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Ideal Types and Empirical Theories

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How can ideal types be of any help in understanding empirical reality?

The present paper is addressed to this question. In the first two sections I shall explain why it needs answering and clear away some of the basic confusions which have bedevilled discussion of this issue. In the following two sections I shall criticise two of the standard approaches to the subject. Finally I suggest an answer.

1 I shall take it that a distinguishing characteristic of ideal type concepts is that they have no instances. That is, ideal type concepts differ from other descriptive concepts in that there are no actual situations, entities, events, phenomena, or whatever, which satisfy them. (In general, however, there will be cases which in some intuitive sense 'approximate' to any ideal type concept—this will be of some significance later.)

Examples of ideal type concepts are thus the sociologist's notion of a 'pure rational bureaucracy', the economist's idea of a 'perfectly competitive market' or the physicist's idea of a 'perfect (terrestrial) vacuum'.

The reason why the use of ideal type concepts requires some explanation or justification is obvious enough: unlike other scientific concepts, ideal type concepts cannot be used in the formulation of testable generalisations whose acceptance enables the explanation and prediction of actually occurring phenomena. For suppose we accept a generalisation with an ideal type concept as antecedent term specifying its range of application, such as, say, 'All pure rational bureaucracies are maximally efficient at achieving organisational goals', or 'All bodies falling in a perfect vacuum have constant acceleration'. Since ideal types have no instances, accepting such a generalisation will never allow us to infer from the fact that one state of affairs obtained, or obtains, the further fact that something else then had to, or will, happen—we will never actually come across any state of affairs to which we might apply the generalisation. Generalisations with ideal type concepts as consequent terms are obviously no good for explanation or prediction either, since the things they would allow us to explain or predict never happen. (Though there is clearly a distinction between generalisations with ideal type concepts as antecedent terms and

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those with ideal concepts as consequent terms, the use of these two types of generalisations raises similar problems and can be accounted for along similar lines. For convenience, however, I shall restrict my discussion to the former type of generalisation. Henceforth I shall use the expression 'ideal type generalisation' to refer to this kind of generalisation only.)

In response to this *prima facie* argument for the uselessness of ideal type generalisations some writers have sought to defend the adoption of ideal type methods in the *social* sciences at least by denying that the discovery of generalisations for explaining and predicting phenomena is an appropriate aim for the social scientist. In support of this they have referred, variously, to the subjective meaningfulness of social action, to the 'value-ladenness' of social scientific judgments, or simply to the sheer complexity of social phenomena.

I shall not follow this line of argument. As it happens, I do not think that any of the considerations mentioned provides any conclusive reason for supposing a qualitative distinction between the social and natural sciences. But instead of arguing this I shall simply *assume* that the social and natural sciences alike are ultimately aiming at the discovery of factual generalisations, and will try to show that even *within* this assumption a defence can be found for the use of ideal type methods. Indeed, the essence of my arguments will be that the use of ideal type methods is merely a special case of what must be generally acknowledged to be an important aspect of practice in the natural sciences. But first I must clear away some confusions.

2 'The adequacy of a hypothesis does not depend on the realism of its assumptions, but merely on the accuracy of its empirical consequences.' This dictum is strenuously defended by Milton Friedman in his [1953] and is often invoked in defence of ideal type methods (in particular in defence of the use of abstract models in economics). However, there is a basic difficulty involved in knowing what is meant by 'realism of assumptions' here. This phrase seems to have been intended, and understood, in (at least) two different ways. When these are distinguished it becomes clear that Friedman's dictum offers no help to the defender of ideal types.

In one sense, ideal type generalisations have 'unreal assumptions' simply insofar as their antecedent terms fail (by definition) to apply to any actual situations. However, generalisations characterised by such 'unreal assumptions' can scarcely be defended by reference to the 'accuracy' of the empirical predictions they yield, for, as pointed out in the previous section, such generalisations will never yield any empirical predictions, accurate or otherwise.

On the other hand it is possible to understand 'unreal assumptions' as referring, not to the antecedent terms of ideal type generalisations, but to such generalisations *in toto*, and thus to read Friedman's dictum as suggesting that the acceptance of ideal type generalisations might, in spite of their *falsity*, be justified in terms of their deductively implying other generalisations which are themselves capable of direct empirical corroboration. However, it is hard to see how this can help. For what is wrong with ideal type generalisations is *not* that they are false, but that they are *unfalsifiable*. The admitted fact that any attempt to apply their antecedent terms to specific situations will result in false singular statements does not show that ideal type generalisations themselves are false; rather it shows that there is no possibility of their truth or falsity being empirically indicated. Once we are clear that ideal type generalisations are unfalsifiable, rather than false, we can see that there is no real possibility of their being justified by their deductively implying other generalisations whose empirical accuracy might be corroborated. The direct consequences of a generalisation whose conditions of application are never instantiated will in general themselves be generalisations with non-instantiated antecedent terms.

It might be argued nevertheless that ideal type hypotheses do play a part in the derivation of empirically testable generalisations when they are conjoined with other assumptions. Thus consider the following kind of argument:

(1) Ideal gases (those composed of perfectly elastic molecules with zero mass and volume) satisfy Boyle's law.

(2) If the temperature is above and the pressure below certain (specified) limits, hydrogen is an ideal gas.

Therefore

(3) Within those limits of temperature and pressure, hydrogen satisfies Boyle's law.

Here (1) plays a part in deriving (3), in that (3) follows from (1) and (2), but not from (2) alone. However, we cannot simply therefore take the empirical acceptability of (3) to justify the acceptance of the ideal type hypothesis (1). For the assumption (2) is manifestly false. We do not in general consider statements to be responsible for what they imply when conjoined with admitted falsehoods, and consequently do not take the truth of such implications to support them. (There would be no limit on what could be justified if we adopted such a principle.) Clearly something more needs to be said about the status of (2) in the above argument if the acceptability of the argument's conclusion is to count as lending support to (1). One thing that might be done (something like this is suggested by Friedman) is to replace (2) by

(2)′ Within the specified limits of temperature and pressure hydrogen behaves *as if* it were an ideal gas.

Since something 'behaving *as if*' it were an ideal gas presumably requires only that it does what ideal gases are supposed to do (and not its actually being one), there is no longer any bar to (2) being credited as a seriously accepted assumption. However, this ploy of introducing 'as if' premises simply makes the whole line of defence under consideration irrelevant to the justification of ideal type methods. For note that if the sample argument is to go through with (2)′ in place of (2), then (1) must be replaced by

(1)′ Anything which behaves *as if* it were an ideal gas satisfies Boyle's law.

But (1)′ cannot be held to be an ideal type generalisation at all, since there *are* things which behave *as if* they were ideal gases (at least insofar as (2)′ is acceptable). And in any case (1)′ is analytic, and so does not any more play any real part in the derivation of (3), which is now directly derivable from (2)′. Such objections as these can clearly be made to any attempt to justify ideal type methods by reference to 'as if' interpretations.

Before proceeding further it is worth noting that the unfalsifiability that is characteristic of genuine ideal type generalisations need not be due to their being accepted as analytically true. That is, it is not generally the case with ideal type generalisations that the consequent term's being satisfied by some entity is a logically necessary condition for the antecedent term's applying to that entity. For instance, somebody who asserts that 'In pure vacuums bodies fall with constant acceleration' need not have as part of his ultimate criteria for something being a pure vacuum that bodies fall in it with constant acceleration. Yet his assertion will still be untestable, simply for lack of any pure vacuums. The unfalsifiability of ideal type generalisations is not so much an unfalsifiability in principle as an unfalsifiability in practice. (It might be objected here that the acceptance of an ideal type generalisation *does* generally provide a new criterion for applying the terms involved, and thus that ideal type generalisations are indeed unfalsifiable in principle. This objection raises the important question of whether it is possible to distinguish between analytic and synthetic lawlike generalisations. However, this objection and the issues it raises are in no way peculiar to ideal types and so I shall avoid discussing them further here.)

It is perhaps also worth noting that ideal type generalisations are not the same thing as '*ceteris paribus* laws'. The latter are typically stated with non-ideal concepts as antecedent terms, but are rendered unfalsifiable by the addition of a '*ceteris paribus*' clause which can be invoked whenever a negative instance appears. Ideal type generalisations, on the other hand, being unfalsifiable to start with, do not require any such clause to make them so. ('*Ceteris paribus*' laws and ideal type methods do raise similar problems; indeed it seems to me that the usefulness of generalisations with ideal type concepts as *consequent* terms depends precisely on the availability of '*ceteris paribus*' clauses. Unfortunately space restricts me from discussing '*ceteris paribus*' laws any further here.)

3 It might be thought that the utility of ideal types can be accounted for easily enough simply by understanding ideal type concepts as implicitly designating not only those (non-existent) situations that satisfy them exactly, but also as applying to any situations that 'approximate' to the ideal type.

However, this account can be faced with an awkward dilemma. Is it in general specified what degree of approximation is required for something to count as an instance of (what is intended by) the ideal type, or is it not? If it is *not*, then it can be argued that ideal type generalisations are quite vacuous—for in the absence of such specifications it will be quite arbitrary what are to be taken as instances (whether negative or positive) of such generalisations. If, on the other hand, such specification *is* laid down, it is difficult to see why any recourse to ideal type methods is needed at all: why, in such cases, is the generalisation not simply asserted in the way it is meant to be understood? We would then have a generalisation framed in terms of a normal descriptive concept more general in scope than the original ideal type, and all difficulties would disappear. But to take this horn of the dilemma is scarcely a solution, for it simply denies to ideal types just that characteristic which makes them problematic in the first place.

These considerations also show what is wrong with the view that the usefulness of ideal type generalisations lies in their ability to yield predictions and explanations when 'simplifying' assumptions about particular situations are made. Suppose someone 'assumes' that a given medium is a perfect vacuum, and thence predicts that a body falling through it will have constant acceleration. The obvious objection to such a procedure is that his prediction is based on a premise known to be false. Since we do not in general credit such predictions, there must be some constraints on when this procedure can be used. Presumably the justification in a particular case would be that the simplification involved deviates so little from reality that we can rely on the resulting prediction to be approximately true. If this defence is to carry any weight, however, there will have to be definite limits on the extent of simplification allowed and the degree of approximation to be expected in the prediction; and it will have to be accepted that the ideal type generalisation 'holds good' within those limits. But this is just to say that the generalisation involved is not essentially ideal typical after all, but, as above, implicitly an ordinary generalisation framed in non-ideal terms.

4 The most sophisticated defence of ideal type methods is based on a comparison of ideal type concepts and theoretical concepts. According to this view, which can be found in Ernest Nagel's [1963] (and, less centrally, in Carl Hempel's [1965]) the acceptance of an ideal type generalisation is justified only when the generalisation is the limiting case of a more general system of hypotheses, which are capable of, and have received, independent empirical corroboration. For instance, it is argued that Galileo's law of free fall in a perfect vacuum is acceptable because it is the limiting case of a system of generalisations which describes how bodies do fall in actually occurring situations. It is the limiting case in the sense that the system of generalisations in question can be summarised by a simple continuous function which gives the acceleration of a falling body in terms of the density and elasticity of the medium and the shape and speed of the body, and which specifies that the acceleration tends to a constant as the density of the medium tends to zero.

What underlies Nagel's claim that ideal type generalisations are justified in such cases is the notion that terms signifying ideal type concepts are a particular kind of theoretical term. By 'theoretical' term is meant an expression the applicability of which cannot be decided by direct observation, but only by means of inferences based on 'mixed' generalisations linking the theoretical terms to observational terms. In general the utility of generalisations involving theoretical terms is argued to be that they allow a large number of observational generalisations to be summarised in a simple and systematic manner: relatively few theoretical postulates and 'mixed' generalisations will generate as deductive implications a wide range of observational hypotheses. Thus the essence of Nagel's argument is that ideal types, like theoretical terms in general, can play an essential role in frameworks of postulates which allow an economical and systematic statement of what would otherwise be an unmanageable mass of observational generalisations.

However, the assimilation of ideal types to theoretical terms involved here can be shown to be illegitimate. When this is done it will be seen that the Nagel account of ideal types is unsound. What is characteristic of ideal types is not that they can only be applied indirectly—rather it is that they can't be applied at all, for lack of any instances. 'Ideality' and 'theoreticity' are quite distinct: a theoretical term certainly does not need to lack instances ('radioactive', 'monarchy'); and conversely we can conceive of ideal types which are observational, in that if there were any instances it would be possible to recognise them as such by direct sensory inspection ('black tulip'). The clear distinction between ideal types and theoretical terms should not be obscured by the fact that many of the terms used in science are both ideal typical and theoretical.

Once we are clear about this distinction it is possible to see that the Nagel account of ideal types is unsound. What is held to justify the acceptance of theoretical postulates is that, together with mixed generalisations, they can provide economical sets of premises from which many observational generalisations can be deduced as consequences. But it is essential to theoretical postulates making a real contribution to such deductions that the concepts they involve be non-ideal; to put it crudely, they must be about theoretical aspects of actual situations. For, as argued above in section 2, ideal type generalisations, being nowhere applicable, cannot play any real part in the deduction of empirically testable consequences. True, as Hempel and Nagel point out, an ideal type generalisation can be a limiting case of a general function stating the relationship between specified deviations from the ideal type and the phenomena which will occur in such deviating situations. But the ideal type generalisation will not add anything to the empirical content of a statement of such a functional relationship. This is made clear by the fact that the empirical implications would remain the same if we replaced the function in question by another differing only in that it was undefined, or had some singularity, at the limiting (ideal) case. Although it would be unnatural (and probably counter to any principles of inductive logic there might be) to leave the function undefined for the ideal case, or to postulate a singularity there, the possibility does show that the acceptance of ideal type generalisations cannot satisfactorily be justified by supposing them to be *necessary* for economically formulating a system of postulates with observational consequences.

This point can be elaborated by comparing the use of a system of theoretical postulates to make an explanation or a prediction with the analogous use of a function with an ideal type generalisation at the limit. In the former case we reason from established initial conditions, via mixed and theoretical generalisations, to the state of affairs to be predicted or explained. The theoretical generalisations are essential to the validity of this deduction in that without them the deduction would not go through. When in the latter case, on the other hand, we infer from the extent of the deviation of the initial conditions from the ideal type case, what then will (or had to) happen, the inference is sustained solely by what the function says about *that* kind of deviation, independently of whatever it might assert for the ideal type case. Any actual prediction or explanations could here be made equally well even if we dispensed with the ideal type generalisation itself.

It might be doubted whether the dispensability of ideal type generalisations for predicting and explaining is in fact a sufficient reason for discounting their role in the formulation of empirically contentful systems. For systems of theoretical postulates are also in principle dispensable, notwithstanding their practical usefulness in making inferences from one observed fact to another. Craig's theorem shows that for any system of postulates involving theoretical terms there is another with the same empirical content involving observational terms only. However, in the present context this is not to the point. Scientists use theoretical postulates, in spite of their dispensability in principle, because their replacement by empirically equivalent systems of observation generalisations would involve great loss of convenience and simplicity—the observational replacement invoked by Craig's theorem cannot in general be axiomatised by a finite schema. Thus practising scientists have good reason to rely on theoretical postulates in predicting and explaining. This is not so with ideal type generalisations. What I have argued is that ideal type generalisations do not *even in practice* add to the explanatory or predictive power of systems of generalisations. No additional complexity would result from their abandonment, and so there is no question of their practical utility justifying their acceptance.

5 However, it is possible to derive a justification of ideal type methods from a comparison of ideal type and theoretical concepts, if we focus on a different aspect of theoretical procedures from the one considered so far. Facilitating the economical summarising of observable generalisations is not the only role that theoretical methods play in scientific practice. They are also significant in affecting the development of science over time. For often a scientist accepting certain postulates at a theoretical level will have a commitment to these postulates in themselves, over and above any commitment he may have to the empirical generalisations they are taken to imply, a commitment which will be manifested in his seeking to substantiate precisely those observational hypotheses which would lend

support to his favoured theoretical assumptions, and even, if necessary, in his revising 'mixed' generalisations in such a way as to ensure that the observational data do fit in with those assumptions. In general this kind of procedure lends direction to scientific research; moreover it can be further justified in that it provides some guarantee that the observational generalisations that are admitted to the corpus of scientific knowledge will actually be susceptible of economical incorporation into theoretical systems. Imré Lakatos, in his [1970] has given a general characterisation of this aspect of scientific practice. Lakatos argues that the history of science displays a succession of competing 'research programmes', each consisting of a framework of fundamental principles which over time will find expression in a developing sequence of systems of observable regularities. More precisely, a 'research programme' is characterised by a 'negative heuristic' or 'hard core', a set of basic postulates which are, by fiat, accepted as unfalsifiable, together with a 'positive heuristic', which is 'a partially articulated set of suggestions or hints on how to change, develop the "refutable variants" of the research programme' (p. 135).

It seems to me that the most fruitful way of understanding ideal type methods is as another application of the methodology Lakatos describes and advocates. An empirical system might be simple and economic either because it is derivable from a small number of theoretical and mixed generalisations, or because it can be summarised in a simple functional relationship. An ideal type approach to some field amounts to a 'research programme' directing the scientist towards a system with the latter kind of economy. The adoption of an ideal type generalisation can be seen as a special case of the adoption of a 'research programme' as follows: the ideal type generalisation itself is the 'negative heuristic', a basic principle which is itself unfalsifiable, but which together with the 'positive heuristic' generates a series of empirical generalisations. The 'positive heuristic' would be some 'partially articulated suggestions' of the following kind: suggestions about the limits within which situations approximating to the ideal type situation will approximately satisfy the consequent term of the ideal type generalisation; suggestions about the dimensions along which such approximations can fruitfully be differentiated; suggestions about the kinds of generalisations that might relate specific approximations thus differentiated; *etc.* Somebody espousing an ideal type generalisation would be committed to the existence of some simple but unspecified function having as values deviations from the consequent term and amongst its arguments deviations from the factors mentioned in the antecedent term, with the property that the consequent term would be the limit of this function as the deviations from the factors in the antecedent term tended to zero.

The distinction between ideal types and theoretical terms emphasised above does not mean that ideal types cannot play a similar role to theoretical terms in research programmes. For what is at issue here is not the ability of ideal type generalisations to contribute to the empirical content of systems of generalisations considered synchronically, but whether they can play a part in the diachronic development of such systems. Even if, as I have argued, the acceptance of ideal type generalisations can add nothing to the empirical content of systematic frameworks of generalisations, it is still possible that they can be essential in the production of such frameworks. If this is correct, then ideal types should be seen as providing the kind of ladder which may as well be thrown away once it has got one where one wants to go.

The account of ideal types I am advocating differs from that offered by those who assimilate ideal types to theoretical terms particularly in respect of one important consequence. On their view, the acceptance of an ideal type generalisation is unjustified unless it is known to fit into an independently corroborated system as a special case. As Hempel in particular is concerned to point out, this would deny any sound basis to nearly all the ideal typical assertions put forward by social scientists—for in general such assertions are patently not set within the context of a framework of empirically confirmed generalisations. The view adopted here does not require any such methodological intolerance. For it suggests that the acceptance of an ideal type generalisation is not to be justified by showing that it fills an otiose position in some framework of generalisations, but, if at all, by the possibility that it might lead to the elaboration of such a framework.

Of course the argument I have put forward does nothing to show that social scientists will meet anything like the success of their natural counterparts in finding such frameworks of generalisations. But it does imply that, if that is their aim, the adoption of ideal type methods is a sensible way to go about it and nothing to be ashamed of.

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REFERENCES

FRIEDMAN, M. [1953]: 'The Methodology of Positive Economics', in *Essays in Positive Economics*.

HEMPEL, C. [1965]: 'Typological Methods in the Natural and Social Sciences', in *Aspects of Scientific Explanation*.

LAKATOS, I. [1970]: 'Falsification and the Methodology of Scientific Research Programmes', in I. Lakatos and A. Musgrave (*eds.*): *Criticism and the Growth of Knowledge*.

NAGEL, E. [1963]: 'Assumptions in Economic Theory', *American Economic Review, Supplementary Volume*, 53, 1, pp. 211–19.