simplicity* is one of the major topics of this book, it would have been misleading to speak here (and in section 38) of *degree of complexity*. I therefore decided to use the term '*degree of composition*' which seems to fit the context very well.

*1 See section 38, and the appendices i, *vii, and *viii.

(3) If neither of the classes of potential falsifiers of the two statements includes the other as a proper subclass, then the two statements have non-comparable degrees of falsifiability ($Fsb(x) \parallel Fsb(y)$).

If (1) applies, there will always be a non-empty complement class. In the case of universal statements, this complement class must be infinite. It is not possible, therefore, for the two (strictly universal) theories to differ in that one of them forbids a finite number of single occurrences permitted by the other.

The classes of potential falsifiers of all tautological and metaphysical statements are empty. In accordance with (2) they are, therefore, identical. (For empty classes are subclasses of all classes, and hence also of empty classes, so that all empty classes are identical; which may be expressed by saying that there exists only *one* empty class.) If we denote an empirical statement by 'e', and a tautology or a metaphysical statement (*e.g.* a purely existential statement) by 't' or 'm' respectively, then we may ascribe to tautological and metaphysical statements a zero degree of falsifiability and we can write: $Fsb(t) = Fsb(m) = 0$, and $Fsb(e) > 0$.

A self-contradictory statement (which we may denote by 'c') may be said to have the class of all logically possible basic statements as its class of potential falsifiers. This means that any statement whatsoever is comparable with a self-contradictory statement as to its degree of falsifiability. We have $Fsb(c) > Fsb(e) > 0$.*2 If we arbitrarily put $Fsb(c) = 1$, i.e. arbitrarily assign the number 1 to the degree of falsifiability of a self-contradictory statement, then we may even define an empirical statement e by the condition $1 > Fsb(e) > 0$. In accordance with this formula, $Fsb(e)$ always falls within the interval between 0 and 1, excluding these limits, i.e. within the 'open interval' bounded by these numbers. By excluding contradiction and tautology (as well as metaphysical statements) the formula expresses at the same time both *the requirement of consistency and that of falsifiability*.

34. *The Structure of the Subclass Relation. Logical Probability.*

We have defined the comparison of the degree of falsifiability of two statements with the help of the subclass relation; it therefore

*2 See however now appendix *vii.

shares all the structural properties of the latter. The question of comparability can be elucidated with the help of a diagram (fig. 1), in which certain subclass relations are depicted on the left, and the corresponding testability relations on the right. The Arabic numerals

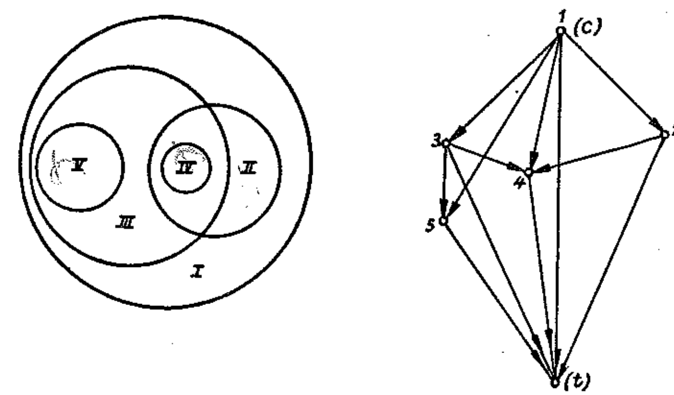

Figure 1

on the right correspond to the Roman numerals on the left in such a way that a given Roman numeral denotes the class of the potential falsifiers of that statement which is denoted by the corresponding Arabic numeral. The arrows in the diagram showing the degrees of testability run from the better testable or better falsifiable statements to those which are not so well testable. (They therefore correspond fairly precisely to derivability-arrows; see section 35.)

It will be seen from the diagram that various sequences of subclasses can be distinguished and traced, for example the sequence I–II–IV or I–III–V, and that these could be made more 'dense' by introducing new intermediate classes. All these sequences begin in this particular case with 1 and end with the empty class, since the latter is included in every class. (The empty class cannot be depicted in our diagram on the left, just because it is a subclass of every class and would therefore have to appear, so to speak, everywhere.) If we choose to identify class 1 with the class of all possible basic statements, then 1 becomes the contradiction (c); and 0 (corresponding to the empty class) may then denote the tautology (t). It is possible to pass from 1 to the empty class, or from (*c*) to (*t*) by various paths; some of these, as can be seen from the right hand diagram, may cross one another. We may therefore say that the structure of the relation is that of a lattice (a 'lattice of sequences' ordered by the arrow, or the subclass relation). There are nodal points (*e.g.* statements 4 and 5) in which the lattice is partially connected. The relation is totally connected only in the universal class and in the empty class, corresponding to the contradiction *c* and tautology *t*.

Is it possible to arrange the degrees of falsifiability of various statements on one scale, *i.e.* to correlate, with the various statements, numbers which order them according to their falsifiability? Clearly, we cannot possibly order all statements in this way;[*1] for if we did, we should be arbitrarily making the non-comparable statements comparable. There is, however, nothing to prevent us from picking out one of the sequences from the lattice, and indicating the order of its statements by numbers. In so doing we should have to proceed in such a way that a statement which lies nearer to the contradiction *c* is always given a higher number than one which lies nearer to the tautology *t*. Since we have already assigned the numbers 0 and 1 to tautology and contradiction respectively, we should have to assign *proper fractions* to the empirical statements of the selected sequence.

I do not really intend, however, to single out one of the sequences. Also, the assignment of numbers to the statements of the sequence would be entirely arbitrary. Nevertheless, the fact that it is possible to assign such fractions is of great interest, especially because of the light it throws upon the connection between degree of falsifiability and the idea of *probability*. Whenever we can compare the degrees of falsifiability of two statements, we can say that the one which is the less falsifiable is also the more probable, by virtue of its logical

[*1] I still believe that the attempt to make all statements comparable by introducing a *metric* must contain an arbitrary, extra-logical element. This is quite obvious in the case of statements such as 'All adult men are more than two feet high' (or 'All adult men are less than nine feet high'); that is to say, statements with predicates stating a measurable property. For it can be shown that the metric of content or falsifiability would have to be a function of the metric of the predicate; and the latter must always contain an arbitrary, or at any rate an extra-logical element. Of course, we may construct artificial languages for which we lay down a metric. But the resulting measure will not be purely logical, however 'obvious' the measure may appear as long as only discrete, qualitative yes-or-no predicates (as opposed to quantitative, measurable ones) are admitted. See also appendix *ix, the Second and Third Notes.

form. This probability I call[*2] *'logical probability'*;[1] it must not be confused with that numerical probability which is employed in the theory of games of chance, and in statistics. *The logical probability of a statement is complementary to its degree of falsifiability:* it increases with decreasing degree of falsifiability. The logical probability 1 corresponds to the degree 0 of falsifiability, and *vice versa*. The better testable statement, *i.e.* the one with the higher degree of falsifiability, is the one which is logically less probable; and the statement which is less well testable is the one which is logically more probable.

As will be shown in section 72, *numerical* probability can be linked with logical probability, and thus with degree of falsifiability. It is possible to interpret numerical probability as applying to a subsequence (picked out from the logical probability relation) for which a *system of measurement* can be defined, on the basis of frequency estimates.

These observations on the comparison of degrees of falsifiability do not hold only for universal statements, or for systems of theories; they can be extended so as to apply to singular statements. Thus they hold, for example, for theories in conjunction with initial conditions. In this case the class of potential falsifiers must not be mistaken for a class of events—for a class of homotypic basic statements—since it is a class of occurrences. (This remark has some bearing on the connection between logical and numerical probability which will be analysed in section 72.)

35. *Empirical Content, Entailment, and Degrees of Falsifiability.*

It was said in section 31 that what I call the *empirical content* of a statement increases with its degree of falsifiability: the more a statement forbids, the more it says about the world of experience. (*Cf.*

[*2] I now (since 1938; *cf.* appendix *ii) use the term 'absolute logical probability' rather than 'logical probability' in order to distinguish it from 'relative logical probability' (or 'conditional logical probability'). See also appendices *iv, and *vii to *ix.

[1] To this idea of logical probability (inverted testability) corresponds Bolzano's idea of validity, especially when he applies it to the *comparison of statements*. For example, he describes the major propositions in a derivability relation as the statements of lesser validity, the consequents as those of greater validity (*Wissenschaftslehre*, 1837, Vol. II, §157, No. 1). The relation of his concept of validity to that of probability is explained by Bolzano in *op. cit.* §147. *Cf.* also Keynes, *A Treatise on Probability*, 1921, p. 224. The examples there given show that my comparison of logical probabilities is identical with Keynes's 'comparison of the probability which we ascribe *a priori* to a generalization'. See also notes 1 to section 36 and 1 to section 83.

section 6.) What I call 'empirical content' is closely related to, but not identical with, the concept 'content' as defined, for instance, by Carnap.[1] For the latter I will use the term 'logical content', to distinguish it from that of *empirical* content.

I define the *empirical content* of a statement p as the class of its potential falsifiers (*cf.* section 31). The *logical content* is defined, with the help of the concept of derivability, as the class of all non-tautological statements which are derivable from the statement in question. (It may be called its 'consequence class'.) So the logical content of p is *at least equal* to (*i.e.* greater than or equal to) that of a statement q, if q is derivable from p (or, in symbols, if '$p \rightarrow q$'[*1]). If the derivability is mutual (in symbols, '$p \leftrightarrow q$'[*1]) then p and q are said to be of equal content.[2] If q is derivable from p, but not p from q, then the consequence class of q must be a proper sub-set of the consequence class of p; and p then possesses the larger consequence class, and thereby the greater logical content (or logical force[*2]).

It is a consequence of my definition of *empirical content* that the comparison of the logical and of the empirical contents of two statements p and q leads to the same result if the statements compared contain no metaphysical elements. We shall therefore require the following: (a) two statements of equal logical content must also have equal empirical content; (b) a statement p whose logical content is greater than that of a statement q must also have greater empirical content, or at least equal empirical content; and finally, (c) if the empirical content of a statement p is greater than that of a statement q, then the logical content must be greater or else non-comparable. The qualification in (b) 'or at least equal empirical content' had to be added because p might be, for example, a conjunction of q with some purely existential statement, or with some other kind of metaphysical statement to which we must ascribe a certain logical content; for in

[1] Carnap, *Erkenntnis* **2**, 1932, p. 458.

[*1] '$p \rightarrow q$' means, according to this explanation, that the conditional statement with the antecedent p and the consequent q is *tautological*, or logically true. (At the time of writing the text, I was not clear on this point; nor did I understand the significance of the fact that an assertion about deducibility was a meta-linguistic one. See also note *1 to section 18, above.) Thus '$p \rightarrow q$' may be read here: '*p entails q*'.

[2] Carnap, *op. cit.*, says: 'The metalogical term "equal in content" is defined as "mutually derivable".' Carnap's *Logische Syntax der Sprache*, 1934, and his *Die Aufgabe der Wissenschaftslogik*, 1934, were published too late to be considered here.

[*2] If the logical content of p exceeds that of q, then we say also that p is *logically stronger* than q, or that its *logical force* exceeds that of q.

this case the empirical content of p will not be greater than that of q. Corresponding considerations make it necessary to add to (c) the qualification 'or else non-comparable'.[*3]

In comparing degrees of testability or of empirical content we shall therefore as a rule—*i.e.* in the case of purely empirical statements—arrive at the same results as in comparing logical content, or derivability-relations. Thus it will be possible to base the comparison of degrees of falsifiability to a large extent upon derivability relations. Both relations show the form of lattices which are totally connected in the self-contradiction and in the tautology (*cf.* section 34). This may be expressed by saying that a self-contradiction entails every statement and that a tautology is entailed by every statement. Moreover, *empirical* statements, as we have seen, can be characterized as those whose degree of falsifiability falls into the open interval which is bounded by the degrees of falsifiability of self-contradictions on the one side, and of tautologies on the other. Similarly, *synthetic* statements in general (including those which are non-empirical) are placed, by the entailment relation, in the open interval between self-contradiction and tautology.

To the positivist thesis that all non-empirical (metaphysical) statements are 'meaningless' there would thus correspond the thesis that my distinction between *empirical* and *synthetic* statements, or between *empirical* and *logical* content, is superfluous; for all synthetic statements would have to be empirical—all that are genuine, that is, and not mere pseudo-statements. But this way of using words, though feasible, seems to me more likely to confuse the issue than to clarify it.

Thus I regard the comparison of the empirical content of two statements as equivalent to the comparison of their degrees of falsifiability. This makes our methodological rule that those theories should be given preference which can be most severely tested (*cf.* the anti-conventionalist rules in section 20) equivalent to a rule favouring theories with the highest possible empirical content.

36. *Levels of Universality and Degrees of Precision.*

There are other methodological demands which may be reduced to the demand for the highest possible empirical content. Two of these are outstanding: the demand for the highest attainable level

[*3] See, again, appendix *vii.

(or degree) of *universality*, and the demand for the highest attainable degree of *precision*.

With this in mind we may examine the following conceivable natural laws:

p: All heavenly bodies which move in closed orbits move in circles: or more briefly: All *orbits of heavenly bodies* are *circles*.

q: All *orbits of planets* are *circles*.

r: All *orbits of heavenly bodies* are *ellipses*.

s: All *orbits of planets* are *ellipses*.

$$\begin{tikzcd}
 & \mathbf{p} \arrow[dl] \arrow[dr] \arrow[dd] & \\
\mathbf{q} \arrow[dr] & & \mathbf{r} \arrow[dl] \\
 & \mathbf{s} &
\end{tikzcd}$$

The deducibility relations holding between these four statements are shown by the arrows in our diagram. From p all the others follow; from q follows s, which also follows from r; so that s follows from all the others.

Moving from p to q the *degree of universality* decreases; and q says less than p because the orbits of planets form a proper subclass of the orbits of heavenly bodies. Consequently p is more easily falsified than q: if q is falsified, so is p, but not *vice versa*. Moving from p to r, the *degree of precision* (of the predicate) decreases: circles are a proper subclass of ellipses; and if r is falsified, so is p, but not *vice versa*. Corresponding remarks apply to the other moves: moving from p to s, the degree of both universality and precision decreases; from q to s precision decreases; and from r to s, universality. To a higher degree of universality *or* precision corresponds a greater (logical or) empirical content, and thus a higher degree of testability.

Both universal and singular statements can be written in the form of a 'universal conditional statement' (or a 'general implication' as it is often called). If we put our four laws in this form, then we can perhaps see more easily and accurately how the degrees of universality and the degrees of precision of two statements may be compared.

A universal conditional statement (*cf.* note 6 to section 14) may be written in the form: '$(x)\ (\varphi x \rightarrow fx)$' or in words: 'All values of x which satisfy the statement function φx also satisfy the statement function fx.' The statement s from our diagram yields the following example: '(x) (x is an orbit of a planet $\rightarrow$ x is an ellipse)' which means:

'Whatever x may be, if x is an orbit of a planet then x is an ellipse.' Let p and q be two statements written in this 'normal' form; then we can say that p is of greater universality than q if the antecedent statement function of p (which may be denoted by '$\varphi_p x$') is tautologically implied by (or logically deducible from), but not equivalent to, the corresponding statement function of q (which may be denoted by '$\varphi_q x$'); or in other words, if '$(x)\ (\varphi_q x \rightarrow \varphi_p x)$' is *tautological* (or logically true). Similarly we shall say that p has greater precision than q if '$(x)\ (f_p x \rightarrow f_q x)$' is tautological, that is if the predicate (or the consequent statement function) of p is narrower than that of q, which means that the predicate of p entails that of q.[*1]

This definition may be extended to statement functions with more than one variable. Elementary logical transformations lead from it to the derivability relations which we have asserted, and which may be expressed by the following rule:[1] If of two statements both their universality *and* their precision are comparable, then the less universal or less precise is derivable from the more universal or more precise; unless, of course, the one is more universal and the other more precise (as in the case of q and r in my diagram).[2]

We could now say that our methodological decision—sometimes metaphysically interpreted as the principle of causality—is to leave nothing unexplained, *i.e.* always to try to deduce statements from others of higher universality. This decision is derived from the demand for the highest attainable degree of universality and precision, and it can be reduced to the demand, or rule, that preference should be given to those theories which can be most severely tested.[*2]

[*1] It will be seen that in the present section (in contrast to sections 18 and 35), the arrow is used to express a conditional rather than the entailment relation; *cf.* also note *1 to section 18.

[1] We can write: $[(\phi_q x \rightarrow \phi_p x) \cdot (f_p x \rightarrow f_q x)] \rightarrow [(\phi_p x \rightarrow f_p x) \rightarrow (\phi_q x \rightarrow f_q x)]$ or for short: $[(\phi_q \rightarrow \phi_p) \cdot (f_p \rightarrow f_q)] \rightarrow (p \rightarrow q)$. *The elementary character of this formula, asserted in the text, becomes clear if we write: '$[(a \rightarrow b) \cdot (c \rightarrow d)] \rightarrow [(b \rightarrow c) \rightarrow (a \rightarrow d)]$'. We then put, in accordance with the text, 'p' for '$b \rightarrow c$' and 'q' for '$a \rightarrow d$', etc.

[2] What I call higher universality in a statement corresponds roughly to what classical logic might call the greater 'extension of the subject'; and what I call greater precision corresponds to the smaller extension, or the 'restriction of the predicate'. The rule concerning the derivability relation, which we have just discussed, can be regarded as clarifying and combining the classical '*dictum de omni et nullo*' and the '*nota-notae*' principle, the 'fundamental principle of mediate predication'. *Cf.* Bolzano, *Wissenschaftslehre* II, 1837, §263, Nos. 1 and 4; Külpe, *Vorlesungen über Logik* (edited by Selz, 1923), §34, 5, and 7.

[*2] See now also section *15 and chapter *iv of my *Postscript*, especially section *76, text to note 5.

37. *Logical Ranges. Notes on the Theory of Measurement.*

If a statement p is more easy to falsify than a statement q, because it is of a higher level of universality or precision, then the class of the basic statements permitted by p is a proper subclass of the class of the basic statements permitted by q. The subclass-relationship holding between classes of permitted statements is the opposite of that holding between classes of forbidden statements (potential falsifiers): the two relationships may be said to be inverse (or perhaps complementary). The class of basic statements permitted by a statement may be called its '*range*'.[1] The 'range' which a statement allows to reality is, as it were, the amount of 'free play' (or the degree of freedom) which it allows to reality. Range and empirical content (*cf.* section 35) are converse (or complementary) concepts. Accordingly, the ranges of two statements are related to each other in the same way as are their logical probabilities (*cf.* sections 34 and 72).

I have introduced the concept of range because it helps us to handle certain questions connected with *degree of precision in measurement*. Assume that the consequences of two theories differ so little in all fields of application that the very small differences between the calculated observable events cannot be detected, owing to the fact that the degree of precision attainable in our measurements is not sufficiently high. It will then be impossible to decide by experiment between the two theories, without first improving our technique of measurement.[*1] This shows that the prevailing technique of measurement determines a certain range—a region within which discrepancies between the observations are permitted by the theory.

Thus the rule that theories should have the highest attainable degree of testability (and thus allow only the narrowest range) entails the demand that the degree of precision in measurement should be raised as much as possible.

It is often said that all measurement consists in the determination of coincidences of points. But any such determination can only be correct within limits. There are no coincidences of points in a strict

[1] The concept of range (*Spielraum*) was introduced by von Kries (1886); similar ideas are found in Bolzano. Waismann (*Erkenntnis* I, 1930, pp. 228 *ff.*) attempts to combine the theory of range with the frequency theory; *cf.* section 72. * Keynes gives (*Treatise*, p. 88) 'field' as a translation of '*Spielraum*', here translated as 'range'; he also uses (p.224) 'scope' for what in my view amounts to precisely the same thing.

[*1] This is a point which, I believe, was wrongly interpreted by Duhem. See his *Aim and Structure of Physical Theory*, pp. 137 *ff*.

sense.[*2] Two physical 'points'—a mark, say, on the measuring-rod, and another on a body to be measured—can at best be brought into close proximity; they cannot coincide, that is, coalesce into *one* point. However trite this remark might be in another context, it is important for the question of precision in measurement. For it reminds us that measurement should be described in the following terms. We find that the point of the body to be measured lies *between* two gradations or marks on the measuring-rod or, say, that the pointer of our measuring apparatus lies *between* two gradations on the scale. We can then either regard these gradations or marks as our two optimal limits of error, or proceed to estimate the position of (say) the pointer within the interval of the gradations, and so obtain a more accurate result. One may describe this latter case by saying that we take the pointer to lie between two imaginary gradation marks. Thus an interval, a range, always remains. It is the custom of physicists to estimate this interval for every measurement. (Thus following Millikan they give, for example, the elementary charge of the electron, measured in electrostatic units, as $e = 4.774 \cdot 10^{-10}$, adding that the range of imprecision is $\pm\, 0.005 \cdot 10^{-10}$.) But this raises a problem. What can be the purpose of replacing, as it were, one mark on a scale by *two*—to wit, the two bounds of the interval—when for each of these two bounds there must again arise the same question: what are the limits of accuracy for the bounds of the interval?

Giving the bounds of the interval is clearly useless unless these two bounds in turn can be fixed with a degree of precision greatly exceeding what we can hope to attain for the original measurement; fixed, that is, within their own intervals of imprecision which should thus be smaller, by several orders of magnitude, than the interval they determine for the value of the original measurement. In other words, the bounds of the interval are not sharp bounds but are really very small intervals, the bounds of which are in their turn still much smaller intervals, and so on. In this way we arrive at the idea of what may be called the 'unsharp bounds' or '*condensation bounds*' of the interval.

These considerations do not presuppose the mathematical theory of errors, nor the theory of probability. It is rather the other way round; by analysing the idea of a measuring interval they furnish a background without which the statistical theory of errors makes very

[*2] Note that I am speaking here of measuring, not of counting. (The difference between these two is closely related to that between real numbers and rational numbers.)

little sense. If we measure a magnitude many times, we obtain values which are distributed with different densities over an interval—the interval of precision depending upon the prevailing measuring technique. Only if we know what we are seeking—namely the condensation bounds of this interval—can we apply to these values the theory of errors, and determine the bounds of the interval.*3

Now all this sheds some light, I think, on the *superiority of methods that employ measurements* over *purely qualitative methods*. It is true that even in the case of qualitative estimates, such as an estimate of the pitch of a musical sound, it may sometimes be possible to give an interval of accuracy for the estimates; but in the absence of measurements, any such interval can be only very vague, since in such cases the concept of condensation bounds cannot be applied. This concept is applicable only where we can speak of orders of magnitude, and therefore only where methods of measurement are defined. I shall make further use of the concept of condensation bounds of intervals of precision in section 68, in connection with the theory of probability.

38. *Degrees of Testability Compared by Reference to Dimensions.*

Till now we have discussed the comparison of theories with respect to their degrees of testability only in so far as they can be compared with the help of the subclass-relation. In some cases this method is quite successful in guiding our choice between theories. Thus we may now say that Pauli's exclusion principle, mentioned by way of example in section 20, indeed turns out to be highly satisfactory as an auxiliary hypothesis. For it greatly increases the degree of precision and, with it, the degree of testability, of the older quantum theory (like the corresponding statement of the new quantum theory which asserts that anti-symmetrical states are realized by electrons, and symmetrical ones by uncharged, and by certain multiply charged, particles).

For many purposes, however, comparison by means of the subclass relation does not suffice. Thus Frank, for example, has pointed

*3 These considerations are closely connected with, and supported by, some of the results discussed under points 8 *ff.* of my 'Third Note', reprinted in appendix *ix. See also section *15 of the *Postscript* for the significance of measurement for the 'depth' of theories.

out that statements of a high level of universality—such as the principle of the conservation of energy in Planck's formulation—are apt to become tautological, and to lose their empirical content, unless the initial conditions can be determined '... by *few* measurements, ... *i.e.* by means of a small number of magnitudes characteristic of the state of the system'.[1] The question of the number of parameters which have to be ascertained, and to be substituted in the formulae, cannot be elucidated with the help of the sub-class relation, in spite of the fact that it is evidently closely connected with the problem of testability and falsifiability, and their degrees. The fewer the magnitudes which are needed for determining the initial conditions, the less composite*1 will be the basic statements which suffice for the falsification of the theory; for a falsifying basic statement consists of the conjunction of the initial conditions with the negation of the derived prediction (*cf.* section 28). Thus it may be possible to compare theories as to their degree of testability by ascertaining the minimum degree of composition which a basic statement must have if it is to be able to contradict the theory; provided always that we can find a way to compare basic statements in order to ascertain whether they are more (or less) composite, *i.e.* compounds of a greater (or a smaller) number of basic statements of a simpler kind. All basic statements, whatever their content, whose degree of composition does not reach the requisite minimum, would be permitted by the theory simply because of their low degree of composition.

But any such programme is faced with difficulties. For generally it is not easy to tell, merely by inspecting it, whether a statement is composite, *i.e.* equivalent to a conjunction of simpler statements. In all statements there occur universal names, and by analysing these one can often break down the statement into conjunctive components. (For example, the statement 'There is a glass of water at the place *k*' might perhaps be analysed, and broken down into the two statements 'There is a glass containing a fluid at the place *k*' and 'There is water at the place *k*'.) There is no hope of finding any natural end to the dissection of statements by this method, especially since we can always introduce new universals defined for the purpose of making a further dissection possible.

With a view to rendering comparable the degrees of composition

[1] *Cf.* Frank, *Das Kausalgesetz und seine Grenzen*, 1931, e.g. p. 24.

*1 For the term 'composite', see note *1 to section 32.

of all basic statements, it might be suggested that we should choose a certain class of statements as the *elementary* or *atomic* ones,[2] from which all other statements could then be obtained by conjunction and other logical operations. If successful, we should have defined in this way an 'absolute zero' of composition, and the composition of any statement could then be expressed, as it were, in absolute degrees of composition.[*2] But for the reason given above, such a procedure would have to be regarded as highly unsuitable; for it would impose serious restrictions upon the free use of scientific language.[*3]

Yet it is still possible to compare the degrees of composition of basic statements, and thereby also those of other statements. This can be done by selecting arbitrarily a class of *relatively* atomic statements, which we take as a basis for comparison. Such a class of relatively atomic statements can be defined by means of a *generating schema or matrix* (for example, 'There is a measuring apparatus for . . . at the place . . . , the pointer of which lies between the gradation marks . . . and . . .'). We can then define as relatively atomic, and thus as equi-composite, the class of all statements obtained from this kind of matrix (or statement function) by the substitution of definite values. The class of these statements, together with all the conjunctions which can be formed from them may be called a '*field*'. A conjunction of n different relatively atomic statements of a field may be called an 'n-tuple of the field';

[2] 'Elementary propositions' in Wittgenstein, *Tractatus Logico-Philosophicus*, Proposition 5: 'Propositions are truth-functions of elementary propositions'. 'Atomic propositions' (as opposed to the composite 'molecular propositions') in Whitehead and Russell's *Principia Mathematica* Vol. I. Introduction to 2nd edition, 1925, pp. xv *f.* C. K. Ogden translated Wittgenstein's term '*Elementarsatz*' as 'elementary proposition', (*cf. Tractatus* 4.21), while Bertrand Russell in his Preface to the *Tractatus*, 1922, p. 13, translated it as 'atomic proposition'. The latter term has become more popular.

[*2] Absolute degrees of composition would determine, of course, absolute degrees of content, and thus of absolute logical improbability. The programme here indicated of introducing improbability, and thus probability, by singling out a certain class of absolutely atomic statements (earlier sketched, for example, by Wittgenstein) has more recently been elaborated by Carnap in his *Logical Foundations of Probability*, 1950, in order to construct a theory of induction. See also the remarks on model languages in my *Preface to the English Edition*, 1958, above, where I allude to the fact that the third model language (Carnap's language system) does not admit measurable properties. (Nor does it in its present form allow the introduction of a temporal or spatial order.)

[*3] The words 'scientific language' were here used quite naïvely, and should not be interpreted in the technical sense of what is today called a 'language system'. On the contrary, my main point was that we should remember the fact that scientists cannot use a 'language system' since they have constantly to change their language, with every new step they take. 'Matter', or 'atom', after Rutherford, and 'matter', or 'energy', after Einstein, meant something different from what they meant before: the meaning of these concepts is a function of the—constantly changing—*theory*.

and we can say that the degree of its composition is equal to the number n.

If there exists, for a theory t, a field of singular (but not necessarily basic) statements such that, for some number d, the theory t cannot be falsified by any d-tuple of the field, although it can be falsified by certain $d+1$-tuples, then we call d the *characteristic number* of the theory with respect to that field. All statements of the field whose degree of composition is less than d, or equal to d, are then compatible with the theory, and permitted by it, irrespective of their content.

Now it is possible to base the comparison of the degree of testability of theories upon this characteristic number d. But in order to avoid inconsistencies which might arise through the use of different fields, it is necessary to use a somewhat narrower concept than that of a field, namely that of a *field of application*. If a theory t is given, we say that a field is a *field of application of the theory t* if there exists a characteristic number d of the theory t with respect to this field, and if, in addition, it satisfies certain further conditions (which are explained in appendix I).

The characteristic number d of a theory t, with respect to a field of application, I call the *dimension* of t with respect to this field of application. The expression 'dimension' suggests itself because we can think of all possible n-tuples of the field as spatially arranged (in a configuration space of infinite dimensions). If, for example, $d = 3$, then those statements which are admissible because their composition is too low—form a three-dimensional sub-space of this configuration. Transition from $d = 3$ to $d = 2$ corresponds to the transition from a solid to a surface. The smaller the dimension d, the more severely restricted is the class of those permitted statements which, regardless of their content, cannot contradict the theory owing to their low degree of composition; and the higher will be the degree of falsifiability of the theory.

The concept of the field of application has not been restricted to basic statements, but singular statements of all kinds have been allowed to be statements belonging to a field of application. But by comparing their dimensions with the help of the field, we can estimate the degree of composition of the basic statements. (We assume that to highly composite singular statements there correspond highly composite basic statements.) It thus can be assumed that to a theory of higher dimension, there corresponds a class of basic statements of higher dimension, such that all statements of this class are permitted by the theory, irrespective of what they assert.

This answers the question of how the two methods of comparing degrees of testability are related—the one by means of the dimension of a theory, and the other by means of the subclass relation. There will be cases in which neither, or only one, of the two methods is applicable. In such cases there is of course no room for conflict between the two methods. But if in a particular case both methods are applicable, then it may conceivably happen that two theories of equal dimensions may yet have different degrees of falsifiability if assessed by the method based upon the subclass relation. In such cases, the verdict of the latter method should be accepted, since it would prove to be the more sensitive method. In all other cases in which both methods are applicable, they must lead to the same result; for it can be shown, with the help of a simple theorem of the theory of dimension, that the dimension of a class must be greater than, or equal to, that of its subclasses.[3]

39. *The Dimension of a Set of Curves.*

Sometimes we can identify what I have called the 'field of application' of a theory quite simply with the *field of its graphic representation*, *i.e.* the area of a graph-paper on which we represent the theory by graphs: each point of this field of graphic representation can be taken to correspond to one relatively atomic statement. The dimension of the theory with respect to this field (defined in appendix I) is then identical with the dimension of the set of curves corresponding to the theory. I shall discuss these relations with the help of the two statements q and s of section 36. (Our comparison of dimensions applies to statements with different predicates.) The hypothesis q—that all planetary orbits are circles—is three-dimensional: for its falsification at least four singular statements of the field are necessary, corresponding to four points of its graphic representation. The hypothesis s, that all planetary orbits are ellipses, is five-dimensional, since for its falsification at least six singular statements are necessary, corresponding to six points of the graph. We saw

[3] *Cf.* Menger, *Dimensionstheorie*, 1928, p. 81. *The conditions under which this theorem holds can be assumed to be always satisfied by the 'spaces' with which we are concerned here.

in section 36 that q is more easily falsifiable than s: since all circles are ellipses, it was possible to base the comparison on the subclass relation. But the use of dimensions enables us to compare theories which previously we were unable to compare. For example, we can now compare a circle-hypothesis with a parabola-hypothesis (which is four dimensional). Each of the words 'circle', 'ellipse', 'parabola' denotes a class or *set of curves*; and each of these sets has the dimension d if d points are necessary and sufficient to single out, or characterize, one particular curve of the set. In algebraic representation, the dimension of the set of curves depends upon the number of *parameters* whose values we may freely choose. We can therefore say that the number of freely determinable parameters of a set of curves by which a theory is represented is characteristic for the degree of falsifiability (or testability) of that theory.

In connection with the statements q and s in my example I should like to make some methodological comments on Kepler's discovery of his laws.*[1]

I do not wish to suggest that the belief in perfection—the heuristic principle that guided Kepler to his discovery—was inspired, consciously or unconsciously, by methodological considerations regarding degrees of falsifiability. But I do believe that Kepler owed his success in part to the fact that the circle-hypothesis with which he started was relatively easy to falsify. Had Kepler started with a hypothesis which owing to its logical form was not so easily testable as the circle hypothesis, he might well have got no result at all, considering the difficulties of calculations whose very basis was 'in the air'— adrift in the skies, as it were, and moving in a way unknown. The unequivocal *negative* result which Kepler reached by the falsification of his circle hypothesis was in fact his first real success. His method had been vindicated sufficiently for him to proceed further; especially since even this first attempt had already yielded certain approximations.

No doubt, Kepler's laws might have been found in another way. But I think it was no mere accident that this was the way which led to success. It corresponds to *the method of elimination* which is applicable only if the theory is sufficiently easy to falsify—sufficiently *precise* to be capable of clashing with observational experience.

*[1] The views here developed were accepted, with acknowledgments, by W. C Kneale, *Probability and Induction*, 1949, p. 230, and J. G. Kemeny, 'The Use of Simplicity in Induction', *Philos. Review* **57**, 1953; see his footnote on p. 404.

40. *Two Ways of Reducing the Number of Dimensions of a Set of Curves.*

Quite different sets of curves may have the same dimension. The set of all circles, for example, is three-dimensional; but the set of all circles passing through a given point is a two-dimensional set (like the set of straight lines). If we demand that the circles should all pass through *two* given points, then we get a one-dimensional set, and so on. Each additional demand that all curves of a set should pass through one more given point reduces the dimensions of the set by one.

zero dimensional classes[1]	one dimensional classes	two dimensional classes	three dimensional classes	four dimensional classes
—	—	straight line	circle	parabola
—	straight line through one given point	circle through one given point	parabola through one given point	conic through one given point
straight line through two given points	circle through two given points	parabola through two given points	conic through two given points	—
circle through three given points	parabola through three given points	conic through three given points	—	—

The number of dimensions can also be reduced by methods other than that of increasing the number of given points. For example the set of ellipses with given ratio of the axes is four-dimensional (as is that of parabolas), and so is the set of ellipses with given numerical eccentricity. The transition from the ellipse to the circle, of course, is equivalent to specifying an eccentricity (the eccentricity 0) or a particular ratio of the axes (unity).

As we are interested in assessing degrees of falsifiability of theories we will now ask whether the various methods of reducing the number of dimensions are equivalent for our purposes, or whether we

[1] We could also, of course, begin with the empty (over-determined) minus-one-dimensional class.

should examine more closely their relative merits. Now the stipulation that a curve should pass through a certain *singular point* (or small region) will often be linked up with, or correspond to, the acceptance of a certain *singular statement*, *i.e.* of an initial condition. On the other hand, the transition from, say, an ellipse-hypothesis to a circle-hypothesis, will obviously correspond to a reduction of the dimension of *the theory itself*. But how are these two methods of reducing the dimensions to be kept apart? We may give the name '*material reduction*' to that method of reducing dimensions which does *not* operate with stipulations as to the 'form' or 'shape' of the curve; that is, to reductions through specifying one or more points, for example, or by some equivalent specification. The other method, in which the form or shape of the curve becomes more narrowly specified as, for example, when we pass from ellipse to circle, or from circle to straight line, *etc.*, I will call the method of '*formal reduction*' of the number of dimensions.

It is not very easy, however, to get this distinction sharp. This may be seen as follows. Reducing the dimensions of a theory means, in algebraic terms, replacing a parameter by a constant. Now it is not quite clear how we can distinguish between different methods of replacing a parameter by a constant. The *formal reduction*, by passing from the general equation of an ellipse to the equation of a circle, can be described as equating one parameter to zero, and a second parameter to one. But if another parameter (the absolute term) is equated to zero, then this would mean a *material reduction*, namely the specification of a point of the ellipse. I think, however, that it is possible to make the distinction clear, if we see its connection with the problem of universal names. For material reduction introduces an individual name, formal reduction a universal name, into the definition of the relevant set of curves.

Let us imagine that we are given a certain individual plane, perhaps by 'ostensive definition'. The set of all ellipses in this plane can be defined by means of the general equation of the ellipse; the set of circles, by the general equation of the circle. These definitions are *independent of where*, in the plane, we *draw the* (*Cartesian*) *co-ordinates* to which they relate; consequently they are independent of the choice of the origin, and the orientation, of the co-ordinates. A specific system of co-ordinates can be determined only by individual names; say, by ostensively specifying its origin and orientation. Since the definition of the set of ellipses (or circles) is the same for all Cartesian co-ordinates, it is independent of the specification of these individual names: it is *invariant* with respect to all co-ordinate transformations of the Euclidean group (displacements and similarity transformations).

If, on the other hand, one wishes to define a set of ellipses (or circles) which have a specific, an individual point of the plane in common, then we must operate with an equation which is not invariant with respect to the transformations of the Euclidean group, but relates to a singular, *i.e.* an individually or ostensively specified, co-ordinate system. Thus it is connected with individual names.[2]

The transformations can be arranged in a hierarchy. A definition which is invariant with respect to a more general group of transformations is also invariant with respect to more special ones. For each definition of a set of curves, there is one—the most general—transformation group which is characteristic of it. Now we can say: The definition D_1 of a set of curves is called 'equally general' to (or more general than) a definition D_2 of a set of curves if it is invariant with respect to the same transformation group as is D_2 (or a more general one). A reduction of the dimension of a set of curves may now be called *formal* if the reduction does not diminish the generality of the definition; otherwise it may be called *material*.

If we compare the degree of falsifiability of two theories by considering their dimensions, we shall clearly have to take into account their *generality*, *i.e.* their invariance with respect to co-ordinate transformations, along with their dimensions.

The procedure will, of course, have to be different according to whether the theory, like Kepler's theory, in fact makes geometrical statements about the world or whether it is 'geometrical' only in that it may be represented by a graph—such as, for example, the graph which represents the dependence of pressure upon temperature. It would be inappropriate to require of this latter kind of theory, or of the corresponding set of curves, that its definition should be invariant with respect to, say, rotations of the co-ordinate system; for in these cases, the different co-ordinates may represent entirely different things (the one pressure and the other temperature).

[2] On the relations between transformation groups and 'individualization' *cf.* Weyl, *Philosophie der Mathematik u. Naturwissenschaft*, 1927, p. 59, English edition pp. 73 *f.*, where reference is made to Klein's *Erlanger Programm*.

This concludes my exposition of the methods whereby degrees of falsifiability are to be compared. I believe that these methods can help us to elucidate epistemological questions, such as the *problem of simplicity* which will be our next concern. But there are other problems which are placed in a new light by our examination of degrees of falsifiability, as we shall see; especially the problem of the so-called 'probability of hypotheses' or of *corroboration*.

CHAPTER VII

SIMPLICITY

THERE seems to be little agreement as to the importance of the so-called 'problem of simplicity'. Weyl said, not long ago, that 'the problem of simplicity is of central importance for the epistemology of the natural sciences'.[1] Yet it seems that interest in the problem has lately declined; perhaps because, especially after Weyl's penetrating analysis, there seemed to be so little chance of solving it.

Until quite recently the idea of simplicity has been used uncritically, as though it were quite obvious what simplicity is, and why it should be valuable. Not a few philosophers of science have given the concept of simplicity a place of crucial importance in their theories, without even noticing the difficulties to which it gives rise. For example, the followers of Mach, Kirchhoff, and Avenarius have tried to replace the idea of a causal explanation by that of the 'simplest description'. Without the adjective 'simplest' or a similar word this doctrine would say nothing. As it is supposed to explain why we prefer a description of the world with the help of theories to one with the help of singular statements, it seems to presuppose that theories are simpler than singular statements. Yet few have ever attempted to explain why they should be simpler, or what is meant, more precisely, by simplicity.

If, moreover, we assume that theories are to be used for the sake of simplicity then, clearly, we should use the simplest theories. This is how Poincaré, for whom the choice of theories is a matter of convention, comes to formulate his principle for the selection of theories: he chooses the *simplest* of the possible conventions. But which are the simplest?

[1] *Cf.* Weyl, *op. cit.*, pp. 115 *f.*; English edition p. 155. See also section 42 below.

41. *Elimination of the Aesthetic and the Pragmatic Concepts of Simplicity.*

The word 'simplicity' is used in very many different senses. Schrödinger's theory, for instance, is of great simplicity in a methodological sense, but in another sense it might well be called 'complex'. We can say of a problem that its solution is not simple but difficult, or of a presentation or an exposition that it is not simple but intricate.

To begin with, I shall exclude from our discussion the application of the term 'simplicity' to anything like a presentation or an exposition. It is sometimes said of two expositions of one and the same mathematical proof that the one is simpler or more elegant than the other. This is a distinction which has little interest from the point of view of the theory of knowledge; it does not fall within the province of logic, but merely indicates a preference of an *aesthetic or pragmatic* character. The situation is similar when people say that one task may be 'carried out by simpler means' than another, meaning that it can be done more easily or that, in order to do it, less training or less knowledge is needed. In all such cases the word 'simple' can be easily eliminated; its use is extra-logical.

42. *The Methodological Problem of Simplicity.*

What, if anything, remains after we have eliminated the aesthetic and the pragmatic ideas of simplicity? Is there a concept of simplicity which is of importance for the logician? Is it possible to distinguish theories that are logically not equivalent according to their degrees of simplicity?

The answer to this question may well seem doubtful, seeing how little successful have been most attempts to define this concept. Schlick, for one, gives a negative answer. He says: 'Simplicity is . . . a concept indicative of preferences which are partly practical, partly aesthetic in character.'[1] And it is notable that he gives this answer when writing of the concept which interests us here, and which I shall call the *epistemological concept of simplicity*; for he continues: 'Even if we are unable to explain what is really meant by "simplicity" here, we must yet recognize the fact that any scientist who has succeeded in representing a series of observations by means of a very simple formula (*e.g.* by a linear, quadratic, or exponential function) is immediately convinced that he has discovered a law.'

[1] Schlick, *Naturwissenschaften* **19**, 1931, p. 148. *I have translated Schlick's term '*pragmatischer*' freely.

Schlick discusses the possibility of defining the concept of law-like regularity, and especially the distinction between 'law' and 'chance', with the help of the concept of simplicity. He finally dismisses it with the remark that 'simplicity is obviously a wholly relative and vague concept; no strict definition of causality can be obtained with its help; nor can law and chance be precisely distinguished'.[2] From this passage it becomes clear what the concept of simplicity is actually expected to achieve: it is to provide a measure of the degree of law-likeness or regularity of events. A similar view is voiced by Feigl when he speaks of the 'idea of defining the degree of regularity or of law-likeness with the help of the concept of simplicity'.[3]

The epistemological idea of simplicity plays a special part in theories of inductive logic, for example in connection with the problem of the 'simplest curve'. Believers in inductive logic assume that we arrive at natural laws by generalization from particular observations. If we think of the various results in a series of observations as points plotted in a co-ordinate system, then the graphic representation of the law will be a curve passing through all these points. But through a finite number of points we can always draw an unlimited number of curves of the most diverse form. Since therefore the law is not uniquely determined by the observations, inductive logic is confronted with the problem of deciding which curve, among all these possible curves, is to be chosen.

The usual answer is, 'choose the simplest curve'. Wittgenstein, for example, says: 'The process of induction consists in assuming the *simplest* law that can be made to harmonize with our experience.'[4] In choosing the simplest law, it is usually tacitly assumed that a linear function, say, is simpler than a quadratic one, a circle simpler than an ellipse, etc. But no reasons are given either for choosing this particular hierarchy of simplicities in preference to any other, or for believing that 'simple' laws have advantages over the less simple—apart from aesthetic and practical ones.[5] Schlick and Feigl mention[6] an unpublished paper of Natkin who, according to Schlick's account,

[2] Schlick, *ibid.*

[3] Feigl, *Theorie und Erfahrung in der Physik*, 1931, p. 25.

[4] Wittgenstein, *op. cit.*, Proposition 6.363.

[5] Wittgenstein's remark on the simplicity of logic (*op. cit.*, Proposition 5.4541) which sets 'the standard of simplicity' gives no clue. Reichenbach's 'principle of the simplest curve' (*Mathematische Zeitschrift* **34**, 1932, p. 616) rests on his Axiom of Induction (which I believe to be untenable), and also affords no help.

[6] In the places referred to.

proposes to call one curve simpler than another if its average curvature is smaller; or, according to Feigl's account, if it deviates less from a straight line. (The two accounts are not equivalent.) This definition seems to agree pretty well with our intuitions; but it somehow misses the crucial point; it would, for example, make certain parts (the asymptotic parts) of a hyperbola much simpler than a circle, etc. And really, I do not think that the question can be settled by such 'artifices' (as Schlick calls them). Moreover, it would remain a mystery why we should give preference to simplicity if defined in this particular way.

Weyl discusses and rejects a very interesting attempt to base simplicity on probability. 'Assume, for example, that twenty co-ordinated pairs of values (x, y) of the same function, $y = f(x)$ lie (within the expected accuracy) on a straight line, when plotted on square graph paper. We shall then conjecture that we are faced here with a rigorous natural law, and that y depends linearly upon x. And we shall conjecture this because of the *simplicity* of the straight line, or because it would be so *extremely improbable* that just these twenty pairs of arbitrarily chosen observations should lie very nearly on a straight line, had the law in question been a different one. If now we use the straight line for interpolation and extrapolation, we obtain predictions which go beyond what the observations tell us. However, this analysis is open to criticism. It will always be possible to define all kinds of mathematical functions which . . . will be satisfied by the twenty observations; and some of these functions will deviate considerably from the straight line. And for every single one of these we may claim that it would be *extremely improbable* that the twenty observations should lie just on this curve, unless it represented the true law. It is thus essential, after all, that the function, or rather the class of functions, should be offered to us, *a priori*, by mathematics because of its mathematical simplicity. It should be noted that this class of functions must not depend upon as many parameters as the number of observations to be satisfied.'[7] Weyl's remark that 'the class of functions should be offered to us *a priori*, by mathematics, because of its mathematical simplicity', and his reference to the number of

[7] Weyl, *op. cit.*, p. 116; English edition, p. 156. * When writing my book I did not know (and Weyl, no doubt, did not know when writing his) that Harold Jeffreys and Dorothy Wrinch had suggested, six years before Weyl, that we should measure the simplicity of a function by the paucity of its freely adjustable parameters. (See their joint paper in *Phil. Mag.* **42**, 1921, pp. 369 *ff.*) I wish to take this opportunity to make full acknowledgement to these authors.

parameters agree with my view (to be developed in section 43). But Weyl does not say what 'mathematical simplicity' is; and above all, he does not say what *logical or epistemological advantages* the simpler law is supposed to possess, compared with one that is more complex.[8]

The various passages so far quoted are very important, because of their bearing upon our present aim—the analysis of the epistemological concept of simplicity. For this concept is not yet precisely determined. It is therefore possible to reject any attempt (such as mine) to make this concept precise by saying that the concept of simplicity in which epistemologists are interested is really quite a different one. To such objections I could answer that I do not attach the slightest importance to the *word* 'simplicity'. The term was not introduced by me, and I am aware of its disadvantages. All I do assert is that the concept of simplicity which I am going to clarify helps to answer those very questions which, as my quotations show, have so often been raised by philosophers of science in connection with their 'problem of simplicity'.

43. *Simplicity and Degree of Falsifiability.*

The epistemological questions which arise in connection with the concept of simplicity can all be answered if we equate this concept with *degree of falsifiability*. This assertion is likely to meet with opposition;[*1] and so I shall try, first, to make it intuitively more acceptable.

[8] Weyl's further comments on the connection between simplicity and corroboration are also relevant in this connection; they are largely in agreement with my own views expressed in section 82, although my line of approach and my arguments are quite different; *cf.* note 1 to section 82, * and the new note here following (note *1 to section 43).

[*1] It was gratifying to find that this theory of simplicity (including the ideas of section 40) has been accepted at least by one epistemologist—by William Kneale, who writes in his book *Probability and Induction*, 1949, pp. 229 *f*: '... it is easy to see that the hypothesis which is simplest in this sense is also that which we can hope to eliminate most quickly if it is false. ... In short, the policy of assuming always the simplest hypothesis which accords with the known facts is that which will enable us to get rid of false hypotheses most quickly.' Kneale adds a footnote in which he refers to p. 116 of Weyl's book, and also to mine. But I cannot detect on this page—of which I quoted the relevant portions in the text—or anywhere else in Weyl's great book (or in any other) even a trace of the view that the simplicity of a theory is connected with its falsifiability, *i.e.* with the ease of its elimination. Nor would I have written (as I did near the end of the preceding section) that Weyl 'does not say what *logical or epistemological advantages* the simpler law is supposed to possess' had Weyl (or anybody else known to me) anticipated my theory.

The facts are these. In his profound discussion of the problem (here quoted in section 42, text to note 7) Weyl mentions first the intuitive view that a simple curve—say, a straight line—has an advantage over a more complex curve *because it might be considered a highly improbable accident if all the observations would fit such a simple curve*. But instead of

I have already shown that theories of a lower dimension are more easily falsifiable than those of a higher dimension. A law having the form of a function of the first degree, for instance, is more easily falsifiable than one expressible by means of a function of the second degree. But the latter still belongs to the best falsifiable ones among the laws whose mathematical form is that of an algebraic function. This agrees well with Schlick's remark concerning simplicity: 'We should certainly be inclined to regard a function of the first degree as simpler than one of the second degree, though the latter also doubtless represents a perfectly good law. . . .'[1]

The degree of universality and of precision of a theory increases with its degree of falsifiability, as we have seen. Thus we may perhaps identify the *degree of strictness* of a theory—the degree, as it were, to which a theory imposes the rigour of law upon nature—with its degree of falsifiability; which shows that the latter does just what Schlick and Feigl expected the concept of simplicity to do. I may add that the distinction which Schlick hoped to make between law and chance can also be clarified with the help of the idea of degrees of falsifiability: probability statements about sequences having chance-like characteristics turn out to be of infinite dimension (*cf.* section 65); not simple but complex (*cf.* section 58 and latter part of 59); and falsifiable only under special safeguards (section 68).

The comparison of degrees of testability has been discussed at length in sections 31 to 40. Some of the examples and other details

[1] Schlick, *Naturwissenschaften* **19**, 1931, p. 148 (*cf.* note 1 to the preceding section).

following up this intuitive view (which I think would have led him to see that the simpler theory is the better testable one), Weyl *rejects* it as not standing up to rational criticism: he points out that the same could be said of *any given* curve, however complex. (This argument is correct, but it does no longer hold if we consider the *potential falsifiers*—and their degree of composition—rather than the verifying instances.) Weyl then proceeds to discuss the paucity of the parameters as a criterion of simplicity, without connecting this in any way either with the intuitive view just rejected by him, or with anything which, like testability, or content, might explain our epistemological preference for the simpler theory.

Weyl's characterization of the simplicity of a curve by the paucity of its parameters was anticipated in 1921 by Harold Jeffreys and Dorothy Wrinch (*Phil. Mag.* **42**, 369 *ff.*). But if Weyl merely failed to see what is now 'easy to see' (according to Kneale), Jeffreys actually saw—and still sees—the very opposite: he attributes to the simpler law the greater prior probability instead of the greater prior improbability. (Thus Jeffreys's and Kneale's views together may illustrate Schopenhauer's remark that the solution of a problem often first looks like a paradox and later like a truism.) I wish to add here that I have further developed my views on simplicity, and that in doing so I have tried hard and, I hope, not quite without success, to learn something from Kneale. *Cf.* appendix *x and section *15 of my *Postscript*.

given there can be easily transferred to the problem of simplicity. This holds especially for the degree of universality of a theory: a more universal statement can take the place of many less universal ones, and for that reason has often been called 'simpler'. The concept of the dimension of a theory may be said to give precision to Weyl's idea of using the number of parameters to determine the concept of simplicity.*2 And by means of our distinction between a formal and a material reduction of the dimension of a theory (*cf.* section 40), certain possible objections to Weyl's theory can be met. One of these is the objection that the set of ellipses whose axes stand in a given ratio, and whose numerical eccentricity is given, has exactly as many parameters as the set of circles, although it is obviously less 'simple'.

Above all, our theory explains *why simplicity is so highly desirable.* To understand this there is no need for us to assume a 'principle of economy of thought' or anything of the kind. Simple statements, if knowledge is our object, are to be prized more highly than less simple ones *because they tell us more; because their empirical content is greater; and because they are better testable.*

44. *Geometrical Shape and Functional Form.*

Our view of the concept of simplicity enables us to resolve a number of contradictions which up to now have made it doubtful whether this concept was of any use.

Few would regard the *geometrical shape* of, say, a logarithmic curve as particularly simple; but a *law* which can be represented by a logarithmic function is usually regarded as a simple one. Similarly

*2 As mentioned in notes 7 to section 42 and *1 to the present section, it was Harold Jeffreys and Dorothy Wrinch who first proposed to measure the simplicity of a function by the paucity of its freely adjustable parameters. But they also proposed to attach to the simpler hypothesis a greater prior probability. Thus their views can be presented by the schema

simplicity = paucity of parameters = high prior probability.

It so happens that I approached the matter from an entirely different angle. I was interested in assessing degrees of testability, and I found first that testability can be measured by 'logical' improbability (which corresponds exactly to Jeffreys' 'prior' improbability). Then I found that testability, and thus prior improbability, can be equated with paucity of parameters; and only at the end, I equated high testability with high simplicity. Thus my view can be presented by the schema: *testability =*

high prior improbability = paucity of parameters = simplicity.

It will be seen that these two schemata coincide in part; but on the decisive point—probability *vs.* improbability—they stand in direct opposition. See also appendix *viii.

a *sine function* is commonly said to be simple, even though the geometrical shape of the *sine curve* is perhaps not so very simple.

Difficulties like this can be cleared up if we remember the connection between the number of parameters and the degree of falsifiability, and if we distinguish between the formal and the material reduction of dimensions. (We must also remember the rôle of invariance with respect to transformations of the co-ordinate systems.) If we speak of the *geometrical form or shape* of a curve, then what we demand is invariance with respect to all transformations belonging to the group of displacements, and we may demand invariance with respect to similarity transformations; for we do not think of a geometrical figure or shape as being tied to a definite *position.* Consequently, if we think of the shape of a one-parametric logarithmic curve ($y = log_a x$) as lying anywhere in a plane, then it would have *five* parameters (if we allow for similarity transformations). It would thus be by no means a particularly simple curve. If, on the other hand, a *theory or law* is represented by a logarithmic curve, then co-ordinate transformations of the kind described are irrelevant. In such cases, there is no point in either rotations or parallel displacements or similarity transformations. For a logarithmic curve as a rule is a graphic representation in which the co-ordinates cannot be interchanged. (For example, the x-axis might represent atmospheric pressure, and the y-axis height above sea-level.) For this reason, similarity transformations are equally without any significance here. Analogous considerations hold for *sine* oscillations along a particular axis, for example, the time axis; and for many other cases.

45. *The Simplicity of Euclidean Geometry.*

One of the issues which played a major rôle in most of the discussions of the theory of relativity was the simplicity of Euclidean geometry. Nobody ever doubted that Euclidean geometry as such was simpler than any non-Euclidean geometry with given constant curvature—not to mention non-Euclidean geometries with curvatures varying from place to place.

At first sight the kind of simplicity here involved seems to have little to do with degrees of falsifiability. But if the statements at issue are formulated as empirical hypotheses, then we find that the two concepts, simplicity and falsifiability, coincide in this case also.

Let us consider what experiments may help us to test the hypothesis, 'In our world, we have to employ a certain metrical geometry with such and such a radius of curvature.' A test will be possible only if we identify certain geometrical entities with certain physical objects—for instance straight lines with light rays; or points with the intersection of threads. If such an identification (a correlating definition, or perhaps an ostensive definition; *cf.* section 17) is adopted, then it can be shown that the hypothesis of the validity of an Euclidean light-ray-geometry is falsifiable to a higher degree than any of the competing hypotheses which assert the validity of some non-Euclidean geometry. For if we measure the sum of the angles of a light-ray triangle, then any significant deviation from 180 degrees will falsify the Euclidean hypothesis. The hypothesis of a Bolyai-Lobatschewski geometry with given curvature, on the other hand, would be compatible with any particular measurement not exceeding 180 degrees. Moreover, to falsify this hypothesis it would be necessary to measure not only the sum of the angles, but also the (absolute) size of the triangle; and this means that in addition to angles, a further unit of measurement, such as a unit of area, would have to be defined. Thus we see that more measurements are needed for a falsification; that the hypothesis is compatible with greater variations in the results of measurements; and that it is therefore more difficult to falsify: it is falsifiable to a lesser degree. To put it in another way, Euclidean geometry is the only metric geometry with a definite curvature in which similarity transformations are possible. In consequence, Euclidean geometrical figures can be invariant with respect to more transformations; that is, they can be of lower dimension: they can be simpler.

46. *Conventionalism and the Concept of Simplicity.*

What the conventionalist calls 'simplicity' does *not* correspond to what I call 'simplicity'. It is the central idea of the conventionalist, and also his starting point, that no theory is unambiguously determined by experience; a point with which I agree. He believes that he must therefore choose the 'simplest' theory. But since the conventionalist does not treat his theories as falsifiable systems but rather as conventional stipulations, he obviously means by 'simplicity' something different from degree of falsifiability.

The conventionalist concept of simplicity turns out to be indeed

partly aesthetic and partly practical. Thus the following comment by Schlick (*cf.* section 42) applies to the conventionalist concept of simplicity, but not to mine: 'It is certain that one can only define the concept of simplicity by a convention which must always be arbitrary.'[1] It is curious that conventionalists themselves have overlooked the conventional character of their own fundamental concept—that of simplicity. That they must have overlooked it is clear, for otherwise they would have noticed that their appeal to simplicity could never save them from arbitrariness, once they had chosen the way of arbitrary convention.

From my point of view, a system must be described as *complex in the highest degree* if, in accordance with conventionalist practice, one holds fast to it as a system established forever which one is determined to rescue, whenever it is in danger, by the introduction of auxiliary hypotheses. For the degree of falsifiability of a system thus protected is equal to *zero*. Thus we are led back, by our concept of simplicity, to the methodological rules of section 20; and especially also to that rule or principle which restrains us from indulgence in *ad hoc* hypotheses and auxiliary hypotheses: to the principle of parsimony in the use of hypotheses.

[1] Schlick, *ibid.*, p. 148.