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Confirmation and Parsimony

1. Jeffreys' Problem

I shall attempt to deal with the most basic issues of confirmation, inquire what precisely the term 'induction' stands for, what the problem of induction amounts to and what, if anything, can be suggested in a way of solution.

Let us begin by picturing to ourselves Galileo rolling down round objects along an inclined plane in an effort to discover experimentally the law governing the relationship between the distance traveled by the rolling body, subject to the earth's gravity and the time taken to cover that distance. We shall imagine that at the end of seven experiments he has collected the following entirely unlikely perfect results:

t . . .	0	5	10	15	20	25	30
s . . .	0	5	20	45	80	125	180

It seems at first that from this table of results Galileo can at once read off the law correlating time and distance

(G) $\quad s = 1/5\, t^2$

i.e., that distance equals the square of time multiplied by a constant which, for the plane used with its particular inclination, equals 1/5. H. Jeffreys has, however, pointed out that there are infinitely many other equations which fit these seven results just as well as (G):

(J) $\quad s = 1/5t^2 + t(t-5)(t-10)\ \ldots\ (t-30)\ f(t).$

It should be noted that (J) is an infinite set of equations, since $f(t)$ may assume infinitely many different forms. Infinitely many members of the set denoted by (J) will give a different value, for corresponding values of s, to the next t to be tested, although for all the t's so far tested they gave values of s identical with those yielded by (G). But at the same time, (J) contains an infinite subset of equations which will yield the same value of s as does (G) for the next t to be tested. It is because of this last fact that the possibility of a crucial experiment to decide between (G) and any of the (J) equations is ruled out. For suppose we try to adjudicate between the (G) equation and any equation belonging to (J), by testing what will happen when $t = n$. Suppose the result satisfied (G). This, of course, eliminates an infinite number of (J)-type equations, but we would be still left with infinitely many equations competing with (G), namely, with all those (J) equations, for which

$$f(t) = (t - n)\, g(t)$$

where $g(t)$ may assume infinitely many forms.

Now what exactly does our problem amount to? It would be a total misrepresentation of the situation if we said that the difficulty raised was merely that the claims of science, including even those which concern quite elementary relations like Galileo's law of free fall, are not securely founded. The situation seems rather comparable to the one in which I buy one lottery ticket where there are many billions of tickets and only one of them will be drawn to win a prize. Given that each number has an equal chance to win, it would be wrong to say that it is merely uncertain that I shall win; the correct thing to say is that it is virtually certain that I am not going to win. Similarly, given Galileo's experimental results and the principle that the true hypothesis must satisfy them all, together with the fact that there are infinitely many hypothesis capable of doing this, implies not merely that we cannot have full confidence that (G) stands for the law governing the correlation of distance and time but rather that it is virtually certain that (G) does not truly represent this law.

It will, of course, be noted that of all the admissible equations (G) is the simplest and therefore the most convenient of the available equations. This, however, in the absence of further argument carries about as much weight in favor of adopting (G) as would, in the case of the aforementioned lottery, the argument that the most convenient thing for me would be for the ticket in my possession to win and therefore my confidence that it will win is, to some extent, justified.

One must refrain from using a number of faulty arguments which seem to present themselves and which one may be strongly tempted to employ in defending Galileo's choice of (G). Among these the most often heard is: but equation (G) works! We are much more interested in the practical aspects of science than in its capacity to produce ultimate truth. (G) has proven itself to work in the past; therefore we believe it will work in the future as well. For practical scientists this is what really matters.

This argument can be seen as faulty without our questioning the underlying assumption that whatever has worked in the past is going to work in the future as well. At this stage we shall ignore Hume's problem of how we know that the unobserved will be like the observed or the future will be like the past. We shall assume we know. The trouble with the argument is that it overlooks the fact that there are infinitely many other equations which too have worked in the past. The argument cannot be applied to all these since infinitely many of them yield incompatible prediction. What then is there in (G) which justifies our belief, that to it, rather than to its rivals, we should apply the argument that an equation which has worked in the past will work in the future as well?

Some may wish to argue that the credibility of (G) is ultimately based, not on the experimental results listed or on any further evidence of this kind, but rather on its derivability from higher order laws and its having become an integral part of a large interconnected system known as Newtonian mechanics.

This line of approach is wrong on a number of accounts. Nobody will deny that with (G) becoming integrated into Newtonian mechanics the evidence for its truth has undergone an important qualitative change. But to claim that, until this took place, the empirical support for (G) was nonexistent would be a misrepresentation of how scientists view the situation. Galileo is not criticized for having had confidence in (G); in fact, he is admired for his efforts to establish his law of free fall by the only method available at the time, which was to test s against the various values of t. Furthermore, if we disqualified this method we should never reach a situation in which we had any higher order laws from which to derive (G). Higher order laws with their unifying power are only discovered on the basis of our knowledge of at least some of the laws they imply.

There is, of course, a more immediate reason as well why this line of approach is useless: Jeffreys' difficulty, which he happened to illustrate by using Galileo's laws, applies with equal force to any hypothesis, including Newton's laws, and until his problem is solved we cannot be said to have any evidence for any type of law.

Another suggestion which might seem to have merit is that, in view of all the competing equations, we should leave no t untested and thus render the problem for practical purposes irrelevant. From a practical point of view, the range of t is limited. If t exceeds a few minutes we are no longer in the neighborhood of the earth's surface and (G) is not applicable. Between the limited range of zero and a few minutes, t assumes a finite number of different values since, for practical purposes, we are not interested in time intervals less than, say, one-tenth of a second. After we have determined the values of s for all the relevant values of t, the continued existence of infinitely many (J) equations no longer need to trouble us. All the (J) equations left in the field yield identical results for all the tested values of t and the fact that they give different results for all the untested values of t is of no consequence since in practice we are never going to encounter those.

What is wrong with this suggestion is not merely that it may not work everywhere since in general the range of values which are of interest to us is not as limited as in this particular case, but that it goes counter to one of the most central ideas of science. One of the main aims of the scientific enterprises is the construction of hypotheses on the basis of which we may make predictions concerning situations never encountered before. The reason why Galileo's equation is of scientific interest lies in its economy and fruitfulness, and in the fact that it is a very simple equation which has been established on the basis of a few experiments and is nevertheless capable of yielding the values of distances corresponding to a large number of yet untested values of time. Galileo regarded his law as well confirmed long before he had tested it for all the values of t which are of practical interest and, far from condemning this as a deficiency in his scientific judgment, it is taken as a sign of his true insight into the nature of scientific hypotheses.

2. The Problem of Additional Variables

One conspicuous difference, of which we have made no mention yet, exists between equation (G) on the one hand and all the (J) equations which remain unrefuted after a considerable number of experiments. This difference, it has struck many people, cannot be devoid of significance although it is not immediately clear what the significance is. On the one hand, it is only after we have performed all the experiments, the results of which we have tabulated, that we could put forward any particular (J) equation, while on the other hand, the (G) equation was advanced earlier on the basis of fewer experimental results than we have now at our disposal.

Suppose we have on our list the outcome of twenty experiments. It seems unquestionably true that (G) equation was advanced as an equation likely to represent the law we are after on the strength of the first five or six results, and it is quite immaterial which five or six of the twenty values of t we have tabulated had been experimented on first, for the same (G) equation would have been advanced. Not so with (J) equation. The twenty factors preceding $f(t)$ in the second term of the right-hand side of the equation would not have been spelled out by us (or at any rate, it is exceedingly unlikely that they would have been spelled out) before the particular values of t for which the distance fallen is going to be determined were known.

What exactly are we going to make of this acknowledged difference? Would it be reasonable to argue that we select (G) equation because it is much more strongly confirmed since it was advanced right after the fifth experiment and thus was confirmed by every subsequent experiment, the outcome of which could have falsified but did not? And we reject any particular (J) equation which we are setting up as a rival to (G) equation and which has been advanced only now and thus has not yet been exposed to any refutation yet?

I do not believe that many would find such an argument very convincing. It may well be that nobody was likely to have thought of the particular (J) equation we are now setting up as a rival to (G) equation before all the twenty values of t to be tested were known. This, however, does not amount to saying that the (J) equation in question has not in fact been tested just as severely as its rival (G) equation. An equation exists independently of whether anyone thinks of it and every experimental result which does not show that it cannot represent the law of free fall lends it exactly as much support as it does to (G) equation whether or not at the time of the experiment anyone had his mind fixed on that particular equation.

It is of course possible to point out that there is nevertheless a difference between (G) equation advanced fairly early in the course of the series of experiments performed and any particular (J) equation put forward merely at the very completion of the series. Both of course have survived the same number of tests. In the case of the former, however, active attempts were made by the experimenter to falsify it, while in the case of the latter no conscious effort was made to expose it to experimental refutation. If we agree that confirmation consists in putting a hypothesis to severe tests, with the honest intention to see whether it can survive it, then (G) equation has received confirmation whereas (J) equation has not. It is, however, hard to see why anyone should agree to such dogmatic and arbitrary viewpoints of what confirmation consists of.

Another way to try to exploit the difference here, noted between (G) equation and (J) equation, is to stipulate that no equation is likely truly to represent a law of nature if it is not discovered after m experiments where m is some agreed upon number. We therefore perform m experiments and advance the various equations which we think are suggested by the outcome of these experiments. Next we perform another experiment for an additional value of t selected at random. If it should turn out that (G) equation is the only equation compatible with all the $m + 1$ results, then, now that we know the $m + 1$ value of t, we are not permitted to set up the appropriate rival (J) equation containing the extra factor $(t - t_{m+1})$, since this would be contrary to our stipulation.

Once again it would not be easy to justify such a stipulation. But there is also a much more basic reason why no suggestion attempting to exploit the difference between (G) equation and (J) equation, namely that the former could be thought of much before all the experimental results were in, while the latter would be advanced only after all the values of t were known, can succeed. There is a whole group of (J) equations we have so far neglected to consider, each one of the infinitely many members of which can as easily be thought of and advanced as soon as (G) equation.

So far we have unquestioningly assumed that for any tested value of t the value of s obtained on one occasion will be the value obtained on all occasions. Jeffreys has taken for granted that the same value of s corresponds at all times to a given value of t and raised doubts only concerning values of s which correspond to yet untested values of t whether they will turn out as predicted on the basis of (G) equation. One may, however, quite legitimately raise doubts about whether future values of s corresponding to already tested values of t will turn out in accordance with (G) equation. And what is important, one may do so without questioning the assumption that the future resembles the past or that the unobserved will be like the observed. The experimental results we have collected so far are perfectly compatible with the supposition that the law of free fall is of the form:

(K) $s = 1/5\, t^2 + A \sin N!\pi/n,$

where N stands for some physical parameter which assumes integral values only and A and n are constants (the latter an integer). Then it is clear that as long as $N > n$, $N!\pi/n$ is an integer and $A \sin N!\pi/n$ equals zero. Thus the values so far obtained for s, instead of showing that there is no other physical parameter operative, should be taken as indicative of the fact that the third parameter had so far values exceeding n. (K) equation, for instance, may stand for the temperature at the center of the earth measured on a (quantum) scale admitting of integral values only. The temperature at the center of the earth is known constantly to be decreasing; let n be the value of the temperature to be reached in two weeks time at the center of the earth. Clearly (K) equation is not in the least more hard to think of as (G) equation, and every test for the last three centuries and for the next two weeks which supports (G) equation supports (K) equation to the same degree. Both have yielded so far and will yield for another two weeks that for $s = 5$, $t = 5$, but with respect to times beyond two weeks from now $t = 5$ will correspond to some other value of s according to (K) equation.[1]

It could of course be pointed out that (K) equation introduces a third variable into the picture and hence suggests that, as a matter of principle, we do not assume that an extra variable is operative until forced to do so. This suggestion would not be of much use until we justified the alleged principle. It would certainly be wrong to claim that there was anything in the evidence pointing to the absence of a third variable. All that one may say about the evidence is that it is not indicative at all that there is such a variable, but then it fails precisely to the same extent to give any indication that there is no extra variable involved.

[1] The problem raised here affects not only generalizations of a more sophisticated nature where, as in Galileo's law of free fall, an equation relating variables representing physical parameters expresses the law governing the covariation of these two physical parameters. Simpler generalizations like 'All ravens are black' are equally affected. The fact that hitherto all observed ravens have been black can be expressed by

No. of black ravens observed/No. of ravens observed = 1

and also

No. of black ravens observed/No. of ravens observed = $1 - A \sin N!\pi/n$

the two yielding different predictions.

With the introduction of a third variable we may now give our additional reason for rejecting a suggestion made in the previous section for the solution of Jeffreys' problem. It was suggested that we could eliminate all the (J) equations which may from a practical point of view be regarded as rivals of (G) equation by performing enough experiments for every relevant value of t which are after all limited in number. Now, of course, the suggestion is at once seen as useless since we have raised doubts about the future values of s for already tested values of t.

This latter way of rendering our problem has the advantage of clearly bringing out the fact that the difficulty with empirical inference is completely all-pervasive, affecting the most familiar propositions of common sense no less than the sophisticated results of advanced science. It is easily seen that the assumption that past regularities will continue in the future or that the unobserved resembles the observed does not, in the least, imply even that just because the sun has risen every day in the past it is going to rise tomorrow or that because unsupported bodies near the surface of the earth have always fallen to the ground they will continue to do so in the future. In prerelativistic physics, it was asserted that

(L) $\quad \text{mass} = \text{constant},$

but now our attention has been drawn to the fact that all observations throughout history were compatible with the rival hypothesis that

(M) $\quad \text{mass} = \text{constant} - A \sin N!\pi/n,$

where N stands for a physical parameter, the value of which drops below n tonight for the first time in history. We have no arguments to support the claim that (L), rather than (M), describes truly the nature of mass. If, however, (M) is the true hypothesis, then overnight masses will assume large negative values, setting up repulsive forces between the earth and the sun, in consequence of which the sun will not rise tomorrow and unsupported bodies will cease to fall to the ground.

I believe that, in order to make any progress toward a solution, it is essential to realize that the notion of doubt with respect to scientific method is very different from the notion of doubt as it applies to some particular claim about nature once we have accepted the validity of a particular method of empirical inference.

Normally, when we entertain serious doubts about the validity of an equation purported to represent a law of nature, the reasonable thing to do is to suspend judgment until more evidence is forthcoming and, in the meantime, refrain from employing or relying on the equation in question. Suppose the questionable equation has been used in the construction of a new type of aircraft; a rational person will avoid traveling on it because of its unproven safety. Sensible people refrain from taking serious risks and, whenever the safety of a given line of action depends on the validity of a putative law of nature, they will not commit themselves to that line of action until the law in question is confirmed beyond reasonable doubt.

Suppose no one finds any solution to the difficulties we have been discussing. How is this to affect the practical conduct of our daily lives? Given, for example, that in the construction of airplanes the validity of all sorts of laws has been assumed without there being any way of justifying why they have been assumed to be true rather than any one of their many rivals, is it advisable to avoid flying by plane? The answer is that there cannot be any point in avoiding air travel when one is just as much exposed to dangers when staying on the ground. We are, after all, unable to justify our belief that the ground beneath our feet will not melt, evaporate, or explode; ought we not therefore get off it and seek the relative safety of the air? It should be clear that since the problem we have been discussing is an all-pervasive one, affecting to the same degree every empirical proposition, there is no escape from it. Avoiding boarding airplanes, or not venturing outside our homes, or even lying immobile in bed will not shield us from the dangers of our environment; as long as we cannot opt out of this world we remain exposed to these dangers. Complete agnosticism with respect to scientific method is an impossibility, for being agnostic with respect to any given hypothesis implies our refusal to commit ourselves to it. Whatever we do or refrain from doing, we commit ourselves to a large number of empirical hypotheses.

The last point has the important consequence that we cannot be called reckless or said to be taking risks when adopting any particular method of hypothesis selection, since there is no alternative we may regard as safer. The labeling of a given action as hazardous carries with it a disapprobation and implicit opinion that a different, less risky action should have been substituted in its place. Similarly, our expression of doubt implies the advocacy of the withholding of judgment and when the efficacy of a given line of action is doubted it is implied that an alternative, more secure line should be taken instead—which again cannot be held with respect to a line of action taken on the basis of any principle of hypothesis selection. Indeed, concepts such as hazard and uncertainty as well as safety, reliability and credibility are not defined until a given scientific method is chosen and in relation to it. To rely on a given hypothesis may be said to be hazardous if and only if it is done not in accordance with the requirements of a scientific method we subscribe to. When, for instance, it is said of an aircraft that it is highly reliable and that it is extremely improbable that it should fail to land safely after take-off, what is meant is that the laws of nature which have been employed in the construction of the vehicle have been adopted as confirmed in accordance with the selection principles laid down by the method universally prevailing in the scientific community and have gone through all the tests to the degree of rigor required by it. On this construal of the notion of safety, reliability, and probability, one just cannot speak of the safety, of the reliability of the selection principles themselves, and of the probability that they will lead to success.

But it by no means follows from this that there is no need or possibility to defend scientific method. Admittedly, now we claim that not only cannot one show that the prevailing scientific method is more assured of success than others, but that the whole notion of likelihood to succeed is inapplicable to scientific method as such. There are, however, other desiderata scientific method has to fulfill. One reasonable demand is that our method should be such that it makes maximum use of past experience in recommending the hypothesis to be selected. In what follows I shall give an outline of a defense, according to which the prevailing method is not the best method but the *only* method which is universally applicable in selecting hypotheses on the basis of past experience.

3. The Only Usable Methodological Principle

It will be recognized at once that a methodological rule requiring that, in any given situation, we choose the least simple of the admissible hypotheses would render us completely paralyzed. Given any hypothesis, it is easy to construct one which is more complex than it and therefore, unlike the most simple, the most complex hypothesis is an undetermined and undeterminable proposition. Thus, if the problem facing us was confined to the question, should we adopt as our universal rule the principle of simplicity or the principle of maximum complexity, we would reject the latter not merely by saying that it is an inferior principle to the principle of simplicity. We would reject it because it is not a principle at all; the rule requiring us to accept the most complex of all the available hypotheses does not really require us to accept anything and is therefore not a rule in the minimal sense of that term.

The next step in my argument is to show that if we were to adopt a rule always to choose the *second* simplest of all the admissible hypotheses, or for that matter the *third*, *fourth*, or anything but the simplest hypothesis, we would not be provided with any means by which to decide what hypothesis to choose and would, therefore, be left without a rule.

I shall illustrate this claim with an example, which has a rather restricted applicability but which has the advantage of reflecting three important aspects of methodology.

Suppose three nonlinear points are given, lying on the path of a planet known to move along a closed orbit. The simplest of all available hypotheses under these circumstances would appear to be that the planet moves along a circular path. It will be agreed by most that, in the absence of anything but geometrical considerations, the hypothesis with simplicity of rank two is that the orbit of the planet is elliptical.

There are, however, infinitely many ellipses which pass through the three points. Thus if we were to adopt the rule that, in any situation, one is to choose the hypothesis of rank-two simplicity we would not be provided with a definite instruction regarding what specific orbit to postulate. On the other hand, the rule to choose the hypothesis of first-rank simplicity gives us the unique circle determined by the three points.

It is of great interest to note a second feature of the principle to maximize the simplicity which is that the situation remains unchanged with respect to the basic difference between it and the alternative methodological principles we are comparing it with if we wait until more points along the path of the planet are given. Suppose at some later time five points lying on the path of the planet become known and that no circle but an ellipse passes through these points. Under these new circumstances, the hypothesis which postulated an elliptical orbit moves up to rank-one simplicity. But, under these circumstances, it is also our good fortune that no more than one ellipse passes through the points given, since through five points either no ellipse or, at least, one ellipse may be drawn.

This then is a small illustration of what I suggest may be universally the case: While other principles of selection lead us to a whole set of hypotheses of certain kind, the principle to pick the simplest of all hypotheses generally leads us to pick a unique hypothesis. And although what is the simplest hypothesis changes with changing circumstances, it is an invariant aspect of our principle that it is usable.

Before leaving this example, let me raise a very crucial third point which may be made with its aid. On the surface, it may seem that the following question could be posed: even though our data consist of no more than three nonlinear points on the path of the planet under investigation, any ellipse we may draw through these points is just as concrete, specific, and uniquely different from any other curve as is the circle determined by the three points. What then is the basis for saying that this unique curve, which is perfectly compatible with our data, is less probable to represent the orbit of our planet than the circle?

The answer to this question lies in the realization of the essential idea that we are not considering the prior probability of any putative hypothesis. We are approaching the matter from the point of view of the *rules* according to which a hypothesis is adopted. Any curve which passes through our three points is unique, but it is only in the case of the circle that we can name the universal methodological principle which has directed us specifically to postulate it, rather than any other curve, as the shape of the orbit of our planet.

Ultimately, then, our adherence to the rule to maximize simplicity is seen to be caused by the indeterminate nature of all alternative rules. All ellipses which pass through the three points are symmetrical with respect to one another. Why pick this ellipse rather than that? The principle to choose the hypothesis of simplicity rank two seems to operate symmetrically with respect to all the infinitely many ellipses. No other principle which singles out this ellipse and is also universally applicable seems to present itself which could provide grounds for our choice. We can, however, explain why we pick the circle we do pick; sufficient reason for our choice can be provided: It is uniquely given by the rule which instructs us to choose the hypothesis compatible with the data and is the simplest of those which satisfy them.

Now we may return to our previous examples. Until the beginning of the twentieth century, our experiences allowed us to describe mass by

(L) mass = constant

or by

(N) mass = constant + $F(x)$

where $F(x)$ reduces to zero for all past observations. The rule to maximize parsimony bids us to select (L). I should point out that we are not ignoring the fact that there are difficulties in exhaustively characterizing the notion of simplicity. However, no matter how intricate these difficulties may be in general, in the present context the selection of the most parsimonious of all the available equations is a straightforward matter. It is our good fortune that, in all the situations associated with the universal problem we are considering, namely, where in the case of every hypothesis compatible with the data which is advanced, infinitely many others are automatically generated, it is immediately obvious which is the most parsimonious of all these. That equation, the terms of which on the right-hand side form a proper subset of all the terms on the right-hand sides of the other equations, is the most parsimonious of all.

Which of all the equations is of parsimony rank two? Let $F^*(x)$ be the simplest $F(x)$; then it is clear that

(N^*) mass = constant + $F^*(x)$

is the equation of parsimony rank two. Suppose we adopted the rule to choose the hypothesis of rank two, then, of course, we would have to face the difficulty of how to identify $F^*(x)$ since we have no generally applicable criterion for the ordering of mathematical expressions with respect to their simplicity. This, however, may not be an insurmountable difficulty since there is no reason to believe that such a criterion may, in principle, never be constructed. The real difficulty is the x may stand for indefinitely many physical parameters and thus (N^*) represents not a single equation but an equation form. Here again, then, in this much more general situation we find exactly what we found in our more specific example, that is, if we adopt the principle to maximize parsimony, we are led to the choice of a specific hypothesis, namely, that (L) describes properly the nature of mass. If we were to adopt, however, a rule to choose the hypothesis of parsimony rank two, we would be presented with an indefinitely large set to choose from without being provided with sufficient reason why we should prefer a specific equation of type (N^*) over another.

The second point made earlier, concerning what happens when increased knowledge replaces one hypothesis by another, as being the most parsimonious in the context of the newly arisen situation is well illustrated here too. With the acquisition of new knowledge that led to the adoption of special relativity, the most parsimonious expression describing the behavior of mass became

(R) $\quad \text{mass} = \text{constant} + F(v)$

where v is the velocity of the body whose mass is measured relative to the system in which it is measured.

Once again, there are infinitely many equations which are equivalent to (R) with respect to all past observations, namely

(S) $\quad \text{mass} = \text{constant} + F(v) + G(x)$

where $G(x)$ reduces to zero for all those observations. Suppose $G^*(x)$ is the simplest form of $G(x)$, then

(S^*) $\quad \text{mass} = \text{constant} + F(v) + G^*(x)$

is the equation of simplicity rank two describing mass in a manner compatible with contemporary knowledge. But, again, although (R) is a single equation (S^*) represents an indefinite number of equations, since x may stand for all sorts of physical parameters. Thus the principle of parsimony yields a unique hypothesis, while the principle to select a hypothesis with any degree of parsimony other than the first fails to yield a unique hypothesis. This, of course, is not too surprising. The crucial advantage of the principle of parsimony over all other principles described here is a necessary consequence of the very notion of parsimony. The most parsimonious of all the equations which will fit all that has been observed is the one which is free of all extraneous[2] elements.

[2] It should be clear that the extraneous element is not necessarily, as in the case before us, a term; it may also be a factor or an index. For example the equation

$$\text{mass} = \text{constant} \times \cos T!\pi/n$$

is, as long as $T > n$, equivalent to

$$\text{mass} = \text{constant}$$

and is a less parsimonious rival to the latter, containing the extraneous element $\cos T!\pi/n$.

It is important to realize that each one of the infinitely many less parsimonious rival hypotheses h' of the most parsimonious hypothesis h is parasitic upon the latter. Hypothesis h' is formed from h by adding to it an extra element such that

There is only one way of being free of all extraneous elements; there are infinitely many ways of having such an element—by having *any* one term which is extraneous.

For a moment it might seem perhaps that we could adopt some other principles as well which would always yield a unique hypothesis. It might be suggested in a fanciful way, for instance, that in order to overcome the problem of not being provided with a specific hypothesis whenever we required one, we should appoint a certain person as our "hypothesis chooser" whose task it would be to give clear instructions in every instance what hypothesis we should subscribe to. Alternatively, it might be suggested that whenever presented with a set of hypotheses all compatible with past observations, we should choose the one we think of first.

Now it is unquestionably true that there are uncountably many rules one could adopt to pick out always a particular hypothesis compatible with past observations and which would yield concrete predictions. We could agree, for example, on the rule that from all the competing hypotheses we should choose the one to be adopted by drawing lots or the United Nations should appoint a "universal hypothesis chooser," whose task it would be to pick one hypothesis out of all those which have so far accounted for all the events of the past, for the whole world to adopt. Thus, it would not seem impossible to adopt the convention that the phrase "the future will resemble the past" should be taken to mean that the future will resemble the past described by the statement picked out by any one of the aforementioned rules.

Momentarily it may seem, therefore, that our original problem has shifted to the next level: we now appear to require a meta-rule to help

both *h* and *h'* are equally confirmed by any member of a set of evidence e, namely, the evidence so far observed, but there is a set of evidence e' any member of which if it confirms h does not confirm *h'* and if it confirms *h'* does not confirm h. Of course, *h* may sometimes have genuine rivals which are not parasitic upon it, in the way Copernican celestial machanics rivaled Ptolemy's or Young's theory of light rivaled Newton's. Such hypotheses require, however, the services of a creative scientist; they are not mass produced. Such rival hypotheses do not present an important philosophical problem since, because there are never more than a few of them, they are eliminated by crucial evidence which supports one hypothesis and not the other. Of course, there is always specific evidence which will adjudicate between any *h'* and *h* and hence, if *h* is the correct hypothesis, *h'* will drop out of the competition because of that evidence. But since there are infinitely many *h''*s, no matter how many are eliminated through the use of crucial evidence, there are still infinitely many left to compete with *h*.

us to decide which first-order rule for selection of hypotheses to adopt. But it will easily be recognized that the first-order rule which has universally been adopted is different from all other possible rules. For, it is true that no matter which convention we adopted the future would be assumed to resemble the past according to some description of the past. There would be nothing in the past events nor in the types of regularities exhibited by these, which, *in themselves*, determined which particular description we should choose for projecting into the future. Only according to the prevailing convention is it the case that the past is assumed to determine the future in a *strong sense*. The future behavior of masses is predicted on the basis of their past behavior without appeal to anything extraneous (e.g., the drawing of lots or a special person). It entirely depends on the nature of past events what regularities these exhibit and which statement corresponds to the most parsimonious true description of these.[3]

4. A Defense of the Induction Principle

Now we are in the position to say something about the problem of induction in general. What is the problem of induction? There are not many people who, in the twentieth century, have restated the problem of induction more lucidly than Bertrand Russell in a chapter called

[3] Suppose now it is objected: admittedly the selection principle according to which we are to adopt the most parsimonious of the available hypotheses is indeed different from all other such principles inasmuch as it alone relies on nothing extraneous to the listed results of our observations. But the selection principle according to which we are to subscribe to the hypothesis provided for us by the "universal hypothesis chooser" appointed by the U.N. is unique too inasmuch that it alone ensures that we always adopt the hypothesis selected by the special person assigned to the task. Thus what makes us adopt the principle according to which the future is determined by the past in a strong sense, which is unique in one way, rather than another principle such as the one just mentioned which is unique in another way?

The answer to this question is similar to the answer to the question Why, if we do employ the principle to assume that the future is like the past in the strong sense, should we select the most parsimonious rather than the second or third most parsimonious hypothesis, since it is only through the practice we have adopted that we end up with something determinate? If we were permitted to use anything extraneous to our observations, such as a universal hypothesis chooser, then indefinitely many candidates present themselves and there is no way to select among the indefinitely many diverse selection principles which employ these. Only if we adopt the rule not to admit any extraneous elements do we have a unique principle of hypothesis selection which also selects a unique hypothesis, namely the most parsimonious hypothesis.

"On Induction" in his book *The Problem of Philosophy*. He begins his exposition with an example:

> Let us take as illustration a matter about which none of us, in fact, feels the slightest doubt. We are all convinced that the sun will rise tomorrow. . . . It is obvious that if we are asked why we believe that the sun will rise tomorrow, we shall naturally answer 'Because it always has risen every day.' We have a firm belief that it will rise in the future because it has risen in the past. If we are challenged as to why we believe that it will continue to rise heretofore, we may appeal to the laws of motion: the earth, we shall say, is a freely rotating body, and such bodies do not cease to rotate unless something interferes from outside, and there is nothing outside to interfere with the earth between now and tomorrow. Of course, it might be doubted whether we are quite certain that there is nothing outside to interfere, but this is not the interesting doubt. The interesting doubt is as to whether the laws of motion will remain in operation until tomorrow (pp. 60–61).

He goes on to explain why this doubt cannot be resolved through any argument based on our knowledge of the past:

> It has been argued that we have reason to know that the future will resemble the past, because what was the future has constantly become the past and has always been found to resemble the past, so that we really have experience of the future, namely, of times which were formerly future, which we may call past futures. But such an argument really begs the very question at issue. We have experience of past futures, but not of future futures, and the question is will future futures resemble past futures? (Pp. 64–65.)

It seems to be implied that if the question, "Will future futures resemble past futures?" could be answered in the affirmative, then we could rest assured about tomorrow's sunrise; our only cause for worry is our inability to provide an affirmative answer. Russell appears quite clearly to be maintaining that if it were given that there has been no outside interference to stop the rotation of the earth and yet the sun failed to rise, this would constitute an example that nature cannot be relied upon to remain always unchanged and future events do not necessarily continue to obey the past regularities. But this, as we have seen in section 2 of this paper, is not so. The sun may fail to rise tomorrow because masses have always obeyed and continue to obey equation (M) rather than equation (L). Clearly then the inductive principle does not merely bid us to assume that the future will be like the past, but that the future will be like the past in accordance with the most parsimonious description thereof.

In the light of what we have said in the previous section, however, the Russellian way of describing induction does not necessarily appear faulty. It need not be objectionable to express oneself in the manner of Russell as long as it may be assumed as understood that 'the future will be like the past' is merely brief for 'like the past described in a particular fashion' everyone knowing in which particular fashion, namely, in accordance with the most parsimonious description. The reason why it can be assumed that this is what is meant is that there does not seem to exist any other way of interpreting the principle. As we have seen in the previous section, any other interpretation does not really allow us to make predictions on the assumption of the strict similarity of past and future alone (since one is required to employ extraneous factors beside one's knowledge of past regularities exhibited by the event type we are concerned with) or does not allow us to make any specific predictions at all because it renders one's principle indeterminate.

Now we are in the position to see the outlines of a possible defense of induction in general. This defense presupposes that all agree that no guarantee whatever can be given that any method of predicting the future yields correct results. On the other hand, it is also understood that we cannot refrain from acting altogether and therefore we must subscribe to some method of hypothesis selection. In view of this it may be sufficient to show that the method adopted is superior to any of its alternatives.

Suppose we agree that it is an essential desideratum that all our hypotheses about the unobserved should be constructed on the basis of what is equally accessible to all people. We shall take it for granted that the raw material upon which our principle of empirical reasoning is to operate is the observed facts of the kind about which we are to hypothesize.

It would seem then that the method which arrives at conjectures based on past observations through the principle to assume the resemblance of the future and the past has an edge over all alternative 'methods'; it is the only method capable of being applied to experience.

Suppose it were recommended that we should employ the principle — which sometimes has been called 'counterinductive' — always to assume that the future will be unlike the past. Does the principle bid us to assume that the future will not be like the past as it is described by inductivists? Such assumption leads us nowhere, since it does not yield determinate predictions; there are infinitely many ways in which the future may differ from the past. Thus, this version of counterinduction offers no methodology at all. But we would not be better off if we were to assume that the future will not be like the past, as described in any other particular way.

Is it perhaps feasible to employ the principle to assume that the unobserved will not be like the past as described in any but one particular way? Which particular way are we to take as being singled out by this principle? Surely not the most complex one, since, as we have already said, that simply does not exist. But descriptions of parsimony of rank two as well as of rank three and, in fact, of any other rank except one exist in infinitely large numbers, and we would be provided with no means of choosing a particular hypothesis among all the hypotheses of equal rank. Only the description which contains no redundant elements is a unique description of the observed in the sense that every other description differs from it in the rank of the parsimony it possesses. But, of course, if the principle to assume that the future will not be like the past is to be interpreted to mean that it will not be like the past as it is described in any but the most parsimonious way, then the principle is equivalent to the inductive principle. The latter, therefore, appears to be the only principle which leads to the choice of a determinate hypothesis among the infinitely many hypotheses which are automatically generated by the addition of terms extraneous with respect to the past and provides us with a methodology of coping with the future with the aid of a rule applicable to past observations.[4]

[4] It is being assumed throughout that the mathematics we work with and the language in which we formulate our statements are the ones in common use. We do not, for example, admit artificially constructed predicates such as grue and bleen. Apart from all the obvious objections to such predicates from considerations of simplicity and teachability, there is the objection that there are infinitely many possible artificially created words and there are infinitely many diverse predictions we are led to make through the use of the various artificial predicates. But there is no criterion to guide us how to choose among these predicates; thus there is nothing to tell us which of the contrary predictions to accept. If we stick, however, to natural languages, we have no problem since their use yields a unique prediction in all cases. Thus the principle to use natural language and it alone produces determinate predictions.