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DEMONSTRATIVE INDUCTION: ITS SIGNIFICANT ROLE IN THE HISTORY OF PHYSICS*

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It is argued in this paper that the valid argument forms coming under the general heading of Demonstrative Induction have played a highly significant role in the history of theoretical physics. This situation was thoroughly appreciated by several earlier philosophers of science and deserves to be more widely known and understood.

The general feature of the arguments which I shall discuss is that they involve the deduction of an explanans from one of its own explananda.

I shall first give a simple formal example of such an argument. Then I shall give numerous examples from the history of physics. Finally I shall review what the philosophical literature has had to say about such arguments.

The principal argument schema which I propose to consider is one in which a universal generalization is deduced from one of its own particular instances. Of course this deduction involves the use of additional theoretical premises. The important thing about these additional premises is that they must not themselves imply the universal generalization in question and that they be such that, in a realistic situation, we could have more initial confidence in them, than in the universal generalization which we proposed to deduce with their help.

Instead of offering a schema which enables one thus to infer $(x)Fx$ from Fa, I shall offer one which licenses the (seemingly more restrictive) inference to $(x)Fkx$ from Fka, where the constant k can be thought of, if you like, as picking out a particular property F_k from a more general class of properties F. I do things in this way merely in order to be able to formulate the additional premises needed wholly within first-order logic, whereas they would otherwise need to be formulated within the less familiar second-order logic.

The additional premises that I propose are two. The first asserts the existence of a universal law of a certain specified form; its formal version reads: $(\exists n)(x)Fnx$. That is, it specifies the law in question up to an undetermined parameter n. (This might seem a very specific kind of restriction: but in fact virtually any restriction on the form of the law can be expressed formally in this way.) The second additional premise asserts that there cannot be more than one value of the parameter n for any given value of the argument x, and reads formally: $(x)(m)(n)(Fmx \,\&\, Fnx \rightarrow m = n)$. I shall call this condition the uniqueness condition and the first condition the existence condition. In some applications the uniqueness condition will be fairly trivially satisfied. In all applications F will be such as to have the

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effect that the variable x ranges over the domain over which we wish to generalize, e.g. pairs of bodies, sets of bodies, distances, etc.

Here is a formal proof of the validity of the argument schema in question.

1. Fka	Premise.
2. $(\exists n)(x)Fnx$	Premise.
3. $(x)(m)(n)(Fmx \;\&\; Fnx \rightarrow m = n)$	Premise.
4. $(x)Fjx$	(2), existential instantiation.
5. Fja	(4), universal instantiation.
6. $Fka \;\&\; Fja$	(1), (5), *and*-addition.
7. $Fka \;\&\; Fja \rightarrow k = j$	(3), three universal instantiations.
8. $k = j$	(6), (7), modus ponens.
9. $(x)Fkx$	(4), (8), substitutivity of identity.

Q.E.D.

This is a valid argument in first-order predicate logic with identity. Let me illustrate it with some examples from the history of physics.

Passing over Newton's deduction of the gravitational inverse square law (which is certainly not a simple and straightforward instance of the argument I have in mind; its detailed logical structure is too complex to analyze here), the next fundamental law to be introduced into physics was the electrostatic inverse square law. Now although the electrostatic and magnetostatic inverse square laws were both arrived at by Coulomb [4] in 1785 on the basis of what appeared to be rather direct inductive generalizations from experiments, the electrostatic law had in fact been arrived at already by Cavendish in 1773 (though not published at the time) by what Maxwell was later to claim as a very much more accurate method, and one which relies on the result of only a single experiment.[1] However, while the Cavendish argument is an historically important example of the use of demonstrative induction, it is nevertheless, philosophically, a comparatively uninteresting example because the existence condition on which Cavendish relied is not such that one could reasonably have had appreciably more confidence in it than in the universal generalization which was to be deduced with its help. At least that is my understanding of the theoretical situation at the time Cavendish was working. A full historical and philosophical analysis of this example is given in [10].

[1] The details of Cavendish's experiment and of the argument by which the electrostatic inverse square law may be inferred from its result can be studied in Cavendish's own version in Maxwell's [25] and in Maxwell's version in the section entitled "On the proof of the law of the inverse square" in Maxwell's [23]. I have myself examined this argument closely, [10], and am satisfied that its fundamental step is indeed in accordance with the demonstrative induction schema given above, the existence condition being Cavendish's assumption of some inverse power law, and the uniqueness condition amounting to the assumption that the forces in question depend *only* on the distance. These are of course not the only theoretical assumptions Cavendish needed to infer from the result of his experiment with two concentric globes that the force between any two small portions of the electric fluid varied as the inverse 2 ± 0.02th power of their distance apart. But it is they which enable Cavendish validly to deduce the law in question from one of its own particular instances.

Surprisingly enough one finds implicit in Coulomb's method of reaching the electrostatic inverse square law [4] another demonstrative induction with better philosophical credentials. We recall that Coulomb experimented with a particular pair of small charged wooden balls and inferred from observations on the force between them at a comparatively small number of distances (three), a law for the force between them at all distances. However, this comparatively low-level generalization about the behavior of a particular pair of balls of elder wood in a particular experimental situation is obviously itself only a very special case of the electrostatic inverse square law. How did Coulomb infer the full electrostatic inverse square law from this special case? It seems to me that the simplest rational reconstruction of his procedure is not to suppose that he took that law as an hypothesis, or to suppose that he arrived at it by a wild inductive generalization from a single instance, but to suppose that he was already committed to a theoretical assumption to the effect that there existed some force law giving the force between charges as a function of their quantities and their distances apart, which law was to be valid for all pairs of charges in all relative states of position. Since the uniqueness condition corresponding to this existence condition is comparatively trivial (Coulomb would hardly entertain the possibility of two different distance dependences for the force between his charged balls) he has enough theoretical assumptions to which he is already committed to permit him to *deduce* the full electrostatic inverse square law from the low-level generalization which he inferred from his experimental results. The argument is a special case of the demonstrative induction schema I drew up originally, the predicate F in this case itself containing a variable bound by a universal quantifier (namely one ranging over all distances) while the universal generalization licensed by the demonstrative induction schema is the further one over all pairs of charged bodies. Here, as is often the case, the statement of the phenomena from which the deduction starts is itself a low-level generalization (although I have been at pains to emphasize that this is not a logical requirement): the argument of philosophical interest is that by which further universal quantifiers are added. This particular example seems to be a realistic example of one where the scientist in question evidently would have had more initial confidence in the theoretical premises needed to mediate the deduction, than in the particular universal generalization which could be deduced with their help.

The next celebrated example of a demonstrative induction from the phenomena is Ampère's *Théorie Mathématique des Phénomènes Électrodynamiques Uniquement Déduite de l'Expérience*, [1], in which Ampère's celebrated (and, according to our modern views, incorrect) formula for the force between two current elements is allegedly deduced from the results of four specific experiments. I have sketched the essential structure of the argument in the diagram opposite.

The basic deduction seems to me cogent provided one allows that Ampère's starting point is low-level experimental generalizations rather than actual experimental results and provided one grants him the additional crucial extra-experimental assumptions which he quite openly requires for his deduction: in particular the assumption that the force between the current elements is along the

THÉORIE MATHÉMATIQUE DES PHÉNOMÈNES ÉLECTRO-DYNAMIQUES UNIQUEMENT DÉDUITE DE L'ÉXPÉRIENCE

A.-M. Ampère 1827.

A rational reconstruction of Whittaker's rational reconstruction of Ampère's deduction

(The English quotations are from Whittaker [27], p. 85. Notice that what he describes as Ampère's experimental results are really low level generalizations from them. Unbroken arrows signify *deductive* inferences, broken arrows hypothetico-deductive inferences or inductive inferences according to your philosophical fancy.)

Force Law | *Ampère's Experiments*

1. **dF** = **dF**(i, i', **ds**, **ds′**, **r**) ← Under the conditions of Ampère's experiments the force depends (perhaps only *inter alia*) on these variables. ← - - - specific experiments unnecessary

← "Expt. 1: The effect of a current is reversed when the direction of the current is reversed" ← - - - - - - - - - - actual Expt. 1

← proportionality to i by definition, to i' by equality of action and reaction ←

2. **dF** = ii'**dF**(**ds**, **ds′**, **r**)

← force is along line joining current elements ← Newtonian mechanics ← - - - all physical experience; All forces reduce to inter-particle forces ← - - - - "*rien ne s'oppose*" (central dogma)

3. **dF** = $ii'\hat{\mathbf{r}}df$(**ds**, **ds′**, **r**)

← invariance under translations and rotations ← - - - - - - - - common experience

← invariance under reflections ← - - - - - - - - - - - - - - - - - - ?

4. **dF** = $ii'\hat{\mathbf{r}}df$(**ds**.**ds**, **ds′**.**ds′**, **ds**.**ds′**, **r**.**r**, **ds**.**r**, **ds′**.**r**)

← linearity and homogeneity in **ds** and **ds′** ← "Expt. 2: The effect of a current flowing in a circuit twisted into small sinuosities is the same as if the circuit were smoothed out" ← - - - - - actual Expt. 2

← equality of action and reaction ← Newtonian mechanics ← - - all physical experience

5. **dF** = $ii'\hat{\mathbf{r}}(A(r)(\mathbf{ds}.\mathbf{ds'}) + B(r)(\mathbf{ds}.\mathbf{r})(\mathbf{ds'}.\mathbf{r}))$ ← All forces reduce to inter-particle forces ← - - - "*rien ne s'oppose*"

← **dF** proportional to $1/r^n$ ← - ?

6. **dF** = $\frac{ii'\hat{\mathbf{r}}}{r^n}(a.(\mathbf{ds}.\mathbf{ds'}) + b.(\mathbf{ds}.\hat{\mathbf{r}})(\mathbf{ds'}.\hat{\mathbf{r}}))$

← "Expt. 4: The force between two elements of currents is unaffected when all linear dimensions are increased proportionately, the current strengths remaining unaltered" ← - - actual Expt. 4

7. **dF** = $\frac{ii'\hat{\mathbf{r}}}{r^2}(a.(\mathbf{ds}.\mathbf{ds'}) + b.(\mathbf{ds}.\hat{\mathbf{r}})(\mathbf{ds'}.\hat{\mathbf{r}}))$

← "Expt. 3: The force exerted by a closed circuit on an element of another circuit is at right-angles to the latter" ← - - actual Expt. 3

8. **dF** = $\frac{kii'\hat{\mathbf{r}}}{r^2}(2(\mathbf{ds}.\mathbf{ds'}) - 3(\mathbf{ds}.\hat{\mathbf{r}})(\mathbf{ds'}.\hat{\mathbf{r}}))$

← The sign of k is $-$ve if two parallel currents attract; Set the magnitude of $k = 1$, by an appropriate defn. of current strength ← additional unspecified experiment

9. **dF** = $\frac{ii'\hat{\mathbf{r}}}{r^2}(3(\mathbf{ds}.\hat{\mathbf{r}})(\mathbf{ds'}.\hat{\mathbf{r}}) - 2(\mathbf{ds}.\mathbf{ds'}))$

(**dF** is the force exerted by circuit element **ds** (current strength i) on circuit element **ds′** (current strength i', relative position **r**)).

A TYPICAL CASE OF A DEDUCTIVE JUSTIFICATION OF A NEW FUNDAMENTAL HYPOTHESIS line joining them and satisfies the law of equality of action and reaction.[2] Ampère's argument is neatly presented in Whittaker, [27]; I have followed Whittaker's modern notation and have taken the liberty of filling in a few minor lacunae in both Whittaker's and Ampère's presentations of the argument. Ampère's weakest assumption is his appeal to an inverse power law. Here I have indicated Whittaker's suggested alternative route, which relies on a rather broad low-level generalization of one of Ampère's experimental results. This alternative route is essentially equivalent to Laplace's argument for the inverse square law here on dimensional grounds.

The steps in this deduction which are essentially cases of the inference scheme I have already described are those from formula 6 to formula 7 and from formula 7 to formula 8. In the course of these steps implicit existential quantifiers governing the variable n and the ratio of the constants a and b are eliminated in just the manner one expects from the scheme I gave earlier. The half dozen other crucial inferential steps seem to be essentially of the form:

$$\begin{array}{l} (\exists n)(x)(Fnx \,\&\, Gx) \\ \Big\downarrow \longleftarrow (x)Fkx \\ (x)(Fkx \,\&\, Gx). \end{array}$$

This inference scheme may seem even more trivial than the earlier one but it shares the feature that the conclusion can function as the explanans of a minor premise required in its own deduction.

The deductive structure in the diagram possesses the important feature that every one of the formulae on the left is itself rededucible from its immediate successor by existential generalization. It is this feature which allows us to treat Ampère's whole theory as included in formula 9 itself, and which might encourage the naïve hypothetico-deductivist to treat this as Ampère's hypothesis and to ignore the deductive steps which led to it. However such a construction of Ampère's theory would lead to the mistaken inference that any experiments which later threw doubt on Ampère's formula merely called into question a single rather arbitrary-looking hypothetical force formula, whereas in fact, had such an experimental refutation been devisable, it would have called in question some of the most fundamental assumptions of classical physics. Indeed it is only by taking crucial account of the relativity of simultaneity, that we are able today to understand how Ampère's deduction went astray and failed to yield the force formula which we now accept, namely Grassman's, which was rightly never taken seriously

[2] These assumptions were criticized by Ampère's contemporaries because they ruled out torques. But the formula which we accept today (and which follows in the appropriate approximation from relativistic electron theory), namely Grassmann's formula,

$$dF = \frac{ii'}{r^2}((ds'.r)ds - (ds.ds')r),$$

also rules out torques. And our explanation of the absence of torques is the same as Ampère's, namely that one doesn't get torques if the force is reducible to the forces between the (point) particles assumed to make up the (infinitesimal) current elements.

by Grassman's contemporaries. (Since modern electron theory still requires that the forces between relatively moving particles are, in a certain sense, along the lines joining them (we can ignore the acceleration forces in the case of steady currents), it is at first surprising that we fail to recover Ampère's result. However, the paradox disappears when we take explicit account of the several frames of reference involved.)

The virtues in the physical sciences of demonstrative inductions of this kind, as against simple-minded induction or hypothetico-deduction, were well appreciated in the philosophical writings of at least one of Ampère's contemporaries. Thus John Herschel wrote in sections 210 and 211 of his *Discourse on the Study of Natural Philosophy* [17]:

> We have next to consider the laws which regulate the action of these our primary agents; and these we can only arrive at in three ways: 1st, by inductive reasoning...; 2ndly, by forming at once a bold hypothesis...; or, 3rdly, by a process partaking of both these, and combining the advantages of both without their defects, viz. by assuming indeed the laws we would discover, but so generally expressed, that they shall include an unlimited variety of particular laws; following out the consequences of this assumption, by the application of such general principles as the case admits;—comparing them in succession with all the particular cases within our knowledge; and, lastly, *on this comparison*, so modifying and restricting the general enunciation of our laws as to *make the results agree*.
>
> All these three processes for the discovery of those general elementary laws on which the higher theories are grounded are applicable with different advantage in different circumstances. We might exemplify their successive application to the case of gravitation; but as this would rather lead into a disquisition too particular for the objects of this discourse, and carry us too much into the domain of technical mathematics, we shall content ourselves with remarking, that the method last mentioned is that which mathematicians (especially such as have a considerable command of those general modes of representing and reasoning on quantity, which constitute the higher analysis), find the most universally applicable, and the most efficacious; and that it is applicable with especial advantage in cases where subordinate inductions of the kind described in the last section have already led to laws of a certain generality admitting of mathematical expression.

Herschel was of course better placed to understand the actual reasoning of men of science than have been many of his twentieth century successors in the philosophy of scientific method.

The phenomenal premise on which a demonstrative induction rests may of course itself be quite a high-level generalization; indeed it may well be itself the conclusion of an earlier piece of demonstrative induction, such for example as Ampère's force formula. This actually happened in this case when Wilhelm Weber in 1848 [26] took Ampère's force formula as the starting point for his own demonstrative induction to a formula for the force between two charges in arbitrary relative motion.

Weber assumed that currents consist of positive and negative moving charges and that Ampère's forces between current elements must therefore be reducible to the forces between charges. Weber himself believed that the positive and negative charges move in a current with equal and opposite velocities; however his deduction goes through without this restrictive assumption provided it is assumed merely that the quantity of positive electricity in each current element is equal to the quantity of negative electricity (cf. Maxwell, [23]). Weber assumed that the force between charges reduced to the Coulomb force only when the first and higher time-derivatives of the distance between the charges vanished, but that in general it would be also a function of these higher time-derivatives of the distance between the charges. The formula he eventually arrives at is

$$\mathbf{F} = \frac{ee'\hat{r}}{r^2}\left(1 + \frac{rd^2r}{c^2dt^2} - \frac{1}{2c^2}\left(\frac{dr}{dt}\right)^2\right),$$

where e and e' are the charges, r is the distance between them, and c is a constant of the dimensions of a velocity, and which Weber evaluated experimentally with Kohlrausch and showed to have the same order of magnitude as the velocity of light. The distance derivatives in the formula are not to be confused with relative velocities and relative accelerations, but are simply time-derivatives of the scalar distance between the charges, and are thus invariant under transformation to, for example, a uniformly rotating frame of reference. (Weber did not say why he imposed this requirement, though he did know that an alternative formula due to Gauss which involved the ordinary relative velocity was inconsistent with energy conservation.) In deducing this formula from Ampère's formula, Weber does not make it as clear as he might what general assumptions are needed in order *rigorously* to obtain the uniqueness that he claims for his formula. However it turns out on investigation that it is sufficient that he assume that *fourth* and higher derivatives of the distance can be neglected; he need make no restrictive assumptions on the form of the functional dependence on r and the other derivatives; his formula is then uniquely deducible from the requirement that it yield Coulomb's law for stationary charges and Ampère's result for the forces between current elements.

The importance of Weber's formula is not that we can still accept it—we cannot; but that its experimental refutation would have called in question either the quite plausible assumptions on which Weber's deduction of it rests, or Ampère's formula and the assumptions on which that rests. As things turned out Weber's formula was empirically brilliantly successful since it proved to be capable of accounting not only for Ampère's results but also for the quite separate phenomenon of electromagnetic induction; however it later came under fierce criticism on peculiarly nonempirical grounds and fell a victim to the power and prestige of its principal opponent, Helmholtz.

Maxwell motivated his own initial attempts to construct a rival electromagnetic theory by appeal to the alleged mechanical difficulties of Weber's theory. His own procedure in these early papers vacillated between the frankly speculative and the claim that he was merely studying mechanical analogies rather than proposing physical hypotheses. However, in his later papers ([21], [23]) his claim is that he has deduced his own equations from admitted facts by applying the laws of general mechanics with the help of the sole additional premise that the energy resides in the field. His method here evidently purports to be that of demonstrative induction but it can hardly be considered a felicitous application of that method, given the succession of dubious and mutually inconsistent arguments to which Maxwell had to appeal in order to motivate his key innovation, the introduction of the displacement current term. A few years later, [24], Maxwell endeavored to apply the same technique of demonstrative induction to the kinetic theory, and offered an explicit derivation of its fundamental hypotheses from the experimental facts and the fundamental equations of analytical dynamics, claiming that, "When examples of this method of physical speculation have been properly set forth and explained, we shall hear fewer complaints of the looseness of the reasoning of men of science, and the method of inductive philosophy will no longer be derided as mere guess-work" ([24], p. 357). While Maxwell's detailed argument is again not free from blemishes (I have discussed it at length elsewhere [7]), the attempt was of the greatest methodological interest and led to that process of mathematical consolidation of the foundations of the kinetic theory, which enabled physicists, by the end of the century, to interpret the well-known experimental discrepancies, not merely as a refutation of some particular hypothetical kinetic model, but rather as casting doubt on the most general equations of classical dynamics.

The quantum theory began with Planck's theoretical contribution, which, as his contemporaries were quick to point out, is merely a case of invalid deduction from a wholly classical starting point. Nevertheless, Einstein's 1905 argument for the existence of photons does appear to take the form of a demonstrative induction, with the Wien limit of Planck's empirical black-body radiation law playing the role of the phenomena and thermodynamics and Boltzmann's relation, between entropy and probability functioning as the general theoretical constraints. The argument from these premises is deductive apart from one crucial step which Einstein himself seems to have regarded as speculative. I have shown in another paper [8], that this step, too, can be rendered strictly deductive.

The key argument of Einstein's special relativity paper of the same year [11], namely the derivation of the Lorentz transformation equations, also takes the form of a demonstrative induction. The principle of the constancy of the velocity of light functions as phenomenal premise in the deduction of a conclusion which exceeds it in generality by the addition of at least three universal quantifiers, namely the generalization to all processes that propagate at the velocity c, to all velocities, and to all kinematical quantities and not just to velocities. The existence and uniqueness conditions licensing this deduction include the principle of relativity itself and the requirement of linearity of the transformation equations. Contrary to the impression given in some accounts, it is the conclusion of this demonstrative induction, namely the Lorentz transformation equations, which must be taken as the explanans, and the constancy of the velocity of light, together with the other special relativistic effects, as the explananda. This is clear from the evidently greater generality of the former.

The next important advance in theoretical physics was Bohr's old quantum theory. Bohr's 1913 arguments were not very satisfactory, nor were the different justifications he then offered for his quantization condition entirely consistent with one another (cf. Heilbron and Kuhn [15]). However, the best of them do appear to take the form of demonstrative inductions. Thus, assuming that a spectral line is emitted as a result of a transition between two stationary states, and that the energy difference between those states is equal to Planck's constant times the emitted frequency, Bohr is able to infer an expression for the energies of the stationary states by arguing backwards from the Balmer formula for the emitted frequencies. Bohr is then able to infer the value of an undetermined constant in his expression, by considering a single special case, namely the classical limiting case, and he hence arrives at a theoretical expression for the empirical Rydberg constant in the Balmer formula. Here Bohr has deduced the precise form of his explanans, the formula for the energy levels, from two of its own explananda.

While later advances in the quantum theory seem to owe most to an explicitly hypothetico-deductive methodology, demonstrative induction continues to play a role, especially in applications of the correspondence principle. The existence of stimulated emission, as postulated in Einstein's 1917 paper "On the Quantum Theory of Radiation," [13], not only provides an explanation of one of the crucial features of Planck's radiation law, but can also be seen as required by the form of that law, when compared with that of Wien. Alternatively it can be construed as deduced from the correspondence principle requirement that the density of radiation increase indefinitely with the temperature. Both these arguments could be reconstructed as demonstrative inductions, although that is not quite the form in which Einstein presented them. In the same paper Einstein claimed that the existence of a momentum transfer, equal to h/λ, when a molecule emits or absorbs a quantum of radiation of energy equal to $h\nu$, is required if the interaction with the radiation field is to continue to allow an equilibrium distribution of molecular velocities. If this argument were reconstructible in a satisfactorily rigorous form, it too would have to take the form of a demonstrative induction. Again, when Heisenberg, [16], in 1930, reconstructed his 1925–1926 route to the quantum theory, he spoke of the "deduction of the fundamental equations of the new quantum mechanics," and he takes the premises for this deduction to consist of empirical facts together with the correspondence principle. While the correspondence principle can hardly be the only theoretical premise required, Heisenberg is wrong to say of his argument, that "this cannot be a deduction in the mathematical sense of the word, since the equations to be obtained form themselves the postulates of the theory" ([16], p. 105 ff.).

An even clearer case of a deduction of the detailed form of a new fundamental equation from very general theoretical requirements, together with what purports to be a particular special case required for conformity with the empirical evidence, occurred in Dirac's 1928 introduction of the relativistic wave equation for the electron ([5] and cf. [6]). Dirac imposes as general theoretical requirements the condition that the equation remain relativistically invariant when electromagnetic terms are included and the condition that it be linear in the operator $\partial/\partial t$, which latter condition he takes to be required for conformity with the general principles of quantum mechanics. These requirements uniquely determine the general form of the new equation. Dirac then evaluates the as yet undetermined coefficients in the equation, by requiring that the equation imply the earlier Klein–Gordon equation, which he takes to be a necessary condition that his new equation continue to yield the classical relativistic equation for a charged particle in the limit of large quantum numbers. Clearly the form of Dirac's general argument here is that of a typical case of demonstrative induction.

As a final example we may consider the arguments by which Einstein, [12], [14], justifies his choice of the fundamental field equations of the general theory of relativity. Again it is a case of carefully motivating sufficient theoretical requirements on the general form that the new equations must take, for their specific form then to be determined uniquely, modulo the fixing of certain coefficients by appeal to particular consequences that these equations are required to yield in order to conform with the known empirical facts. Einstein's general theoretical requirements included the local validity of special relativity, the incorporation of gravitation into the geometrical structure of space-time, and the requirement that the gravitational field should arise from the density of ponderable matter in a manner analogous to that required by Poisson's equation in the Newtonian theory. (Einstein also thought that he had, in some nontrivial sense, imposed the requirement of a general relativity of motion; indeed at one point in his 1916 paper he spoke as if this were *the* requirement from which his equations proceeded "by the method of pure mathematics:" here he was mistaken.) Given his theoretical requirements Einstein then argued that his field equations were uniquely determined, provided we chose the value of a certain coefficient equal to $\frac{1}{2}$, by imposing the condition that the field equations yielded energy momentum conservation as a consequence, and we evaluated the remaining constant, k, by considering the classical limiting case. In these final steps, as well as in several of the earlier steps, the equations which are to play the role of the explanans, are mathematically deduced from more general theoretical preconditions only by essential appeal to particular cases of their own explananda, and, as in all other cases of demonstrative induction, the final conclusion is formally taken to explain all, or nearly all, the premises which led to it.

If I am right as to the significant role that arguments taking the form of demonstrative inductions have thus played in the history of theoretical physics, it is remarkable that philosophers of science have not generally attached much significance to this argument form. This has not always been so, as my quotation from Herschel indicated, and in his recent article, "Henry Brougham and the Scottish Methodological Tradition" [3], G. N. Cantor has drawn attention to some earlier philosophical defenses of this particular tradition in scientific method. However, Mill appears to make no mention of this form of argument in his *Logic*, nor, as far as I know, is it given any emphasis by Whewell.

Preoccupation with the problem of justifying induction in general has no doubt been largely responsible for this neglect. For it is clear that demonstrative induction has little or no bearing on this problem, for it merely shifts the burden of inductive justification onto the general premises which are required for the validity of its argument forms. However this does not mean that it is not of considerable significance and importance in actual scientific reasoning. While Herschel may go too far in claiming that arguments by demonstrative induction not only combine the advantages, but also lack the defects, of the simpler forms of induction and hypothetico-deduction, it may nevertheless be true, that if one wishes to attain to informative specific hypotheses without surrendering altogether the demands of reasonable scientific caution, then demonstrative induction is, in the words of one of the ablest of past philosophers of scientific method, "the best way of arguing which the Nature of Things admits of." There are also two significant specific roles which it can play when really revolutionary scientific developments are at issue. First, it seems to provide the only reliable way of generating severe experimental tests for the most general assumptions of a science at a particular time. Second, when these most general assumptions have been shown collectively to be irreconcilable with certain newly established experimental facts, the surest way of attaining to new fundamental hypotheses, which can enjoy some credence, is to deduce them from the new experimental facts, with the help of those earlier general assumptions which seem to have been least directly impugned.

Although, as my examples have illustrated, Einstein himself made frequent use of this method, there has been almost complete silence about the method of demonstrative induction in the writings of twentieth century philosophers of science. It is true that, in the article on Logic in the 1902 edition of the *Encyclopaedia Britannica*, Thomas Case presented an eloquent defense of the method of demonstrative induction, tracing it back, through the medieval distinction between the *progressus a principiis ad principiata* and the *regressus a principiatis ad principia*, to the writings of Aristotle and Alexander the Commentator, emphasizing that "no distinction is more vital in the logic of inference in general, and of scientific inference in particular," and that "the full value of the ancient theory of these processes cannot be appreciated until we recognize that as Aristotle planned them Newton used them." It is also true that W. E. Johnson devoted some space to this form of argument, [18], which he subtitled *Demonstrative Inference: Deductive and Inductive*. His presentation is especially perspicuous and deserves to be quoted:

The Formula of Direct Universalization

Composite premiss: Every S is characterized by some the same determinate under the determinable P.

Instantial Premiss: This S is p.
Conclusion: $\therefore$ Every S is p.

To take a typical illustration from science:

Every specimen of argon has some the same atomic weight.
This specimen of argon has atomic weight 39.9.
$\therefore$ Every specimen of argon has atomic weight 39.9.

In this, as in all such cases of scientific demonstration, the major premiss is established—not *directly*, by mere enumeration of instances—but rather by deductive application of a wider generalization which has been ultimately so established. In the given example it is assumed that *all* the chemical properties of a substance, defined by certain 'test' properties will be the same for all specimens; and this general formula is applied here to the specific substance *argon*, and to the specific property *atomic weight*. The assumption in this case is established by problematic induction, i.e. directly by an accumulation of instances. In practically all experimental work, a single instance is sufficient to establish a universal proposition: when instances are multiplied it is for the purpose of eliminating errors of measurement. It is owing to the fact that the general proposition, functioning as a major or supreme premiss, has the special form of an alternation of universals that, by means of a minor premiss expressing the result of a single observation, we are enabled to establish a universal conclusion. This conclusion, in accordance with our general account of demonstrative induction, is a specification of what is predicated indeterminately in the universal premiss, and a generalization of the proposition recording the result of a single observed instance. ([18], vol. 2, pp. 216-217)[3]

What I have tried to show throughout this essay is not that demonstrative induction provides a royal road to advance in theoretical science, but that, where applicable, it can provide a very significant addition to the simple forms of induction and hypothetico-deduction. An hypothesis is placed at a considerable advantage if it can be shown to be required by the facts provided we assume certain plausible general principles; the method of demonstrative induction can also, in principle, play a significant role in the logic of discovery as well as in the logic of justification. From the point of view of that approach to scientific inference which seeks to construe legitimate scientific inferences as reconstructible in principle as valid subjective probabilistic inferences (cf. [9]), demonstrative inductions are only a rather special case of legitimate scientific inference. But this special case is of sufficient historical importance, and of sufficient theoretical utility for the practicing scientist, to deserve greater emphasis than it has recently received in discussions of scientific method. A proper appreciation of the logical legitimacy of this general form of argument would also promote less superficial reconstructions and appraisals of the intentions and achievements of quite a number of scientists of the past, than those all too current. In fact I know of few advances in theoretical science in which demonstrative inductions have not played either a major, or a minor but significant, role.

[3] Johnson's discussion was taken up by Broad in a couple of articles in [2] and again by H. E. Kyburg in [19]. Broad says that in Johnson's example, "The ultimate major premise is no doubt the proposition that if some sample of a chemical element has a certain atomic weight then all samples of that element will have that weight" ([2], pp. 302, 406), and he thus formalizes it as:

$$(Ex)(\phi x . \psi x) \supset (x)(\phi x \supset \psi x),$$

which seems to me an unnecessary trivialization of Johnson's intentions. Nor does Broad seem aware that demonstrative induction is of wider application in science than merely to arguments from natural kinds. Kyburg's article [19], started out encouragingly with:

> Most of the arguments encountered in scientific literature are supported by reference to (a) general empirical premises, and (b) particular statements of empirical evidence. The argument which proceeds from (a) and (b) to the inductive conclusion is often demonstrative.

But, disappointingly, Kyburg fails to cite examples from theoretical physics, and he ends with:

> The most fruitful of these analyses of demonstrative induction is that provided by Broad of the argument from natural kinds. In advanced sciences this sort of argument is often employed explicitly, and with great plausibility. It is a well-confirmed and often employed generalization, for example, that each chemical element is a natural kind in the sense that there is a large family of properties, in addition to the defining properties of the kind, which are such that if one sample of an element has one of these properties, then all samples of that element have it. Although it is difficult to see any conceivable way in which such an hypothesis could be used in every inductive argument, or even how it could be used in all scientific disciplines, it is clear that many important forms of inductive argument can be reconstructed in this way.

Again, the reader of Kyburg's later discussion of the topic, in his [20], will gain the impression that this form of demonstrative induction is essentially restricted to "arguments in which a key concept is that of a *natural kind*," with examples coming primarily from biology and chemistry, rather than that it, and related argument forms, are, as I argue, of considerable significance in the theoretical development of fundamental science.

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