DISCUSSION: ANALOGIES AS GENERALIZATIONS*

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I

Analogies have been traditionally recognized as a proper part of inductive procedures, akin to generalizations. Seldom, however, have they been presented as superior to generalizations, in the attainability of a higher degree of certitude for their conclusions or in other respects. Though Bacon definitely preferred analogy to generalization[1], the tradition seems to me to go the other way—until the recent publication of works by Mary B. Hesse ([2], pp. 21-28 and *passim*) and, perhaps, R. Harré ([1], pp. 23-28 and *passim*).

The aim of the present note is to argue the following two points. First, generalizations proper are preferable to predictions from past observations to a single future observation, since the latter are *ad hoc*. Second, analogies are either generalizations proper—perhaps higher-level ones—or *ad hoc*. In any case, they are not more certain than generalizations, but they are still equally legitimate.

II

Traditionally, extensions of known observation-reports to unknown cases are of two kinds: from the sample to the whole ensemble—generalizations proper— and from the sample to the next case to be observed. Of course, all intermediate cases between these two extremes are possible, but inductive philosophers have hardly paid any attention to them. The reason may be this: the principle of paucity of assumptions leads us to reject the generalization, i.e., the extension of an observation-report about the observed sample to the whole ensemble, in favour of a less bold extension. The least bold extension is not to extend the report at all (the conventionalist twist); but as this will not do, the extension to the next case to be observed is advocated as a secondbest. In my opinion, this mode of thinking is erroneous. Our assertion about the next case should be interpreted in the following way: the next case to be observed will agree with the sample on the basis of the hypothesis that the sample is representative; otherwise, we claim, in effect, that the next case to be observed is somehow more closely linked to our sample than to the ensemble as a whole.

Take the standard example of induction concerning the whiteness of swans. The next swan is going to be white, we assert. We may so assert because we think all swans are white. Alternatively, we may think that all swans in our neighbourhood are white. This is not different at all from the previous case, except in the choice

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[1] Bacon's opposition to generalization—induction by simple enumeration—on the basis of their uncertainty which rests on the smallness of the sample as compared with the ensemble (*Novum Organum*, I. Aph. 105) is well-known, and so is Ellis's discussion of it in his introduction to that work. Bacon's advocacy of analogy (II, Aph. 42) is well-known too. One should stress, however, in all fairness, that he was not unaware of its problematic character: he says that analogy 'is doubtless useful, but is less certain, and should therefore be applied with some judgment.' As usual, he is ambiguous wherever he senses a difficulty.

of ensemble—swans-in-our-neighbourhood instead of swans. Alternatively, we may expect to be lucky and avoid on the next occasion encountering any of the existing non-white swans. The next swan we meet may be white even though all unobserved swans in the universe except that one are non-white, or even though only one quarter of the unobserved swans in the universe are white, or one half, or three quarters, etc. In other words, the hypothesis that the next swan to be observed will be white is not an extension from the existing sample, but rather a disjunction among (infinitely) many conjunctions about all unobserved swans in the universe and about our luck with the next observation (see *Appendix*). *If we do not claim that all unobserved swans in the universe are white, and if we deny that our next encounter with a swan is of any special cosmic significance, then we cannot but ascribe the whiteness of the next swan to mere chance.* The assumption that no matter what the distribution of swans in the universe, the chances are that the next swan to be observed will be white *because* all swans so far observed were white, seems to be a way out. But it merely is the well-known gambler's fallacy, as has been proved by Popper recently[2]. This exhausts all possibilities, as well as explaining why we refuse to consider seriously the theory 'since all observed swans are white the swan that Tom will observe next is white but the swan that Dick will observe next is not'. It is too arbitrary; it gives *ad hoc*, or with no reason, a special significance to Tom's observation.[3]

III

And now to analogy. Take the assertion that since Tom possesses the property P and the property Q, so Dick, who possesses the property P, also by analogy possesses the property Q. Suppose that Harry also possesses the property P; either (*a*) by the same analogy Harry also possesses the property Q, or (*b*) we arbitrarily discriminate between Dick and Harry, or else (*c*) we give reasons for discriminating—by differentiating between Dick and Harry. Case (*a*) is obviously an argument by analogy which is the same as a generalization; case (*b*) must be dismissed like all *ad hoc* hypotheses. Take, then, case (*c*). Differentiating between Dick and Harry amounts to the claim that the one but not the other possesses the property R. This property R is either (C_1) possessed by Tom as well, or (C_2) not. Case (C_1) is;

	Known	*Assumed*
Tom	$P \quad Q \quad R$	
Dick	$P \quad\quad\; R$	Q
Harry	$P \sim Q \sim R$	

The argument here is by analogy not from P to Q as has first been stated, but from P and R to Q (or perhaps even from R to Q). It either applies to all who possess P

[2] This part of the discussion rephrases works by K. R. Popper: ([3] pp. 69-73), [4], [5], [6]. A more formal statement of the argument is in the appendix to this note.

[3] We can easily give a special significance to the next observation in a game of chance; we can first select the object of observation but refrain from observing it, and then calculate the probability of its having a certain property, as Popper does when building a model complying with Laplace's rule of succession. This means that the application of Laplace's rule to science implicitly assumes the intervention of providence, a hypothesis which Laplace said he did not need.

and R, or it is *ad hoc*, or it is not from P and R but from P, R, and S. And so on, until we exhaust all the characteristics shared by Tom and Dick. Case (C_2) is:

	Known	*Assumed*
Tom	$P \quad Q \sim R$	
Dick	$P \qquad R$	Q
Harry	$P \sim Q \sim R$	

Here we see no argument by analogy at all; it is merely the hypothesis that Dick possesses the property Q.

This discussion, then, shows conclusively that the analogy from Tom to Dick is either a generalization or *ad hoc*.

IV

In the literature, the label 'analogy' is usually employed when the names 'Tom', 'Dick', etc. are universal names. This does not alter the above discussion; it merely presents analogy as a higher-level generalization which, being based on lower-level ones, cannot be more reliable, or less open to objection, than the lower-level ones[4].

Also, in the literature, more often than not, the word 'analogy' is used when Dick or his possession of the property Q is unobservable (rather than not yet observed). Examples are Ampère's (alleged) analogy from the existence of currents in electromagnets to the existence of currents in magnets proper, or the (alleged) analogies concerning the elastic properties of the luminous ether. Here, again, the previous discussion remains applicable, and unobservability surely does not add to the reliability of a mode of argument or detract from the objections raised against it.

A generalization is often claimed to be more reliable when certain conditions are met to a higher degree. The conditions stated differ from author to author. Yet on the whole I have the feeling that three conditions are traditional: (*a*) that the sample should be random, (*b*) that it be (relatively) large, (*c*) that the ensemble or population be characterized by a stringent set of properties. Quite possibly when condition (*b*) is met we tend to speak more of a generalization, and when condition (*c*) is met we tend to speak more of an analogy (we can never know that condition (*a*) has been met). But this is an intuitive matter, and one can construct examples which intuitively go either way. In any case the form of a generalization which meets condition (*b*) more readily than (*c*) is the same as the form of one which goes the other way. Hence one cannot ascribe more reliability to the one or the other, and objections validly applicable to one are equally validly applicable to the other.

As both Hesse ([2] p. 25) and Harre ([1] p. 26) have noticed, statements like 'atoms are round and hard like billiard balls' are, strictly speaking, not analogies: they are metaphors, to use these authors' terminology. Metaphors are easy to construct to elucidate any theory, but they go no further. Statements like 'nerves conduct electricity

[4] This invalidates the following contention of Harré ([1] p. 27): '(Here, by the way, is a part, and a vital part, of science to which the pure Popper rejection account does not apply; for complete and final verification is possible, at each descending order of mechanism.)' By 'descending order' Harré means order of increasing depth, which seems to be the same as level of generalization, though I cannot be sure. By 'mechanism' he means analogy which is later verified.

like telegraphs' are analogies proper: nerves and telegraphs both transmit information and, we claim, both transmit electricity. But the analogy is *ad hoc*: no one dreams of applying it to any other information channel. The analogy perhaps was fruitful in suggesting a theory—nerves transmit electricity—but we then consider the theory on its own merit, ignoring the analogy altogether (unless as teachers we wish to employ it as a metaphor or as historians of science we wish to investigate whether it was originally the trigger of a new idea.) Analogies which are non-*ad hoc* are easy to construct but one scarcely finds them in science text-books; usually they are stated as generalizations proper. An author, if he is concerned with the inductive mode of presentation, may, for instance, explain that element x whose atomic number is not a whole number has isotopes and he may then suggest, by analogy, that element y, whose atomic number is not a whole number, also has isotopes. In this case the author has in mind a modern variant of Prout's hypothesis, and he will soon state it. Most authors, however, prefer to approach the same hypotheses with protons and neutrons, and come to the table of chemical elements later on, in the proper deductive order.

V

Hesse seems to suggest that analogies are significant in science because they enable us to explain the meanings of abstract terms and theories, even though this necessarily makes the analogy (from the more concrete to the more abstract) imperfect. In this case analogies are admittedly *ad hoc* analogies, so that one can hardly see the difference between analogy and metaphor (or analogy and 'dead metaphor' to quote the author). Harré's view is more traditional: confirming analogies is, in his view, 'a characteristic move in the advanced sciences' ([1] p. 25). He gives an example of a hypothetical protective 'skin' which prevents aluminium from corrosion, which has actually been peeled off pieces of aluminum ([1] p. 26). I fail to see any analogy here at all; there is here a metaphor and one which is completely redundant, as we can speak of a thin layer of oxydized aluminium instead of a skin since nobody has claimed that just as the human body is protected from corrosion and has a thin layer so is aluminium. There was a problem: as aluminium is combustible, what prevents aluminium from bursting into flames like sodium or kalium? And the answer was that it is protected by a thin layer. The analogy, incidentally, between sodium and aluminium, as all analogies concerning the table of chemical elements, is a proper analogy, and not *ad hoc* at all—it is a generalization.

To conclude, I have contended here that arguments from existing knowledge to single predictions, or from one known case to another by analogy, are either *ad hoc* or fallacious; but I have argued neither against generalizations proper nor against analogies proper. I have merely contended that their logic is the same and that hence they are equally reliable or unreliable. For my own part, I think that all non-*ad hoc* hypotheses are welcome, be they generalizations or not. I am not concerned with reliability, nor do I object to either generalization or analogy, but rather to the *ad hoc* character of some analogies and to their being advocated as the (allegedly) reliable methods of science.

APPENDIX

Consider a set of statements d_1, d_2, d_3 ... of all possible occurances of the properties 'white' and 'non-white' in a finite population ('state-descriptions'), such that,

for every i $p(d_i) \neq 0$;
for every different i and j $p(d_i d_j) = 0$;

$$\textstyle\sum_i p(d_i) = 1.$$

For any statement a, obviously,

$$p(a) = p(a\, U_i d_i) = \textstyle\sum_i p(ad_i) = \sum_i p(a, d_i)p(d_i).$$

If a is the assertion that the next swan to be observed is white and the population described by the various d_i — s is of swans, then the above formula provides the disjunction of conjunctions discussed in the text above. (The element of luck in finding a to be true is $1 - p(a)$.)

The contention in the text above is

$$p(a) = p(a, b),$$

where a is the prediction about the next swan to be observed and b is an observation-report. It can easily be proved if the d_i — s refer to the population of unobserved swans. The inductive contention is that by considering the d_i — s as referring to the whole population of swans—observed and unobserved, we shall obtain a different result, because in such a case b is incompatible with some d_i — s and entailed by others. Let us work this out slowly. When the d_i — s refer to the whole swan population,

$$p(a, b) = p(aU_i d_i, b) = \textstyle\sum_i p(ad_i, b) = \sum_i p(a, d_i b)p(d_i, b).$$

Now, when d_i is inconsistent with b,

$$p(d_i, b) = 0$$

and hence,

$$p(a, d_i b)p(d_i, b) = p(a, d_i)p(d_i, b);$$

when d_i entails b the same holds since in this case

$$p(a, d_i b) = p(a, d_i).$$

Hence,

$$p(a, b) = \textstyle\sum_i p(a, d_i)p(d_i, b)$$

whereas

$$p(a) = \textstyle\sum_i p(a, d_i)p(d_i).$$

Assume now that all d_i — s are equiprobable, so that the *(apriori)* probability of whiteness is 1/2; we can easily see that $p(d_i, b)$ equals twice $p(d_i)$ for one half of the d_i — s and zero for the rest, so that

$$\textstyle\sum_i p(a, d_i)p(d_i, b) = \sum_i p(a, d_i)p(d_i),$$

and hence,

$$p(a, b) = p(a).$$

This case has been extensively discussed by Carnap who, on the basis of the contention that inductive learning is possible, has rejected the hypothesis of equiprobability of the d_i — s.

The same proof can very easily be generalized to the case of the probability of whiteness being $1/n$ where there exist n alternative possible colours. Obviously the only way round the difficulty is to deny that the d_i — s are equiprobable, and this will render cases in which $p(a) \neq p(a, b)$. It would be a hypothesis, however, which may be empirically refuted. We can postulate, to take an extreme but easy instance, that all swans are of the same colour, where there are n possible colours and where $p(d_i) = 1/n \vee 0$; in this case, $p(a) = 1/n$ but $p(a, b) = 0 \vee 1$ in all cases of b being an observation-report consistent with our postulate. In this case, then, $p(a) \neq p(a, b)$ as desired. But the postulate may be refuted by many observation reports, such as the report of having observed one white and one green swan. This is the gist of Popper's criticism of Carnap's *Continuum of Inductive Methods* (a 'continuum' of possibilities of ascribing weights to the d_i — s) in the paper referred to in note 2 above: the 'continuum' is not of methods but of falsifiable hypotheses.

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