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Synopsis of the Robert and Sarah Boote Conference

in Reductionism and Anti-Reductionism in Physics

Nicholaos Jones and Kevin Coffey, with added remarks by Chuang Liu, John D. Norton, John Earman, Gordon Belot, Mark Wilson, Bob Batterman and Margie Morrison.

This document is a synopsis of discussions at the workshop prepared by Nicholaos Jones and Kevin Coffey, with remarks added by by Chuang Liu, John D. Norton, John Earman, Gordon Belot, Mark Wilson, Bob Batterman and Margie Morrison. The program is included in an appendix.

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in Reductionism and Anti-Reductionism in Physics

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Reductionism is a failure -- or, at least, it is bankrupt as a research program. Relations between theories, and relations between phenomena, are richer than reductionist paradigms allow. There is more substance, complexity, and diversity to the relations among the sciences and their objects of study than use of the term "emergence" can hope to capture. Although these relations are not well-understood, their exploration promises to be interesting and exciting.

The Sense(s) in which Reductionism Fails

Although there are many different uses of the term "reduction" and its cognates, there are few cases in which such terms are applicable. (1) One theory may be said to be reducible to another theory just if the (law-like) generalizations of the latter theory, supplemented with appropriate bridge principles, entail the (law-like) generalizations of the former theory. In this sense, to say that chemistry is reducible to physics is to say that the laws of chemistry are deducible from the laws of physics plus appropriate bridge principles. Reductions in this sense are sparse, because bridge principles tend to be absent. For example, the paradigm case for this sort of reduction is the (purported) reduction of thermodynamics to statistical mechanics. However, exact (non-approximate) derivations of the laws of thermodynamics from the laws of statistical mechanics require bridge principles that only obtain in the thermodynamic limit -- this is the limit in which, among other things, a system's number of particles $N \rightarrow \infty$. Since real systems have only finitely many particles, these bridge principles fail for real systems.

(2) One theory may be said to be reducible to another theory in a different sense just if the characteristic equations of the latter theory are obtainable from the characteristic equations of the former theory in an appropriate limit. In this sense, to say that special relativity reduces to Newtonian mechanics is to say that, in the limit as the ratio between velocity and the speed of light $v/c \rightarrow 0$, characteristic special relativistic equations are equal to corresponding characteristic equations of Newtonian mechanics. Reductions in this sense are sparse, because limiting relations between the characteristic equations of theories tend to be singular: the equality required for this sense of reduction tends not to hold.

(3) One concept may be said to be reducible to another concept if the former is definable in terms of the latter. In this sense, to say that *temperature* is reducible to *mean molecular kinetic energy* is to say that *temperature* is definable as *mean molecular kinetic energy*. There are few reductions in this sense between concepts from different theories. This scarcity is due, in part, to variations in the degree of abstractness of the conceptual repertoire for different theories.

(4) One entity may be said to be reducible to another entity just if the former entity is nothing more than the latter. In this sense, to say that a puddle of water is reducible to a collection of molecules is to say that the puddle of water is nothing more

than a collection of molecules. Perennial concerns about minds and qualia to the side, there is little live doubt that "large" entities like tables, bears, and planets are reducible, in this sense, to "smaller" entities like protons, electrons, and whatnot. But the significance of this sort of reduction is less clear: noticing that these reductive relations obtain is only the tip of the iceberg. For instance, it is uncertain whether there is a "bottom level" of entities, a group of entities that compose everything but are not composed by anything else. And, regardless of whether there are such fundamental entities (and of whether "non-fundamental" entities are "real"), questions remain about the importance of problems that involve "more fundamental" kinds of entities as opposed to problems that involve "less fundamental" kinds of entities, and about the relations between the theories and concepts used to describe these various kinds of entities.

(5) Finally, facts about one entity may be said to be reducible to facts about another entity just if the facts about the former entity are nothing more than facts about the latter entity. In this sense, to say that facts about water are reducible to facts about molecules is to say that facts about turbulent flows in water are nothing more than (perhaps very complicated) facts about molecules. Remarks about the reducibility relation between entities apply, *mutatis mutandis*, to the reducibility relation between facts about entities.

How to Explore Autonomy?

To deny general reductive relations between theories and concepts is to affirm the general autonomy of those theories and concepts. There are (at least) two ways of accounting for such autonomy. First, one might hold that autonomy is an artifact of scientific practice: perhaps autonomy among disciplines aids in the discovery of new phenomena, the development of research programs and the tracing of those programs to their logical conclusions, and so on. Second, one might hold that autonomy reflects the way the world is: perhaps some phenomena demand their own set of concepts and generalizations. These views need not be mutually exclusive.

There is good reason to suppose that the prevalence of autonomy is not exclusively due to the pragmatics of scientific research. These reasons do not merely characterize instances of autonomy as instances of emergence. For example, methods of asymptotic reasoning show that there can be explanatory relations between theories, and the phenomena described by those theories, that are not reductive relations (and, in some cases, not causal relations either). The presence of non-commuting limits, uncontrollable idealizations, and symmetry breaking also might be important features in many cases of autonomy. But there is no consensus about the best way(s) to describe non-reductive relations between theories and between phenomena. Much remains unexplored.

Kevin Coffey

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[NOTE: I've associated names with various positions and questions in the hopes that it will provoke comments on the draft. If one's name is going to be associated with particular claims, one may be more inclined to revise or qualify various statements (or to correct any misinterpretations!), and that could get a useful discussion going. I apologize in advance if I've omitted anyone or failed to give proper credit for a point raised.]

I take the overarching question unifying the conference to be the following: in the wake of the (general) failure of Nagelian reduction, what remains of the reductionist project? Attempts to get at this issue can lead in a number of overlapping directions.

(1) *Remnants of Nagelian Reduction* – Even if Nagelian reduction fails as a *general* philosophical program, Margaret Morrison asked: is it an apt analysis of particular cases?

(2) *Types of Reduction* – Are there non-Nagelian notions of reduction that can be given precise characterizations (in terms of, say, ontology, explanation, mathematical limits, or multiple realizability)? Chuang Liu pointed out that the notion of reduction that interests physicists varies from one context to the next – is there any systematic pattern here? Does it depend on the questions one is asking about a given phenomenon, as Bob Batterman suggests? If so, how? Can we make sense of a continuum of types of reduction, with inter-theoretic relations at one end and metaphysical/ontological reduction at the other, as John Norton suggests?

(3) *Inter-Theoretic Relations* – What sorts of non-reductive relationships exist between theories, disciplines, and phenomena? On the assumption that not all such relationships are to be understood in terms of limits (often singular), in what other ways should they be understood? Bob Batterman claimed that we should just talk about inter-theoretic relationships, not reduction.

(4) *Accounting for Background Philosophical Motivations* – The Nagelian reductionist program was of a piece with (if not motivated by) other quite general philosophical views, such as views about what's 'fundamental' and the idea that phenomena can be given a hierarchical structure. What are (were) these background philosophical motivations? Do they still seem plausible? Can new accounts of reduction or inter-theoretic relationships account for them?

In what sense(s) is there a hierarchy of phenomena? If we accept that there is such a hierarchy, would it be in error to give up on the reductionist program entirely? Does accepting a hierarchy of phenomena imply a commitment to a hierarchy of theories?

Leo Kadanoff claimed that we should give up the idea that there are fundamental constituents of the world. But if we give this up, what remains? Larry Sklar asks: why not just believe in the observable? Moreover, is there a connection between reduction (or inter-theoretic relations) and realism? Jos Uffink warns that we should keep these ideas separate, which was a virtue of Nagelian reduction.

(5) *New Angles* – What further concepts/tools are available to refocus the questions or suggest possible answers? A sampling of options: autonomy/independence, idealization and approximation, (partial) hierarchies of phenomena (or theories or disciplines), stability/robustness of phenomena, limits (singular or otherwise), explanation, supervenience, emergence, and complexity.

Mark Wilson asked whether discussions of reduction were being clouded by issues in the philosophy of language, and pointed out that many of the 'valuable' relationships in science (such as asymptotic and universal relationships) are approximate. He also noted that phenomena like friction and sliding on a surface are themselves enormously complicated, and that what's 'fundamental' in some cases is the macroscopic phenomena, not the underlying microscopic phenomena.

Bob Batterman claimed that we should talk of disciplines or theories as being autonomous, not as being reducible. Chuang Liu identified two different notions of autonomy – (1) being insulated from details and complications of another ('independent') theory, which is perhaps where idealization plays a role, and (2) not being reducible. Larry Sklar characterized an autonomous theory as being one with its own conceptual structure, explanatory rules, and principles – but such an autonomous theory need not be disconnected from the underlying physics.

Can we understand the idea of a hierarchy of phenomena via the concept of supervenience? Mark Wilson was very suspicious of the notion of supervenience. Margaret Morrison noted that one reason some are suspicious of supervenience is that the lower level is thought of as *causing* the higher level – if that's true, then supervenience must go if you give up reduction. But she also noted that there is a more innocuous sense of supervenience. Larry Sklar argued that we could have causal relations that weren't explanatory, and so supervenience might not be problematic even if it is cashed out using causal relations.

Leo Kadanoff noted the purely heuristic value the assumption of autonomy has in advancing a field. John Norton asked: is there a broader, non-pragmatic role that assumptions about reduction, autonomy, inter-theory relations, etc. play in actual scientific practice? How do these issues bear on what scientists actually do? Bob Batterman argued that theoretical autonomy has explanatory value, even if the heuristics are strictly-speaking false, and that we should separate questions of explanation from questions of reduction.

Chuang Liu
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I like Kevin's way of associating points with names; so let me add to some of the points he attributed to me in the text (and a bit more).

For 'Types of reduction' I would like to add that at least Bob Batterman and Jos Uffink had explicitly rejected the considerations of any metaphysical or ontological relations among phenomena that may undergird theoretical reduction. I raised the question: can we afford to ignore them completely since practicing scientists are aware of the belief for the compositional hierarchy of reality and such a belief may have caused some anxiety among scientists and made them look for reductive relations whenever possible. It's a curious fact that none of the papers at the conference even mentioned the Oppenheim & Putnam paper on the reductive unity of science.

As for the prevailing mood at the conference: reduction is dead, let's just talk about inter-theoretic relations, I have one observation. Isn't it true that if we completely cut out any considerations of hierarchical relations among phenomena (again ontic or metaphysical considerations), and take the notion of reduction to be referring to nothing beyond a certain kind of relations between theories, then anti-reductionism becomes rather trivialized. If reduction is nothing more than a kind of inter- theoretic relation, then the failure of reduction is no more than a realization that that relation doesn't hold but others still do. Is this the upshot of all the work in condensed-matter physics for or against reductionism?

As for autonomy, I have one further observation: taking the thermodynamic limit has the function of making thermodynamics autonomous -- in the sense of being insolated from the statistico-mechanical details of the underlying constituents -- AND reducible -- in the sense of being rigorously derivable from SM, singularities included (the same sense as in Gordon's review article of Batterman's book).

A Dissent

John D. Norton
Center for Philosophy of Science and
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There was a sense in the workshop that over the last few decades we have entered a new era in our understanding of the relationship between microphysics (the “lower” level) and macrophysics (the “higher” level). Most memorably there seemed to be some strong agreement that “reductionism is a failure” and that a new view is to be built around the idea of an autonomy of levels in theory and explanation. There does not seem to be agreement, however, on a more precise characterization of the new view.

My view is a dissenting one. The demise of reductionism and its impending replacement by a new view depends largely on a very narrow view of reductionism, a focus on the failings of an old-fashioned and defective expression of it, the ignoring of its successes and a blindness to the incompleteness and familiarity in the new proposals. Let me focus this dissent on three myths.

Myth: Nagel was dead wrong.

Granted: the old Nagel model of reduction is literally a failure. We cannot deduce the higher level thermodynamic laws from the lower level molecular ones, even with suitable bridge principles. The second law of thermodynamics is absolute, but statistical physics is time reversible and recurrent.

That doesn’t make Nagel dead wrong. First, there remain too many successes of the old idea to allow us to discard it completely. Take Nagel’s favorite example, the deduction of the ideal gas law. It is fraught with the usual difficulties. Its microphysical assumptions are false: there are no molecules of point size and there are no molecules that do not interact. The macroscopic law deduced from it, the ideal gas law, is precisely true of nothing; there are no ideal gases. Yet no one wants to dismiss the deduction as failed physics. It explains very effectively why the pressures of many gases, the partial pressures of their components, the osmotic pressures of many dilute solutions all conform more or less closely to the ideal gas law. It is because they consist, near enough, of very many, spatially localized, non-interacting components.

Second, Nagel's deeper concern was obscured by the milieu in which he wrote. That concern was to head off the idea that macroscopic temperature, as measured by macroscopic instruments, is "the genuine reality, and that the molecular energies … are just a fiction."[1] This he contrasted with the "somewhat more sophisticated line of thought" in which "temperature [is] an 'emergent' trait, manifested at certain 'higher' levels of the organization of nature…" Indeed at one point he wrote in evident displeasure of "some alleged ontological hiatus between the [microscopic] mechanical and thermodynamical." (pp. 365-66) The notion at issue here is what we would now call ontological reduction. A system conceived macroscopically—a bucket of water—is just a large collection of water molecules interacting. So all facts about the former depend completely upon facts about the latter.

The implementation and development of this idea suffered greatly from the fact that the 1960s, when Nagel wrote, were, in his circle, still the hey day of logical positivism. To do philosophy of science was to talk of sentences, their syntactical structure and the deductive relations between them. Clearly worried that his work was not logical enough, Nagel explained that he intended his book to have "a wider audience," so he "avoided highly formalized presentations of analyses or the use of special symbolic notation of modern formal logic…" (p. ix) Nonetheless the work is ultimately about statements and the deductive relations between them. He talks of explanation and delineates four senses in an earlier chapter. Yet in virtually all his examples in the treatment of reduction, to explain is merely to deduce.

In this environment the inevitable happened. The notion of reduction was expressed in the vernacular: to reduce is to deduce the laws of the higher level theory from those of the lower. As the example of the deduction of the ideal gas law already suggests, the analysis would have benefited had Nagel spent a little less effort delineating just which sentences could be deduced from which others, and little more explicating those parts of the story that elude these deductive relations.

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[1] Ernst Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation.* New York: Harcourt, Brace and World, Inc., 1961, p. 342.

Seen this way, it would have been better if, from the start, Nagel had distinguished two forms of reduction. The first is ontological and expressed by the slogan that a bucket of water is nothing but a collection of very many interacting water molecules. The second form of reduction is theoretical. It pertains to the way we relate our theories. In this form, a reductionist or antireductionist thesis tell us how our lower level theories of atoms and molecules and their statistics relate, or fail to relate, to the higher level theories of phenomenological thermodynamics.

Myth: You can be an ontological reductionist and theoretical antireductionist.

The temptation is to say that Nagel's idea lives on in ontological reduction, but that it fails for theoretical reduction. It is an attractive compromise. We don't have to defend any crazy ideas about water being somehow more than the sum of its component molecules and their interactions. We can leave the world alone. We identify the domain in which our new era of understanding is exercised as one of human activity: it lies in how we find and build theories and how we use them to explain. Is it not plausible that the human mind is just too feeble to find theories that tightly bind the macroscopic higher level with the intricate molecular lower level, so that the autonomy of levels derives not from an hiatus in ontology, but from human limitations?

It is tempting. But it is wrong. You cannot separate ontological and theoretical reduction like this. Whatever theories may mediate in the process, ontological reduction has powerful explanatory consequences. Macroscopic theories have the same sort of autonomy as a prisoner confined to a cell: he may formulate all the travel plans he likes as long he doesn't travel more than six feet. My favorite example is as simple as it is compelling. In the 1980s, a research team led by Jacques Benveniste reported that they found residual biological activity in solutions of antigens diluted to such a degree that, with high probability, no single molecule of antigen remained. The molecular account—essentially only the value of Avogradro's number—explains decisively why such experiments must fail. We know the experiments must fail and understand why they must fail because of what water is and what antigens are.

More generally, the fact of ontological reduction means that the lower level theories powerfully circumscribe the higher level theories. If the interactions between the molecules are conservative, then so must any higher level theory. One of the most celebrated examples of the new physics is the use of renormalization group methods to account for critical phenomena. Since the analyses and even the values of exponents recovered depend essentially on the microphysical Hamiltonians, this analysis cannot claim autonomy from the molecular physics.

Myth: Limits, false idealizations and robustness are a novelty.

Much has been said about the use in the new physics of limits and idealizing assumptions that are literally false on the molecular view but nonetheless prove essential to successful theorizing. There is no doubt of the fascination and power of these methods, as they have been developed in the new physics, and that if we describe them in ever finer detail we will finally arrive at something novel in our history.

Yet the novelty lies not in the use of limits and idealizations that are false on the molecular picture. They have been a staple of thermal physics since its inception. They are used every time we model water or air as infinitely divisible, continuous fluids. The program of statistical physics from its inception embodies this idea. The founding difficulty was that a molecular theory of gases, dependent on a complete account of every molecule, was intractable. So a tremendous amount of the system was idealized away through the statistical approach; and the recovery of almost any result required that it be robust under a literally false limit—notably that the there are infinitely molecules in a finite sample of gas.

(Added to Version 7) Finally the notion that macroscopic systems may be robustly independent of the details of the microscopic structure is again an idea that has been with us from the earliest days. Take the ubiquity of the canonical distribution with its factor of exp(-E/kT). It is holds of virtually any system that has come to thermal equilibrium with a heat reservoir. Macroscopic consequences follow. If the system is classical, consider the venerable equipartition theorem. We need only know the number of degrees of freedom f of the Hamiltonian and we can immediately write down the molar heat capacity as fR/2; all the other properties are irrelevant.

John Earman
Department of History and Philosophy of Science
University of Pittsburgh

I join John Norton's dissent. Whether the speakers intended it or not, the collective impression given was that they subscribed to what I take to be a huge non-sequitur: Inter-theory relations are very complicated (and they surely don't fit the Nagel model); therefore reductionism is a failure. Nonsense! 1) I didn't hear any convincing reason to give up on *ontological reductionism*, expressed in terms of some sort of supervenience—e.g. no difference in facts about the higher level without some difference in facts about the lower level. 2) Nor did I hear any convincing reason to give up on *theoretical reduction* in the following sense: the theory of the fundamental level (or the more fundamental level if there are fleas on fleas ad infinitum) is capable in explaining—in a good old fashioned Hempelian DN sense!—all of the facts and lawlike regularities of the higher level. It is no evidence against 1) and 2) to trot out examples where the more fundamental theory fails to Nagel reduce the higher level theory since typically the latter is *false*. What the more fundamental theory does is to explain a) why the higher level theory works as well as it does and b) where and why its predictions go wrong.

The last point is connected with what I take to be a strong consideration in favor of reductionism. The higher level sciences do enjoy a *methodological* autonomy. But it is difficult, if not impossible, to state any exactly true lawlike generalizations about the higher level phenomena in a vocabulary appropriate to these phenomena—ceteris paribus clauses are typically needed. Not surprising if there is a relation of ontological reduction, for then there will likely be defeating conditions for any generalization of broad scope in the higher level theory, conditions that cannot be stated in the vocabulary the higher level theory but require the vocabulary of the lower level theory. The point already emerges in basic theories of physics. E.g.—the "Second Law" of thermodynamics is not a law—it is, strictly speaking, false and with the aid of a low power microscope we can see violations. The conceptual resources of statistical mechanics are needed to delimit where these violations occur (although how this is to be done is still controversial).

Finally, the claim that idealizations are *essential* to explaining higher level phenomena from lower level phenomena is without merit. E.g. the claim that the thermodynamic limit is essential to explaining phase transitions is an example of what Craig Callender has rightly termed taking thermodynamics too seriously by granting that the explanation must produce an actual singularity in a thermodynamic quantity. All that is necessary is to show that for large but *finite* N statistical mechanics reproduces the observed behavior.

Gordon Belot
Department of Philosophy, University of Pittsburgh

I have a couple of remarks about one of the themes that arose at the conference: that less fundamental theories play an essential role in our understanding of the world. Let me call this the Novel Claim.

Some Pittsburghers find this suggestion puzzling. No one at the conference cast any serious doubt on any of the following. (a) More fundamental theories are in general better guides to ontology than less fundamental theories. (b) If a given event can be explained by a less fundamental theory then, at least in principle, it must be explicable by the more fundamental theory. (c) When we have two explanations of a given event, one rooted in a more fundamental theory the other in a less fundamental theory, we expect the former to be superior to the latter (more detailed, more accurate, involving assumptions closer to the truth, etc.). If all of this is granted, then what understanding of the world can a less fundamental theory offer us that we cannot already get from a more fundamental theory?

I want to suggest a two-pronged response to this question that is, I think, in line with the approaches of Batterman and Morrison (and probably others). Both prongs contrast the sort of explanation involved in the NC with the Hempelian notion of explanation.

1.

Faced with an interesting event (a rainbow, an instance of supercooling etc.) we can ask: (i) Why did this event occur? or (ii) Why do events of this type often occur? Explanations proffered in response to (i) might well fit the D-N form; and here we expect the best explanation to be rooted in the more fundamental theory rather than the less fundamental theory.

Explanations offered in response to (ii) will not strictly speaking fall under Hempel's classification. One way to reply to a question of type (ii) is to show how to construct an explanation of a particular instance of the type of event, then to show that this explanation is structurally stable (under suitable small perturbations of the particular facts invoked, one expects the phenomenon to continue to occur). Part of the Novel Claim amounts to: less fundamental theories often play a surprisingly important role in the construction of such explanations.

2.

It is misguided to assimilate physical understanding to the possession of D-N explanations: when physicists claim understanding, they do not typically have in mind the ability to find a solution satisfying such and such initial boundary conditions, while being able to show that this solution has some feature of interest. The Novel Claim is most plausible and most interesting when understood in the context of a non-Hempelian account of explanation. Here is a possibility starting point for such an account (From p. 2-1 of vol. 2 of the Feynman lectures):

'What it means really to understand an equation---that is in more than a strictly mathematical sense---was described by Dirac. He said: "I understand what an equation means if I have a way of figuring out the characteristics of a solution without actually solving it.' So if we have a way of knowing what should happen in given circumstances if without actually solving the equations, then we "understand" the equations as applied to those circumstances. A physical understanding is a completely unmathematical, imprecise, and inexact thing, but absolutely necessary for a physicist.'

Mark Wilson
Department of Philosophy
University of Pittsburgh

When I claimed that our understanding of the issues of "inter- theoretical relationship" has been unduly clouded by philosophy of language concerns, I had intended to echo, in my own way, some of the concerns that John Norton raises. The central mathematical structures tacitly emphasized in Nagel's old presentation are logical ones, whereas it seems wiser to look at these matters in basic mode that applied mathematicians utilize when they search for lower dimensional manifolds hiding inside the bigger manifolds defined by the Navier-Stokes equations in hopes of locating dominant behaviors that stay close to many of the true fluid trajectories for reasonable periods of time. The common invocation of limiting relationships usually represents a rather quick and dirty means of ascertaining dominant behaviors. The better forms of recent work (e.g., Batterman's) have concentrated upon these more telling forms of mathematical relationship, although sometimes these conclusions have been couched in an unnecessarily tendentious vocabulary of "emergence," "supervenience" and "autonomy" that has been inherited from the mind/ body metaphysicians. Insofar as the summary goes, I object to its tacit suggestion that we recent philosophers have ascertained vital "morals" in the latter, metaphysical vein--claims that I find distracting (and somewhat mystical in import). I concur with John's observation that Nagel's intentions were essentially on the right track and that his emphasis on logical relationships should be pardoned as an artifact of the era.

Bob Batterman
Department of Philosophy
University of Western Ontario

My remarks here will echo Gordon Belot's comments. Both John Norton and John Earman, I believe, would like to endorse something like a Nagelian outlook on intertheory reduction. I say "outlook" here because they rightly point out that the overly linguistic nature of the Nagelian program is the result of the misguided influence of the positivist views about science. Nevertheless, what remains in a Nagelian outlook (and I think this is a major motivation for the continued appeal of the DN model of explanation as well) is the idea that given the laws of some theory one can explain and otherwise account for various phenomena by subsumption under those laws. This is, to my mind, a completely warranted point of view based upon the myriad successes of solving dynamical equations (ODEs or PDEs) given initial conditions and boundary conditions as input.

That said, I've argued that not all explanatory questions one might like answered can be provided by this sort of general view. Sometimes, often in fact, we are interested in explaining certain regularities or patterns that replay themselves over and over in situations in which the world has changed. There must be some account of how, despite completely different environmental contexts etc., these patterns continue. As Gordon notes in his remarks these are questions that I have elsewhere called "type ii" why-questions: Why do events of this type occur? Given the (sometimes) radical changes in context there must be something robust about these patterns. And to explain why they often occur requires a demonstration of what is responsible for that robustness. An example that I've talked about is the shape of water droplets as they break off from a larger mass of water. It turns out that the shape at the breakup is identical over a wide range of ways water drops might be formed---whether they drip from a faucet or are formed by waves crashing into rocks on a beach. How come?

Now from the point of view of Norton and Earman it seems that a full response to this sort of question should come from the fundamental (or most fundamental) theory of the nature of the substance that is available. No less fundamental, false theory can play an explanatory role because it is false and there is a fully correct theory in the wings. Let us assume that we have such a fully correct, fundamental theory, I'll just call it "molecular dynamics" or MD, that describes the complete quantum mechanical details of the molecules of water that make up the mass, how they interact with one another etc. Surely such a theory that gets the ontology correct must also be able to provide complete explanations for all of the various behaviors of masses of water? I take it that this question expresses at least part of Norton's claim that one "cannot separate ontological and theoretical reduction."

Norton rightly notes, I think, that the "fact of ontological reduction means that lower level theories powerfully circumscribe the higher level theories," but I think he is wrong that this eliminates the explanatory autonomy of the higher level (typically false) theory. I think his claim about the renormalization group explanation of critical phenomena vis a vis the microphysical Hamiltonians is mistaken---such analyses and explanations (of the nature and values of the critical exponents) do not depend essentially on the microphysical Hamiltonians; at least not in anyway like one would expect from a modern Nagelian/Hempelian outlook. (In fact, the analyses and explanations depend on showing that the details of the microphysical Hamiltonians are by and large irrelevant.)

I'll discuss the explanatory autonomy of the higher level theories below in the context of the breaking drops where we have the fundamental theory of MD and the false higher level continuum theory describing fluid masses and their evolution---Navier-Stokes theory. But first, I would like to bring out another quote---one that I think more adequately represents the point of view I'm advocating than the passage from Feynman about Dirac mentioned by Belot. Here's a passage from Michael Fisher's review article about the renormalization group account of critical phenomena:

> "The traditional approach of theoreticians, going back to the foundation of quantum mechanics, is to run to Schroedinger's equation when confronted by a problem in atomic, molecular, or solid state physics! One establishes the Hamiltonian, makes some (hopefully) sensible approximations and then proceeds to attempt to solve for the energy levels, eigenstates and so on . . . . The modern attitude is, rather, that the task of the theorist is to *understand* what is going on and to elucidate which are the crucial features of the problem. For instance, if it is asserted that the exponent α depends on the dimensionality, d, and on the symmetry number, n, but on no other factors, the theorist's job is to explain *why* this is so and subject to what provisos. If one had a large enough computer to solve Schroedinger's equation and the answers came out that way, one would still have *no understanding* of why this was the case!"
> (Fisher, "Scaling, Universality, and Renormalization Group Theory" LNP vol. 186, (Springer, 1982))

Now let's consider briefly the explanation of the fact that virtually no matter how a fluid breaks, its shape at breakup is the same. Norton, Earman, and Fisher's traditional theorist would presumably run to Schroedinger's equation (or MD) and try to account for the motions of each molecule in the fluid mass. Suppose that we could do this then as Earman notes we would be able to show for large but *finite* N that the fundamental theory reproduces the observed behavior.

Let's consider whether this is in fact the case. What observed behavior have we reproduced by our fully correct, *nonidealized*, fundamental MD account? It is surely the case that this complete story will yield a spatial picture of the molecules that look like they are breaking with the shape that we observe for that particular drop. But there are other behaviors that we observe, and it seems to me that this fully correct, nonidealized account does not provide anything like an account of those. It seems to me (as I noted earlier) that one thing we observe is the repeated replaying of this scenario in different fluids, in different contexts, etc. That pattern is just as much an observable behavior as the behavior of an individual drop. Does our fully nonidealized MD account explain, or in any way account for our observation of this pattern of behavior? How could it? After all, the details of every other drop breakup will be completely different. Fisher's traditional theorist would have to repeatedly run to Schroedinger's equation for each instance. And every such account will be completely different, as the initial and boundary conditions for the MD derivations will be completely different. These DN type MD accounts are completely disjoint and nothing about any one of them tells us anything about any other.

This suggests, as Belot notes, that a non-DN account of explanation may be required to account for the observable pattern of behavior. Of necessity, such an account will have to provide an explanation of why the breakup shape seems *not* to depend on most of the detailed features at play in the individual instances of the pattern---all those detailed factors that play a role in setting up the DN/MD accounts. The less fundamental Navier-Stokes theory is a continuum theory that ignores all of those details. And, it is possible to show that the Navier-Stokes account of drop breakup leads to a singularity in finite time (the breakup). Of course, this is idealized, as fluids are not continua and as there are no real singularities in the mathematical solutions to the fundamental MD theory. Nevertheless, one can provide a scaling or similarity solution the Navier-Stokes equations that accounts for the fact that virtually all fluids will break with the same shape. This is an explanation of the robustness of the observed behavior and it is one that makes essential appeal to an idealization.

Of course, as Norton and Earman point out, limiting idealizations have played a role in thermodynamics and statistical mechanics since day one. There is nothing terribly novel about it. What is relatively novel, I believe, is the idea that such idealizations may in many important instances play an essential explanatory role in the demonstration of the robustness of certain patterns of phenomena. If that is true, then they are not readily dismissed as merely pragmatically necessary idealizations. In addition, if that is true, then sometimes less fundamental theories can have explanatory autonomy. And, contra

Earman, in the context of explanatory reduction, the fundamental theory doesn't simply explain why the older theory worked as well as it did.

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Being the last person to respond I have the benefit of an overview of the ideas expressed by everyone and how they relate, so let me add a few comments on what seem to be the main objections and points that have emerged in the contributions. I think Kevin's question of whether the acceptance of a hierarchy requires us to give up on reduction nicely encapsulates one of the main points at issue. That said, why does this have to be an either/or scenario? I certainly don't think there is any reason to discard the notion of ontological reduction (that macroscopic phenomena are made up of microscopic constituents), to do so would be rather pointless and without motivation. But, I disagree with John N's claim that subscribing to ontological reduction prevents you from advocating theoretical anti-reduction. I'm happy to concede that there will be cases where ontological and theoretical reduction go hand in hand, i.e. where the fundamental theory that explains the behaviour of microscopic constituents also explains the behaviour of the higher level macroscopic phenomena (his antigen example is one such case). But successes in some contexts don't imply that reduction is the correct model for explaining phenomena in general. The so-called 'failure of reduction' amounts to the claim that there are a large number of cases where that model of explanation ceases to work and we should look elsewhere for the correct explanatory schema.

As Bob's example(s) point out, there is a class of phenomena or behaviours that simply *can't* be understood in terms of explanation via fundamental theory and in these cases reduction fails us. This claim also asserts that whatever the ontology is at the fundamental level, it just doesn't matter when it comes to understanding certain kinds of 'universal' behaviour. Indeed, the power and novelty of the renormalization group is that is shows us the ways in which the underlying order that we find in the behaviour of phenomena at critical point *don't* depend on the original Hamiltonian. The core of the idea of universality is that the fixed points are a property of transformations and are largely insensitive to the Hamiltonian because as one moves to larger and larger block lattices small scale degrees of freedom are gradually excluded. This is where the notion of emergence rather than supervenience becomes important. If we understand supervenience in the way John E. mentions (no change in the higher level pheonomena w/o a change in the lower level) then supervenience doesn't figure as an explanatory notion despite the fact that the higher level phenomena are constituted by those at the lower level. Emergence, however, does capture what's going on in these cases because the stable behaviour of phenomena around critical point can't be derived, explained or predicted by the microphysics. Low level excitation spectra become more and more generic and less sensitive to microscopic details as the energy levels are lowered.

The reason ontological reduction is important is because we think it provides us with an understanding of how and why phenomena behave as they do. To that extent ontological reduction is bound up with theoretical reduction because theories appeal to ontology to describe behaviours and explain why certain experiments fail or succeed where they do.

But a failure to explain these facts about phenomena doesn't imply that ontological reduction has failed. Why? Because the simple *fact* of ontological reduction gives us very little if any information that is useful by itself. (We need something more than the claim that gases are made up of molecules to understand why they behave as they do.) Failure to explain is by and large a failure of theoretical reduction. To that extent we can separate ontological and theoretical senses of reduction because the success of the latter depends on the former but not vice versa. That is to say, we needn't deny the 'constitutive' sense of ontological reduction in the face of failures of explanation.

Part of what is at issue here is what qualifies as a 'reduction'. If we take the simple example of the non-relativistic Schroedinger equation, we can explain (and predict) a wide variety of phenomena, e.g. strength of chemical bonds, elastic properties of matter, simple chemical reaction rates provided we use approximation techniques and plug in the required experimental data. Consequently, these aren't first principle deductions and the addition of experimental input seems to go beyond what might qualify as the specification of initial conditions in a D-N style explanation. But, even if we concede that these qualify as 'reductive' explanations there is a further worry that arises in the relation between exact results and approximations; a worry that I think poses a problem for reductive explanations. For example, in deriving the general phenomena associated with superconductivity like infinite conductivity, the Meissner effect and magnetic flux quantization we rely on both macroscopic models like Ginzburg-Laudau and microscopic models (BCS). These phenomena can be predicted with essentially unlimited accuracy, so the question arises as to how can one use approximations to derive exact results. In other words, why do the models work? The answer is that the models exhibit a breakdown in electromagnetic gauge invariance and from this basic assumption one can derive the exact consequences mentioned above [see Weinberg (2005), *Quantum Theory of Fields*, v.2]. What this means is that these general features of superconductivity are model independent consequences of the breakdown of electromagnetic gauge invariance. The detailed models explain *how* the breakdown occurs and serve as the basis for approximate quantitative calculations but not to derive exact results. So here again we have another case of the insensitivity of generic behaviour to the microphysical base. The explanatory autonomy here is not provided by QM or any QM model like BCS; rather it's provided the general principle of spontaneous symmetry breaking that is distinct from any theoretical (microphysical) story about *how* that breaking occurred. This case is analogous to Bob's water droplets, it doesn't matter how they were formed the shape at break-up is identical.

The Robert and Sarah Boote Conference in Reductionism and Anti-Reductionism in Physics

22-23 April, 2006
Center for Philosophy of Science
817R Cathedral of Learning
University of Pittsburgh

Saturday, 22 April 2006

Morning Session: Reduction

10:00 Reduction and Renormalization
Robert Batterman, University of Western Ontario

11:15 Coffee Break

11:45 The Gibbs Paradox
Jos Uffink, Utrecht University

1:00 Lunch Break

Afternoon Session: Emergence

2:30 Making Complexity: From Splashes to Intelligent Design
Leo Kadanoff, University of Chicago

3:45 Coffee Break

4:15 Varieties of Explanatory Autonomy
Lawrence Sklar, University of Michigan

Sunday, 23 April 2006

10:00 Round Table Discussion
Chuang Liu, University of Florida; Margaret Morrison, University of Toronto; Mark Wilson, University of Pittsburgh; and the Speakers

Committee: Gordon Belot, University of Pittsburgh, Chair; Robert Batterman, University of Western Ontario; John D. Norton, University of Pittsburgh