

Reduction and Realism

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1. Introduction

In his recent book *Foundations of Space-Time Theories* Michael Friedman argues for a realism about theoretical structure based on specific methodological practices concerning theory unification. Theoretical structures that are essential to the unifying process are to be given a literal realistic interpretation while the remaining ones can be considered as having merely representational status. Friedman's account of unification involves the notion of a literal reduction or identification of observational properties of entities or objects with their theoretical counterparts. The relationship between these two levels of theory can be construed as that of submodel to model. Once the appropriate reductions are achieved we are then free to conjoin certain theoretical structures with others thereby enabling us, over time, to produce a unified theory encompassing a variety of domains.

The alternative to this view Friedman describes as the representationalist approach. Instead of characterizing the relationship between observational and theoretical structures as that of submodel to model the observational properties are simply correlated by way of an embedding (as opposed to an identity) map with their appropriate theoretical counterparts. On this view it is not possible to advocate a literal interpretation of theoretical structure and hence theories cannot evolve over time to produce the kind of unified account that explains a variety of different phenomena using relatively few theoretical postulates.

Although Friedman's criticisms of the embedding approach are true of classical instrumentalism they needn't apply in all cases. Below I argue that the need for a literal interpretation of theories does not favor the model-submodel account; in fact, the embedding approach provides a more accurate characterization of scientific practice. Moreover, the strict account of reduction that the model-submodel approach yields can sometimes rule out the kind of literal interpretation it was designed to facilitate.

2. The Friedman Model

Friedman characterizes a typical scientific explanation in the following manner [pp. 236-250]: We postulate a theoretical structure $\alpha = \langle A, R_1 \dots R_n \rangle$ (where A is the domain of individuals and $R_1 \dots R_n$ are physical relations defined on A) possessing certain mathematical properties. We also have an observational substructure $\beta = \langle B, R'_1 \dots R'_m \rangle$ ($m \leq n$). α functions as an explanation or reduction of the properties of β .

Using the kinetic theory we can explain the observable properties of gases characterized by β by embedding them in α where α is literally construed as the world of molecular theory. This enables us to account for the behavior of gases by identifying them with large configurations of molecules that interact according to the laws of Newtonian mechanics. Due to the properties and relations provided by the theoretical structure we can derive laws that govern the behavior of observable objects. By contrast, if we remained strictly on the phenomenological level we would not be able to accurately formulate a law like van der Waals gas law because we would be unable to appeal to the account of intermolecular forces provided by the higher level theoretical structure.

Friedman sees the correct relationship between α and β as that of model to submodel where $A \supseteq B$ and $R'_i = R_{i/B}$ ($i \leq m$). This characterization affords us a literal identification of the elements in α and β which in turn results in the larger structure α "inducing" theoretical properties and relations on objects in β ; properties necessary for stating accurate laws about observable objects (cf. p.240). Contrast this with what Friedman terms the representational account. On this view we do not interpret α literally (as the molecular world), rather, it is construed as a mere representation. Instead of asserting that β is a submodel of α we claim only that β is embeddable into α ; there exists a one-one map $\Phi: B \rightarrow A$ such that $\Phi(R'_i) = R_{i/\beta(\Phi)}$ ($i \leq m$). α does not "induce" the necessary theoretical properties on objects in β unless of course these properties are definable from the observational properties $R_1 \dots R_m$. Consequently we could have two different embeddings Φ and Ψ of β into α such that for some property R_j ($j > m$) and some $b \in B$, $R_j(\Phi(b)) \& \neg R_j(\Psi(b))$. This difficulty is avoided on the submodel interpretation due to the uniqueness of the mapping (the identity map). As Friedman points out, the representationalist account does not prevent us from generating accurate laws, we simply do so by adding new primitive properties and relations to β instead of deriving them directly from higher level structure: However, on this account we provide explanations only in response to particular observable events. There exists no background structure that can be appealed to in attempting to furnish a unified account of various observable phenomena. As a result the representationalist account provides explanations that are less powerful and hence it proves unhelpful when confirmation of laws is at issue. The literal construal is preferred because it yields greater unifying power and increased confirmation; for example, we can *conjoin* molecular theory with atomic theory to explain chemical bonding, atomic energy and many other phenomena. Consequently the molecular hypothesis will pick up confirmation in all the areas in which it is applied. The theoretical description then receives confirmation from indirect evidence (chemical, thermal and electrical phenomena) which it "transfers" to the phenomenological description. Without this transfer of confirmation the phenomenological description receives confirmation only from the behavior of gases. So, in cases where the confirmation of the theoretical description exceeds the prior probability of the phenomenological description the latter receives the appropriate boost in confirmation as well. Hence the phenomenological description is better confirmed in the context of a total theory that *includes* theoretical description than in the context of a theory that *excludes* such description. The literal interpretation can thereby be seen as better confirmed, more plausible and less *ad hoc* (p.241).

Friedman emphasizes the importance of reduction in facilitating conjunctive inference, a crucial component in his account of unification. He insists that conjunction forms an important part of scientific practice because it is through the conjunctive process that theories evolve. Although I am unable to address the issue of conjunction here it will suffice to point out that because of the way in which conjunction seems to depend on Friedman's reductivist strategy, if his account of reduction cannot be sustained the status of conjunction as a methodological principle becomes highly suspect. In the following section I show how the reductivist approach of the model-submodel account can, in some cases, actually *prevent* a literal interpretation of theoretical structure by failing to provide an appropriate characterization of the relationship between the empirical and theoretical domains of the specified theory. As a result one is unable to perform the conjunctions Friedman sees as

necessary for theory unification, thereby rendering the model-submodel account an ineffective way of achieving realist goals.

3. Reduction vs. Representation

3.1 Is Reduction a viable approach?

The traditional philosophical problems associated with reduction focused on the relationship between thermodynamics and statistical mechanics and dealt with the identification of concepts such as temperature, mean kinetic energy and entropy. However, the idealized nature of the assumptions concerning the probability of a molecule's being at a certain phase cell (as well as the independence condition) expose a more serious problem than these linguistic debates would indicate, namely, that the global properties of the statistical system are not simply the result of the collective properties of the individual parts. In other words a literal reduction of the whole to its parts is impossible. Consider the following case.

Assume that the total energy $E(x_1 \dots x_n)$ of a system can be represented as the sum of two terms E_1 and E_2 and $(x_1 \dots x_n)$ denotes the dynamical coordinates of a point in the space Γ (the product of the phase spaces of all the components). Each phase function and the total energy E of the given system is a function of these n variables. $E_1 = E_1(x_1 \dots x_k)$ and depends on *some* of the dynamical coordinates while $E_2 = E_2(x_{k+1} \dots x_n)$ depends on the remaining coordinates. Given this characterization we say that the set of dynamical coordinates $(x_1 \dots x_n)$ of a particular system is decomposed into the components $(x_1 \dots x_k)$ and $(x_{k+1} \dots x_n)$. However, a peculiarity results when we try to interpret each component as a separate physical system contained in the given system.¹ Although each materially isolated part of the system usually determines a certain component of the system some components or sets of coordinates do *not* correspond to any materially isolated part of the system. These components characterize pure energy (in the sense given above by the definition of a component). For example, consider a system of one material particle with components of velocity and mass being u, v, w, m ; if its energy E reduces to kinetic energy we have $E = m/2(u^2 + v^2 + w^2)$. Although u is a component of the system whose energy is $mu^2/2$, it doesn't correspond to any material aspect of the system. But, because each component is a group of dynamic coordinates and has a definite energy it has its own phase space; with the phase space of the system Γ being the product of the phase spaces Γ_1 and Γ_2 of its two components. Moreover, each component also has its own structure function each of which, taken together, determine the structure function of the given system. Indeed the law governing composition of structure function is one of the most important formulas in statistical mechanics. In other words, there are certain aspects of the theoretical model or structure that are crucial for representing a unified account of the system yet they fail to correspond to anything real.

An additional problem is the methodological paradox that arises from the decomposition of the system into components is the exclusion of the possibility of any energetic interaction between particles defined as components. The irony is that statistical mechanics invariably assumes that particles of matter are in a state of intensive energy interaction, where the energy of one particle is transferred to another through the process of collisions. In fact, its methods are based precisely on the possibility of such an energy exchange. Quite simply, if total energy of a gas is expressed as the independent energies of the two components (the energies of the molecules), the assumptions of conservation and velocity distribution are violated because each assumption requires that the particles interact. If the Hamiltonian expressing the energy of the system is a sum of functions, each of which depends only on the dynamic coordinates of a single particle (and representing the Hamiltonian of this particle), then the entire system of equations governing the motion of the system splits into component systems.² Each component system describes the motion of some separate particle and is not connected to any other particle.³ As a result the energy of

each particle expressed by its Hamiltonian function appears as an integral of equations of motion and remains constant.⁴ From the fact that the particles are independent and the sum of the energies is constant it follows that the individual energies must be constant as well. But, since this conclusion violates conservation of energy we must deny the claim that the total energy is the sum of n independent individual energies. In other words, if we adopt a literal reductivist strategy then the way the mathematical model describes the system violates some of the structural constraints of statistical mechanics.

The difficulty is resolved by idealizing assumptions that consider particles of matter as *approximately* isolated energetic components. Although the precise characterization of energy contains terms that depend simultaneously on the energy of several particles, and allow for energy interaction between them, these forces of interaction manifest themselves only at very small distances. Consequently, the "mixed terms" in the energy equation (those that represent mutual potential energy of particles) will be negligible compared to the kinetic energy of the particles and therefore will be of little importance in the evaluation of averages. In a majority of cases, such as the calculation of the Boyle-Charles law, we can neglect these terms and still arrive at a good quantitative approximation; we simply *assume* that the energy of the system equals the sum of component energies. However, on a qualitative analysis the mixed terms are extremely important since they provide the basis for an understanding of energy exchange between particles; the very core of statistical mechanics.

I think these examples illustrate some fairly obvious difficulties for the kind of literal reductivist approach outlined by Friedman. Even if we disregard the problem of correlating temperature and mean kinetic energy across theoretical boundaries, a more significant difficulty arises in the case of identifying the constituents of the systems postulated by classical statistical mechanics with its individual particles. The structural presuppositions involved are radically different in each case. Although we can ignore these assumptions in some cases of quantitative prediction this is not the important issue. As Friedman himself suggests, if we are interested in purely phenomenological laws then there is no reason to prefer a reduction to a representation (p. 241). But the motivation for Friedman's account is to achieve a literal interpretation of theoretical structure, which in turn yields greater confirmation of hypotheses; something he sees as guaranteed by the model-submodel approach.

If we recall what constraints are involved in the relationship between a model and its submodel we see that they are structurally similar insofar as the interpretation of each relation, function and constant symbol in the submodel β is the restriction of the corresponding interpretation in the model α . Equivalently for every atomic formula Φ and assignment $[s]$ in β , $\beta \models \Phi[s]$ iff $\alpha \models \Phi[s]$. Applied to our physical example we see that a *literal* identification of the properties of individuals of the system (β) cannot be accomplished given the structural constraints on α , the statistical system taken as a whole. A literal identification of β with α would preclude the formal mathematical model of the statistical theory from accounting for specific parameters (the possibility of energy exchange between particles) that *must* be interpreted literally if we are to have a proper understanding of its foundations.

This difficulty can be countered on a representationalist account where we have an *embedding* of the properties in β into α . However, there is no reason to associate this approach with an accompanying denial of literal interpretation of theories as Friedman suggests [cf. pp. 236-250]. It is possible to retain a semantical realism of the kind van Fraassen (1980) advocates while remaining agnostic about the truth values of specific theoretical claims. This kind of realism denies the tenets of classical instrumentalism by interpreting theoretical structure literally. As a result we are able to appeal to theoretical structure for derivations and entailments of phenomenological laws and need not rely solely on phenomenological properties to provide us with the impetus for theoretical explanation. Although the theory is interpreted literally there is no requirement that every part of the theory has a counterpart in reality. The important subtlety in van Fraassen's position that

guards against the difficulties of instrumentalism (cf. Friedman p. 220 n. 3)⁵ is the distinction between theoretical structure being the kind of thing that *can't* be interpreted literally (and hence is only a mathematical representation) and theoretical structure as something capable of existing; something about which true or false claims can in principle be made without our being in a position to justifiably make such claims. On such a view we do not claim a literal *identification* of observational properties with their theoretical counterparts but instead correlate by way of an embedding map, certain features of β with features of α . Every aspect of β need not have a counterpart in any *one* model of the statistical theory. Instead the theory may have several models, each suited to a particular application. In this case the relationship between elements of α and β is not uniquely specified by the identity map and hence there can be a variety of ways that the so-called "reduced" entities/theory are correlated with the reducing theory or model. Given the logical properties of the model-submodel relationship we should expect that the relations and functions specified by the identity map would be preserved in the way we think of inference rules as truth preserving. But this seems too strong to characterize the relationship between observational and theoretical structure and to incorporate certain aspects of theoretical change. Hence, a further benefit of the representational approach is that it allows the relationship between α and β to change over time, something that is *prima facie* ruled by a *literal identification* of their corresponding elements.

We have seen that the idea that the theory has many models nicely solves the apparent incompatibility of identifying individual particles in some contexts and not in others. The demand for *different* models to account for the same phenomena also arises in the more narrowly defined contexts where we have a more straightforward reduction of observable entities to their theoretical counterparts. Interestingly enough Friedman's own example of the van der Waals laws provides just such a case.

3.2 The Case of van der Waals' Law

In the context of Friedman's kinetic theory example he discusses how the van der Waals law came to replace the Boyle-Charles law as an account of the behavior of real gases.⁶ Given an appropriate theory of molecular structure and intermolecular forces we can explicitly define the terms that represent molecular size (*a*) and force (*b*) and go on to derive the law. However, despite its experimental corroboration, when applied to cases of greater than first order deviation from Boyle's law the molecular model suggested by van der Waals' approach was seen to be insufficient.⁷ Basically the model overlooked the fact that when cohesive forces exist between the molecules some molecules never reach the boundary (the wall of the container). As a result van der Waals assumed that these molecules exert a negative pressure, an assumption that implied negative values for *P*. Because an examination of physical conditions shows that the true value for *P* must be positive an alternative formulation and molecular model was proposed by Dieterici.⁸ This model assumed the constant temperature of the gas so that the total energy distribution applied to molecules striking the wall as well as those that don't. Various other attempts have been made to improve the van der Waals equation by the introduction of more adjustable constants to supplement *a* and *b*; constants which can be chosen so as to make the equation agree more closely with experiment. One approach introduced a term *a'* to replace *a*. Because *a'* specified that *a* vary inversely as the temperature for some gases it provided a better fit with the observations than the original van der Waals equation.

The overall difficulty seems to be one of specifying a molecular model and an equation of state that can accurately and literally describe the behavior of gases. What the examples illustrate is that in order to achieve reasonably successful results we must vary the properties of the model in a way that precludes the kind of literal account that Friedman prescribes (an account that assumes that our model is a *literally true* description of reality). Not only is there a problem in deriving the van der Waals equation from the assumptions of the kinetic theory⁹ but an explanation of the behavior of real gases (something the van der

Waals law is designed to explain) requires many *different* laws and *incompatible* models. In fact, we use several different representations for different purposes; the billiard ball model for ideal gases, the weakly attracting rigid sphere model for van der Waals equation, and a model representing molecules as point centers of inverse power repulsion for transport equations. In general the van der Waals equation tends to smooth out the differences between individual substances and predicts that they behave more uniformly than they do.¹⁰ In what sense then can we link the van der Waals gas law with a molecular model that truly describes or can be identified with the behavior of gases at the phenomenological level?

If the relationship between the behavior of gases and their molecular model is one of model to submodel then the same relations and properties that hold in the latter must hold in the former (with the submodel being a restriction of the relations in the model). So, if the van der Waals equation requires a specific molecular model to establish its results while the Dieterici equation requires a different model it seems that we are unable to claim that either provides a literally *true* account of molecular structure. The so-called derivation of van der Waals law can be achieved using a particular model which we know to be inapplicable in other contexts. Hence it appears that the uniqueness of the mapping in the model-submodel account is actually a drawback rather than an advantage.

4. Conclusions

What I have tried to show is that realism about theoretical structure cannot be consistently maintained if one adopts the restricted view of theories characterized by Friedman's model submodel approach. Not only does it fail to provide a justification for realism but suffers as well from serious shortcomings as an attempt to explicate scientific practice. Although the embedding approach does not guarantee a strong form of realism about theoretical structure it can nevertheless sanction a literal interpretation of theories while remaining faithful to practice; goals that the model-submodel approach was designed to, but ultimately failed to achieve.

Notes

¹For a more specific account of this case see I. Khinchin, *The Mathematical Foundations of Statistical Mechanics*.

²In statistical mechanics we describe the state of the system G with s degrees of freedom by values of the Hamiltonian variables $q_1, q_2, \dots, q_n; p_1, p_2, \dots, p_n$. The equations of motion assume the following form:

$$dq_i/dt = \partial H / \partial p_i, \quad dp_i/dt = - \partial H / \partial q_i, \quad (1 \leq i \leq s),$$

where H is the so-called Hamiltonian function of the $2s$ variables q_1, \dots, p_n .

³See Khinchin, Ch. 2, op. cit. for details.

⁴In other words, the function $H(q_i, p_k)$ is an integral of the system described by the equations of motion.

⁵Although Friedman is correct in seeing constructive empiricism as a generalization of van Fraassen's earlier views on space and time he is mistaken in equating the position with traditional forms of instrumentalism. One obvious reason for Friedman's characterization is perhaps van Fraassen's use of the embedding rather than the submodel approach, a move however that is consistent with various forms of anti-realism. Although van Fraassen's

literal interpretation renders theories capable of being true or false it doesn't entail the corresponding requirement that we believe them to be true. We present a theory by specifying its models and delineating certain parts of those models (the empirical substructures) as candidates for the direct representation of observable phenomena. Once our theory is accepted (minimally, what is actual and observable finds a place in some model of the theory) it guides our linguistic practice. As a result the language receives its interpretation through the model(s) of the theory. Modal locutions as well as statements about theoretical structure and unobservable entities reflect the fact that our models specify many possible courses of events. van Fraassen sees the literal interpretation as specifying the *model* as the locus of possibility, not a reality behind the model [1980 p. 220]. Because language is interpreted through the model rather than by some mysterious hookup with reality, the constructive empiricist can advocate a literal interpretation of theories while remaining agnostic about metaphysical commitment to theoretical structure.

⁶The equation of state for ideal gases (Boyle's Law) $PV = RT$ failed to take into account the finite size of molecules and the existence of intermolecular forces. It was replaced by van der Waals law $[P + (a/v^2)](v - b) = RT$ where a and b are constants characteristic of each gas.

⁷Cf. Tabor (1979) pp. 127-128.

⁸Ibid.

⁹For a nice historical account of the development and derivation of van der Waals law see Martin J. Klein "Historical Origins of van der Waals Law" *Physica*, 73, 1972 pp. 23-47.

¹⁰That this happens can be seen from the "reduced equation of state. If a and b are eliminated from the equation in terms of critical constants one obtains this "reduced" equation which gives reduced values for pressure (where reduced $p - p/p_c$), temperature and volume. The equation is supposedly the same for all gases since the quantities a and b , which vary from gas to gas have disappeared. If this equation could be regarded as absolutely true then whenever any two "reduced" quantities are known the third could also be given and, similarly, when any two quantities are the same for two gases then the third will also be the same. (The Law of Corresponding States.) This law is true only if the nature of the gas can be specified by two physical constants. But, as in the case of van der Waals law, the law for corresponding states is true only as a first approximation. For details see Jeans, op. cit., Collie, op. cit., and S. Brush, *The Kind of Motion We Call Heat*, Book 1, Chapter 7.

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