

Kallfelz, William Michael. [Clifford Algebra: A Case for Geometric and Ontological Unification](#)

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## Foreword

### Statement of the Problem and Overall Summary of my Method toward solving it

Frederik Suppe (1977) details the demise of logical empiricism in the philosophy of science, (i.e., ‘Received View’) with its distinctly realist and anti-realist presumptions concerning the nature of observable versus theoretical terms, as well as its methodological presuppositions concerning the nature of unity in the sciences (when suitably rationally reconstructed). This previous generation’s demise set the stage for an evolving trend in the philosophy of physics (and philosophy of science in general) culminating in what I depict ‘post-Standard’ accounts, based on my readings of Robert Batterman (2002-2005), Nancy Cartwright (1999), Mathias Frisch (2005), Margaret Morrison (2000*a,b*), and other prominent members in contemporary philosophy of science. Such a trend can be plausibly construed as a dialectical response to the ‘Received View’ of traditional logical empiricism: what the logical empiricist would brush aside as part of the “context of discovery” and hence unworthy of philosophical scrutiny, the post-Standardist would consider essential for any serious enquiry. While the logical empiricist would rationally reconstruct a physical theory according to some logical regimentation, the post-Standardist on the other hand would focus on the *actual* processes the physicist implements in the development and refinement of models, laws, and mathematical formalisms. In short, whereas the logical empiricist saw a statically unified picture of the structure of physical theories, the post-Standard philosopher instead regards the structure as a dynamically evolving plurality.

I need not discuss herein all the obvious benefits of this legacy, what great amount the post-Standardist continuously offers in the way of tools for philosophical analysis of particular domains of research. Yet, concerns remain, which can be narrowed down to the following questions: what is the *actual extent* of the pluralism as evinced or presupposed by most contemporary post Standard accounts? Does such pluralism cut across methodological, ontological, or epistemological aspects of their domains of study? If so, to what extent? Would an excessively pluralist outlook erode the *normative* aspect (however conditioned or qualified) of philosophical analysis, eventually rendering philosophy of physics and philosophy of science indistinguishable from other fundamentally descriptive studies of sciences like SST (science and technology studies)?

I think that raising such concerns would *not* imply some false dichotomy between two ‘received views’: i.e., that come what may, we either embrace uncritically the pronouncements of leading post-Standardist philosophers, *or* we adopt some “neo” logical empiricist position. A healthy middle ground, or ‘third way’ of course is possible.

The ‘third way’ I advocate here is what I consider a constructively critical analysis of a leading post-Standardist: Robert Batterman. He has certainly had his fair share of critical respondents, and in that regard, as I show below, mine perhaps most closely resemble those of Gordon Belot (2003), insofar as I draw on a class of mathematical formalisms—comprising what I describe below as an instance of an important and revolutionary *research tradition* (according to Larry Laudan’s (1977) criteria)—namely the adoption of Clifford (or geometric) algebras. (Belot, on the other hand, bases his critical assault on Batterman through his use of the theory of differential equations, both ordinary and partial.)

What I basically aim to show here is that Robert Batterman’s *methodologically pluralist* conclusions, as stated in his claims concerning what he advances as the presumed novelty of “asymptotic explanations,” (2002) as well as classes of modes of theorizing he deems as “epistemically fundamental” versus “ontologically fundamental” (2004-2005) are undercut from the standpoint of the research tradition of Clifford algebra: Characterizing instances of his case studies via this mathematical formalism indicate otherwise—that explanation and reduction may not act at cross-purposes (contrary to his claims in (2002)) and that epistemically and ontologically fundamental modes of theorizing are in fact subsumed under *methodologically* fundamental procedures. So from a standpoint “internal” to Batterman’s case studies, I block his methodologically pluralist inferences primarily by way of the method of counterexample.

From a standpoint external to Batterman’s overall theses with their associated philosophical import, I show by way of inference to the best explanation how his overall views on the nature of reduction and explanation can best be subsumed under the model(s) of explanation and inter-theoretic reduction offered by Fritz Rohrlich (1988-1994) and some of his associates. My overall point in showing this runs as follows: stated in general terms, one can buy into typical post-Standardist claims concerning the inevitably irreducible nature of pluralism when it comes to *ontology* and *epistemology* in the process and activity of theory-development. *Nevertheless, this does not entail a methodologically pluralist thesis.* In other words, as physicist and philosopher Fritz Rohrlich has demonstrated: it is perfectly consistent for a physicist to accommodate an ontological pluralism in a particular class of theory-formation, but at the same time remain methodologically monist. Moreover, such methodological monism is best characterized in a mode and manner that does not hearken back to renditions of logical empiricism based on strong claims of logical reductionism (shown, as I do below, to be largely irrelevant if not outright hindering the progress of the development of theory formation).

The school of thought that Rohrlich implicitly ascribes to as well as some of his associates like Diedrik Aertz and Juergen Ehlers explicitly advocate is *structuralism*, a highly mathematical version of the semantic view of theories enjoying ongoing and active innovation by European physicists and philosophers of the likes of Erhard Scheibe (1997-1999).<sup>1</sup>

One may ask: why worry about the implications of methodological pluralism? Is there some fundamentally qualitative difference distinguishing methodological from epistemic or ontological pluralism? Would methodological pluralism render philosophy of physics indistinguishable from other descriptive studies of science, whether social or psychological? If so, why would the same not hold in the latter cases of epistemic or ontological pluralism? I answer each one of these questions in my argument, but at the outset I may remark here that my basic concern lies in Batterman's treatment of the role of singularities: the normative inferences he draws thereon seem to run counter to the attitudes of how most practicing physicists would approach them. In this regard, I remain faithful to post-Standardism in my exercise of what Frisch describes (and I describe in greater detail in §4 herein) as the "principle of charity" the philosopher of physics should levy to the physicist.

So reiterating: internal to Batterman's claims, I argue by way of counterexample. External to Batterman's claims, I argue by way of inference to the best explanation, to show how the philosophical import of his conclusions can be preserved in the appropriate framework of explanation and reduction (minus what I consider are his unwarranted conclusions). Regarding the latter point, I launch into a somewhat detailed discussion concerning the issue of contextual verisimilitude and reduction, in §I.3-4.

The reader may be dismayed that in that discussion I demur from a general discussion of realism: for are not maneuvers like inference to the best explanation as well as discussions of verisimilitude the realist's favorite possessions? Perhaps. (Indeed, one could further argue that such maneuvers exhibit the greatest coherence in a realist framework, in terms of epistemic values like explanatory strength). However, since my essay is primarily devoted to issues concerning the nature of inter-theoretic reduction, spatial consideration forces me to table the issue of the question of realism versus anti-realism here.<sup>2</sup>

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<sup>1</sup> "Structuralism" is also a term that appears often in certain branches of the philosophy of mathematics (e.g., Charles Chihara (1990, 2003)). Certainly, structuralists in the philosophy of mathematics share metaphysically resonant themes with those mentioned above, as both schools of thought assent to a generally *constructivist* position (as opposed to a Platonic "essentialism") concerning the ontological status of theoretical entities. Nevertheless, the projects' motivations differ. Mathematical structuralists are primarily concerned with resolving issues centering on ontological status, while structuralists in the philosophy of science are typically motivated more by epistemic and methodological concerns. Aside from the issue of a "rapprochement" of methods in philosophy of science vis-à-vis philosophy of mathematics I briefly discuss, a larger comparative and contrastive analysis concerning these two structuralist traditions lies beyond the scope of this essay.

<sup>2</sup> I will point out in passing, however, that just as anti-realists like van Fraassen (1980) and Laudan (1981) have shown in their own ways that inference to the best explanation does *not* presuppose that one need have faith in

If, however, I were forced to lay my metaphysical cards on the table concerning the question of realism, I will mention in passing that recent arguments by Phillip Kitcher (2001) make a strong case for adopting a generally realist outlook without presupposing some context-independent notion of verisimilitude. If one adopts the metaphor he advocates that theories work best like ‘maps,’ in which the methods of map-making are entirely constituted by the interests and aims of their particular function (a subway map of Washington DC is qualitatively different from a topographical map), then such a generally realist outlook can be easily accommodated in the light of Paul Teller’s (2005) and Stephen Yablo’s (1999) contextualist claims they make concerning the nature of verisimilitude and ontology, issues which I discuss at length in §I.3-.4 below.

Contrary to Batterman’s generally positivist stance, realism with a contextual notion of verisimilitude, i.e. “contextual realism,”<sup>3</sup> is a more satisfying position to adopt for both for the workaday physicist and for the philosopher engaged in studying the physicist—*especially* in the area of critical phenomena that Batterman investigates. Note, however, as I show below in chapter 1, that Rohrlich’s research program adopts a version of realism which borrows much from structuralism. This however does *not* make him a “structural realist” (J. Worrall (1989, 1994))—yet *another* sense of structuralism. In other words, structuralism and “contextual realism” do *not* entail structural realism. Instead, Rohrlich et. al. seem to adopt the *divide et impera* (“divide and conquer”) method of defending realism as discussed by Stathis Psillos (1996):

- (i.) [I]dentify[ing] the theoretical constituents of past genuine successful theories that essentially contributed to their successes; and (ii) show[ing] that these constituents, far from being characteristically false, have been retained in subsequent theories of the same domain. (Psillos (1996), S310)

As Psillos argues, such a strategy is immune from the charges of anti-realists (like Laudan) who complain that the realist’s use of inference to the best explanation is guilty of the fallacy of affirming the consequent:

If a theory  $T$  is true, then the evidential consequence of  $T$  are true.

The evidential consequences of some theory  $T'$  are true.

Therefore,  $T'$  is true.

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the ‘truth’ and reference of theoretical terms in a particular theory, realists like Boyd (1985) and Musgrave (1985, 1989) have also responded to the contrary.

<sup>3</sup> Similar themes are echoed in a broader sense by Richard Schlegel (1986): “The present work represents a[n]...attempt

I will also mention in passing that such a version of “contextual realism” is a preferred position for a physicist to adopt in these matters, versus positivism. Such a form of realism offers the physicist a means to adopt a strategy for developing theories that circumvent or overcome singularities produced by their theories past. Moreover, it provides a guideline for distinguishing a singularity (as a theoretical artefact) versus its referent, i.e. the phenomenon in (whether critical or otherwise) that the erstwhile theor(ies) referred to and ‘blew up’.

On the other hand, as I argue below, read in a certain way Batterman’s position seems essentially to promote a message of acquiescence: In certain domains concerning critical phenomena, one must cobble together an admixture of methodologies. Contextual realists, on the other hand, see this actual description of the state of affairs that Batterman describes as an impetus to move on and develop theories with greater systematic coherence. This would include, for that matter, theories with mathematical formalisms harboring a greater degree of regularizability (as exhibited by theories characterizable by Clifford algebra).

### Section 1: Clifford Algebraic Reformulations of Physical Theories: A Thriving Research Tradition

Since the latter half of the twentieth century, the reformulation of physical theories (both fundamental and applied) using Clifford algebra has become a thriving research tradition, which is a notion I am adopting from Larry Laudan (1977):

[A] research tradition [RT] is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain... *Research traditions are neither explanatory, nor predictive, nor directly testable.* Their very generality, as well as their normative elements, precludes them from leading to detailed accounts of specific natural processes. (81-82)

In relation to theories RTs generate:

A research tradition, at best, specifies a *general* ontology for nature, and a *general* method for solving problems in a given domain. A theory, on the other hand, articulates a very specific ontology and a number of specific and testable laws about nature. (84)

Laudan developed the notion to provide what he considered was a more satisfactory account of progress than Thomas Kuhn’s.

In his landmark (1962) Kuhn among other things argued that progress can only occur *within* paradigms or “disciplinary matrices” during periods of normal science, basically thought of by Kuhn as when a paradigm achieves hegemony in a particular branch of science. Kuhn has been criticized for

his manifold senses of the ‘paradigm’ notion that he offered, but vis-a-vis Laudan the following notion offered by Kuhn is apropos:

I shall henceforth refer to as ‘paradigms’ a term that relates closely to ‘normal science.’ By choosing it, I mean to suggest that some accepted examples of actual scientific practice---examples which include law, theory, application, and instrumentation together---provide a model from which spring particular coherent traditions of scientific research. (10)

The concept of RTs draws much from Kuhn’s above notion, however Laudan strongly disagreed with Kuhn’s characterization of (inter-paradigmatic) progress as a ‘puzzle-solving’ activity (in which according to Kuhn the ingenuity of the researcher is put to test, but the methodological norms as well as the ontology of the puzzle’s class of solutions is underwritten or constituted more or less by the paradigm). Laudan instead expands on Popper’s (1959) characterization of scientific progress as a *problem-solving* procedure, in which RTs<sup>4</sup> progress via a dual-optimization procedure of maximizing and minimizing their sets of *empirical problems* and *conceptual problems*, respectively:

The solved problem—empirical or conceptual—is the basic unit of scientific progress...*the aim of science is to maximize the scope of solved problems, while minimizing the scope of anomalous or conceptual problems.* (66)

Moreover, Laudan notes further that:

Conceptual problems are characteristic of theories and have no existence independent of the theories which exhibit them, not even that limited autonomy which empirical problems sometimes possess...conceptual problems are higher-order questions concerning the well-foundedness of the conceptual structures (e.g., theories which have been devised to answer the first order questions. *In point of fact, there is a continuous shading of problems intermediate between straightforward empirical and conceptual problems;* for heuristic reasons, however, I shall concentrate on the distant ends of the spectrum. (italics added, 48)

The italicized qualification above is significant for purposes of my essay. I adopt Laudan’s terminology to motivate my discussion toward very particular issues concerning inter-theoretic

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<sup>4</sup> In addition to the ideas of Popper (1959) and Kuhn (1961), Laudan’s notion of RTs draw also much from Lakatos’ (1970) notion of *research programme* (RPs). However, for reasons lying outside the scope and theme of this essay, I mention in passing that Laudan’s nuanced views of the interrelation between theory and evidence (logical entailment, logical consistency, explanation, confirmation) coupled with his objections to what he considered were the excessively rigid aspects of Lakatosian characterizations of RP’s ‘conceptual core’ encircled by an arsenal of ‘anomaly devouring’ host of auxiliary conditions, motivated him to characterize RTs in a far more general and in his opinion flexible manner than Lakatos’ RPs. Disagreeing with Lakatos in terms of the descriptive details concerning accounts of conceptual progress and change however did not entail that Laudan did not agree overall with Lakatosian norms concerning rationality and progress.

reduction and explanation vis-a-vis certain case-studies in applied and theoretical physics discussed by Robert Batterman (2002, 2004, 2005). Certainly, as shall soon become apparent in my essay, when focusing on such specific domains this “continuous shading between straightforward empirical and conceptual problems” is evident. My project is quite different from Laudan’s, however, since I make no broad overarching normative claims concerning the general nature of scientific progress, instead my relatively more descriptive account focuses on what lies *within* such a spectrum.

Nevertheless, in the light of the above qualifications notwithstanding, I argue here (chapter 3 below) that exemplary instances of Clifford algebraic reformulations solving *empirical* problems occur for instance in Scheuermann (2000), and Mann & Rockwood’s (2003) characterization of singularities in CFD (Computational Fluid Mechanics). These comprise a direct response to what I consider are Robert Batterman’s (2005) treatment of singularities arising as brute stumbling blocks in the more standard CFD approaches employing (non-Clifford algebraic) Navier-Stokes approaches. Moreover, general cases of successful conceptual problem-solving procedures, as I shall argue in detail (chapters 2 and 3 below) manifest themselves in the generally robust *regularizability* of Clifford Algebra—a feature of algebraic expansion, contraction, deformation, that successfully circumvent instances of *singularities* that inevitably arise in standard field-theoretic approaches. I characterize precisely such a conceptual problem-solving feature as a form of *methodological fundamentalism*.<sup>5</sup>

## Section 2: What Makes the Study of Such a Reformulation of Interest in Contemporary Philosophy of Physics

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<sup>5</sup> I adopt Jordi Cat’s notion of “fundamentalism,” in which features of one system are explained entirely in terms of rules and factors from some other realm or level of reality (2007, 15). This notion carries with it both epistemic (“explained...in terms of”) as well as metaphysical overtones (“realm or level of reality”). For example, in Carl Hoefer’s (2003) critical response to Nancy Cartwright (1999), he argues that Cartwright has not successfully made the case against the theoretician’s faith in the existence of *fundamental laws*, which exhibit the explanatory scope suggested in Cat’s general characterization. Specifically, Hoefer argues that Cartwright engages in an essentially question-begging procedure, which overlooks the essentially *interpretative* (not necessarily accurately descriptive) feature of the theorist’s ‘fundamental’ faith in systematic interconnection of theories and laws that Cartwright seeks to trounce. His main problem with Cartwright’s critique is not so much a “principled restriction on induction,” but rather “a flat unwillingness to induce anything at all!”(8):

Notice how dangerously close her answer is to the following: We have reason to think that the laws of physical theory hold only in cases where we can show that they hold...[Cartwright’s position, in other words] saddles the fundamentalist with unreasonable reductionist demands. (8-9)

Hoefer concludes:

Cartwright’s patchwork of laws and capacities offers us a picture of science and its possibilities that is very faithful to the current state of theory and practice. That [however, is also]...its weakness: it holds out no reason to think that our deepest explanations can get significantly better (though at least our engineering can)...To engineers and experimentalists, I commend Cartwright’s philosophy of science wholeheartedly. *But I hope to have made space for the theoretician and philosophers of physics to keep their faith in a world with fundamental physical laws.* (13, italics added).

As I discuss in greater detail below and in the ensuing chapters, detailed attention to the processes of mathematical application, experimentation, and general issues dealing with what in a previous period Reichenbach (1938) relegated as descriptive issues pertaining to the “context of discovery” is an obviously well-established fact hardly requiring mention. I nevertheless mention it here in passing to call to attention that this turning away from Reichenbach’s (and other logical empiricists’) particular kind of reductionism, a turning away first initiated of course by Feyerabend (1963), Hanson (1959), Kuhn (1962), etc., *initiated* a trend which has culminated in the present-day pluralism prevalent in the philosophy of physics and other special sciences, as well as philosophy of science in general.<sup>6</sup>

One area of interest in this trend that I focus on in this essay concerns what may be considered a rapprochement between the philosophy of physics and the philosophy of mathematics concerning the issue of choice and application of mathematical formalisms in particular physical theories or theory-complexes.<sup>7</sup> John Burgess (1992) addresses this issue in general terms:

[M]athematics maintains a material unity which is something very different from the formal unity of common set-theoretic foundations...[T]he construction of proof is not like that of a wall, where the bottom course of bricks is laid first, and the next,...and so on. It is more like the construction of an arch or dome, where the topmost piece may be first held in place, as a

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<sup>6</sup> This is also a rather obvious point. Concerning this pluralism Margaret Morrison (2000) points out that philosophy of science ceases to be metascience dictating standards of rationality and instead becomes a practical discipline whose normative force arises out of cooperation with other disciplines (24). Peter Galison (1997) makes a similar point concerning how philosophers and historians should adopt a stance in which they work alongside the manufacturer (the scientist) ‘on the shoproom floor’ as opposed to secluding themselves in the ‘boardroom office’ sketching out ‘general normative blueprints.’ The great advances of this research tradition certainly produced a far more articulated understanding (and most important a means of *normative characterization* in a far more nuanced fashion) concerning essential aspects of methodology, epistemology, and ontology in the *process* of research. Nevertheless, I seat my critical claims and concerns among others’ (Belot (2003), Bishop (2004), Cohnitz (2003), Hofer (2002), Teller (2002, 2004*a,b*), etc., just to name a few) that *some* of the conclusions by those (most notably, Batterman) have mischaracterized aspects of pluralism to the extent that issues concerning unity and unification have been downplayed or misconstrued (hence the theme of the title of this document). I have far more specifically critical points to address (section 4 below, chapter 2, 3) concerning my general concern mentioned above (and also, for that matter, what my general and specific sympathies are given issues raised by Batterman and others).

<sup>7</sup> Mark Steiner’s (1998) *The Applicability of Mathematics as a Philosophical Problem* is an exemplary instance. He argues that more work should be done in the philosophy of mathematics concerning the issue of *applications*, as opposed to just the issue of *foundations*. He complains that on the one hand, philosophers of physics and physicists themselves are busily engaged in developing and refining normative criteria concerning choices of various and sundry sophisticated algebraic and topological classes of formalisms (whether  $C^*$ , Clifford, etc.) and on the other hand, when reviewing the literature in the philosophy of mathematics, one finds mostly work done in meta-mathematics and foundational questions concerning the status of ‘grounding’ the integers, etc. Regardless of how obviously significant the latter research tradition has proven itself to be, and continues to, Steiner for one perceives a yawning gap between foundational questions and issues concerning applications that he charges the philosopher of mathematics should strive to fill. His study of classes of analogies (“Pythagorean” and “doubly-Pythagorean”) that he argues characterize questions concerning modeling and application is his attempt to partly bridge this gap.

conjecture, by intuitive or inductive considerations, and then various intermediate supports are found and installed...the process is indeed rather like the kind of construction that goes on in empirical science. (434)

This reference to abductive reasoning occurring in the methodology of proof-construction is precisely where Burgess believes recent methods of research in the philosophy of science, which often co-op the methods of cognitive science, can inform philosophy of mathematics. Burgess continues:

A different 'new direction' of inquiry attends less to the building and its builders, the mathematicians, than to its users or inhabitants, the scientists who *apply* mathematics...The various new directions I have been indicating in this Homeric simile mostly belong to 'cognitive studies' in a broad sense...The sustained pursuit of any of them, however, requires an acquaintance with the content and methods of mathematics...indeed many of the most interesting explorations have been the work of professionals who are *amateurs* (in the original, favorable sense) of philosophy...[Nevertheless] [t]he 'renaissance of empiricism' in philosophy of mathematics could easily go...too far in the direction of assimilating mathematics to other sciences...results from mathematics having a unique methodology [can] get ignored. (434-435)

What Burgess proposes is a methodological rapprochement with aspects in the philosophy of science and mathematics which any kind of project proposing to 'naturalize' mathematics. As the last passage suggests, he maintains that the 'material unity' of mathematics differs from any knowledge domain in science (whether presumed to be unified or not) as a matter of *kind*, not just degree. One can develop methodologies that are mutually derived from philosophy of science and mathematics without conflating their domains of study, whether in epistemology or ontology.<sup>8</sup>

[S]ince mathematics is itself an important science and has important applications to other sciences, general philosophy of science cannot ignore or set aside the case of mathematics as special. A philosophical account of science that succeeds only insofar as mathematics is not involved does not succeed at all. (438)

According to Burgess, the foundational question concerning classical versus constructive mathematics<sup>9</sup> informs the philosophy of science in a novel way concerning the choice of

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<sup>8</sup> Whether or not Burgess is successful in his general claims for outlining his project is a question I will not explore here. I merely draw general inspiration from his views and show how my project can be seated in such a context he proposes, in a more specifically concrete manner, concerning the issue of Clifford algebra.

<sup>9</sup> Including all the associated meta-mathematical presuppositions. Classical mathematics, of course, can be generally conceived of as being fundamentally underwritten by 'classical logic,' often characterized precisely

characterizing a physical theory with a mathematical formalism. Concerning the issue of classical versus constructive mathematics, the  $P \neq NP$  conjecture<sup>10</sup> entails that:

The question whether even *classical* mathematics is sufficient for applications remains in a peculiar but genuine sense open...For were it never so firmly established that 100% of present-day applications can be accommodated by some version of constructivism, *this would still leave us wondering whether this is because non-constructive mathematics is inherently inapplicable, or rather because we have not yet been clever enough to apply it.* (439, italics added)

In other words, as indicated by the italicized portion above, the ‘peculiar but genuinely open’ question here concerns our lack of sufficient information guaranteeing whether or not the inherent applicability of non-constructive mathematics is a research *puzzle*, or a genuine problem, phrased in Kuhn’s terms.<sup>11</sup>

Concerning the issue of the applicability of constructive versus classical mathematics, Burgess proposes that the philosopher of science should consider two theories  $T^*$  and  $T$  equivalent in scope of empirical adequacy, respectively characterized by non-classical and classical mathematics. Building such a  $T^*$  (vis-a-vis the  $T$  scenario) requires the *mathematical* notions of ‘general transformations’ between them:

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according to (sound and complete) FOPL (first order predicate logic) with its fourteen rules of inference (eight introduction and elimination rules for  $\neg$ ,  $\wedge$ ,  $\vee$ ,  $\rightarrow$ , four introduction and elimination rules for  $\forall$ ,  $\exists$ , along with EFSQ: *any* proposition can follow from a premise list containing a contradiction and the Double Negation rule: for any sentence  $\phi$ :  $\neg\neg\phi \vdash \phi$ .) Constructive mathematics is underwritten by a weaker (Intuitionist) logic, which rejects the double negation rule. For ‘classical’ works in constructive mathematics, see Errett Bishop (1967). For more recent studies concerning the choice of alternative logics (whether Intuitionist or otherwise) in the sciences, see Paul Weingartner, ed (2003).

<sup>10</sup>  $P$  is the set of all decision problems which can be solved by algorithms that are deterministic (i.e., possess no random or arbitrary choice procedures at any of their step in execution) and “*poly-time*” (short for “polynomial time complexity:” their complexity in execution is bounded above by some power-form  $n^m$  : where  $n$  is the number of input parameters in the algorithm, and  $m$  is some positive integer).  $NP$  on the other hand is the set of all problems whose solutions can be *checked* by poly-time algorithms, which need not be deterministic. “Problems in  $NP$  can have algorithms that search in a non-deterministic manner for a solution” (Hein (2002), 810). While it is obvious that  $P \subseteq NP$  : For every decidable problem whose solution can be characterized by a non-deterministic poly-time algorithm  $A$ , there exists a trivial construction to show that the *correctness* of solution  $A$  can be checked by some poly-time algorithm  $A'$  which need not be deterministic. The proof by construction guarantees the existence of such an algorithm  $A'$ , whose details I omit here. On the other hand, no one has been able to come up with a counter-instance (some  $NP$  problem which isn’t  $P$ ) which would render the inclusion strict (i.e.  $P \subset NP$  or  $P \neq NP$ ). Decision problems are all based on classical logic—the  $P \neq NP$  conjecture is found in most textbooks of advanced discrete *classical* mathematics—hence like the continuum hypothesis such a conjecture renders the issue of choice of mathematical formulation in a fundamental and concretely *practical* sense peculiarly open.

<sup>11</sup> Puzzles test the ingenuity of the researcher and are the hallmark of normal science. Problems, manifesting oft as anomalies, test the integrity of a paradigm and are the hallmark of revolutionary science. Perhaps one can regard the  $P \neq NP$  conjecture as anomalous from the paradigm of classical mathematics.

The obvious strategy would be to look for *an alternative to current theory differing in its mathematical form* [i.e.  $T^*$ ]...since most alternatives to current mathematics have already been proposed, the obvious strategy would be to look whether those alternatives to our current mathematics would in principle be equally usable for applications...[For example] [i]f it can be shown that some version of constructivism...would (even if only in principle) be sufficient for applications, *then that is one fairly concrete way of showing that the mathematics we have currently arrived is not one we were, literally or metaphorically, divinely foreordained to arrive at.* (440, italics added)

The theme of historical contingency that Burgess suggests in the passage above concerning the issue of the choice of classical mathematics is one I adopt and refine in my argument below, concerning the choice of mathematical formalisms, whether Clifford-algebraic or otherwise. As my next section explains below, historical accident (the relatively early death of W. K. Clifford) coupled with the relative intricacy of Clifford algebra compared to the vector-analytic methods concurrently introduced by Gibbs, provides some explanation of why the research tradition of characterizing physical formalisms via Clifford algebra smacks somewhat of a ‘rediscovery’ and even as a renaissance for this particular class of formalism.

More importantly however, I show by way of example (chapter 3 below) how characterizing a fluid mechanical theory via Clifford algebra (analogous to Burgess’ general remark concerning characterizing some physical theory  $T^*$  via constructive mathematics) resolves more conceptual problems and minimizes empirical problems (Laudan 1977) than its empirically adequate alternative (i.e., analogous to  $T$ , characterized by Navier-Stokes continuum methods).

Last of all, I add here by way of historical analogy how the algebraic characterization of geometric notions by Descartes fundamentally and methodologically transformed the emerging sciences of modern mechanics. Ironically, David Hestenes (1985, 1986) suggests adopting the honorific ‘geometric’ to Clifford algebra, reflecting his self-professed ‘Cartesian’ intuition to ‘geometrize’ a physical concept whenever possible (or whenever in doubt concerning its meaning).<sup>12</sup> Of course, it bears emphasizing that the contemporary notion of ‘geometry’ is far more general and abstract than it was in Descartes’ times. Nevertheless, certain basic intuitions functionally carry over (points, lines, planes, etc., albeit stripped of their metaphysical and logical significance as having to be isomorphic with substances, as 17<sup>th</sup> century mathematicians like Descartes thought).

My underlying point here in this historical analogy is that *the Cartesian revolution in mathematical physics methodologically transformed physics in such a manner as to have irreversible*

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<sup>12</sup> This ‘Cartesian’ intuition is shared by many contemporary esteemed mathematical physicists. See, for instance, Sir Micheal Atiyah (2001).

*ontological and epistemological consequences.* Though this seems like an obvious point, it still merits restatement. To name one example, prior to the advent of Descartes’ ‘analytic geometry’ in the spirit of Euclid and Archimedes, most medieval and early modern physicists expressed laws in terms of ratios, *but were loath to transform such ratios into products by way of what is denoted now as ‘cross-multiplication’.* For example, Archimedes’ lever principle<sup>13</sup>:  $W_1 : W_2 = L_2 : L_1$  was not transformed into a product  $W_1 L_1 = W_2 L_2$  “because a product [WL] did not exist in [Greek] mathematics.”<sup>14</sup>, (Bochner, 1963, 180). As Bochner mentions further:

In modern physics, if [W] is a weight and L is a length, then [WL] is the so-called statical momentum.<sup>15</sup> *The express formation of this momentum and other mechanical and then electrodynamical momenta has been a most creative aspect in the unfolding of modern physics.* Archimedes has this momentum in his context, but something in the metaphysical background and ambient of his thinking barred him from conceptualizing it overtly and ‘operationally.’ Generations of physicists after him...were groping for the statical momentum but it kept eluding them for over nineteen hundred years. The first clear-cut formulation of a momentum occurs in [post Cartesian] Newton’s *Principia*. (181)

By the same token, I maintain that Batterman places excessive emphasis on singularities gleaned from formalisms that are not geometric (Clifford) algebraic. The regularizable characteristics of Clifford algebras go a long way to dispense with such singularities, and may introduce many hitherto unknown vistas of interest to physicists and philosophers of physics. This is just one example of why mathematicians and physicists promote them, as I argue in the sections below.

### Section 3: Clifford Algebra: A Brief Historical Overview and Summary

The Cambridge mathematician William Kingdon Clifford originally developed his algebra<sup>16</sup> in the years 1878-1882 as a means to systematically develop a matrix algebra representing rotations and spin, generalized in any  $n$ -dimensional space:  $R^n = \{(x_1, \dots, x_n) | x_k \in R, 1 \leq k \leq n\}$  (where  $R$  are the real numbers). In keeping with Clifford’s intentions, Hestenes (1984, 1986) and others ascribed the term ‘geometric’ to such classes of algebras to call attention to the primary feature of this mathematical

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<sup>13</sup> That the ratio of the weights on a lever in equilibrium is the inverse of the ratio of the lengths of their fulcra.

<sup>14</sup> In more general terms, ontological assumptions blocked mathematical methodology such as the formation of polynomial expressions like  $x + x^2 + x^3$  since the first term represented a length magnitude, the second an area, and the third a volume. It made no sense for pre-Cartesians to ‘superpose’ (add) concepts like length, area, and volume. (Damerov, et. al., 1992)

<sup>15</sup> Otherwise known as the *torque* induced by the weight force.

<sup>16</sup> A vector space endowed with an associative product. For further details, see Appendix below.

system, portraying the class of all possible *rotations* (and spins) in  $n$ -dimensional space, which is an essentially geometrical dynamical property.

Geometric algebras can be fundamentally thought of as systematic collections of *directed* line segments (vectors), areas (bivectors), volumes (trivectors), ...,  $n$ -dimensional hypervolumes ( $n$ -vectors or  $n$ -blades) as bounded above by the dimensionality  $n$  of the algebra's underlying vector space. While the concept of a directed line segment seems intuitive enough (partly due to the historical success of the 'rival' vector algebra of Gibbs), the concept of directed surfaces, volumes, and hypervolumes may seem less so. The concept of directed area however survives, for instance, in the geometric interpretation of a vector cross-product in  $R^3$ . As a further indication of its vestigial ancestry to Clifford, the cross-product is actually an example of a bivector, or *axial vector*, as it changes sign under reversal of parity of the coordinate system (from a left-handed to a right-handed system, and vice versa) while regular vectors do not.

Clifford algebras are *graded*: their generators form a basis of linearly independent  $k$ -vectors (where  $0 \leq k \leq n$ ), where  $n$  is the dimensionality of the underlying vector space. For example, the Clifford algebra  $G(R^3)$  over vector space  $R^3$  is generated by a total of  $2^3 = 8$  grade  $k$  elements (where  $0 \leq k \leq 3$ ): 1 grade-0 element (the real scalars), 3 grade-1 elements (3 linearly independent vectors whose span is obviously  $R^3$ ), 3 grade-2 elements (3 linearly independent bivectors), and 1 grade-3 (trivector) element. In general, for any vector space  $V$  of dimensionality  $n$ , its Clifford algebra is generated by a total of  $2^n$  grade  $k$  elements (where  $0 \leq k \leq n$ ), the dimensionality of each Clifford subspace of uniform grade  $k$  is:  $C(n, k) = \frac{n!}{k!(n-k)!}$ . That is to say,  $C(n, k) = \frac{n!}{k!(n-k)!}$  linearly independent grade- $k$  (or  $k$ -vector) elements generate the Clifford subspaces of uniform grade  $k$ . In addition, the (associative) Clifford product can be decomposed into a grade-lowering (inner) product and a grade-raising (outer) product, from which the notions of dot and cross products survive in the standard (Gibbs) vector algebra of  $R^3$ . For further details, see Appendix below.

After being eclipsed into relative obscurity for almost a century by Gibbs' vector notation,<sup>17</sup> the Clifford algebraic mathematical formalism (as well as its associated algebraic substructures like the Clifford groups) has enjoyed somewhat of a renaissance in the fields of physics (both purely theoretical as well as applied) and engineering in the last several decades. (Baugh 2003, Baylis 1995, Bolinder 1987, Conte 1993-2000, Finkelstein 1999-2004, Doren & Lasenby 2003, Gallier 2005, Hestenes 1984 -1986, Khrenikov 2005, Lansenby, et. al. 2000, Levine & Dannon 2000, Mann et. al. 2003, Nebe 1999-2000, Scheuermann 2000, Sloane 2001, Snygg 1997, Van den Nest, et. al. 2005, Vlasov 2000). All the authors listed above (who comprise just a miniscule sample of the enormous body of literature on the subject of applications of Clifford Algebra in physics and engineering) either

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<sup>17</sup> As explained in the Appendix below, vestiges of Clifford's notation and algebra survive in the concept of Pauli and Dirac spin matrices, as well as the notion of a vector cross-product.

describe the mathematical formalism as especially appealing, due to its providing a ‘unifying language’ in the field of mathematical physics<sup>18</sup>, or apply the formalism in key instances to make some interpretative point in the foundations of quantum theory, no matter how specific<sup>19</sup> or general.<sup>20</sup>

Clifford algebras can provide a complete notation for describing certain phenomena in physics that would otherwise require several different mathematical formalisms. For instance, in present-day quantum mechanics and field theory, a variety of different mathematical formalisms are often introduced: 3 dimensional vector algebra, Hilbert space methods, spinor algebra, diffeomorphism algebra on smooth manifolds, etc. This is due in part to the domain-specific nature of the aforementioned, all tailored to apply to a particularly specific context, but relatively restricted in their power of generalization. In contrast, as shall be shown below, Clifford Algebra provide a single and overarching formalism that can meet the needs of the mathematical physicist working in the applied as well as in the foundational domains.

#### Section 4: ‘Post-Standard’ Accounts in the Philosophy of Physics

Based on Mathias Frisch’s survey in chapter 1 (2005), I draw a distinction between the “standard” versus “post-standard” accounts in the philosophy of physics in terms of the role played by models in any scientific theory as described by the respective traditions. In the Standard account, (either in the ‘syntactic’ or the ‘semantic’ traditions<sup>21</sup>), the notion of “model” is denoted by a model-theoretic sense in which a structure bears (truth-conditional) relations to a *set of sentences*. The latter are taken to be the theory’s axioms or laws.<sup>22</sup>

Models, however, can also be understood as structures bearing (representational) relations to *sets of phenomena*<sup>23</sup> characterizing “post-standard accounts.”<sup>24</sup> In this tradition, models are understood as providing an intermediary layer between a theory’s laws and the “world” of phenomena. The existence of models can at best be understood as somewhat independent of the existence of a theory’s laws. “Building testable models, according to the [post-standard account] ...usually involves highly context-dependent idealizing and approximating assumptions, and often requires appealing to

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<sup>18</sup> E.g., Finkelstein, Hestenes, Lasenby

<sup>19</sup> E.g., Conte, Hogreve, Snygg

<sup>20</sup> E.g., Hiley, Khrenikov, Vlasov

<sup>21</sup> The view that scientific theories are best represented by deductively closed sets of sentences, (Hempel, Carnap, etc) versus the view holding that theories are best represented by sets of *models*, non-linguistic structures in which a theory’s axioms or laws all hold true (Suppes, vanFraasen, etc.)

<sup>22</sup> In the early syntactic traditions, it was thought that such sentences could in principle be regimented in FOPL (first order predicate logic).

<sup>23</sup> Much confusion can arise when equivocating the two senses. “If one is not careful in drawing this distinction, it will probably strike one as somewhat mysterious how an inconsistent theory which has no *model-theoretic* models can nevertheless provide us with *representational* models of the phenomena.” (Frisch (2005) 6)

<sup>24</sup> Cartwright (1982, 1999), Giere (1988), etc.

assumptions from...sometimes incompatible theories.” (Frisch (2005)10) In this respect, high-level abstractions, or the theory’s “laws,” should be thought of as “tools for model-building, rather than as representative of structures of the world.” (11)

This latter view allows one to accept a particular theory based on its models’ *reliability*, rather than forcing a commitment to the *literal truth* of the empirical consequences of the theory as implied by its laws.(42) I will focus on the issues of reliability versus literal truth in greater detail in §1.4 below, in the discussion on verisimilitude and ontology. The standard and post-standard views of physical theories, whether advocated by philosophers or physicists, appear *prima facie* to be governed by conflicting foundational versus pragmatic aims.<sup>25</sup> The foundationalist aims to provide a coherent account for the possible ways the world can be: a theory therefore must provide a set of fundamental laws which would govern the behavior of all possible classes of phenomena in a particular domain. The pragmatist, on the other hand, aims to provide a practical formalism: a theory’s laws can then be applied to model *specific* phenomena.<sup>26</sup>

One of Mathias Frisch’s central claims is that, in the case of the theory of classical electrodynamics,<sup>27</sup> *it is not even possible to pose such a distinction*. “[E]ven at the highest theoretical level of deriving an in some sense principle or general equation of motion governing the behavior of charged particles, pragmatic considerations enter.” (68) Consequently, Frisch suggests to the philosopher (whether working in the standard or post-standard tradition) to adopt a *principle of charity* when analyzing notions like “fundamental,” and “unity,” etc. as used by the workaday physicist:

As philosophers we might be tempted to think that physicists are simply confused when they speak of an appropriate equation as ‘fundamental,’ ‘correct,’ or even ‘exact.’ *This, however would mean imposing a philosopher’s rigid conception of theories on science rather than*

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<sup>25</sup> As I mention in §1.4. below, the distinction is misleading in ways confirming Frisch’s assessment in the case study of classical electrodynamics.

<sup>26</sup> Such clearly opposing aims can often occur in the rubric of the same theoretical tradition. For instance, quantum field theory (QFT) is distinguished by perturbative versus non-perturbative methods (Atiyah (2001)). The former is fundamentally constituted by series methods in which experimental parameters are compared with their (approximate) perturbation series expanded to an arbitrary degree of precision, whose latter terms can be obtained by variously powerful (and ultimately approximate in nature) simplification methods employing Feynman diagrams. On the other hand, (non-perturbative) topologically-based methods culminating in gauge theories arose out of attempts to obtain fundamental structural information of the microworld from QFT. The aim of perturbation series-based QFT is thoroughly pragmatic, insofar as its primary goals involve the modeling of specific phenomena in such a manner respecting practical aims of computational efficacy, irrespective of representational accuracy—let alone *consistency*, as renormalization methods inevitably reveal (Kallfelz (2005a) and in n. 46, below). On the other hand, gauge theories attempt something far more ambitious: to provide generally metaphysically accurate descriptions, as far as QFT will allow, of the microworld of high-energy quantum phenomena, independent of and prior to comparisons made by specific measurements. Consistency would certainly prove itself to be a more important guideline in this latter case should one hope to obtain any generally fiducial representation of such microphysical phenomena that the scope of QFT will ultimately allow. (Though, as Frisch (2005) shows in his study of classical electromagnetism, this guideline is by no means guaranteed to be achievable, let alone even *possible* in certain cases).

<sup>27</sup> The prototypical field theory.

*trying to understand the practice of theorizing... we should [examine]... which sets of equations physicists themselves take to be the most basic and important in a certain domain, and then ask what criteria of theory-choice would allow us to make sense of the physicists' decisions... [W]e should adopt a principle of charity and interpret the physicists' claims in a way that makes them defensible... [for instance, a theory's] internal consistency does not come out as a necessary condition governing theory choice, since considerations of simplicity, mathematical tractability, and conceptual fit appear to be able to override concerns for strict logical consistency. (italics added, 70-72)*

The above-mentioned insights and arguments posed by Frisch (2005) offer a useful conceptual framework for contemporary philosophy of physics, both descriptively and normatively. For instance, the ontological autonomy of models (as representations mediating phenomena and a theory's high-level laws), coupled with a pragmatic concern for their *reliability* (as opposed to *literal truth*), makes an essential contribution to the pluralism characteristic of so much contemporary philosophy of physics—in turn so significantly influenced by post-standard accounts.

To name a few recent examples: Margaret Morrison (2000a) argues (*pace* Kitcher) that unification should be considered as a process separate from explanation. The physicist “unifies first, explains later.” Robert Batterman (2002) argues that reduction and explanation should likewise be considered as separate, and argues (*pace* Hempel) that a species of ‘asymptotic explanations’ indicate that the superseded (or reduced) theory  $T$  still somehow plays a necessary role vis-à-vis the superseding (reducing) theory  $T'$ . In explanations involving asymptotes and critical behavior, the ‘old’ theory  $T$  doesn't get completely reduced by the newly superseding theory  $T'$ , but continues to play an essential role.<sup>28</sup>

In subsequent work (2004-2005) Batterman sunders notions of ‘fundamental’ by arguing that *ontologically* versus *epistemically* fundamental theories act at cross-purposes: the former seek to give a metaphysically accurate account of phenomena at the expense of explanatory efficacy, while the latter do exactly the opposite. For example, in the case of fluid droplet formation, one may appeal to the ontologically approximate Navier-Stokes theory, which models the fluid as a continuum, to account for the universally regular features of droplet formation shared by all classes of fluids of varying density. The Navier-Stokes theory, in short, is epistemically fundamental: It is able to provide a universal account of scale-invariant features of certain critical phenomena *only by hiding* the underlying ontology. The fluid, after all, fundamentally consists of a discrete collection of molecules. Any ontologically fundamental theory modeling the fluid from this accurate level of description, aside

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<sup>28</sup> “The superseding theory  $T'$ , though ‘deeply containing  $T$ ’ (in some non-reductive sense) cannot adequately account for emergent and critical phenomena alone, and thus enlists  $T$  in some essential manner.” (Kallfelz (2006), 3)

from becoming computationally intractable, would, by its very nature of describing the *particular ontological details*, sacrifice the very possibility to provide universal or scale-invariant descriptions of droplet-formation. Conversely, the epistemically fundamental theory is able to capture universal features so well precisely *because* of its approximate representation of the fluid as a continuous medium.

The authors I have cited above, among many others, can be thought of falling into the post-standard account tradition insofar as they focus their primary interest on the *modeling activity* of the physicist, in the *non-model theoretic sense*. They approach physical theories from the ‘bottom-up,’ beginning with a careful study of the reliability of the theory’s models, to make their generally pluralist claims.<sup>29</sup> Moreover, in the normative sense, they all seem to implicitly adopt the “principle of charity” in varying degrees.

I offer a critical response to Robert Batterman’s claims which take into account the essential modeling and theorizing activity of the physicist—in short, with objections respectful of Batterman’s own terms. This essentially involves the use of geometric (or Clifford) algebraic formalisms, which appear, as I argue to unify ontological and geometrical content in certain theoretical frameworks in a more efficacious manner than their non-Clifford counterparts. Such unity calls into question some of the pluralist inferences made by Batterman, in his analysis of explanation and reduction in his case studies of phase transitions and critical phenomena.

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<sup>29</sup> For instance Cartwright (1999) argues not only against forms of ‘top-down,’ but also against ‘cross-wise’ reductionism. That is to say, not only does she dispute the legitimacy of inferences made from the level of a theory’s high-level laws to the level of empirical applications, but also ‘across’ from the concrete contexts of controlled laboratory conditions to the world of (relatively uncontrolled) phenomena. Recall Hofer’s response in n. 5 above.

## Dedication

*I dedicate this first and foremost to my family: to my mother, Elfriede Kristwald, for all the years of her continuing and steadfast support and friendship. To my sisters: Catherine Munsen (and family), and Carol Bershard, with much affection. To my father: John Michael Kallfelz (d. 1997) who in my formative years always urged me to apply myself in school—you will be remembered.*

*I would also like to dedicate this posthumously to W. K. Clifford (b. 1845-d. 1879) who as a mathematician and as a philosopher might be pleased to see his algebra applied to a philosophical problem.*

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# Chapter 1: A Brief Overview of the State of Unity, Reduction, and Explanation in Contemporary Philosophy of Physics

## Section 1: Preliminary Remarks

In this chapter I present a critical review of what I consider are some of the underlying issues and implications concerning unity, explanation, and reduction that motivate Robert Batterman's (2002, 2004, 2005) basic claims. I give the topic of unity the most cursory treatment, as clarified in my disclaimer in the subsection below. Aside from introducing to the reader what I consider are the most salient features of Batterman's conclusions (before launching into more detailed critique of them, as I do in chapters 2 and 3 below) I also intend to deflate some of their normative force. Regarding his theory of 'asymptotic explanation' articulated in (2002), I point out in §2.1 what I consider are common weaknesses his version shares with the 'dynamical explanation' model of Ruth Berger (1998). I also argue that 'asymptotic explanation' can be subsumed under Fritz Rohrlich's (1994) 'covering theory model' (CTM) in §2.2.

I reserve however the lengthiest discussion on the topic of inter-theoretic reduction (§3). After presenting a more detailed description of Rohrlich's model of inter-theoretic reduction (§3.2) I argue that Batterman's model can essentially be subsumed under Rohrlich's (§3.3). In the final section §3.4 I present a more detailed discussion of the issue of ontological ramifications suggested explicitly by Rohrlich's 'pluralist ontology' (and conversely implied by Batterman) vis-a-vis issues centering on verisimilitude and contextualism.

This chapter presents a brief critical overview of the ideas of Robert Batterman. It does not relieve me of the onus of an internal critique thereon, which (as mentioned in the *Foreword* above) I launch in chapters 2 and 3, using Clifford algebra as the primary counterexample. Nevertheless, the reader can consider this chapter as self-contained to the extent that it offers a more 'external' critique of Batterman focusing on conceptual content alone.

### **Subsection 1: Disclaimer Concerning Unity**

Research into issues such as unity, explanation, and reduction form a set of aims in the philosophy of physics, as well as in the philosophy of science proper (both past and present). Like in the notion of the trinity, unity, explanation, and reduction would appear to be 'consubstantial' at some fundamental level of inquiry, though 'distinct' insofar as they derivatively exhibit unique research aims as well as begetting unique research methods and modes of inquiry. Perhaps it is best to consider this 'trinity' as characteristic of aspects of complex, central questions and concerns in the philosophy of science (and in philosophy proper). What could this central concern be?

[W]hat kinds of unity in the sciences are there[?]: [I]s unification a relation between concepts or terms (i.e., a matter of semantics) or about theories they make up? And is the relation one of reduction, explanation, or logical inference?

This is Jordi Cat's opening question in his review article: "The Unity of Science" (2007). He continues:

[From the standpoint of] the often-assumed preference for physics as a privileged locus...from the fact that [it]...is the study of the most fundamental units of matter and energy [does anything follow about the presumed unity of science?] ...[Conversely] [h]ow should we evaluate the evidence for disunity and pluralism in science?...To what extent should we supplement the attention to logic and language with an interest in practices, images and objects?...[I]t is worth pointing out that *positions about the unity of science have important consequences, and affect the way we formulate and solve problems in philosophy* (e.g., questions of naturalism), *science* (e.g., design of education and research projects), and *policy* (e.g., allocation of resources). (1-2, italics added)

That the issue of the unity of science is a live and urgently pressing one is furthermore evidenced in the history of Western philosophy: such questions concerning the unity of science and empirical knowledge<sup>30</sup> go back to the pre-Socratics<sup>31</sup> and form a continuous thread that weaves throughout the writings of Plato and Aristotle,<sup>32</sup> the later Christian monotheists,<sup>33</sup> and of course finally in the emergence of modernist thought and the Rational mechanics of the seventeenth and eighteenth centuries<sup>34</sup> (Cat (2007), 2-3).

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<sup>30</sup> Broadly characterized in Antiquity as a complex mixture of metaphysical assumptions and empirical epistemic notions, 'empirical knowledge' loosely conceived as reduced to or restricted to what one gathers from or reflects upon experience. This notion of 'empiricism' of course cannot be conflated with the metaphysically charged and strongly reductionist position emerging in 17<sup>th</sup> century British thought. Likewise, it is important to mention that this issue in particular, perhaps unlike no other, unites philosophy of science with epistemology. See in particular the Introduction (pp. 3-24) in Paul Moser (2002).

<sup>31</sup> "In what senses are the world and our knowledge of it, one?" (Cat (2007), 2) appears to be the operative question running throughout the writings of pre-Socratics. In turn Thales, Parmenides, Empedocles, Heraclitus, Democritus responded to the question with their own renditions with their associated ramifications of reduction, unity, and plurality in their respectively epistemic and metaphysical senses.

<sup>32</sup> "Knowledge also is surely one, but each part of it that commands a certain field is marked off and given a special name proper to itself. Hence language recognizes many arts and many forms of knowledge." (*Sophist* 275c). According to Aristotle (*On the Heavens*) knowledge is of what is 'primary'. Different 'sciences' may catalog knowledge of different kinds of causes, but metaphysics "comes to provide knowledge of the underlying kind [of cause]." (ibid.)

<sup>33</sup> For the Scholastics, organization of knowledge reflected a notion of the world governed by Divine Law. This notion was in particular distinctly invested in the search for a universal language characterizing the project of unifying knowledge, as evidenced in the *Etymologies* of the 6<sup>th</sup> Century Andalusian Isidore, or in the later writings of Ramon Llull, St. Augustine, etc.

<sup>34</sup> The Rationalist element of Modernism and its assumption of the unity of knowledge emphasized the faculties of human reasoning as "the project of a universal framework of exact categories and ideas, a *mathesis*

It is also safe to admit that research into the question of the unity of science, vis-a-vis reduction and explanation in the philosophy of physics and science, has burgeoned and blossomed in breadth and depth in the last few decades in ways unforeseen by the logical empiricists Carnap (1956, 1966), Hempel (1962, 1965 1988), Kenemy & Oppenheim (1955), Nagel (1974), etc., who all offered their original theories thereon, often creating frameworks for future research traditions.<sup>35</sup> By and large, many contemporaries call the notion of unity into serious question, advocating some methodological, epistemic, or metaphysical pluralism (and any combination thereof, as the “isms” are obviously intertwined) instead.<sup>36</sup>

It is not a trivial task to survey this ‘dappled’<sup>37</sup> landscape of recent literature from any vantage point advocating any claim(s) for unity, however weak they may prove themselves to be in the final analysis. It is best to proceed in a cautiously (but systematically) selective manner.

In this essay, my disclaimer is that I will make no constructive or direct claims concerning the issue of unity (whether in physics, science, or in scientific knowledge generally speaking), from the “top down,” or derived from some general and abstract metaphysical stipulations or assumptions. Instead, in a “bottom-up” style I will focus almost exclusively on issues of unity, vis-a-vis reduction and explanation, by critiquing Batterman’s recent claims (2002, 2004, 2005) in his concrete case studies involving critical phase transition phenomena in this chapter and in chapters 2 and 3 below. Additionally in § 3 below, I “reconstruct” Batterman’s claims within the context of a broader framework comprised by recent arguments of a similar nature concerning reduction and explanation made by Aertz & Rohrlich (1998), Bialkowski (1985), Dieks & de Regt (1998), Ehlers (1986), Ramsey (1995), Rohrlich & Hardin (1983), Rohrlich (1988, 1994), Teller (2004b), and Wojcicki (1998). Such a reconstructive exercise indicates that some of the extreme methodologically and epistemically pluralist notions advocated most notably by Batterman and some of his contemporaries<sup>38</sup> prove unwarranted, and some notion of methodological unity can be retrieved—precisely through the

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*universalis*,” as echoed in Descartes’ methodology and Leibnitz’s General Science (*characteristica universalis*) (3). Certainly during the Enlightenment, belief in the unity of knowledge and the universality of rationality was at its apex, epistemically echoing the metaphysical and methodological ideals of the golden age of mechanism. Mechanistic philosophy (as a systematization of basic concepts and fundamental laws of mechanics) became the framework of choice for the unification of natural philosophy, whose crown jewel became Laplace’s “molecular physics,” with its strict determinism and reductionism. After the demise of Laplacian thought in the early 19<sup>th</sup> century, the role formerly occupied by mechanistic philosophy was supplanted by ether mechanics and energy physics.

<sup>35</sup> For one, Cat describes the structuralist characterizations of inter-theoretic reduction of Ehlers (1986), Scheibe (1983, 1997, 1999), and Sneed (1971) as essentially “neo-Nagelian” (2007, 16).

<sup>36</sup> Methodological and epistemic pluralists include Robert Batterman (2002, 2004, 2005) who is the primary critical focus of my essay, as well as Morrison (2000a, 2000b). Other pluralists of an essentially metaphysical variety include Cartwright (1999) and Dupre’ (1993).

<sup>37</sup> Borrowing self-consciously from Cartwright’s *The Dappled World* (1999) as well as some of her critical respondents: Sklar (2003), “Dappled Theories in a Uniform World,” and Teller (2004a) “How We Dapple the World”.

<sup>38</sup> See note 10 above.

‘unifying’ aspect of the Clifford algebraic research tradition, an aspect I precisely characterize as an instance of a “methodologically fundamental” procedure in chapter 3 below.<sup>39</sup>

## **Subsection 2: The General Framework of My Study: Fritz Rohrlich’s Classification of Theories.**

Having made the general disclaimer concerning unity, i.e. a topic I will *not* focus on directly, herein I make my positive disclaimer concerning the overall framework and domain of my study. In summary: the ideas of Robert Batterman (2002, 2004, 2005) are my primary critical target while the ideas of Fritz Rohrlich (1988, 1994) are my primary inspiration. In §3, for example, I show how Batterman’s conclusions and assessments can be reconstructed and characterized differently according to the general framework of reduction and explanation offered by Rohrlich and some of his derivative work—including some from his direct commentators as well as from those who have further developed aspects of his ideas (Aerts & Rohrlich (1998) et. al., as mentioned in the previous page). To recall the discussion in the fourth section of the *Foreword* above (pp. xv-xxi), the post-Standard intellectual climate gives the philosopher of physics a broad variety of options from which to focus his or her domain of study. Viewing physics (and science in general) as constitutive of an essentially model-forming *activity*, among other things, allies the philosopher of physics with the physicist.<sup>40</sup>

Nevertheless, I limit the discussion here to such ‘products’ of physics in the form of well-developed ‘textbook-ready’ theories—specifically in the realm of fluid mechanics and with an occasional mention of some versions of theoretical particle physics (Finkelstein); the mathematical form is sufficiently developed enough to be characterized by Clifford algebra.<sup>41</sup> In any case, as I discussed (§ 4 of the *Foreword*) the post-standard tent is broad enough to accommodate its methodologies equally well to the study of experimental procedures and instrumentation (and their

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<sup>39</sup> Lest I am guilty of committing a fallacy of dichotomy, I am *not* using my concluding claims to make some general case of a grand notion of unity. The legacy of late twentieth and early twenty-first century philosophy of physics teaches one to know better. Rather, I am tempering some of the extreme aspects of the pluralists’ claims which, if one adopts Frisch’s (2005) “principle of charity” towards the Clifford-algebraic inspired physicist, seem *prima facie* to clash with the Clifford algebraic method of unifying ontological and geometric content. I will say more about this tension in the subsequent sections and chapters.

<sup>40</sup> It scarcely requires mention that none of the above implies a slide towards scientism, i.e. a position which among other things holds philosophy of science ‘hostage’ to whatever fashionable (and more often than not) philosophically uninformed, arbitrary metaphysical assessments (usually just ideology in disguise) offered by members of the scientific community who are given *carte blanche* to safeguarding them from any semblance of conceptual analytical scrutiny. To conflate post standard philosophy of science and physics with scientism would prove itself akin to equivocating John Burgess’ proposal for a methodological rapprochement between philosophy of science and philosophy of mathematics as some version of an attempt to ‘naturalize mathematics’ (recall the discussion in section 2 of the *Foreword* above, pp. vi – x). The post-standard philosopher of physics ideally envisions a reciprocal dynamics between philosopher and physicist, with the physicist offering the philosopher the conceptual raw materials of empirical evidence, models, laws, theories, and the philosopher in turn offering the physicist some means to conceptually and systematically analyze and reflect upon the ensuing inferences and insights issuing forth. Recall Morrison’s (2000*b*) and Galison’s (1997) remarks in n. 6 above.

<sup>41</sup> Indeed, Finkelstein (2001, 2004*a-c*, 2007) adopts Clifford algebra as the central mathematical formalism of his theories, as I briefly review in Subsection 2, section 2 of chapter 3 below.

derived notions of empirical evidence<sup>42</sup>), modeling activity, laws, and theories (both in the developmental and in the final textbook-ready form).

To this effect Rohrlich (1988), and Rohrlich & Hardin (1983) offer four useful distinctions: *developing* theories, *accepted* theories, *mature* theories, and *established* theories. In what *prima facie* may strike the reader as hearkening back to logical empiricism and other early standard accounts in the philosophy of science, Rohrlich writes:

Philosophical questions on intertheory relations and specifically on theory reduction are properly addressed to the mature theories rather than to the successive developmental stages of a theory; the latter belong to the context of discovery. Eventually, only the mature theory needs justification. (1988, 300)

However, the methodology Rohrlich advocates is consistent within the general framework of the post standard approach, as I shall elaborate further in §2 below. The notion of ‘justification’, in a preliminary sense and for purposes of this section, can be thought of as an ascription to theories which have achieved a type of stability and a level of sophistication capable of adequately supporting persisting and coherent ontological as well as mathematical aspects.

Sub-components of theories include *empirical laws*, which are “inductively generalized observation statements usually cast into a mathematically simple form” (300). Common examples include Kepler’s laws, Galileo’s laws of free fall, etc. An important categorical distinction between empirical laws and theories includes the notion that empirical laws contain *factual* ontological content, whereas theories possess only *formal* ontological content.<sup>43</sup> Empirical laws can provide the basic justification for theories, and are reducible to theories to the extent that the theories can explain the laws (300).<sup>44</sup> On the other hand, *conjectures* are conceptual schemes with tenuous empirical support and usually lack a mathematical framework: What distinguishes a *developing* theory from its counterpart (whether accepted, mature, or established) is precisely its relatively high abundance of

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<sup>42</sup> See Galison (1997) and Winsberg (2003).

<sup>43</sup> This is a distinction that shall be elaborated on in greater detail in the subsequent sections. However, at this point perhaps a simple illustration may suffice, hearkening back to the discussion of Frisch (2005) given brief mention in the *Foreword* above: The *empirical law* of Coulomb  $F = \frac{1}{4\pi\epsilon_0} \cdot \frac{Q_1 Q_2}{r^2}$  describes a force relation (inverse square) between two static and charged *bodies* possessing charges  $Q_1$  and  $Q_2$  respectively. That this law possesses factual empirical content is apparent enough: it can be verified (as sure as it has evolved historically as an inductive conjecture based on manifold trials) well enough via actual empirical procedures under controlled conditions, yielding results under suitable margins of error or *domains of validity* (Rohrlich (1988, 2004), Rohrlich and Hardin (1983)). On the other hand, the general framework of the theory of classical electromagnetism’s ontology is comprised of entities such as ‘point particles,’ ‘continuous charge distributions,’ etc. which clearly only refer to a *formal* ontology. More shall be said of this issue pertaining to ontology in the sections below.

<sup>44</sup> This point shall be examined more closely in section 3 below of this chapter.

conjectures at the expense of its relative parsimony of empirical laws.<sup>45</sup> The exact reverse is true for most other theories (whether accepted, mature, or established).

Regarding the latter (non-developing) case:

- a.) **Accepted theories** usually have sufficient empirical support, and are backed up by some mathematical framework, and logically cohere well with other theories.<sup>46</sup> They are considered to be the most plausible at their time of use, though they are “not necessarily accepted because scientists are convinced of [their] correctness” (300). Accepted theories may eventually evolve into mature theories.<sup>47</sup>
- b.) **Mature theories** are believed to be (approximately) true, due to very strong evidence (301). However, their appropriate *validity domains* are usually not known until after some scientific revolution<sup>48</sup> occurs, evincing the possibility of some superseding (or ‘finer’)

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<sup>45</sup> Examples are easy enough to list: The *developing theory* of Aristotelean mechanics even up until the late seventeenth century comprised mostly a body of conjectures in the above sense apart from a few empirical laws developed by William of Ockham (Damerow, et. al. (1992), Bochner (1963)).

<sup>46</sup> Logical “coherence with other theories” is also referred to as “external consistency” in the philosophy of science from previous decades. Consider for instance Kuhn’s (1977) five criteria for theory-choice: a.) Consistency (external and internal), b.) Accuracy, c.) Simplicity, d.) Fecundity, and e.) Broad Scope.

<sup>47</sup> Examples again should be easy to come by. Quantum field theory (QFT), either in its axiomatic or in its series perturbative form, is certainly accepted by the majority in the physics community. However, aside from its applicability only in a flat space-time structure (a generally dynamic geometry as described by General Relativity won’t provide the appropriate boundary conditions to allow for discrete normal mode solutions to the Dirac equation—giving rise to the possibility of ‘quanta’) its ‘kludgy’ renormalization methods, an incoherent ontology is suggested by Haag’s theorem, “[s]cattering amplitudes can be calculated for individual scattering phenomena, but cannot consistently be fitted together to describe a unified physical process” (Teller (2004) 437). Hence physicists have no choice but to use QFT as a springboard to search for deeper and more *mature* theories that account for its results in its appropriate validity domain. The work of Finkelstein (1996-2007) is one such instance.

<sup>48</sup> Note that Rohrlich (1988, 1994) uses this term in a more conservative and weaker sense than what one usually considers from Kuhn (1962) and Feyerabend (1963):

The ‘logical incompatibility’ proposed by some (Feyerabend) ...is only a cognitive incompatibility. The *logical coherence* between the finer and the coarser theories is ensured by the reducibility of [their respective mathematical structural aspects]. The limit involved [in reducing the mathematical aspect of the finer to that of the coarser theory] ...ensure the logical *continuity* and compatibility. (Rohrlich (1988), 307)

I will elaborate on this in greater detail in the subsequent sections of this chapter. However, as the passage suggests, I mention here in passing that in Rohrlich’s view of scientific revolutions does not present semantic or logical challenges rising to the level of ‘incommensurability’. At best, the finer theories of broader scope superseding their coarser counterparts during revolutions present *epistemic* challenges in the form of ‘cognitive incompatibilities’, an issue that I will discuss in greater detail in §3.3 below. However such incompatibilities at best suggest a *pluralist ontology* (Rohrlich (1988, 1994), Teller (2004)). The structuralist (Sneed (1971), Scheibe (1983)) influence on Ehlers (1986), likewise inspired by Rohrlich and Hardin (1983), prompted him to echo:

[L]imit relations *can* be understood *rationally*, that in spite of the alleged meaning changes and conceptual incommensurabilities the successor [Rohrlich’s ‘finer’] theory [  $T'$  ] does *explain* and *improve* its predecessor [  $T$  ], i.e., that there are reasons for asserting that there is progress in science. (Ehlers (1986) 387)

theory  $T'$  to emerge with broader scope and greater explanatory power (which supplies the validity domains to the mature theory  $T$ ).<sup>49</sup>

- c.) **Established theories** are mature theories that are not discarded after a revolution, continuing within their appropriate domains delimited by the validity limits of their superseding ('finer') theories. "Whether there have ever been *mature* theories which had to be discarded as a consequence of a scientific revolution is debatable; it depends on the line one draws between accepted and mature theories." (301) In other words, the fact that no sharply fixed boundary demarcates developing from mature doesn't vitiate the need and use for such a distinction: "[I]t is in general difficult to specify the exact time when a developing turns into a mature one. Nevertheless the distinction is a useful one and can in practice be reliably made in a wide variety of instances." (Rohrlich & Hardin (1983), 606)

I put the distinction to work here by relegating developing theories to the context of study of the *process* of theory-formation and modeling. Post-standard philosophers of science like Giere (1988, 1992), Nersessian (1992, 2002) and others have devoted extensive study to such study, which is an area lying outside the scope of this essay. As mentioned, I focus exclusive attention on the 'textbook-ready' *products* of accepted, mature, and established theories—specifically some of those in Batterman's studies (2002, 2004, 2005). I challenge the explanatory and reductive relations among such theories that Batterman proposes, in favor of aspects provided in the general framework offered by Rohrlich (1988, 1994) and those directly using his notions (Aerts & Rohrlich (1998), Bialkowski (1985), Dieks & de Regt (1998), Ehlers (1986), Rohrlich & Harden (1983), Wojcicki (1998)), as well similar ideas discussed in Ramsey (1995) and Teller (2004b).

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Ehlers (1986) makes his case in a constructively rigorous manner using two examples: Lorentz-invariant scattering theory (the 'finer' or superseding theory  $T'$ ) versus the superseded 'established' theory of Galilean-invariant scattering theory ( $T$ ) (390-396), as well general-relativistic gravitation ( $T'$ ) versus Newtonian gravitation (496-402). The latter example presents additional technical challenges, which the former does not. However, according to Ehlers' schema, such challenges do not rise to the level of a semantics of logical issues concerning incommensurability.

<sup>49</sup> This point shall be elaborated in greater detail in the subsequent sections of this chapter. Nevertheless, examples should be relatively easy to consider. Ptolemaic astronomy was accepted (towards the late sixteenth century not many believed it *true*, but treated it as plausible). Newtonian celestial mechanics on the other hand is a mature theory, as most physicists and astronomers consider it 'approximately true' in its appropriate validity domain. Its ontology of forces reliably represent—to a reasonable extent—the actual state concerning the properties of planetary dynamics. The 'finer' theory of general relativity of course sets the limits of the validity domain of Newtonian celestial mechanics. For precise details for the establishment of such validity domains, see Ehlers (1986), 393-397, 401-402) concerning Newtonian qua General Relativity, as mentioned briefly in n. 48 above.

## Section 2: Explanation

### **Subsection 1: Ruth Berger Meets Robert Batterman: Do their Explanatory Accounts Suffer the Same Shortcomings?**

Robert Batterman (2002) claims to have developed a novel species of explanation—the “asymptotic explanation”—which he argues fits no traditional account. Asymptotic explanations neither fit deductive-nomological [DN] nor the inductive-statistical [IS] schemas originated by Hempel (1962, 1965 respectively), and also do not conform to more recent unification (Friedman (1974), Kitcher (1989)) or causal accounts (i.e., Railton (1981)<sup>50</sup> and Salmon (1984, 1989)). I subject Batterman’s claims concerning the uniqueness of asymptotic explanations under detailed critical scrutiny in Chapter 2 below, from a perspective internal to the workings of his reasoning. Here I can give a brief external appraisal of Batterman’s claims vis-à-vis other recent literature concerning some accounts of explanation in the philosophy of physics, which I believe deflate Batterman’s uniqueness claims.

Reflecting on the contemporary state of pluralism in the philosophy of physics, as I commented in the *Foreword* and briefly in §1 above, it may come as no surprise that there are others who make similar uniqueness claims concerning their proposed accounts of explanation. Ruth Berger (1998) for instance offers a model of ‘dynamical explanations’ based on dynamical systems theory,<sup>51</sup> which she claims is neither subsumed by causal accounts (whether bottom-up or top-down), nor by the idealization of unification accounts of Kitcher (1989):

Dynamical modeling is an important source of explanatory information in just those cases where rigorous deduction from a more comprehensive physical theory is not feasible...[i]n the ideal limiting case, a dynamical explanation would be a demonstration that an interesting

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<sup>50</sup> I am using the notion of ‘causal’ somewhat loosely, to include *mechanisms* that can also comprise Railton’s ideal explanatory text [IET]. Writes Berger (1998), p. 311:

Railton is not a causal theorist per se; he sees explanation as revealing mechanism, but these mechanisms do not necessarily have to be causal. Nevertheless he indicates that explanatory texts for causal phenomena in the sense described as ‘an inter-connected series of lawbased accounts of all the nodes and links in the causal network culminating in the explanandum, complete with a fully detailed description of the causal mechanisms involved and the theoretical derivations of all the covering laws involved.’ (Railton 1981, 247)

<sup>51</sup> Dynamical systems consisting of  $N$  constituents are typically characterized mathematically in terms of a set of  $N$  initial conditions and  $N$  evolutionary equations of a single parameter  $t$  represented by a system of ODEs (ordinary differential equations). Interactions among the constituents complicate the scenario giving rise to ‘chaotic’ non-linear dynamics, which applied mathematicians and mathematical physicists often subject to ‘qualitative analysis’ in  $6N$  – dimensional phase space. Dynamical modeling (i.e. applying dynamical systems theory to classes of complex phenomena) has received relatively recent philosophical attention in the works of Kellert (1993) and Berger (1997, 1998), and others.

physical system realizes a particular mathematical model. However, the enormous complexity of most physical systems makes covering laws scarce and realization claims very rare. (325)

More often than not, argues Berger, the dynamical modeler must fall back to weaker “structural similarity” relations between system and dynamical models, as opposed to the ideal limiting case of ‘realization’. Such structural-similarity claims “are a type of analogy...[w]hen dynamical modelers argue for the relevance of certain causes, their arguments are *abductive*, not *deductive*.” (324-325, italics added) Naturally (à la Cartwright (1999)) one could always continue to treat dynamical explanations as a peculiar form of reduction (in the derivational sense) with an excessive number of ‘true’ and hence very specialized laws with correspondingly limited scope:

However, [such a treatment] is very misleading...[d]ynamical modelers rarely speak in terms of ‘laws’ and this [law-based] analysis suggests that dynamical modeling provides only patchwork ad hoc information. A more accurate characterization depicts dynamical explanations as providing a global mathematical picture of physical systems. This picture clarifies relationships between different variables, *and it increases our understanding by showing how physical systems tend to behave under many different types of circumstances*. On this characterization, dynamical models are not ad hoc at all; they illuminate complex relationships efficiently and without the oversimplification needed to describe these relationships in terms of the general laws. (325)

In this sense, dynamical explanations exhibit top-down features insofar as “the understanding sought is global, not local.” (325) Nevertheless, as emphasized in the passage above, it would prove misleading to consider the modeling a species of deductive reasoning, thus flying in the face of the ‘deductive chauvinism’ advocated by Kitcher (1989).<sup>52</sup> Moreover, dynamical explanations neither can be subsumed under some ‘general causal’ account for at least two important reasons: a.) Dynamical explanations are often presented in situations in which no general causal covering laws hold. b.) Their explanatory power is derived from abductive reasoning, not deductive reasoning. The latter abductive

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<sup>52</sup> Kitcher admits his logical/set theoretic characterization of explanatory unification using his notion of explanatory store  $E(K)$  defined as a set  $E$  over the closed set or totality of sentences  $K$  characterizing scientific belief-claims regimented according to some logic (first order predicate or otherwise, for a highly readable version of the ‘otherwise’ cases see Agazzi (2003)) as able to generate deductively a maximum number of conclusions with a minimum number of ad-hoc elements is somewhat “ham-fisted.” (Kitcher (1989) n. 18, 501) Nevertheless, he sticks by his methodological claim, in order to minimally deviate from what he considers is the best that Hempel’s deductive approach has to offer. Kitcher’s avowal of ‘deductive chauvinism’ is perhaps best clarified in his notion of understanding (inspired by Kuhn’s notion of ‘theory-articulation’ or ‘know-how’, i.e. understanding as *practice* being incapable of being reduced to some ‘know-what’ rational or axiomatic reconstruction of  $T$ ): “I claim that to *know a theory involves the internalization of the argument patterns associated with it*...in consequence, an adequate philosophical reconstruction of a scientific theory requires us to *identify a set of argument patterns as one component of the theory*.” (Kitcher (1989) 438, italics added)

aspect is especially crucial “if we want an account of explanation which does justice to the richness, power, and pervasiveness of modeling explanations.” (331)<sup>53</sup>

Whether or not Berger’s claims (1997, 1998) stand up to critical scrutiny is an issue I will not delve into, nor did I mention her work in passing to suggest that Batterman’s (2002) uniqueness claim is entirely *exclusive*, i.e., that he makes the case that asymptotic explanations are the *only* instance of a type of explanation neither subsumed by causal nor unification accounts. Batterman of course makes no such grand pronouncement, as obviously the exclusivity issue is irrelevant: he needs merely to show that his notion of asymptotic explanations serve as *a* counter-instance to causal or unification accounts. If there are other counter-instances, this would hardly bear any impact on the project concerning the role of asymptotic reasoning via explanation and reduction, a role he claims is somewhat distinct.

Why I mention Berger’s work in passing has more to do with Belot’s (2003) criticism of Batterman (2002), which I delve into greater detail in §2.3 of chapter 2 as well as §1.2 of chapter 3 below. Suffice it to say here that Belot (2003) wields the full-blown mathematical machinery of the theory of differential equations to argue that asymptotic explanations can be conceived as a weak form of derivation: that is to say, rather than both the superseded theory  $T$  and the superseding theories<sup>54</sup>  $T'$  somehow being essentially required in the case of asymptotic reasoning (as Batterman upholds), the superseded theory  $T$  can be shown to be (mathematically, but not necessarily logically let alone semantically) derived as a special case of  $T'$ , *when the latter is sufficiently explicitly characterized by the full-blown mathematical theory of differential equations*. Obviously a connection exists between the general theory of differential equations (whether ordinary or partial, non-linear coupled or non-linear de-coupled, etc.) and the theory of dynamical systems (as the latter essentially involves systems of differential equations).<sup>55</sup>

Hence how seriously one takes that connection implies that Batterman’s (2002) claims may stand or fall with Berger’s (1998): if it can be demonstrated that Belot’s (2003) claims undercut or deflate Berger’s (1998).<sup>56</sup> Batterman (2007) of course rejects Belot (2003) (as I discuss in greater detail in §2.3 and in §1.2 of chapters 2 and 3 below). So as far as *he* (Batterman) is concerned, any possible association I make with his ideas and Berger’s (1998) is a moot point. Nevertheless, I mention Berger’s work in passing to show how she (as well as Batterman) rely on various

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<sup>53</sup> Recall Burgess’ allusion to abductive reasoning as discussed in the *Foreword* above.

<sup>54</sup> Conceived non-developmentally, i.e. as accepted, established, or mature as I disclaim in § 1 above.

<sup>55</sup> I defer to the specialists in the applied and pure mathematics community to point out the nature and significance of such a linkage. But even those professing complete mathematical ignorance would not dispute that the semantic content of dynamical systems theory overlaps with that of the general theory of differential equations in the shared concept of differential equations.

<sup>56</sup> Belot (2003) does not bring up Berger’s (1997, 1998) or Kellert’s (1993) notion of dynamical explanations, yet *prima facie* (formidable technical challenges perhaps notwithstanding) as I mention above there seems to be no reason why Belot’s claims cannot be in principle extended to dynamical systems.

sophisticated approximative techniques found in aspects of the general mathematical theory of differential equations to hedge philosophical uniqueness claims. The fact that at least *one* strong dissident voice (Belot (2003)) seeks to deflate such philosophical hedging seems to raise the question of whether or not the presupposed uniqueness of asymptotic explanations can be couched in terms not essentially relying on such mathematical approximation tricks per se. Batterman (2007) claims to do just *that*, of course, i.e. point out what he considers are essential epistemic and methodological features of asymptotic explanation not exclusively reliant on mathematical structure alone (a claim which I further critically survey in appropriate sections of chapters 2 and 3 below). How successful he is in this endeavor, as well as even in effectively parsing semantic and ontological aspects of a theory from their strictly mathematical aspects is another point I critically address in chapter 2.<sup>57</sup>

Read positively, however, my overarching point in this Batterman-Berger discussion concerns the essential aspect of the mathematical content in the anatomy of some mature theories, a structural aspect that in my opinion Rohrlich (1988) distinguishes well from a theory's semantic and ontological content (which conversely other authors like Batterman do not). I will delve into this issue in greater detail in § 3 below in my discussion on reduction: Here I will mention in closing that once sufficiently clarified, the mathematical aspect of a mature theory is ripe for 'hooking up' to some broader or deeper mathematical research tradition. In particular, my claim is that hooking up a theory's mathematical aspect to Clifford algebra can transform that theory in a way that directly opposes the pluralist implications of Batterman's (2002, 2004, 2005) and other similar assessments.

## **Subsection 2: Fritz Rohrlich's 'Covering Theory Model' of Explanation may 'Cover' the Case of Batterman's 'Asymptotic' Explanations**

Fritz Rohrlich (1994) also offers a rather novel account of explanation, which is based on his account of reduction (1988), which I will devote detailed attention to in §3.3 below to the latter. One however need not be familiar with that account, above and beyond the aspects of Rohrlich I have briefly mentioned in §1.1 above, to appreciate the salient aspects of his account of explanation. Hence my introducing it here.

In a similar manner alluded to in §1.2 in the discussion of Berger (1998), Rohrlich places emphasis on the *primacy of understanding*, which should be the ultimate aim of all forms of scientific explanation. Rohrlich is quick to point out covering-law models [CLMs], i.e. deductive-nomological [DN] accounts (first proposed by Hempel (1962)<sup>58</sup>) do not necessarily ensure understanding, "nor do

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<sup>57</sup> See § 3 of chapter 2 below for more details.

<sup>58</sup> As fitting squarely in the logical empiricist tradition, Hempel's accounts of explanation, whether deductive-nomological or inductive-statistical (DN or IS) are considered to be *epistemic*: not presupposing metaphysical brute facts (like causation) in the explanans. The schema of course is:

they necessarily provide sufficient information for credibility.”(69) This is apparent, according to Rohrlich, because CLMs suffer three major defects (70):

1. Laws are included in the explanans/premises without further justification, which obviously does not contribute to credibility let alone understanding.<sup>59</sup>
2. They provide no explication of undergirding ontology, running the risk of “the explanation...involv[ing] an entirely different ontology from that of the question.” (ibid.)

1.  $L_1, L_2, \dots, L_m$
2.  $C_1, C_2, \dots, C_n$
- $\therefore E$

where:  $L_1, L_2, \dots, L_m$  are statements describing general laws, and  $C_1, C_2, \dots, C_n$  are statements describing particular facts, and  $E$  is the explanandum. If  $L_1, L_2, \dots, L_m$  are all statements describing general laws which are strictly universal (i.e., of the universal conditional form  $\forall x:(Px \rightarrow Qx)$ ), then the schema is DN. On the other hand, if there are instances when at least one of laws  $L_1, L_2, \dots, L_m$  are statistical then the schema is IS. One way to appreciate the *epistemic* character of DN (or IS) explanation is due to causal explanations being DN, but the converse doesn't hold (Hempel (1962) in Curd & Cover (1998) 687). In other words, a presumably metaphysical notion like causation can get reduced to the above logical schema. Recalling Kitcher's (1989) 'deductive chauvinism' discussed in n. 52 above, a similar epistemic approach is taken: Kitcher in fact argues for an anti-realist notion of causation. “[According to Kitcher (1989) our causal beliefs are derived from the explanatory stories we are taught by our culture...one cannot, post-Hume combine causal realism with the belief in the possibility of causal knowledge.” (Barnes (1992), 10).

<sup>59</sup> For instance, what good what it do for ‘understanding’ to explain why a glass of water cracked when the water froze by invoking some law in the explanans:  $\forall x:(H_2Ox \rightarrow Ex \ \& \ Cx)$  where:  $H_2O$ ,  $E$ ,  $C$  are the predicates ‘water,’ ‘cooled to solid form,’ ‘expands’)?

A scientific explanation should also answer such implied questions as: how credible is the law on which it is based? Under what assumptions (on ontology, idealizations, etc.) is the given answer valid? How well established is the theory on which the answer is based? Are there alternative theories that cannot be dismissed? (Rohrlich (1994), 71)

Certainly causal theorists (Railton (1981), Salmon (1984, 1989), etc.) have launched the same complaint as implied in the expanding water example:

[T]heories that unify a group of phenomena also typically do so by describing the *causal bases for the various phenomena in the group*...what is doing the work of manufacturing understanding is the multiple descriptions of the causal bases of the various phenomena offered by the theory, not simply its unifying power per se. (Barnes (1992) 10, italics added)

Nevertheless the questions Rohrlich raises concerning epistemic issues of credibility of the laws are of a different slant than those advocating primacy of causal accounts. In fairness to Hempel, however, it should be emphasized that ‘understanding’ was considered a psychological and pragmatic factor in the logical empiricist tradition, hence lying outside the normative bounds and being incapable of logical characterization. On the other hand, “if the goal of explanation is understanding, one cannot ignore the philosophical aspects of ‘understanding’ in an attempt to explicate explanation.” (Rohrlich (1994) 74) Echoing this point, Dieks & de Regt write: “The positivist approach provides us with the valuable insight that scientific understanding is context-dependent. However, positivism overshoots the mark when it concludes from this that the notion of understanding is extra-scientific and irrelevant to the philosophy of science.” (1998, 51)

3. Emphasis is not placed on the notions of causal mechanism or the way the world may function, “[y]et it is generally accepted that understanding is largely based on ‘the causal structure of the world.’ (Salmon)”<sup>60</sup> (ibid.)

What Rohrlich proposes in lieu of the shortcoming of the CLM is a covering *theory* model of explanation (CTM), in which basically aspects of a *theory* comprise the explanans, not just some set of *laws*. To recall the previous discussion in §1.2 above, *mature, accepted, established* theories were distinguished by Rohrlich from *developing* theories; I focus on the former (non-developing) category.

In addition to this ‘external’ structure concerning a theory’s evolutionary stage, according to Rohrlich (1988) and Rohrlich & Hardin (1983) a theory (non-developing)  $T$  also possesses a complex *internal* structure including the following components or aspects:

- An *ontology*  $\mathcal{O}(T)$
- A set of central *terms*  $\alpha(T)$ , with an accompanying *semantics*  $\sigma(T)$ .
- Set(s) of *principles*  $\Pi(T)$
- (Often<sup>61</sup>) a *mathematical structure*  $\mathcal{M}(T)$
- A *domain of validity*  $\mathcal{D}(T)$

The above aspects (which should not be thought of as exhaustive) clearly indicate Rohrlich’s *structuralist* affiliations, which he obliquely refers to when he mentions that his CTM “is in the spirit of a semantic view of scientific theories...but not anti-realist as in van Fraassen’s constructive empiricism.” (69) I will discuss the above aspects vis-a-vis structuralism in greater detail in §3 below.<sup>62</sup> Suffice it to say that this detailed and plausible characterization of a theory’s internal

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<sup>60</sup> Here Rohrlich defers to the causal theorists’ complaints, as described in the previous note above. However it is erroneous to think of Rohrlich as *merely* a causal theorist.

<sup>61</sup> In this essay, *all* the examples of theories I discuss possess this aspect.

<sup>62</sup> Structuralism of course refers to a somewhat mathematically refined version of the semantic view of scientific theories. Despite the rigorous logical and formal character of structuralism, which prima facie one may associate with rigorous versions of logical empiricism (Carnap) characterized as the ‘syntactic’ or ‘sentential’ view of theories, the two research traditions could not be any more different. Aside from the obvious differences of aims (the *syntactic view*: characterizing theories via sets of sentences in some logically regimented language formal  $\Lambda$ , with primary emphasis placed on deductive consequence  $\vdash$ , versus the *semantic view* characterizing theories as sets of models in the model-theoretic sense, i.e., as *interpreted* sets of sentences and terms with primary emphasis placed on semantic entailment  $\models$ ) structuralism is profoundly anti-reductionist: theories *cannot* be reduced to axiomatic systems. Though Michael Friedman (1981) called into question the significance of such a distinction, since every model  $M$  can be ‘translated’ into some sets of sentences and vice versa. Granted, though this may be true from a *methodological* standpoint, certainly a *logical asymmetry* certainly exists between  $\vdash$  and  $\models$ : The former requires rules of inference characterizing  $\Lambda$  while the latter requires an interpretation mapping. The anti-reductionism inherent in structuralism is the crucial issue here: formal characterizations of *aspects* of theories do not imply a notion of “nothing but” inherent in much stronger logical reductionist programs: ‘Theories, rationally reconstructed, are essentially sets of sentences in a formal language,’ etc. Irreducibly *contextual* factors influence, for instance, which terms in a

structure goes a long way to disambiguate elements of Batterman (2002) inherent in his notion of asymptotic explanation, thus undercutting much of the presumed originality and persuasive force of his claims, as I shall argue here.

The CTM procedure essentially involves three sequential stages: a.) choice of a theory, b.) specification of a model, c.) choice of inference process. (71) For example, concerning the why-question: “Why does the earth exhibit an elliptical trajectory with the Sun in the position of one of the foci of the ellipse?” the CLM would account for the aforementioned explanandum most likely via the following schema:

$$\begin{array}{l} \forall x: Px \rightarrow KFLx \\ Pe \\ \hline \therefore KFLe \end{array}$$

...with predicates P, KFL referring to ‘planet’ and ‘Kepler’s First Law’. “Certainly a criterion for blind acceptance,” (71) as the CLM schema provides no answers to justification of its laws, though such answers would prove essential for understanding as well as aiding in a decision to believing as opposed to just accepting. (72)

On the other hand, the CTM would account for the above explanandum by: (a.) First fixing the choice for Newtonian celestial mechanics  $T_N$ : though only an approximation of the ‘finer’ superseding theory of general relativity, it proves sufficient to account for the qualitative nature of the above why-question.<sup>63</sup> (b.) Then fixing upon a model choice of the Earth-Sun system  $M_{E-S}$  within the framework of  $T_N$ , which in this case is composed of several assumptions exhibiting the inevitably counterfactual features of the modeling procedure:

- (b.i.) Assume the Earth (E) and the Sun are the only bodies.
- (b.ii.) Assume the Earth’s mass:  $m_E \ll m_S$ : the Sun’s mass.
- (b.iii.) Assume the Earth’s radius:  $R_E \ll r_{E-S}$ : the average Earth-Sun distance and assume the same holds for the Sun (i.e.,  $R_S \ll r_{E-S}$ ).

“This model specification establishes the particular *idealized ontology* within a cognitive level for Newtonian theory.”<sup>64</sup> (72) Echoing Rohrlich’s notion of domain of validity, the specification of the model gives the best approximation and approximating assumptions through which the explanandum would hold. “Such knowledge on the limited validity of the questioner’s tacit assumption contributes

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theory are empirical versus those that are  $\Theta$ -theoretical, and in turn how each get instantiated. For more information, see Kallfelz (2006c), Sneed (1971), and Scheibe (1983, 1997, 1999).

<sup>63</sup> I am leaving aside here the technical details of Rohrlich’s notions of ‘domains of validity,’ which underwrite his theory of verisimilitude. I will discuss these notions in some detail in the ensuing sections §3, §4 below.

<sup>64</sup> I will examine the notion of idealized ontology vis-à-vis cognitive levels in greater detail in §3 and §4 below.

greatly to the success of the explanation.”<sup>65</sup> (ibid.) (c.) The choice of inference becomes a formal matter (both logical and mathematical). In this case, one avails oneself to the “technical machinery of the theory [ $T_N$ ].” (ibid.) The mathematical machinery includes second-order differential equations characterizing the two-body problem subject to an inverse-square attractive radial force, along with the mathematical derivations leading to the desired result (i.e., the parameterized elliptical trajectory of the Earth’s orbit). Such formal steps however conceal a qualitative picture as well: In the case of the ‘cognitive level’ of  $T_N$ , a *causal mechanism* is deduced, invoking gravitational laws and conservation principles. “[T]hat qualitative part may be the only part that is important [to]...understanding. It is certainly necessary.” (ibid.)

After having explicated aspects of Rohrlich’s CTM, recall the previous claims of Batterman (2002) discussed briefly in the previous section (and thoroughly in chapter 2 below). According to Batterman, asymptotic explanations are neither ‘covering’ and derivational, nor inductive statistical, let alone much less unificational or causal. The essential reason cited by Batterman is that in the complex cases of critical phenomena (e.g. fluid droplet formation, caustic surfaces in optics, sonic booms, etc.) the superseding theory  $T'$  may not reduce to the superseded theory  $T$  in the limit of one of  $T'$ ’s essential parameters. For example, in the case of caustic surfaces, a ‘singularity’ or spike resulting from constructive interference of wave-fronts of continuous order, the superseding theory of Fourier optics will *not* produce the relatively straightforward results of a series of concentrated rays converging at a point, in the  $\lambda \ll L$  limit.<sup>66</sup> Quite the contrary: in this case one must resort to various complex approximation techniques in a manner involving aspects of geometric and Fourier optics in the field of research known as catastrophe optics. Hence according to Batterman, the superseded theory  $T$  plays an essential role alongside the superseding theory  $T'$ , when scientists seek to account for such complex critical phenomena. Moreover the relationship between  $T$  and  $T'$  is *not* one of derivation, unification, shared causal IETs (ideal explanatory texts), but a *singular* one: failure of the laws and results of  $T'$  to converge to those of  $T$  in the asymptotic limit  $\lambda \ll L$  reveals that the “behavior [of  $T'$ ] in the limit is not the same as the behavior *at* the limits.” Batterman (2002, 2003b)

I offer a detailed critique of Batterman’s account of asymptotic analysis in chapter 2 below, but given the exposition of Rohrlich (1994) above one might object to Batterman’s claims as being overhasty. For adopting a CTM in the case of caustic surfaces, one’s theory of choice (step (a.))

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<sup>65</sup> Lest this notion strike the reader as a rendition of the *ceteris paribus* clauses that Cartwright (1999) and others have delved so deeply into, Rohrlich’s formulation of the notion of validity limits is somewhat more subtle and involved, rendering him on the one hand to ascribe to a pluralist (idealized) ontology but a substantial monism. More shall be said of this in §3 and §4 below.

<sup>66</sup> Where  $\lambda$  is the wavelength of the wavefront and  $L$  is the scale factor of the regions through which the EM waves interact. The  $\lambda \ll L$  limit is the ‘geometric optics’ limit in the typical (non-singular) cases, since the results of Fourier wave optics will converge to the results of geometric optics. I am deliberately oversimplifying the subtleties here, as they will be discussed in the subsequent section on inter-theoretic reduction (§ 3).

would be the (at the very least *accepted*) *theory* of catastrophe optics. Choosing the models (step (b.)) to account for the caustic surface would involve borrowing elements and aspects from different ‘cognitive levels’ with their associated ontologies: rays *and* waves. The theory of catastrophe optics, in other words, exhibits what Rohrlich describes as a ‘pluralism of ontologies.’ The failure of reductions from Fourier to geometric optics that Batterman highlights bespeak of a failure of the *mathematical structures*  $\mathcal{M}(T')$  of Fourier optics ( $T'$ ) to converge smoothly to the *mathematical structures*  $\mathcal{M}(T)$  of geometric optics ( $T$ ). *This is precisely why the theory of catastrophe optics was developed, which has mathematical structures able to model the caustic phenomena—in albeit ‘messy’ ways, in the sense of being laden with a plethora of approximation techniques to condition its even messier sets of differential equations, a matter of part (c.) in the CTM.*

I will discuss the nuances of the mathematical structures with respect to ontology in greater detail in the following sections below. Suffice it to say here, however, that the chief problem in Batterman’s claims is his equivocating some of a theory’s (formal) ontology  $\mathcal{O}$  with its mathematical structures  $\mathcal{M}$ .<sup>67</sup> This comprises one of the central and critical points that I expand on in chapter 2 below. Keeping the two (formal ontological versus mathematical) reduces Batterman’s claim of the necessity of utilizing both geometric and Fourier methods to a mere recognition that catastrophe optics contains a plural ontology: both wave and ray idealizations enter into the choice of model (part (b.) in the CTM). Granted, I am glossing over these points in somewhat cursory detail here, and the sections below shall expand on in broader depth and precision the nuanced character of reduction and ontology (as conceived by Rohrlich and others).

### Section 3 : Inter-Theoretic Reduction

#### **Subsection 1: Some Opening Remarks and Essential Characterizations**

By far the most central aspect of Batterman’s analyses (2002, 2004, 2005, 2007)—and my subsequent study thereon—is *reduction* among conceptual structures: laws, models, mathematical results and systems, etc. Echoing Rohrlich’s characterization of the aspects constituting a theory which I made reference to in §2.2 above, I will denote the study of reduction among this entire class of conceptual structures as *inter-theoretic* (insofar as a theory consists of a complex of such aspects). Since I am basing my notion of the structure of scientific theories on Rohrlich’s characterization, this prevents me from equivocating the *term* ‘theory’ with other terms such as ‘law’, ‘model,’

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<sup>67</sup> On the role of  $\mathcal{O}(T)$  viz. explanation Rohrlich (1994) comments further:

[S]cientific explanation must first...specify the perception within which the explanation is to take place: its cognitive level and its ontology...It requires the choice of a scientific theory and a suitable model within that theory...Only then can the explanation proceed to deduce from first principles the way things are and the way things function on that level. (76)

‘mathematical framework’, etc., so long as I explicitly enumerate the *aspect(s)* of the theory(ies) I am focusing on in the study of the reduction relation.<sup>68</sup>

Hence in such a centrally important topic it merits devoting some space to discussing some of the fundamental points within this study of reduction, which naturally includes a cursory treatment of some of the ‘classical’ treatments of Nagel (1974) and Nickels (1975) vis-a-vis the ideas of Batterman (2002, 2003a). Ernest Nagel (1974) advanced a detailed model of intertheoretic reduction which is considered *epistemic* insofar as for Nagel reducing a theory  $T$  to  $T'$  implied a form of explanation: one explains the meaning of  $T$ 's central concepts by reducing them to those of  $T'$  (where  $T$  usually serves as the role of superseded theory and  $T'$  the superseding theory).<sup>69</sup> In this respect, Nagel's emphasis on epistemics is part of the logical empiricist tradition, as in the case of models of explanation advanced by Hempel (1962, 1965), reflective of the tradition's generally anti-metaphysical bias. Nagel's model characterizes reduction as a *logical* and *semantic* relation:  $T$  reduces to  $T'$  if the meanings of  $T$ 's essential terms are included in those of  $T'$ , i.e. can be logically derived from those of  $T'$ .<sup>70</sup> As mentioned (n. 32 above) some later structuralist-inspired renditions of inter-theoretic reduction have been classified as “neo-Nagelian” despite their heavy reliance on seemingly purely abstract mathematical structures through which they attempt to characterize the relation of reduction. This is primarily due to the structuralists comprising a rather formal wing of the semantic view of theories (recall n. 62 above) with its emphasis on model-theoretic (read: logical/semantic) approaches to physical theories.

As Nagel (1974) set the stage for a rigorous model of inter-theoretic reduction, so Thomas Nickles (1975) was perhaps the first to upstage him:

[W]e need to recognize that at least two main kinds of reduction, which differ both in nature and in scientific function or purpose. ‘Reduction<sub>1</sub>’ (as I shall call it) is the achievement of postulational and ontological economy and *is* obtained chiefly by derivational reduction as

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<sup>68</sup> Regarding the risk of equivocation between a theory and any of its structural aspects, Aerts & Rohrlich write: “[T]he ‘theory’ of the solar system developed since Copernicus and Kepler is a *model* to which Newtonian gravitation *theory* is applied. Another model of the same theory is the ‘theory’ of the tides.” (Aerts & Rohrlich (1998) 29) Though many writings on inter-theoretic reduction, including ‘classics’ like Nagel (1974) and Nickles (1975) will predicate ‘theory’ to all in the aforementioned quote.

<sup>69</sup> “[There are] certain relations of dependence between one set of distinctive traits of a given subject matter [that] are allegedly explained by ... ‘reduced’ to assumptions concerning more inclusive relations... They [i.e., such relations of dependence] raise the question of what, in fact, is the logical structure of such reductive explanations—whether they differ from other sorts of scientific explanation, what is achieved by reductions, and under what conditions are they feasible.” (Nagel (1974) in Curd & Cover (1998), 906)

<sup>70</sup> *Homogeneous* reductions occur when *all* of  $T$ 's terms contain meanings which can be derived from those of  $T'$ , i.e. if the extensions of  $T$ 's essential terms are a subset of the extensions of those of  $T'$ . *Inhomogeneous* cases arise when there exist essential terms of  $T$  whose extensions are not part of those of  $T'$ . Nagel developed the notion of *bridge laws* to compensate for this shortcoming so the meaning of  $T$ 's essential terms could still be strictly derived from those of  $T'$ , adjoined with the appropriate set of bridge laws which Nagel viewed as empirical hypotheses: “[S]uch bridge laws...are empirical hypotheses concerning the *extensions* of the predicates mentioned in these correspondence rules—that is, concerning the class of individual things or processes designated by these predicates.” (Nagel (1974) in Curd & Cover, 914-915)

described by Nagel...amount[ing] to the *explanation* of one theory by another...‘Reduction<sub>2</sub>’ [on the other hand]...involves a varied collection of intertheoretic relations rather than a single, distinctive logical or mathematical relation...The great importance of reduction<sub>2</sub> lies in its heuristic and justificatory roles in science. (Nickles(1975), in Curd & Cover (1998) 950)

Elsewhere in his essay Nickles describes reduction<sub>2</sub> as ‘domain preserving’ and the former as ‘domain combining.’ The justificatory role played by reduction<sub>2</sub> “derives from the fact that the reduction shows the successor theory [ $T'$ ] to account adequately for the structured domain of phenomena inherited from its successful predecessor [ $T$ ], i.e. it is ‘domain-preserving.’”<sup>71</sup> (953) Whereas the semantic notion of reduction which Nagel sought to characterize combines domains (of meaning): what was perhaps thought of as a separate extension for a term in  $T$  (e.g. the meaning of ‘light ray’ in for instance geometric optics) is shown to be properly contained in the extension(s) of (an) essential term(s) in  $T'$ : For example, the extension of the term ‘light ray’ is shown to be contained in the extension of the term ‘electromagnetic propagation’ in the theory of electromagnetism  $T'$ .<sup>72</sup>

Certainly Batterman (2002, 2003a, 2004, 2005) and others whom I mention here primarily draw from Nickles’ (1975) second notion (the domain-preserving kind).<sup>73</sup> Fundamentally, the justificatory and heuristic nature of this sense is best depicted as an ‘asymptotic’ procedure insofar as the successor theory’s ( $T'$ ) laws or essential terms are shown to smoothly converge to those

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<sup>71</sup> In Rohrlich’s terms, the ‘successful predecessor’  $T$  plays the role of an *established* or *mature* theory (recall § 1.2 above). Moreover:

Sometimes philosophers of science speak as if an established theory [ $T$ ] like classical mechanics were a ‘dead’ theory. This is simply not the case. Any mature theory can continue to grow, to develop, and to encompass previously unknown phenomena...Not surprisingly, established theories are the building blocks not only of the curriculum, but of almost all research in physical science...it is a mature theory [therefore] which is coherently vertically upward, in the sense of being a limit theory of a superseding theory [ $T'$ ], thereby receiving validity limits. (Rohrlich & Hardin (1983), 608)

<sup>72</sup> Hence Nagel’s (1974) characterization of reduction as a form of explanation (conceived of in Hempel’s terms as a form of derivation):  $T'$  ‘explains’  $T$  as the logically closed set  $K$  of all of the logically possible inferences  $T$  can generate is shown to be a deductive consequence of such a set for  $T'$  (i.e.  $K'$ ) or in the inhomogeneous case  $K'$  adjoined with the logical closure of all inferences drawn from the bridge principles  $BP$ : I.e.  $K' \vdash K$  or  $K' \cup BP \vdash K$  for the respectively homogeneous versus inhomogeneous cases. The extension of the term ‘electromagnetic propagation’ is the *explanans* for the *explanandum* of the extension of term: ‘light ray’.

<sup>73</sup> In this sense, Batterman’s (2002) claims that reduction and explanation should not be considered as the same kind of activity is by no means a unique claim. Nickles argues that in the domain preserving sense, certainly nothing like ‘explanation’ (characterized according to Hempel’s models (1962, 1965)) is going on. At best the justificatory enterprise of the domain preserving case can only be thought of as ‘explanatory’ in a very loose sense. To argue that relativistic momentum reduces<sub>2</sub> to Newtonian momentum under suitable conditions (e.g., Rohrlich’s ‘domain of applicability’) certainly does not ‘explain’ Relativistic momentum! Though one could (in a loose sense) say that such cases “might be said to explain why the predecessor theory worked as well as it did.” (Nickles (1975) in Curd & Cover (1998), n.4, 967).

corresponding laws or terms in  $T$ . For example, the relativistic momentum:<sup>74</sup>  $\vec{p} = m\vec{v}$  where:  $m = \gamma(v)m_0$ ,  $\gamma(v) = \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}$ , and  $m_0$  is the rest mass.<sup>75</sup> Certainly in the limit:  $v \ll c$ ,  $\gamma(v) \approx 1$ , hence the

special-relativistic expression converges to the classical Newtonian formula.<sup>76</sup>

Batterman (2002, 2003a) characterizes “smoothly converging” instances of domain-preserving reduction (as in the example shown above) as ‘regular’, fitting what he describes as **SchemaR**:  $\lim_{\varepsilon \rightarrow 0} T' = T$ . The ‘equation’ is basically shorthand for stating that for *some* fundamental parameter  $\varepsilon$  of  $T'$ , as  $\varepsilon \rightarrow 0$ ,<sup>77</sup> the behavior of some of  $T'$ ’s laws smoothly converge to those of  $T$ : The behavior *at* the limit matches the behavior *in* the limit. Note however the existential quantifier in the above claim: “[O]ne must take the equality...with a small grain of salt...[as] it is likely not the case that every equation or formula from [ $T'$ ] will yield a corresponding equation of  $T$ .” (Batterman (2007), 5)

Robert Batterman (2002, 2004, 2005) of course devotes most of his attention to cases (usually associated with critical phenomena) when the above schema **SchemaR** fails, the singular case:  $\lim_{\varepsilon \rightarrow 0} T' \neq T$ . Two possibilities arise that are of interest: (a.) Strongly singular: something goes wrong in the asymptotic limit in the case of  $T'$  in the sense that the relevant laws, expressions, or formulae ‘blow up’ in the  $\varepsilon \rightarrow 0$  limit, which could be abbreviated as: ‘ $\lim_{\varepsilon \rightarrow 0} T' = \infty$ ’. (b.) Weakly singular: the behavior of  $T'$ <sup>78</sup> might not necessarily produce (one may assume) physically meaningless singularities but “the behavior in the limit is of a *fundamentally different character* than the nearby solutions one obtains as  $\varepsilon \rightarrow 0$ .” (Batterman (2007) 5) Batterman concocts a simple example illustrating the weakly singular case via the quadratic  $x^2 + x - 9\varepsilon = 0$  with  $\varepsilon$  acting as the ‘perturbation

<sup>74</sup> As represented here as a three-dimensional vector in ordinary  $\mathbb{R}^3$ . Ehlers (1986) treats the case more representatively by conducting his derivations in Minkowski geometry. For purposes of his demonstrations, Nickles ignores the geometric subtleties.

<sup>75</sup> Feyerabend (1963) and others concerned about incommensurability raise the objection that even in a proper limit relation where:  $\lim_{\frac{v}{c} \rightarrow 0} m = m_0$ , the extensions and intensions of relativistic mass and Newtonian mass are disjunct. (For instance, in the case of syntax, relativistic mass is a two-place predicate: ‘the mass of\_ with velocity\_’ ) while Newtonian mass is a one-place predicate. Hence one cannot identify  $m_0$  with Newtonian rest mass, regardless of their mathematical indistinguishability. The semantic issues will be address in the subsequent sections below, viz. ontology.

<sup>76</sup> Expanding  $\gamma(v)$  according to its *Taylor Series Representation*:  $\gamma(v) = [1 - (v/c)^2]^{-1/2} = 1 - 1/2(v/c)^2 + O((v/c)^{2n})$ , where  $\lim_{v/c \rightarrow 0} O((v/c)^{2n}) = 0$  establishes a ‘domain of validity’ (Rohrlich) epresented topologically as an  $\varepsilon$ -disk with  $\varepsilon \equiv (v/c)$  such that  $\gamma(v) = [1 - \varepsilon^2]^{-1/2} \approx 1 - 1/2\varepsilon^2 \approx 1$ . Batterman (2007) pays especially close attention to the subtleties of series expansions in a philosophically illuminating manner.

<sup>77</sup> With no loss of generality, one could of course just as well represent **Schema R** for some fundamental paramter  $\eta$  in  $T'$  that  $\lim_{\eta \rightarrow \infty} T' = T$ . As I discuss in the ensuing subsection, Rohrlich and Hardin (1983), Ehlers (1986), and Rohrlich (1988) are far more careful concerning fixing such parameters: for one, they *must* be dimensionless. Validity domains are established by such parameters  $\varepsilon$  which must be dimensionless since “otherwise these limits [of the validity domains] would depend on the (arbitrary) units chosen to measure quantities.” (Rohrlich & Hardin (1983) 608).

<sup>78</sup> Shorthand for  $T'$ ’s laws, formulae, etc.

parameter' in  $T'$ . Then in the limit:  $\varepsilon \rightarrow 0$  the solutions of the quadratic converge smoothly to the solutions

$\{0, -1\}$  of the unperturbed equation  $x^2 + x = 0$  in  $T$ . On the other hand, in the case:

$\varepsilon x^2 + x - 9 = 0$  the behavior  $\varepsilon \rightarrow 0$  does not smoothly converge: a qualitative distinction (quadratic with two solutions versus linear with only one) exists between the perturbed and unperturbed case.

Thus, the character of the behavior in the limit  $\varepsilon = 0$  differs fundamentally from the character of its limiting behavior. Not all singular limits result from reductions in order [i.e. degree of its leading term] of the equations. Nevertheless, these latter singular cases are much more prevalent than the former. (6)

It is precisely these sorts of cases (strongly and weakly singular) that Batterman's studies primarily focus on, studies which I critique in a detailed manner in chapters 2 and 3 below. In this subsection, however, I give his views on reduction a cursory overview so that I can discuss Rohrlich's views on the matter in some detail. As I argue §1.2 above, a strong case can be made that Batterman's (2002, 2004, 2005) notions on *reduction* can be reconstructed and re-cast in Rohrlich's (1988) mold. Doing so would have the effect of preserving much of Batterman's important philosophical work, minus what I consider are his incorrect conclusions.

## **Subsection 2: Fritz Rohrlich's Model of Inter-Theoretic Reduction**

As alluded to in §1.2 and §2.2, Fritz Rohrlich offers a nuanced and comprehensive account of inter-theoretic reduction which among other things makes up for the shortcomings of Batterman's account. As suggested above in §2.2 regarding Rohrlich's (1994) CTM model of explanation, I likewise claim here that most of Batterman's analyses (minus his conclusions) can be subsumed under and incorporated in Rohrlich's general model.<sup>79</sup> By now, from some of my cursory description of Rohrlich in the above sections, the reader may have already formulated a coarse-grained conception to his approach in inter-theoretic reduction. Here I make the description more fine-grained.

Recall the description in §2.2 concerning the 'internal' structure of certain aspects of a non-developing (i.e., accepted, mature, or established) theory  $T$ :

- An *ontology*  $\mathcal{O}(T)$
- A set of central *terms*  $\tau(T)$ , with an accompanying *semantics*  $\sigma(T)$ .
- Set(s) of *principles*  $\Pi(T)$

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<sup>79</sup> Despite what I consider (and have stated already in several places) the deep conceptual resonances between Batterman's motley and Rohrlich's systematic and more comprehensive accounts, Batterman (to his detriment I believe) only gives a cursory mention to Rohrlich in his (2002) in p. 79, essentially just giving passing mention of his 'fine' versus 'coarse' distinction of theories.

- A *mathematical structure*  $\mathcal{M}(T)$
- A *domain of validity*  $\mathcal{D}(T)$

In Rohrlich (1988, 302) the list of aspects is presented in a subtly different manner.<sup>80</sup> In terms of theory-reduction, one should deal only with mature or established theories, whose characteristic components include:

- An *ontic* component  $\mathcal{O}$
- An *epistemic* component  $\mathcal{E}$
- A *language and conceptual content* component  $\mathcal{L}$ , which includes formal and informal language, and a subset of central terms  $\tau$ <sup>81</sup>
- Set(s) of *principles*  $\Pi(T)$
- A *mathematical-logical structure* component  $\mathcal{M}$ <sup>82</sup>
- A *domain of validity*  $\mathcal{D}(T)$

Rohrlich succinctly states that Nagel's (1974) model of reduction (as mentioned briefly in the preceding subsection above) holds between (mature) theories  $T$  and  $T'$  whenever there exists a

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<sup>80</sup> As mentioned above the list of aspects is by no means meant to be exhaustive, which reflects the anti-reductionism of structuralism in the sense of its repudiation of the attempt to reduce the semantic and syntactic content of scientific theories to formal axiomatic systems (recall n. 62 above). Hence no single list of structural aspects sufficiently constitutes a theory, let alone if such aspects were characterized in closed axiomatic form. Rohrlich and Hardin (1983) are even more explicitly adamant against axiomatic reductionism, which they are quick to mention is *not* what is implied by their model of inter-theoretic reduction. Scientists, they argue, should in general avoid axiomatization as the scheme "is difficult and in general equivocal." (605) (They proceed to mention the numerous schemes of attempts at axiomatizing quantum mechanics, all of which by nature are rather different, some even opposed). Instead they go on to say that scientists use *mathematical structures* of two or more theories, seeking to establish a 'conceptual dictionary' among notions conveyed by such mathematical structures which appear similar. (605) In yet another article, Aerts & Rohrlich (1998, 27) describe three kinds of reduction: a.) logical (i.e. reducing to some axiomatic framework), b.) theory reduction ('semantic reduction'), and c.) reductive explanation ('explanatory reduction'). They proceed to state that their paper will *not* cover logical reduction, since: "Logical reduction is a formal procedure that can be used in a scientific theory only *post facto*, after the theory has been formulated based on empirical information...*in no known case does axiomatization of a theory help to elucidate the scientific problems one encounters.*" (Aerts & Rohrlich (1998) 28, italics added)

<sup>81</sup> One recognizes this as a slightly more refined description of the set of central *terms*  $\tau(T)$ , with an accompanying *semantics*  $\sigma(T)$  mentioned in Rohrlich (1994).

<sup>82</sup> The essential importance of this component for mature scientific theories cannot be over-emphasized. Aside from its obvious feature including deriving the central equations of a theory, quantitative explanatory and predictive power:

[ $\mathcal{M}$  can probe where] human intuition fails...when the theory refers to those aspects of nature which lie outside our direct experience, the mathematical structure becomes the backbone of the scenario, [the model] which characterizes this indirect knowledge. [Moreover]...[t]he conceptual model associated with a theory is largely derived by confronting  $\mathcal{M}$  with empirical evidence and with neighboring theories (testing and coherence)...involv[ing] informal language and is not the result of logical-mathematical deduction. (Rohrlich (1988), 301)

As mentioned in n. 80 above, so this above passage likewise distinguishes a structuralist's approach to mathematical structure and their use from a logical reductionist, as evidenced in the implication of abductive reasoning "involv[ing] informal language....not the result of ...deduction."

mapping  $\Phi: \alpha(T') \rightarrow \alpha(T)$ , i.e. the central terms of  $T$  must be functions of those of  $T'$ .<sup>83</sup> On the other hand (recalling Nickles (1975)) physicists are generally intuitive about the issue of inter-theoretic reduction, typically deriving just the *mathematical structures* from one theory to another. Moreover, in this more pedestrian but representative case, the physicists:

...pay little attention to whether the concepts resulting from the physical interpretation of the symbols permit such a functional relation [a' la Nagel]...The mathematical structure or framework of the theory is considered to be primary, and the central terms (the meaning of certain central symbols) can be later derived from the applications of that framework to actual situations. (1988, 303)

The above point is used, for instance, to reconcile Feyerabend's theoretical pluralism (and its associated incommensurability issues, mentioned briefly in n. 48 above) and at the same time ensuring a well-defined logical-mathematical linkage between two theories  $T$  and  $T'$  by recognizing that such two theories can refer to different cognitive (or epistemic) levels: In other words the fact that a reduction relation may hold between  $\mathcal{M}(T)$  and  $\mathcal{M}(T')$  does not guarantee that such a relation exists between  $\mathcal{L}(T)$  and  $\mathcal{L}(T')$ ,  $\mathcal{O}(T)$  and  $\mathcal{O}(T')$ , or  $\mathcal{E}(T)$  and  $\mathcal{E}(T')$ , etc.: "The mathematical framework of  $[T]$  is rigorously derived from that of  $[T']$  (a derivation which involves limiting procedures); but the interpretation and the ensuing ontologies [of  $T$  and  $T'$ ] are in general not so related."<sup>84</sup> (1988, 303)

I bring this point up in anticipation of where I believe Batterman (2002, 2004, 2005) falls short: by not giving serious enough attention to the issue of carefully parsing a theory's mathematical components, from its associated epistemic, ontological, linguistic-conceptual components. (Recall I made a similar point already in §2.2 when discussing models of explanation). Here, however, in the context of a discussion on inter-theoretic reduction, the problem becomes more nuanced and serious. Prior to delving into this issue headlong, however, some further clarification is required concerning Rohrlich's notions of epistemic, ontological, and validity domain components.

#### **a.) The epistemic Component $\mathcal{E}$**

Recall the distinction between developing versus mature theories as discussed above. In an insightful commentary on Rohrlich, Ryszard Wojcicki (1998) writes:

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<sup>83</sup> If the mapping is surjective (onto, i.e.  $\Phi[\alpha(T')] = \alpha(T)$ ) then the reduction is homogeneous. Otherwise (i.e., if the mapping is strictly *into*:  $\Phi[\alpha(T')] \subset \alpha(T)$ ) the reduction is heterogeneous.

<sup>84</sup> Feyerabend of course may brush this aside as a red herring, or as just a re-statement of the problem of incommensurability. Recall in n. 75 Feyerabend's very point was just what Rohrlich (1988) seems to be reiterating: a mathematical reduction will not guarantee a semantic one. However, if one accepts the structuralist maxim of a theory being composed of a plurality of *aspects* including semantic, mathematical, ontological components, then Rohrlich's points make good sense: one can guarantee reduction in one aspect but not in others. Only if one held fast to some reductionism claiming that the *semantic content* is what is essential to a theory (i.e., its  $\mathcal{L}$ ,  $\mathcal{O}$ ,  $\mathcal{E}$  components) does the incommensurability issue then become a more serious concern.

Rather than treating a theory which has reached the mature stage as a partially adequate description of the external world, Rohrlich (if I convey his position correctly) treats it as a cognitive counterpart of...*ontological levels*, or perhaps I should say ‘ontological regions of reality.’ (38)

In other words, what distinguishes a mature theory are distinctively stable reciprocal dynamics between its cognitive (or epistemic) and ontological levels. Such a stable correspondence implies (within its domain of validity) that one can associate a distinctive cognitive level associated with a robust ontological level:

The existence of different concepts on different levels justifies one’s talking about *qualitative* differences between levels...It thus follows that one level does not make another level superfluous. Both are needed; which theory is the suitable one depends on the domain of parameters...[t]he concepts we employ, the questions we ask, and the answers we are prepared to accept will be controlled by the domain of discourse—the ontological level—which we intend. (Rohrlich & Hardin (1983), 610)

So the ontological component, the epistemic component, and the validity domain of a mature theory ( $\mathcal{O}$ ,  $\mathcal{E}$ ,  $\mathcal{D}$ ) all mutually co-refer in important ways. Yet each aspect or component has its distinct features as well, so they can (to a certain extent) be considered independently of each other. In the case of  $\mathcal{O}$ , I will mention in passing that it forms such a crucial tier of my discussion as to deserve its own major section (see §4 below), because it remains inextricably tied to notions like verisimilitude and representation. The validity domain  $\mathcal{D}$ , on the other hand, depends crucially on extensions of Nickel’s (1975) ‘domain preserving’ reduction, that Batterman (2007) extended in his **Schema R** (discussed in the previous subsection above).

The issue of the epistemic aspect of a mature theory  $\mathcal{E}$ , as hooking into a coherent and consistent ontological aspect  $\mathcal{O}$ , is best illustrated by way of a counter-instance, as what would occur in the case of a *developing* or immature theory. Developing theories do not yet possess a stable ontological aspect  $\mathcal{O}$ , hence their epistemic component is volatile. To name one contemporary instance: consider the case of String Theory. This developing theory’s greatest strength is also its chief weakness: String Theory possesses a richly mathematical component  $\mathcal{M}$  at the expense of its epistemic and ontological components. Efforts to ‘interpret’ the theory range from some extremely dubious version of Platonism (Brian Greene) in which an ontology is imposed in a ham-fisted manner relegating most of the theory’s essential terms to unobservable abstractions, devoid of any operational content.<sup>85</sup> Other interpretations verge on the instrumentalist, regarding some of its mathematical results as empirically adequate at best, but the essential terms are devoid of ontological content aside

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<sup>85</sup> See Finkelstein (1996, 2001, 2004*a-c*, 2007) for criticism of this developing theory.

from predictive value. A similar case can be made for developing versions of Ptolemaic astronomy in Antiquity (as opposed to the late Middle Ages), despite its mathematical sophistication.

On the other hand, in the case of mature theories, cognitive levels occur in  $\mathcal{E}$  due to “cognitive (or epistemic) emergence.”<sup>86</sup> (Rohrlich (1988) 3) Rohrlich’s notion of cognitive emergence is similar to the notion of ‘epistemic emergence’ discussed in Humphreys (1997), Silberstein & McGeever’s (1999), and in Kronz & Tiehen (2002) in that the notion spells no ontological difficulties: Cognitive emergence is *contextual* insofar as it is entirely constituted by the relationship our cognitive apparatus has with its referent. An apparent emergence of new objects (atoms, stars, organisms, etc.) having certain unique properties identified by humans’ cognitive apparatus:

suggest...*something qualitatively new* has evolved...[only] because it differs perceptively from anything that there was at the earlier stages [of cosmic evolution]; there is a recognition of this fact that is sudden despite the realization that nothing discontinuous has happened. (Rohrlich (1988), 298)

In other words, such ‘new’ objects are characterized via an *idealization*: “their detailed structure has become unimportant. Characterizations are *approximations*...beyond a certain observational precision they become empirically inadequate.” (298-299) It is a short step to realize the ubiquitous and unremarkable fact of epistemically emergent cognitive levels once one accepts the truism that “it is only through idealizations, and what...we can think of as their alter-ego—inexact truths—that we have access to the world.”<sup>87</sup> (Paul Teller (2004b) 447)

### **b.) The Ontological Component** ⊙

I devote a separate section (§ 4 below) on more general issues of ontology, vis-a-vis verisimilitude from the standpoints of Rohrlich (1988, 1994), Teller (2004b), Yablo (1998), in a further effort to solidify my case concerning some of the shortcomings of Batterman. Here I present Rohrlich’s views (in a somewhat perfunctory fashion) of the ontological component of mature theories, withholding detailed philosophical comment.

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<sup>86</sup> I discuss the issue of emergence in greater detail in Chapter 2 below. See also Kallfelz (2009).

<sup>87</sup> Paul Teller’s (2005) argument is certainly not some endorsement of idealism of sense-data characteristic of certain elements of British empiricism from the 17<sup>th</sup> to the 20<sup>th</sup> centuries: “The British empiricists thought that thinking consists in having a stream of ‘ideas’ [representations], and concluded mistakenly that all we ever think *about* are our own ideas.” (Alan Musgrave (1985), in Curd & Cover (1998), n. 2, 1223-1224) Teller’s claim comes as a concluding statement of his argument against quantitative verisimilitude, i.e., that there exists some context-independent way of determining ‘closeness to truth’ of our theories. Teller argues that ‘closeness to truth’ is an inevitably contextual notion and recognizing this entails that the distinction between a ‘foundational theory’ and ‘phenomenological theory’ is likewise context-relative: Foundational theories distort, approximate, and idealize as much as ‘phenomenological’ theories do. Conversely, “[Though] I accept that foundational theories do tell us a great deal about how the world really is. I note also that many ‘phenomenological’ theories [however] ...tell us about the world in the same kind of way that the foundational theories do.” (Teller (2004b), 446) I will discuss Teller’s insights in greater detail in §4 below.

As described above, the epistemic component of mature theories corresponds with a robust ontology in a stably reciprocal manner, underwritten by the inevitably idealizing activity of both: For instance in the epistemic component of classical mechanics the emergent cognitive level of ‘massive bodies subjected to macroscopic forces’ corresponds to the ontological component of the theory containing ‘fallible veracities’<sup>88</sup> like ‘point mass,’ ‘frictionless planes,’ etc., rendered possible only through an idealizing activity ignoring details of the massive bodies’ constituents at the molecular, or atomic, or nuclear, or sub-nuclear, or Planck scales, etc.

A central metaphysical point that Rohrlich makes from the above is his advocating a *pluralist ontology, constituted by a substantial monism*:

[I]t is our cognitive capacity, our ability to perceive, to recall, to recognize, and to draw analogies [all inevitably idealizing activities] that is...responsible for this pluralistic nature of our ontology. We...encounter it in the *cognitive emergence* of new objects...[nevertheless the standpoint of] cosmic evolution is in support of the notion of the unity of nature (substantive monism).<sup>89</sup> (1988, 297)

There is nevertheless a substantial monism as entities evolve continuously (or quasi-continuously in the case of quantum mechanics<sup>90</sup>) “unfold[ing] into increasing complexity.” (298)

The idealization underwriting the conceptual levels of the epistemic as well as the associated ontological components of a mature theory corresponds to a *level of coarseness* (determined by the extent of the idealization and simplification) for the basic level of the domain of scientific inquiry. “I prefer the terms ‘coarser’ and ‘finer’ level of theory [rather than]...terms such as ‘more fundamental’, ‘superseding’, ‘supervening’, ‘primary’, etc. [as the latter notions] prejudice the case.”<sup>91</sup> (299) Hence in this context, the convention I have been adopting for preceding and superseding theories ( $T$  and  $T'$ , respectively) apply equally well to Rohrlich’s ‘coarser’ and ‘finer’ theories; i.e., theories  $T$  and  $T'$ , with the former whose ontological component  $\odot(T)$  is coarser relative to the latter’s  $\odot(T')$ . Moreover, though most physical theories have an ontological component at a certain level of

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<sup>88</sup> A term Teller (2004b) suggests one should use in lieu of ‘useful fictions.’ “[I]mperfect characterizations [still] genuinely inform...just calling them ‘fiction’ thus misleads. But we do want to acknowledge that these characterizations are not simply true.” (445)

<sup>89</sup> The notion of cognitive emergence (vis-à-vis substantial unity in cosmic evolution) is resonant with some of A. N. Whitehead’s (1929/1978) ideas: “*Process and Reality* divides actual entities/occasions into four grades of ascending complexity...[which] is not a fundamental division according to kind or essence, but a qualitative classification by complexity, and a coarse one at that.” (Finkelstein & Kallfelz (1997), 289). For a review of certain contemporary notions of emergence with respect to the implied substantial monism of Whitehead, see also Kallfelz (2009)

<sup>90</sup> “The discontinuities in quantum mechanics do not prevent predictability but they restore it to a probabilistic one.” (Rohrlich (1988), n.1 298)

<sup>91</sup> Note however, such terms apply just to the physical sciences, where the size of an object is a determining factor. “[F]or other scientific levels qualitative distinctions may dominate over quantitative ones.” (299) It is this distinction of coarse versus fine that Batterman (2002) gave passing mention to (recall n. 79 above).

coarseness, some ‘framework theories’ like mechanics (whether classical, statistical, or quantum) have ontological components containing several levels of coarseness.<sup>92</sup>

### c.) The Validity Domain $\mathcal{D}$

The reduction of a coarser theory  $T$  to a finer theory  $T'$  requires  $\mathcal{M}(T')$  to converge to  $\mathcal{M}(T)$  whenever the validity domain of  $T'$ , i.e  $\mathcal{D}(T')$ , is restricted to that of  $\mathcal{D}(T)$ . Echoing Nickles’ (1975) domain preserving notion of intertheory reduction, the above necessarily involves a limiting process.<sup>(303)</sup> This limiting process involves a parameter  $p$  which must be dimensionless (recall n. 77 above) as well as have the functional form  $p = f(x, x')$  where:  $x'$  is a quantity or array of quantities in  $\mathcal{M}(T')$  and  $x$  is a quantity or an array of quantities in  $\mathcal{M}(T)$ . “Given the finer theory [alone], it is not obvious what the characteristic parameter  $p$  actually is. It becomes evident only when the coarser theory is known.” (304) For example, in the previous example mentioned above involving momentum in finer theory of Special Relativistic Dynamics (SRD) vis-a-vis the coarser one of classical particle dynamics (CPD) a natural choice is  $p = \frac{v^2}{c^2}$ . In the case of the reduction of electromagnetism (EM) to geometric optics (GO),  $p = \frac{\lambda}{L}$ , where  $\lambda$  is the wavelength of the EM wavefront, and  $L$  is the slit width. In the case of the Bohr Correspondence Principle between non-relativistic quantum mechanics (NRQM) and classical mechanics (CM),  $p = \frac{f(\hbar)}{R}$ , where  $f$  is some analytic function<sup>93</sup> of  $\hbar$  with range values expressed in length dimension, and  $R$  is the average radius of the spatial region.<sup>94</sup>

Hence borrowing from Batterman’s **Schema R** notation, one can characterize the reductions as:  $\lim_{p \rightarrow 0} \mathcal{M}(T') = \mathcal{M}(T)$  whenever  $\mathcal{D}(T')$  is restricted to  $\mathcal{D}(T)$ . However, whenever such a reduction holds, it does *not* follow that there exists some mapping  $\Phi: \mathcal{M}(T') \rightarrow \mathcal{M}(T)$ , which would signal a stronger case of semantic reduction (a’ la Nagel) (302). Also, the reduction need not be unique: There can exist several parameters  $p_1, p_2, \dots$  such that:  $\lim_{p_1 \rightarrow 0} \mathcal{M}(T') = \mathcal{M}(T_1)$ ,  $\lim_{p_2 \rightarrow 0} \mathcal{M}(T') = \mathcal{M}(T_2)$ , etc.<sup>95</sup>(305)

<sup>92</sup> In the case of mechanics: the distinction between particle and rigid body dynamics. The latter corresponds to a finer ontological level relative to the former since accounting for torques, angular momenta and rotational inertia on the body necessitates that it *cannot* be modeled as a single point particle. In the case of statistical mechanics, the science “interpolates between levels of the microworld and the macroworld.” (1988, n. 2, 299) Also in the case of quantum mechanics (non-relativistic and relativistic) its ontological component is not restricted to one level of coarseness either, ranging from the nucleonic to macroscopic in the case of Bose condensations.

<sup>93</sup> I.e. a ‘smooth’ or continuously differentiable (to all orders) function  $f(x)$  (real or complex-valued). In the complex case, every differentiable function is automatically analytic. Every analytic function can be expressed as a convergent power series, hence its limit behavior is everywhere well-defined.

<sup>94</sup> Note however in other cases of reduction of NRQM to NM, one could also choose the more elementary  $p = (\# \text{ of quanta})$  in the  $p \rightarrow \infty$  limit.

<sup>95</sup> For an interesting case, see Finkelstein et. al. (2001) who develop several Clifford algebraic contraction parameters in their general Clifford algebraic quantum space-time formalism, and proceed to show how their

The parameter  $p$  is naturally interpreted as establishing a validity domain  $\mathcal{D}$  of a theory. “A *validity limit* is thus equivalent to a specification of the error made by using the lower level [coarser theory  $T$ ] instead of the higher level theory [ $T'$ ].” (Rohrlich & Hardin (1983), 607) Hence in terms of  $T'$  any prediction made by  $T$  should be multiplied by the factor  $(1 \pm p)$ . For instance, in the case of NM predicting the motion of the Earth vis-à-vis SRD, the former is subject to measurement error  $p = \frac{v^2}{c^2}$ , where  $v$  is the average speed of the Earth relative to the Sun, hence the predictions of NM are accurate to within  $(1 \pm 10^{-8})$ . This establishes of course a measure of the reliability of NM’s predictions, hence its validity domain  $\mathcal{D}(\text{NM})$ . Validity limits characterize theories as approximate (in the light of their finer counterparts), however “[i]n most cases the approximation involved, is *extremely good*.” (Rohrlich & Hardin (1983), 608)

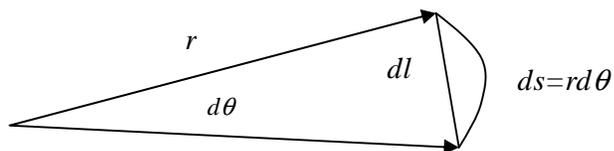
The validity domain’s connection with the ontological component  $\mathcal{O}$  is apparent in the following sense: an ontological level naturally corresponds to a case in which  $p$  is negligible to a sufficiently good approximation.

Since  $p$  either is or is not negligible, there is no intermediate situation. But what makes this definition of ontological level...is the large size of the domains of validity of theories: it spaces ontological levels far apart. (609)

Regarding the aforementioned issue of conceptual emergence:

[T]here is in many cases no simple relation between the concepts of theories on two different [ontological] levels. The limiting procedure that relates [ $T'$ ]...to [ $T$ ] can in fact create *new* concepts...not present in the higher level theory. (ibid)

By way of an elementary calculus example (reminiscent of Batterman’s (2003b) example of  $\epsilon x^2 + x - 9 = 0$  discussed in Subsection 1 above) Rohrlich & Hardin demonstrate this in terms of an arclength of a circular sector  $ds = rd\theta$ , compared to the length of its inscribed secant  $dl$ :



**Fig. 1.1: Representation of the secant-tangent relation in Rohrlich’s illustration concerning epistemic emergence and ontological levels of coarseness.**

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Clifford commutation relations converge to the classical symplectic algebra in the limit of one of their contraction parameters, versus the former converging to the Heisenberg algebra for another contraction parameter.

Now in the limit:  $r \rightarrow \infty$ , then  $ds \rightarrow dl$ , hence  $ds$  assumes the property ‘straight’. “The property ‘straight’ did not exist on the circle but was produced by the limiting procedure.” (609) In an emblematically physical example of the same conceptual kind, in the  $N \rightarrow 0$  limit (where  $N$  is the number of bodies of appreciable mass) in a local space-time region  $M$ ,  $g^{\mu\nu}(x) \rightarrow A^{\mu\nu}$ , where  $g^{\mu\nu}(x)$  is the variable metric of general relativity (GR) at a space-time point  $x \in M$ , and  $A^{\mu\nu}$  is the constant Lorentz metric characterizing flat Minkowski space-time in special relativity (SR). Various space-time symmetries occur in such a manner exhibiting Poincare’ Group invariance in SR dynamics, but this property doesn’t manifest in the curved and dynamical space-time of GR. (610)

Last of all, despite this seemingly facile characterization of the limit:

$\lim_{p \rightarrow 0} \mathcal{M}(T') = \mathcal{M}(T)$  in *theory*, it remains a delicate and complicated procedure to attempt to carry it out in *practice*<sup>96</sup>:

The limiting process involved can be very complicated as well as very subtle. Some of the limiting processes have so far not been carried out in a mathematically satisfactory way, but for enough to satisfy the intuitive expectations of the physicist. (Rohrlich & Hardin (1983), n.3, 605)

The reduction of the symmetry properties of  $[\mathcal{M}(T')]$  to those of  $[\mathcal{M}(T)]$  plays a very significant role...[f]rom a technical point of view, it shows that the limiting process is highly nontrivial and must be carried out very carefully: the symmetry reduction may be the result of group contraction, and the limit can only be carried out in suitable group representations. But we shall not pursue these mathematical matters here. (Rohrlich (1988) 304)

### **Subsection 3: Do Batterman’s Claims Reduce to Rohrlich’s?**

After having presented the arsenal of Rohrlich (et. al.) concerning the characterization of inter-theoretic reduction, can one argue that they subsume Batterman’s notions? Certainly Batterman (2002, 2004, 2005) and Rohrlich (1988) share many thematic resonances. Rohrlich speaks of ‘ontological levels’, ‘coarse and fine’ theories, etc., whereas Batterman (2004, 2005) distinguishes ‘epistemically fundamental’ from ‘ontologically fundamental’ theories. Ontologically versus epistemically fundamental theories supposedly aim at conflicting purposes: The former strive to get the fundamental ontology accurate at the expense of explanatory power, while the latter sacrifice in

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<sup>96</sup> A noteworthy example is Ehlers (1986) who, inspired by Rohrlich & Hardin, constructed two concrete case studies rigorously demonstrating the reduction of Lorentz invariant scattering theory to Galilean invariant scattering theory (390-396), as well as a partial reduction of GR to Newtonian gravitation (396-400). The technical rigor and mathematical sophistication should prove itself to be convincing enough of the inherent challenges regarding the attempt to carry out the limiting procedure in practice.

ontological detail to boost explanatory strength. Seen however from Rohrlich's framework, one could argue that the distinctions Batterman presents are perhaps just clumsy attempts on his part to speak of finer (read: ontologically fundamental) versus coarser theories (read: epistemically fundamental). After all, as mentioned in the above section, epistemic emergence can occur at the coarse level vis-à-vis the finer level, consistent with Batterman's (2004, 2005) characterization of the Navier-Stokes theory of fluid mechanics as epistemically fundamental, with its ontology of continuous and incompressible fluids, as opposed to a discrete molecular ontology comprising the ontologically fundamental theory. Approximating a discrete collection of  $N$  molecules in the limit  $N \rightarrow \infty$  by a *continuum* is downright *logically* inconsistent,<sup>97</sup> so one must conclude that the ontology of continuous fluids does not semantically correspond to anything in the ontology of the finer theory.

However, setting aside such considerations for the moment, one must confront a primarily serious difference between Rohrlich and Batterman. Rohrlich confines his cases to reductions in the mathematical components of the respectively fine and coarse theories, i.e.  $\lim_{p \rightarrow 0} \mathcal{M}(T') = \mathcal{M}(T)$ , while the overarching theme in *all* of Batterman's case studies are the *singular cases*:  $\lim_{p \rightarrow 0} \mathcal{M}(T') \neq \mathcal{M}(T)$ . Could there be a difference any more dramatic and basic? Answer: only when one is *convinced* that there exists no possible way to characterize  $T$  and  $T'$  in *any* mathematical formalism such that the hopes of a reduction may be realized after all. In other words, when one assumes that Batterman's notion of the singular case:

$\lim_{p \rightarrow 0} \mathcal{M}(T') \neq \mathcal{M}(T)$  is universally quantified over the domain of all possible mathematical formalisms  $\mathcal{M}$ .

I, for one, am not convinced, and in the ensuing chapters 2 and 3 below I argue by way of a counterexample  $\mathcal{M}^*$  to show that even though  $\lim_{p \rightarrow 0} \mathcal{M}(T') \neq \mathcal{M}(T)$  holds for *some* mathematical characterization  $\mathcal{M}$  of  $T'$  and  $T$ , respectively, nevertheless  $\lim_{p \rightarrow 0} \mathcal{M}^*(T') = \mathcal{M}^*(T)$  in the case of a Clifford-algebraic characterization  $\mathcal{M}^*$  of  $T'$  and  $T$ . I am only pointing out the issue here and will clarify the logical and mathematical nuances in the appropriate sections of the chapters below. Like Ehlers (1986) has shown via Lie algebras, I am taking up the charge to "pursue the mathematical matters" further, as Rohrlich (1988) certainly leaves enough room in his framework beckoning one to do just precisely that.

So once the mathematical stool has been kicked out from under Batterman's feet, where does that leave his claims? As I mentioned briefly in the earlier sections above (and follow in more detail in Chapters 2 and 3 below), Batterman (2007) strenuously objects to Belot's (2003) claim that once this mathematical stool of asymptotic analysis get kicked out [Belot (2003) wields the club of the general theory of differential equations to knock over the stool] that his theory of explanation

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<sup>97</sup>  $N \rightarrow \aleph_0$ , i.e.  $N$  will converge to the discrete infinity, and  $\aleph_0 < C$ , where  $C$  the power of the continuum.

implodes. Batterman argues that there are significant semantic and ontologically pluralist features of his theory of asymptotic explanations (2002) that are not susceptible to the vicissitudes of mathematical characterizations. To which I respond: correct. The epistemic and ontologically pluralist aspects of Batterman’s theory are toy instances of Rohrlich’s general framework of reduction, to put the matter (again) somewhat glibly. (I will discuss the issue of pluralism in greater detail in my Concluding chapter 4 and in §4 below, after going through Batterman’s case studies in greater detail). I can finish here by pointing out that Rohrlich’s model exhibits an epistemic and ontological pluralism, but not a methodological one, in the sense that reductions  $\lim_{p \rightarrow 0} \mathcal{M}(T^p) = \mathcal{M}(T)$  can still hold.

#### Section 4 : Ontology

As mentioned above in §3.2 and elsewhere, a discussion concerning the ontological aspect  $\mathcal{O}(T)$  of any theory  $T$  is incomplete, without at least mentioning in some detail how one might best characterize its relation to ‘the world’. Or putting the matter in more specific terms: the previous sections only covered half the story in its treatment of how the ontological aspect relates to *other aspects of a theory  $T$*  in Rohrlich’s framework. I have so far covered only the issue of what role the ontological component plays *internal* to a given theoretical framework. One is naturally left to ask, given Rohrlich’s provocative metaphysical claims concerning a substantial unity *qua* ontological pluralism, how can one best *externally* (i.e., external to a theoretical framework) characterize the relation of the ontological aspect? How does Rohrlich present the kind of relation  $\mathcal{O}(T)$  has to the world ‘outside’ of  $T$ ?<sup>98</sup> One would wish for a *metaphysical* account here (no matter how weak).

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<sup>98</sup> Asking such a question obviously does *not* commit one to the stronger and ultimately untenable claim of there being some strict distinction between theoretical and theory-neutral ‘observational’ language, as held by some early logical empiricists. By the same token, to suggest what Feyerabend (1963) and undoubtedly others advocating a strong case for incommensurability, that one *presupposes* such a distinction when asking how a theory’s ontological component ‘hooks up’ to the ‘world’, is to commit the fallacy of dichotomy by equivocating the weaker notion of ‘external to theory  $T$ ’ with the much stronger notion of ‘theory-neutral’:

Feyerabend has difficulties providing a firm observational basis for objectively assessing the empirical worth of proposed hypotheses...from what I believe is his exaggerated view that the meaning of every term occurring in a theory or in its observation statement is *wholly and uniquely determined by that theory*...although both ‘theoretical’ and ‘observational’ terms may be ‘theory laden,’ it does not follow that there can be no term in a theory which retains its meaning when transplanted into some other theory. (Nagel (1974), in Curd & Cover (1998), 918-919, italics added)

This ‘exaggerated view’ would equivocate ‘theory-external’ with ‘theory-neutral’. (Recall Musgrave’s (1985) similar objection regarding some in the British empiricist tradition committing the category mistake of confusing thoughts-as-representations versus their *contents*, i.e. ‘all we can ever *think about* are representations’, mentioned in n. 87 above.) Certainly, one could characterize the  $T$ -internal versus  $T$ -external worlds or realms of discourse without smuggling in notions like ‘theory-neutrality’ in any number of ways. To illustrate one example, one may choose to adopt Nickels’ (1975) theory/meta-theory distinction. “The theory/metatheory distinction is context-dependent and is not intended to demarcate essentially different modes

Conversely, offering up an account of  $\mathcal{O}(T)$  in solely epistemic terms like ‘levels of cognitive emergence’ and ‘idealization’ (as discussed in §3.2) merely displaces the question: One would then immediately ask *how* is the mind-world (and in turn the ontology-world) relation set up to make Rohrlich’s characterization of a pluralist ontology *qua* substantial monism plausible, or even possible?

To clarify this issue here in some detail goes beyond a mere exegesis of Rohrlich. Among other things, this certainly revives issues discussed by (and debated between) Rudolf Carnap (1956) and W.V.O. Quine (1948, 1951*a*, 1960)—aspects which have brought a fresh reinterpretation from Stephen Yablo (1998) which prove to be directly relevant to the discussion here, as I argue below. Moreover, as suggested in the previous sections, so here the discussion has direct impact on Batterman (2002, 2004, 2005). What I argue is that the broader discussion presented here, involving issues of verisimilitude, idealization, and contextuality, spell further trouble for Batterman: His categorical distinctions of ‘fundamental’ versus ‘phenomenological’ (2002), as well as ‘ontologically fundamental’ versus ‘epistemically fundamental’ theories<sup>99</sup> (2005) are further called into question. On the other hand, Rohrlich’s framework is a typical instance of a contextualist account of verisimilitude.

### Subsection 1: Verisimilitude Contextualized

One...[can] suggest...the more limited goal of developing theories which approximate more closely to the truth, i.e. possess increasing verisimilitude. However, *there are severe problems involved both in defining and in developing a measure or ranking-mechanism of, verisimilitude.* –James Logue (in Honderich, ed. (2005), 944, italics added)

Echoing Logue’s statement above, Rohrlich writes:

There exists a naïve view of science in which all scientific laws and descriptions are judged as strictly true or false. *This view leads to the disastrous consequence that all present scientific knowledge may be false since the ultimate theory (the only true one) has yet to be found.* But scientific statements can be judged true or false *only within the validity limits of the corresponding theory.* (Rohrlich (1994), 75, italics added)

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of...inquiry.” (Nickels (1975) in Curd & Cover (1998), n. 22, 969). I will focus on the issue of contextuality in some detail in this section.

<sup>99</sup> Obviously there is quite a bit of conceptual overlap between these two distinctions. Nevertheless, they are not coextensive, for reasons I discuss in chapter 3 below. One could perhaps easily think of counter-examples based on what I have summarized thus far, however. A fundamental or superseding theory that is epistemically (but not ontologically) fundamental would include thermodynamics replacing Carnot’s caloric fluid theory. Another example would of an epistemically fundamental theory that is considered ‘fundamental’ (i.e. superseding) would include General Relativity (GR) (superseding Newtonian Mechanics). GR is epistemically fundamental in its vast explanatory scope, which obviously hides the significant quantum-topological effects that would of course dominate on the Planck scale. Conversely, examples of ontologically fundamental theories which are superseded would include NRQM being superseded by QFT (if one bases the ‘fundamental’ ontological level on the atomic scale).

To recall the discussion in §3.2 above, such validity limits always belong to some cognitive level vis-a-vis its “appropriate ontology.” (ibid) Hence:

The truth of a scientific theory can therefore be justified only by the empirical evidence gathered on the particular cognitive level of that theory. Evidence from a different such level cannot be held against it. *Nor can one level of ontology be fully reduced to another...Cognitive levels and ontologies therefore do not supersede one another:*<sup>100</sup> they complement one another representing different faces of the same part of the world. (ibid)

Prima facie the above passages articulate the pith of Rohrlich’s nuanced views on the matter of verisimilitude. The last phrase, for example, illustrates the metaphysics of plural aspects (“representing different ontologies”) which serves to reconcile ontological pluralism with substantial monism (“same part of the world”). The first obvious question is whether or not Rohrlich’s views succeed in addressing “the severe problems” concerning the specification of a measure or ranking-mechanism. The second battery of questions might reflect concerns regarding the presumed consistency or very coherence of his claims.

To initially respond to the first (and undoubtedly simpler) question: Rohrlich suggests a ‘ranking’ order of verisimilitude to be sure, *but one that is not independent of the context of a particular (non-developing) theory or theories.* Recall in the discussion in §3.2 the ranking systems specified by degrees of coarseness. This epistemic notion (insofar as ‘degree of coarseness’ involves the act of idealization and abstraction; i.e. deliberately ignoring details of sub-constituents of the system under study) corresponds to the appropriate level of ontology, which itself is determined by the significance of parameter  $p$  with respect to the appropriate validity domain  $\mathcal{D}$ .<sup>101</sup> I abbreviate this notion by writing: ‘ $\mathcal{E}_p(T) \approx \mathcal{O}_p(T)$ ’ whereby the epistemic level of coarseness corresponds to its ontological level in the mature theory  $T$ , as indexed by the reduction parameter  $p$ .<sup>102</sup> So one can naturally and rigorously specify a verisimilitude ranking *within* the context(s) of one or more mature theories  $T'$  and  $T$ .<sup>103</sup> But there is no such thing as a *context-independent* notion of a verisimilitude

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<sup>100</sup> Only the *mathematical aspects* of the theories do: A finer theory has a mathematical component superseding the coarser one in its descriptive generality and greater resolving power.

<sup>101</sup> Recall from §3.2: “Since  $p$  either is or is not negligible, there is no intermediate situation. But what makes this definition of ontological level...is the large size of the domains of validity of theories: it spaces ontological levels far apart.” (Rohrlich & Hardin (1983), 609)

<sup>102</sup> Moreover, recall that this reduction parameter relates the *mathematical aspects* (or components) of mature coarser and finer theories  $T, T'$  according to the following schema:  $\lim_{p \rightarrow 0} \mathcal{M}(T') = \mathcal{M}(T)$ . ‘Coarse’ and ‘fine’ in turn are abbreviations of the associated cognitive levels instantiated by the epistemic aspects of  $T'$  and  $T$  respectively: i.e.  $\mathcal{E}(T')$  and  $\mathcal{E}(T)$ .

<sup>103</sup> For the single theory case, recall (n. 89 above) that *some* theories, i.e. ‘framework theories’ like classical mechanics, statistical mechanics, and quantum mechanics exhibit ontological pluralism in and of themselves. Hence one can speak of degrees of verisimilitude with respect to the validity domains internal to their distinct

metric: Echoing Carnap's claims (1956) it is meaningless to specify a verisimilitude measure *external* to any theoretical framework(s). Rohrlich's framework-driven or contextual notion of verisimilitude dovetails with some of the more general discussions of contextualism of Paul Teller (2004b) and Stephen Yablo (1998), as I shall argue below.

As for the second set of questions concerning the consistency or coherence of Rohrlich's claims, one could cite concerns that ontological pluralism is inextricably caught between the Scylla and Charibdes of the undesirable extremes of Meinong's absolute world populated with fictitious objects versus sheer ontological relativism. Lawrence Sklar, in his response to Cartwright (1999), objects to any notion of ontological pluralism perhaps on account of the aforementioned concerns:

Nothing in the admitted variety of our conceptual and explanatory schemes, even if that variety is admitted to be intrinsically ineliminable for adequate description and explanation...by itself is good reason for denying the universal domain or the appropriate domain for the truth of foundational physics. (Sklar (2003), 441)

Paul Teller (2004b) however responds to Sklar's claims by pointing out that his argument appears to presuppose a rigid distinction of such notions as 'the way things really are' versus 'useful fictions'. Such a rigid distinction is only possible to maintain if one were to repudiate any notion of contextual verisimilitude in favor of truth *simpliciter* or some *context-independent* measure of verisimilitude:

Many of Sklar's arguments depend for their cogency on the assumption that it is truth, *not some kind of approximation to truth*, that is in question...[Sklar denies ontological pluralism because] [w]here ontologies conflict, at most one can be right. Foundational theories are true, phenomenological theories merely false, 'useful fictions', so we should accept the ontology of the true, foundational theories and not those of false phenomenological theories...But in the light of the foregoing acknowledgment, *this shows nothing about the ontologies of the theories we actually have*. (428-430, italics added)

Teller's response to Sklar puts the onus on the ontological monist to give such an account of such a credential of ontological monism as presumably underwritten by a 'fundamental' theory. Teller's arguments rest on notions of idealizations and contextualism that I will elaborate on in greater detail in the next subsection below.

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cognitive ontological level(s). For instance, within classical mechanics, the analysis of rigid body motion (translational + rotational dynamics) is of a finer cognitive level than the analysis of translational motion alone: The latter's ontological level consists of point-particles or (at best) bodies exhibiting spherical symmetry. On the other hand, the former consists of rigid bodies (suitably idealized to ensure linearity in specifying the Euler equations in generalized coordinates) *not* idealized as point-masses (to ensure a non-trivial depiction of their associated inertia tensors). Hence the analysis of the dynamics of rigid body motion exhibits a higher degree of verisimilitude compared to its counterpart of merely translational dynamics.

However I mention some of Teller's claims here in passing to illustrate the way a plural ontologist may avoid the extremes of ontological absolutism and relativism by adopting Teller's position of contextual verisimilitude—if not Rohrlich's. Indeed, as I shall discuss in greater detail in the following subsection below, Teller shares quite a few points in common with Rohrlich when it comes to such issues as contextual verisimilitude and the irreducible element of idealization in *all* of our epistemic activity.<sup>104</sup>

Regarding the question concerning the consistency of Rohrlich's (1988, 1994) above claims, one must not forget that he places emphasis entirely on *non-developing, physical* theories. As mentioned in the previous sections, ruling out examples from biology,<sup>105</sup> (not to mention psychology) eliminate the inevitably many counterexamples one could easily concoct.<sup>106</sup> Add to that, the non-developing characteristic of physical theories (whether accepted, mature, or established) is likewise significant: Their epistemic and ontological components have achieved a level of relative stability or dynamical equilibrium exhibiting “ontological levels spaced [far enough] apart.” (Rohrlich & Hardin (1983), 609). The element of cognitive emergence hence justifies the claim that “[no] ...one level of ontology be fully reduced to another,” (Rohrlich (1994), 75).

## **Subsection 2: Contextualism, Idealizations, and ‘Fallible Veracities’**

Aside from presenting a (theory) framework-dependent notion of degrees of coarseness (regimented according to  $p$ ) and making some general claims concerning the importance of adopting a position of verisimilitude, Rohrlich (1988, 1994) offers little more in the way of clarification on these matters. I have argued in the subsection above that Rohrlich's notion of verisimilitude is *contextual* in the sense that any measure or “degree of closeness to ‘the truth’” is entirely constituted by the theory-dependent parameter of reduction  $p$  delimiting a theory's validity domain. Yet this proves somewhat dissatisfying, as the charge is to provide some *general* insights (metaphysical or otherwise, no matter how weak) which could underwrite Rohrlich's pluralist ontology and contextual verisimilitude in ways that do not inevitably defer to his machinery of cognitive degrees of coarseness in a seemingly question-begging fashion.

Several of Rohrlich's more recent commentators (Dieks & de Regt (1998), Wojcicki (1998)) attempt to articulate more general claims along these lines, whether by launching into a discussion on

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<sup>104</sup> Though Teller (2004b) never cites Rohrlich in his article.

<sup>105</sup> Whose theories and their interrelations may be characterized by a set of more complex *qualitative* aspects, hence inapplicable to such a schema delimiting validity, epistemic, and ontological domains by a straightforward quantitative measure  $p$ .

<sup>106</sup> Consider, for instance, the case of psychology and cognitive science: Questions concerning issues like downward causation and emergence are by no means settled. Certainly emergence and downward causation act as counterexamples to Rohrlich's above claim that evidence on one level cannot be held as evidence for or against evidence on another level.

supervenience (Dieks & de Regt)<sup>107</sup> or on the idea of substantial unity qua mereological reduction (Wojcicki).<sup>108</sup> In reading such commentaries, however, one gets the sense that the authors' efforts are spent more in the direction of finessing some metaphysically *descriptive* fine points, rather than actually introducing a more general project by which the ontological pluralism of Rohrlich (and others with similar claims) can be *justified*.

In this respect, Paul Teller (2004b) (some of whose critical responses to Sklar (2003) were briefly summarized in the subsection above) provides in my opinion a more satisfactory account justifying ontological pluralism vis-a-vis a contextual notion of verisimilitude. Echoing Logue's pessimism concerning severe problems of defining a context-independent notion of verisimilitude, Teller's chief critical point is aimed at Sklar's presumed fixed distinction between 'fundamental' versus 'phenomenological' theories. The distinction rests on what Teller considers to be an untenable notion—fundamental theories are “‘on the road to our desired [I presume Sklar means true] ultimate theory’,” (431) while the latter are comprised of ‘convenient fictions.’

For instance, Teller argues that according to Sklar, there is something categorically different about talk of nucleons as being comprised by quark and anti-quark pairs held together by strong and weak forces, versus talking about them in terms of fluid drops held together by surface tension. According to Sklar, the former ontology is derived from the ‘fundamental’ theories of QCD and QFT,<sup>109</sup> ‘on the road to the ultimate theory,’ whereas the latter merely represents the ontology of ‘convenient fictions’ of a ‘phenomenological’ and approximate nuclear scattering theory. However:

...how can a difference among false theories, or being further or less far ‘down the road’ to the truth found a preferential attitude towards ontology when all our theories in question have known failings?...[T]he proposed contrast is between ontologically sound but quantitatively not completely accurate descriptions of things that (we have good reason to believe) in fact exist, [versus]...useful but ontologically erroneous descriptions. (432)

Sklar's position is that one should defer to science itself to provide the appropriate demarcation criteria between fundamental versus phenomenological.<sup>110</sup> But upon closer examination, this doesn't seem to hold up very well at all. “[O]ur current best theories clearly tell us that [‘fundamental’ characterizations]...are idealizations every bit as much as [‘phenomenological’ characterizations].”

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<sup>107</sup> “[W]e propose to express the reductionist thesis [viz. Rohrlich's pluralist ontology] in the following way: all properties of systems, defined on whatever level of description, supervene on the fundamental physical description.” (Dieks & de Regt (1998), 47)

<sup>108</sup> “The unity of nature consists in the fact that all the existing objects are formed from some fundamental ones rather than in that the properties of more complex objects are definable in terms of properties characteristic of the fundamental objects.” (Wojcicki (1998), 39)

<sup>109</sup> Quantum chromodynamics and quantum field theory, respectively.

<sup>110</sup> “Science is replete both with schemes intended to truly characterize ‘how things are’ and with other schemes intended only as knowingly false but useful models of the real situation.” (Sklar (2003), 431)

(Sklar (2005), 433) Teller proceeds to run through several detailed examples concerning QCD-QFT versus scattering theory, as well Batterman's (2004, 2005) 'ontologically' versus 'epistemically' fundamental examples of discrete versus continuous characterization of fluids. In both cases, he shows through the details of the articulation of the theories that the presumably 'fundamental' ones are just as approximate and indirect as those regarded 'phenomenological':<sup>111</sup>

I tentatively conclude that the metaphor of 'further down the road to truth' won't help in driving a wedge between acceptable and unacceptable ontologies...a great many of...competing 'ontologies' have complementary virtues in informing us about the world. (434)

Now this tentative conclusion at once strengthens Rohrlich's claims while weakening those of Batterman. Though Batterman never explicitly discusses the issues of verisimilitude vis-a-vis ontology, the distinctions he presupposes between superseding versus superseded theories (2002), as well as between ontologically versus epistemically fundamental theories (2004, 2005) track the 'received view' as articulated by Sklar: Science cleaves the domain of theories into fundamental and phenomenological. In fact, as shown in greater detail in chapters 2 and 3 below, the very epistemic force of Batterman's conclusions rest on the assumption that there must exist such a significant distinction. However, the last phrase in the quoted passage above is also essentially an endorsement of Rohrlich's pluralist ontology, as discussed in detail in the preceding subsection.

According to Teller, what may fundamentally motivate a belief in context-independent verisimilitude, as captured for instance by Sklar's teleological notion/metaphor, may amount to nothing more than an instance of the UEEU fallacy—inferring ' $\exists\forall$ ' from ' $\forall\exists$ '. For instance, one starts with the reasonable assertion that for *any* theory  $T$  there exists a  $T'$  such that  $T'$  is a *refinement* of  $T$ . (Recall, for instance, Rohrlich's degrees of coarseness/refinement). However from there the incorrect inference is drawn that there *exists* a theory  $T^*$  such that  $T^*$  is a refinement for *any* theory  $T$  ;

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<sup>111</sup> For example, in the case of QFT/QCD, its 'fundamental' ontology of 'particle' is of course derived from the notion of 'quanta,' which are excitations of the normal modes in the 'solutions' to fundamental dynamical equations (e.g. the Dirac equation). However one must presuppose (among other unwieldy approximation techniques) a flat space-time just to render the possibility of solutions exhibiting discrete boundary conditions—i.e. the possibility of 'quanta'. "States described by such solutions are idealizations every bit as much as the idealization of a liquid as a continuous medium." (Teller (2004b), 433) "There are no quanta any more than there are continuous fluids. Both are idealizations, known not to be realized in the real world. What then is to give an 'ontology'?" (440) Things look even more muddled when Teller examines the details of what could be essentially characterized (à la Rohrlich) as the 'validity domain' of some of the relatively rare instances when the Dirac equation can actually be solved, without an arsenal of approximation techniques one would assume held for the case of an 'approximate' theory:

Most of the so-called foundational theory is constituted by the approximation schemes—including most of our understanding of quarks, gluons, and other quanta. [Certainly] [t]he resulting 'theory', that is the approximation schemes, is breathtakingly exact for certain very special questions, but also in great many respects severely limited in what it covers. (436-437)

i.e. the belief in a DUT (“desired ultimate theory”) toward which all ‘fundamental’ theories make their pilgrimage ‘on the road’ in a faithfully context-independent manner.

To endorse the metaphor of a royal road to the truth is to embrace the search for...a ‘desired ultimate theory’. But the evidence supports, at best, *indefinite refinability, in indefinitely many diverse ways, a network of complementary and always limited probes into...parts of ‘reality’ that are humanly accessible*, not the Holy Grail of some ‘final theory.’ (438-439)

In short, Teller makes a simply persuasive point justifying an essentially contextual notion of verisimilitude (for *any* theory  $T$  there exists a  $T'$  such that  $T'$  is a *refinement* of  $T$ ) of the kind advocated by Rohrlich (as described in the previous subsection).<sup>112</sup> This contextual notion of verisimilitude also coheres well with the notion of ‘truth’-as-reliability (of a theory’s models) I briefly mentioned characterizes much ‘post-Standard’ philosophy of science in the *Foreword*.

[W]e must make choices as to the respects in which our representations do well. But then accessibility (also a metaphor) is an additional essential consideration when what is at issue is providing human access to an independent world (439) ...[A]ll the theoretical descriptions we get from science fail in one way or another, and succeed and fail in a complex pattern of contrasting aspects, [so] thinking in terms of...a distinction [‘true’ versus ‘useful fiction’] in a context independent way, badly misleads...and the impression that the distinction will support conclusions about ontology will melt away as one takes it to hear that the unproblematic use is context sensitive. (444)

To support such general claims Teller draws the analogy with the context-relative distinction between ‘direct’ versus ‘indirect’ observation.<sup>113</sup> By the same token: “When a description’s

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<sup>112</sup> Echoing Rohrlich’s complaint about the ‘disastrous’ consequence of viewing theories aiming unqualifiedly true or false, Teller states further: “[T]he evolution of ‘truth’ and various kinds of ‘truthlikeness’ are characteristics of our representations. Our representations function as our guides...our guides are imperfect...their virtues as guides to the world cannot be simply evaluated in terms the dichotomy, true or false.” (439)

<sup>113</sup> Recall Grover Maxwell’s (1962) essay the untenability of the distinction of ‘direct’ versus ‘indirect’ observation, in any ontologically fixed and fundamental sense:

The point I am making is that there is, in principle, no continuous series beginning with looking through a vacuum...looking through a windowpane, looking through glasses, ...binoculars,...low-power microscope, etc. The important consequence is that, so far, we are left without criteria which would enable us to draw a nonarbitrary line between ‘observation’ and ‘theory’. *Certainly, we will often find it convenient to draw such a to-some-extent-arbitrary line; but its position will vary widely from context to context.* (Curd & Cover (1998), 1055-1056, italics added.)

It is precisely this contextual and ‘non-arbitrary’ sense concerning demarcating observational versus theoretical terms that led Maxwell to advocate his version of realism. Spatial and thematic considerations (recall opening remarks in the *Foreword* above) however prevent me from launching into a general discussion concerning the question of realism versus anti-realism.

shortcomings are not of present concern we apply the epithetic ‘true’...the context relative distinction between veridical descriptions and useful fictions bears no weight in drawing conclusions about ontology.” (445) He proceeds to make the general point that only though our idealizations and inexact truths “that we have access to the world.”(447) Hence, ontologies offered by any actual theory are all idealizations and (strictly speaking) ‘false.’ (446) As a final semantic point, based on the context-relative distinction between ‘true’ and ‘useful fiction’, Teller argues that the latter term is better characterized as a ‘fallible veracity’, or ‘veridical fiction’, since descriptions (inevitably idealizations) can be utilized in a number of ways:

[N]ot just for ‘predicting the phenomena’ but in improving our grip on properties, explanation, theoretical understanding, every aspect of our intellectual encompassing of what there is...[Since] imperfect characterizations [still] genuinely inform...[j]ust calling them ‘fictions’ thus misleads. But we do want to acknowledge that these characterizations are not *simply* true. (445)

Aside from justifying Rohrlich’s pluralistic ontology in a number of ways (the inevitably ‘idealizing’ hence ‘strictly false’ characteristic of all ontologies, i.e. what Rohrlich would describe as ‘epistemically emergent’ cognitive levels) Teller’s more general claims concerning idealizations, representations, and contextuality echo with Stephen Yablo (1998). Though spatial considerations here constrain me from launching into a full discussion (which would indeed require another study) I can mention here in passing that any serious discussion on ontology inevitably raises issues discussed and debated by Quine (1948, 1951*a*, 1951*b*, 1960) and Carnap (1956). Yablo makes the more general point concerning Carnap’s notions of (theoretical framework) distinctions between ‘internal’ and ‘external’<sup>114</sup>:

The key point about frameworks for Carnap’s purposes is that they provide a *context* in which we are to say –X- *under these conditions* [or] *under those conditions*, and so on, entirely without regard to whether these statements are in a framework-independent sense true. *This is all it takes for there to be an internal/external distinction.* (240, italics added)

The above passage is resonant with Teller’s context-relative distinction between the “epithetic ‘true’, [w]hen a description’s shortcomings are not of present concern,” versus “fallible veracity:” One could liken the former notion as ‘framework-independent’ inasmuch as a description’s shortcomings are “not of present concern,” i.e. lie under some threshold value for *p* internal to the framework’s domain of validity (à la Rohrlich). Whereas the statements concerning X “under these

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<sup>114</sup> Recall a specific example of a theoretical framework-dependent notion of verisimilitude characterized by *p* à la Rohrlich.

[or] those conditions” can be likened to the ‘fallible veracities’ or classes of particular idealizing representations comprising a particular ontology *within* a theory-framework.

Stated somewhat glibly, the overall point here is that Yablo sympathizes with Carnap’s pragmatic stance regarding issues both external and internal to frameworks,<sup>115</sup> while being generally critical of Quine’s claims that the meaning of Carnap’s internal/external distinction depends on the analytic/synthetic distinction.<sup>116</sup> The bulk of Yablo’s paper is devoted to a re-conceptualization of Carnap’s distinction de-coupled from the association with Quine’s analytic/synthetic distinction. “[O]nce freed it [the external/internal distinction] becomes something independently interesting...[essentially involving] the metaphorical/literal distinction.” (232)

In his critique of Quine, Yablo turns the tables: Instead of viewing metaphor as Quine did, in terms of “dramatic idiom[s] of propositional attitudes...deliberate myths,<sup>117</sup> ...from which we can protect ourselves from ontological scrutiny by...holding our tongues in moments of high scientific seriousness,” (245) Yablo submits rigorous semantic arguments for the *centrality* and irreducibility of metaphor.<sup>118</sup> Metaphors can be classified as *representationally* essential, *presentationally* essential, and *procedurally* essential. (250) For representationally essential metaphors, there may be no literal

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<sup>115</sup> Within a framework, for example, utterances need not be assertions (Carnap (1956) 206). One could adopt a Platonic language “without embracing a Platonic ontology,” in a manner compatible with scientific thinking and empiricism in general. (Yablo (1998) 241) The meaning of X-claims within any framework of course is dependent on the syntactic rules governing the use of terms within a framework. Recently illuminating studies on Carnap have also been authored by Demopoulis (2003) concerning issues of general reconstruction of theoretical knowledge. Though I advocate a generally structuralist viewpoint, as discussed above, Demopoulis is generally critical concerning the semantic view of scientific theories (of which structuralism is an instance, highly formalized). “Invoking the semantic view appears therefore to have brought us no closer to a satisfactory account of our theoretical knowledge.” (392) *Pace* Frisch (2005) Demopoulis adopts a ‘received view’ of consistency concerning an essential characteristic of theories, when suitably rationally reconstructed. “Suppose we are given theory  $\Theta$ , all of whose observational consequences are true; it follows from this supposition that  $\Theta$  is empirically adequate and consistent.” (385)

<sup>116</sup> Carnap agreed with Quine here. (Yablo (1998), 233, 237) The analytic/synthetic distinction was of course famously challenged and disavowed by Quine (1951a) though some like Laudan (1990), and Yablo (1998) complain about the lack of argumentative rigor in many of Quine’s central claims. “Quine’s repeated failures to turn any of his assertions about normative underdetermination into plausible arguments may explain why...he has been distancing himself from virtually all the strong readings of his early writings.” (Laudan (1990) in Curd & Cover (1998), n. 39, 352) Yablo likewise complains that Quine’s argument against Carnap “seems to win on a technicality,” since it’s based primarily on the *way* Carnap developed the internal/external distinction, and not against the notion in and of itself. Specifically, Quine takes issue with the *presumed analyticity of the rules by which Carnap construes* frameworks, a red herring for Yablo. (240)

<sup>117</sup> Including for that matter the aforementioned ‘fallible veracities’ undergirding Teller’s notion of theoretical ontology, i.e. notions including the mathematical concept of the infinitesimal, the notion of frictionless plane, etc. (Quine (1960), 219)

<sup>118</sup> Following the heels of the endorsement of scientific seriousness, Quine thought that by selecting our best scientific theory available would enable, in the long run, the fixing of a proper ontological contents which fixed literal meaning. Yablo’s response to Quine: “The boundaries of the literal are so unclear that there is no telling, in cases of interest, whether our assertions are to be taken ontologically seriously [in Quine’s sense].” (255) Yablo defines metaphorical content as ensembles of possible worlds selected via their shared property which would render some pretense legitimate. (250)

alternative to paraphrase away the metaphor's contents.<sup>119</sup> Presentationally essential metaphors carry with them epistemically effective "framing effects" above beyond their actual metaphorical contents:

[I]t is not only conventionally 'picturesque' metaphors that pack a cognitive punch no literal paraphrase can match. This is clear already from scientific metaphors like *feedback loop*, *underground economy*, and [even] *unit of selection*. (252)

Procedurally essential metaphors are semantically open-ended insofar as "the speaker's sense of the potential metaphorical *truthfulness* of a form of words outruns her sense of the particular truth(s) being explored."<sup>120</sup> (254)

The above three classes are not meant to describe an exhaustive list of metaphorical content. Yablo suggests other characteristics as well concerning controversial ontological claims seemingly equiposed between literality and metaphor in a way that Quine's methods seem to fail to resolve. For Yablo, such cases include "mathematical objects" as well as "theoretical entities in physics." (n. 75, 259)

### Subsection 3: Concluding Remarks

The above excursion into elements of Yablo's semantic work on metaphor vis-a-vis Carnap and Quine, as well as the discussion on Teller's 'fallible veracities' may appear tangential. However, my overarching point is this: In a nested 'Russian doll' fashion (proceeding from the concrete to the most general) I have shown central aspects of Rohrlich's pluralist ontology as justified by Teller's notions of contextual verisimilitude and 'fallible veracities.' The latter notion of fallible veracities can perhaps be best systemically characterized by Yablo's general semantic theory of metaphorical content. Conversely, *all* three attempt a serious analysis of the subtle notion of the ontological component of a theory, which harkens back to Carnap (1956) as Yablo has explicitly shown.

On the other hand, this section on ontology demonstrates that Batterman's ideas cannot be embedded in the same nested manner as Rohrlich's. This is not to imply that the systematic interconnection of the three respective positions, i.e., Rohrlich-Teller-Yablo bespeak of their fundamental 'correctness'. One can err systematically. Add to that, Teller and Yablo have their share of critical responses that I did not address here. All this being said, however, aside from the other issues concerning of Rohrlich's program versus what I consider are the shortcomings of Batterman I

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<sup>119</sup> "No...literal criterion immediately suggests itself for pieces of a computer code called *viruses*...or topographical features called *basins*, *funnels*, and *brows*." (Yablo (1998), 250)

<sup>120</sup> An example includes the class of prophetic metaphors, whose semantic contents have an identity taking time to emerge. "[A] growing technical literature of verisimilitude testifies to the belief that 'close to the truth' admits of the best interpretation." (n. 65, 254)

discussed in this chapter, one more is evident in this extensive analysis of ontology: Because Batterman (like Sklar) unquestioningly presupposes a fixed dichotomy between ‘fundamental’ (superseding) and ‘phenomenological’ (superseded) theories, this asymmetry seems to undercut his approach from being subject to a more nuanced analysis of the ontological aspects of theory-frameworks in a manner dissimilar to Rohrlich’s approach.

## Chapter 2: A Deflationary Critique of Batterman’s Notions of Asymptotic Explanation: How to Rejoin Reduction with Explanation

### Section 1: Overview of Robert Batterman’s Notions of Asymptotic Analysis and Asymptotic Explanation

For the most part Robert Batterman (2002, 2003, 2004) concentrates on methodological areas comprising the nature of scientific explanation, scientific theories, and intertheoretic reduction vis-à-vis *asymptotic analysis* and *explanations*. Broadly speaking, these analytical methods deal with examining limiting cases in the mathematical framework of a theory. That is to say, adopting such methods involves examining the qualitative and quantitative behavior of a theory’s term(s) in the  $\xi \rightarrow \infty$  (or  $\xi \rightarrow 0$ ) limit, where  $\xi$  is one of the theory’s central parameters. In regular cases, i.e. cases in which a theory’s constituents continuously transform or reduce to some limiting case(s) devoid of singularities,  $\xi \rightarrow \infty$  or  $\xi' \rightarrow 0$  are two tokens of the same type of limit.<sup>121</sup> However, in singular cases, depending on one’s choice of fundamental parameters, the limits may fail to commute. Asymptotic explanations exploit the characteristics of asymptotic analysis in providing accounts of phenomena modeled by two (or more) theories  $T, T'$  such that  $T$  is recovered in the  $\xi = \infty$  (or  $\xi' \rightarrow 0$ ) limit of some of  $T'$ ’s central parameter(s)  $\xi$ .<sup>122</sup> Only in the non-singular or regular case can it be said that ‘the behavior of the theory *at the limit equals* the behavior of the theory *in the limit*,’ i.e.:  $\lim_{\xi \rightarrow \infty} T' = T$ .<sup>123</sup>

According to Batterman, asymptotic analysis and asymptotic explanations comprise a unique methodological category traditionally overlooked by most philosophers of science. This becomes especially true in the cases of *singular* limits, when the behavior of a theory *in the limit* of one of its central parameters  $\xi$  does not equal the behavior *at the limit*. That is to say, given theories  $T$  and  $T'$  referring to some domain  $D$ , where  $T$  is the theory describing what is occurring *at the asymptotic limit* ( $\xi = \infty$ )<sup>124</sup> for one of  $T'$ ’s fundamental parameters  $\xi$ , then  $T'$  ‘blows up’ in the  $\xi \rightarrow \infty$  limit or the ‘limit’  $\lim_{\xi \rightarrow \infty} T'$  does not exist. Otherwise, in the regular case, we can write:  $\lim_{\xi \rightarrow \infty} T' = T$ .

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<sup>121</sup> Simply redefine  $\xi'$  as  $1/\xi$  in the second case.

<sup>122</sup> For example, in the case of special relativity, in the  $c \rightarrow \infty$  (or  $1/c \rightarrow 0$ ) limit Galilean relativity is recovered. The quantities in special relativity smoothly converge to those in Galilean relativity in a manner free of singularities.

<sup>123</sup> Recall **Schema R** discussed in chapter I above. Moreover, hearkening back to the definition of ‘limit’ one encounters in a typical undergraduate calculus text, involves three necessary conditions: i.)  $\lim_{\xi \rightarrow \infty} T'$  exists, ii.)  $T'$  (at  $\xi = \infty$ ) exists, iii.)  $T' \text{ (at } \xi = \infty) = T = \lim_{\xi \rightarrow \infty} T'$ .

<sup>124</sup> Due to ambiguities concerning the nature of singularities, such statements like: “ $\xi = \infty$ ” are obviously ill-posed, in a strictly mathematical sense. One should consider this ‘equation’ expressed here as shorthand for “the behavior *at the limit*.”

Aside from singular asymptotic analyses and explanations possibly providing a key insight into depicting emergent properties and phenomena,<sup>125</sup> Batterman also makes the very general methodological claim that reduction and explanation can mean different things. “[T]here are good reasons to think that reduction and explanation can part company...there are no good reasons to maintain that reduction (in all of its guises) need be essentially epistemological.” (2002, 114). Consequently, “the nature of asymptotic explanation holds out on the possibility that we can explain the universality of the special sciences from the point of view of the lower level theory while maintaining the irreducibility of those sciences to physics. Explanation and reduction must part company.” (2002, 134.)

## Section 2 : Asymptotic Analysis and Explanation and Emergent Phenomena

### **Subsection 1: Strong and Weak Emergence**

The notion of emergence has received much attention recently by Batterman (2002, 2003, 2004), Bishop (2004), Humphreys (1996), O’Connor & Wang (2003), and Silberstein & McGeever (1999), among others. The aforementioned authors share the common aim of providing accounts for emergence which offer fresh insights from highly articulated and nuanced views reflecting recent developments in applied physics. Moreover, the authors present such accounts to reveal what they consider as misrepresentative and oversimplified abstractions often depicted in standard philosophical accounts.<sup>126</sup>

Silberstein & McGeever (1999) for instance contrast weaker and stronger ‘epistemological’ and ‘ontological’ notions of emergence. Epistemological emergence is best understood as a kind of artefact of a certain formalism or model arising through a macroscopic or functional analysis of the theory’s ‘higher level’ descriptions or features in its domain (182). This is a weak notion, since it connotes practical or theoretical limitations on the resolving and computing power of the theory and in

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<sup>125</sup> In a broad sense, ‘emergence’ is construed as the failure of the limits of reductionism and supervenience. Emergent phenomena in the field of physics, whether characterized by phase-transitions or by caustic surfaces in catastrophe optics (to name just a few important instances thereof) often exhibit the same kind of multiply realizable behavior (in the sense of being independent of any particular microphysical characterizations of its constituents) one usually encounters in issues pertaining to the philosophy of mind (Kim 1992). Such multiple realizability vis-à-vis emergent behavior has been found puzzling by many in the philosophy of physics, yet “Batterman [on the other hand] suggests that the similarities in behavior may be explained as a consequence of the fact that the differences in realization at the physical level are irrelevant to the higher-order behavior, in the same way that the differences between diverse systems underlying phase transitions are irrelevant to the behavior near the critical temperature.” (Strevens 2002, 656). For further details, see Section 3 below.

<sup>126</sup> “It is...possible that...standard divisions and hierarchies between phenomena that are considered fundamental and emergent, aggregate and simple, kinematic and dynamic, and perhaps even what is considered physical, biological, and mental [should be] redrawn and redefined.” (Silberstein & McGeever, 1999, 200.)

turn of its agent.<sup>127</sup> Epistemic emergence is metaphysically neutral. An epistemically emergent property of an object, for example, can *in principle* be reducible to or determined by intrinsic properties, though being *practically* impossible to explain, predict, or derive.<sup>128</sup>

Ontological emergence, on the other hand, comprises features of systems/wholes possessing capacities (causal, and otherwise) *not* reducible to the intrinsic capacities of the parts, nor among the reducible relations among such parts (1999, 182). Ontological emergence is *usually* thought to entail epistemic emergence,<sup>129</sup> though the converse *never* holds: “Epistemological emergence cannot entail ontological emergence, because it is defined to preclude it.” (1999, 185) On a perhaps even more strongly metaphysical note, Humphreys (1996) characterizes an ontological notion of emergence in terms of a *dynamical fusion* of previously two (or more) lower-level properties into a higher-level property.<sup>130</sup>

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<sup>127</sup> Strong and weak notions of emergence find an interesting counterpart in Humphreys’s (2000) distinctions of theoretical *limits* versus practical *limitations*. “A limit is an ‘in-principle’ epistemological constraint, whereas a limitation is an epistemological or pragmatic constraint.” (Carrier, et. al. 2000, 2). However there remains the rather daunting task to establish a more precisely metaphysical notion of principled epistemic limits. For instance, do such limits reflect some objectively irresolvable features in the world, or do they merely represent some fundamentally limiting aspect concerning the cognitive capacities of the agent? Such a distinction (in both senses) casts doubt on the literal truth of a theory, in favor of its *reliability*. “[W]e [can be] ... committed to the claim that a theory is *reliable*, but...not committed to the literal *truth* of its empirical consequences. *This does not mean we have to be instrumentalists...* a scientific realist [for instance] might be committed to the reality of electrons and fields, yet demand only that electromagnetic models represent the behavior of these ‘unobservables’ reliably, while an empiricist could be content with the fact that the models are reliable as far as the theory’s observable consequences are concerned.” (Frisch (2005) 42) In this respect Cherniak (1986) recommends that notions like weak emergence should operate as a regulative norm in any theory (and its associated reliability) that would lay primary emphasis on cognitive agents and agency. In a metatheoretic sense, specifically applying to special ‘human’ sciences like microeconomics, cognitive psychology, etc., the ‘is’ of computational, logical, and cognitive limitations (usually spelled out precisely by limitation theorems, conjectures, formulae) should inform the ‘oughts’ of theory-construction. The latter of course pertain to assumptions made by the theorists concerning what constitute idealized agents. So in this case, a notion of emergence *informs* an asymptotic maneuver as opposed to the latter constituting the former, in the case of Batterman.

<sup>128</sup> This simplest example of such a case involves the ‘three-body problem’ in classical mechanics: Such a problem is unsolvable in the sense that one cannot derive the trajectories in the 6-dimensional phase space in the general case of three interacting force centers. This is *not* to say that numerical and statistical approximation-schemes cannot aid in giving an account for classes of solutions, to an agreed-upon error. But in a deductive nomological scheme, the classes of such regions wherein the (uncomputably) exact solution trajectory is bound, described by various topological regions by the approximation schemes, are epistemically emergent.

<sup>129</sup> An exception includes entanglement in QM. For further details, see **Subsection 2**.

<sup>130</sup> Precisely stated, for objects  $x_r^i, x_s^i$  at level  $i$  and at time  $t$ , endowed with respectively  $i$ -th level  $n$  &  $m$  -type properties  $P_m^i, P_n^i$ , then during time interval  $\Delta t = t' - t$  they will fuse in such a manner to form a composite  $i$ -th level object  $x_c^i \equiv x_r^i \oplus x_s^i = \{x_r^i, x_s^i\}$  such that:  $P_m^i(x_r^i, t) * P_n^i(x_s^i, t) \rightarrow_{\Delta t} [P_m^i * P_n^i](x_c^i, t') \equiv P_k^{i+1}(x_c^i, t')$ , where  $*$  is the fusion operation. Note, for the sake of simplicity in this characterization, that only the *properties* fuse to become a higher-level properties. (1996, 60). For example, consider a wooden deck comprised of beams that are glued together. Before the glue has dried, each beam  $x_r^i$  had the property  $P_r^i$  that it was (relatively free) to move with respect to the other beams. Once the glue has dried, the planks become rigid and in this characterization, their previous properties of relative mobility with respect to their neighbors vanishes, to fuse into the aggregate property  $P_k^{i+1}$  of being able to support the weight(s) of person(s) standing on the deck (1996, 65-66).

Still others like Robert Bishop (2004) offer classification schemes which seat emergence in a more descriptive context alongside the more ‘traditional’ categories of reduction and supervenience. Bishop for instance offers the following categories:

- i.) Reduction: When more fundamental properties or descriptions provide *necessary* and *sufficient* conditions for less fundamental properties/descriptions.
- ii.) Contextual Emergence: When more fundamental properties or descriptions provide *necessary* but not *sufficient* conditions for less fundamental properties/descriptions.
- iii.) Supervenience: When more fundamental properties or descriptions provide *sufficient* but not *necessary* conditions for less fundamental properties/descriptions.
- iv.) Strong Emergence: When more fundamental properties or descriptions provide neither *necessary* nor *sufficient* conditions for less fundamental properties/descriptions.

As evidenced by the properties/description division, contextual and strong emergence can respectively modify ontological/epistemic senses of emergence.

## **Subsection 2: Quantum entanglement: an example of ontological without epistemic emergence**

In the previous section I wrote that for most cases, ontological emergence entails epistemic emergence. I discuss the exceptional case here, involving quantum entanglement, since these issues will accompany subsequent points later. The essence of this exceptional case can be understood as follows: Though post-Bell experiments have conclusively established entanglement phenomena can violate classical statistics ascribed to only locally interacting subconstituents, thus making a case for ontological emergence (Silberstein & McGeever 1999, 189), the linear formalism of QM means that entangled states can be characterized in epistemically reducible (non-emergent) ways.

Prior to Bell, arguments regarding measurements on isolated parts, one might have viewed superposition states as merely an artifact. The Bell results suggest that the formalism presages the existence of genuine emergent properties...[Moreover] the kind of emergence found in quantum mechanics and quantum field theory completely explodes the ontological picture of reality as divided into a ‘discrete hierarchy of levels’; rather ‘it is more likely that even if the ordering on the complexity of structures ranging from those of elementary physics to those of astrophysics and neurophysiology is discrete, the interaction between such structures will be so entangled that any separation into levels will be quite arbitrary.’<sup>131</sup>

Yet, on the other hand this doesn’t necessarily entail epistemological emergence, since “non-separability is [often just] a logical consequence of the dynamical equations of motion.” (1999, 187)

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<sup>131</sup> (Humphreys 1997, 15) in Silberstein & McGeever 1999, 189.

In other words, preserving relativistic locality entails an ontologically emergent interpretation of the properties of the EPR-Bohm systems (188).

For instance, consider a pair of two-dimensional quantum systems  $A, B$  with states spanned by bases:  $\{|0\rangle_A, |1\rangle_A\}, \{|0\rangle_B, |1\rangle_B\}$ . Suppose that the initial composite system is in the (separable) state:

$$\begin{aligned} |\Psi_{AB}(0)\rangle &= \frac{1}{2}\{|0\rangle_A + |1\rangle_A\} \otimes \{|0\rangle_B - |1\rangle_B\} \\ &= \frac{1}{2}\{|0\rangle_A |0\rangle_B - |0\rangle_A |1\rangle_B + |1\rangle_A |0\rangle_B - |1\rangle_A |1\rangle_B\} \\ &\equiv \frac{1}{2}\{|00\rangle - |01\rangle + |10\rangle - |11\rangle\}^{132} \end{aligned} \quad (2.2.1)$$

Consider the system Hamiltonian:  $H = \{|00\rangle\langle 00| + |01\rangle\langle 10| + |10\rangle\langle 01| - |11\rangle\langle 11|\}$ . Then the time-evolution operator  $U(t, t_0 = 0) = \exp(-i2\pi Ht/\hbar)$ , after time  $t = \hbar/4$  becomes:

$$U(\hbar/4, 0) = -i\{|00\rangle\langle 00| + |01\rangle\langle 10| + |10\rangle\langle 01| - |11\rangle\langle 11|\}. \quad (2.2.2)$$

So the initially separable  $|\Psi_{AB}\rangle$  now evolves into the entangled state:

$$|\Psi_{AB}(\hbar/4)\rangle = \frac{-i}{2}\{|00\rangle - |01\rangle + |10\rangle + |11\rangle\} \quad (2.2.3)$$

For this simple two-dimensional composite system evolving unitarily into an entangled state, there is nothing epistemically emergent occurring per se in the representation: A simple and direct *linear* combination of the entangled state  $|\Psi_{AB}(\hbar/4)\rangle = \frac{-i}{2}\{|00\rangle - |01\rangle + |10\rangle + |11\rangle\}$  is expressed in the composite basis for the two systems:  $\{|00\rangle, |01\rangle, |10\rangle, |11\rangle\}$ . Linearization is the essence of epistemic reduction. The ontological emergence, on the other hand, is clearly represented by virtue of the non-factorizability of  $|\Psi_{AB}(\hbar/4)\rangle$  into product states spanned by the individual bases:  $\{|0\rangle_A, |1\rangle_A\}, \{|0\rangle_B, |1\rangle_B\}$ .

The phenomenon of entanglement have led some to question whether quantum systems possess anything like an absolute state (Rovelli 1997, Finkelstein 1996, 2001, 2004a-c). Interpretations of quantum theory are instead advanced giving primary focus to an action-based (Finkelstein 1996<sup>133</sup>, 2003<sup>134</sup>) or information-based ontologies (Bub (2004), Clifton et. al. (2003),

<sup>132</sup> The last line in (2.2.1) adopts the shorthand representation for denoting the ordering of base elements in the composite system.

<sup>133</sup> Finkelstein (1996, 24-25) writes:

Let us assume that a physical theory should at least lead us to, if not consist of, statements of the form: ‘If we do so-and-so, we will find such-and-such.’ Suitably idealized, generalized, and algebraicized, such doings and findings become the physical units of our theory....The algebra is a language of and for action...We describe a quantum entity not by a complete description or state but by the external acts by which we prepare or register it, and by the actions transforming any experimenter into any other...The fundamental question in physics, then, is no ‘What are all things made of?’ It is rather ‘What goes on here?’...We retain Bacon’s maxim ‘Dissect nature’ but we read it (or misread it) as the injunction to dissect dynamical history into least actions, not some hypothetical static matter into atoms.

The act-algebraic semantics arises as a way to compensate for the non-commutativity of quantum acts, preventing a complete characterization of the system.

<sup>134</sup> “Quantum theory is a theory of quantum processes. It is no more a theory of a state than special relativity is a theory of the present. That is why Heisenberg called his theory nonobjective and why Blatt and Weisskopf refer to  $\psi$  as channels, not states...[a]  $\psi$  describes a process, not the product of the process.” (Finkelstein, 2003, 180)

Rovelli (1997), Green (2000)). These interpretations seek to account for ontological emergence through their stipulation and use of fundamentally relational properties and notions like act and information in a manner respecting locality and rejecting hidden variables, as discussed in general terms in Silberstein and McGeever (1999, 188).

### Subsection 3: A Survey of Some Critical Responses

There are authors who also advance deflationary claims in response to the above: Many who would deny that contemporary treatments on the notion of ‘emergence’ offer anything novel in the making. I will mention in passing a few counterclaims to Batterman (2002, 2003, 2004) made by his contemporaries.

Gordon Belot (2003) denies that there is anything particularly novel, in a *methodological* sense, about the claims of asymptotic *analysis* made by Batterman. Belot focuses on the general theory of partial differential equations to show that any astute mathematician, ignorant of the physical details of the particular cases Batterman (2002) refers to, can in principle *derive* such solutions from the *general theory alone* (i.e.,  $T'$ ). In other words,  $T'$  possesses *sufficient explanatory structure* and hence the reliance of structures in  $T$  is (at best) contingent, despite such claims of necessity made by Batterman in the singular limit, when  $\lim_{\xi \rightarrow \infty} T'$  does not exist (Belot (2003) 20-25).

The cases Batterman (2002) examines in detail include the caustic structure in catastrophe optics. These result in asymptotic divergences, which according to Batterman necessitates a complex amalgamation of geometrical optical structures (i.e., in the theory  $T$ ) alongside the wave-theoretic ones (in the theory  $T'$ ) as well as similar situations arising in semi-classical quantum theory. For instance, as in the case of catastrophe optics, likewise in the case of quantum chaos, do we witness in the effect of ‘Gutzweiler scarring’ an intricate interplay and irreducible interdependence of quantum mechanical and classical mechanical structures (2002, 100-111)?<sup>135</sup>

According to Belot, Batterman is at best simply reifying auxiliary mathematics, hence, “in calling our attention fascinating intricacies of asymptotic analysis, [Batterman is basically no more than] calling our attention to an unjustly ignored species of Hempelian explanation, rather than elucidating a competitor to it.” (2003, n39, p22). This “ignored species” species Belot is referring to is the “DSN” model (“deductive *statistical*-nomological”), i.e. the DN model applied to statistical regularities, as described in detail in the following page.

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<sup>135</sup> Although it’s important to keep in mind that in *most* cases, a *singular* limit occurs when  $h \rightarrow 0$  (i.e., most governing equations in quantum mechanics will *not* smoothly converge to those in classical mechanics in such a limit) this is not *generally* the case. For instance, the time-dependent Schroedinger equation (TDSE) will reduce smoothly to the Hamilton-Jacobi equation (HJE) in such a limit (Goldstein 1980, 489-492). However, this case of TDSE  $\rightarrow$ HJE reduction applies only to integrable systems. The cases Batterman refers to in catastrophe optics and Gutzweiler scarring are non-integrable systems.

Batterman (2003) responds to Belot's charge by supplying yet more examples from applied physics to convince the reader that the charge of reification of auxiliary mathematics fundamentally misses his most essential points. In so many words, these points entail that *especially in the singular limit*, one can find numerous examples in physics whereby the physics *in the limit* (described by  $T$ ) governs the phenomena depicted by theoretical terms in  $T'$ . One sees this most clearly in the cases when *initial* or *boundary* conditions are best described by  $T$  in such a manner, when  $\lim_{\xi \rightarrow \infty} T'$  does not exist. "In arguing that an account ... appeal[ing] to the mathematical idealization is explanatorily superior that does not invoke the idealization, I am not reifying the mathematics...I am claiming that the 'fundamental' theory that fails to take seriously the idealized 'boundary' is less explanatorily adequate." (2003, 8).

For example, in the case of supersonic shock waves (phonons) propagating in a gas, the shock wavefront is idealized as a 2D surface, which is a divergence in the continuum mechanical limit. Such a (2D) shock front however governs the dynamics of the gas, insofar as the (idealized) boundary conditions constitute the solutions of the differential equations of motion describing the propagation of density waves through the gas. This is a physical *fact*, not an inappropriately reified mathematical artefact, argues Batterman (2003).

Paul Cohnitz (2002) responds to Batterman with a roughly similar charge, albeit focusing more on the logic of asymptotic explanation, as opposed to the mathematics of asymptotic analysis per se. Cohnitz basically argues that Hempel's statistical deductive-nomological model (SDN), revised by Railton, adequately takes care of what Batterman describes as "asymptotic explanation."

Batterman has not (yet) posted a direct response to Cohnitz. However Cohnitz focuses his one critique against Batterman's mathematical example involving the 'chaos game,' (2002, 23-35) and it is hard to imagine how Cohnitz's response can adequately deal with the later arsenal of examples Batterman invokes, especially those that are found in his (2004) reply to Belot. Though one may grant to Cohnitz his claim that the Hempel-Railton DSN models provide an adequate account of the chaos game, in the aforementioned cases in which boundary phenomena *govern* the physics described by  $T'$  this becomes a different story. The DSN model for statistical regularities exhibits the following structure:

I. Demonstration through an analysis of the lawlike dynamical instabilities of systems  $s$  of type  $S$ , that  $S$  possesses certain strong statistical properties  $P_S$

II.  $\forall x \forall t [P_S(x,t) \rightarrow \Pr(G(x,t+\theta)=1) \theta \geq 0$

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III.  $\therefore \forall s \in S \forall t [P_S(s,t) \rightarrow \Pr(G(s,t+\theta)=1) \theta \geq 0$

$G$  is the resultant property of interest, and  $\text{Pr}$  the probability. (Cohnitz 2002, 31).

Now in the cases of phenomena constituted by singular initial and boundary-value conditions subsequently discussed by Batterman, either the DSN yields a trivial or a null account depending on the interpretation of  $G$ . In the former trivial case, if the boundary or initial valued effects drive the system's dynamics, then a strong correlation results so trivially DSN formalizes this effect. But is the explanation really deductive-nomological? It all depends how one interprets  $G$ . If property  $G$  is instantiated by entities in  $T$  (the behavior of the theory *at the limit*) then a null answer results, since it is assumed here that one is dealing with entities and properties in the domain of  $T'$ .<sup>136</sup> If, on the other hand, one argues against Batterman and claims (like Belot 2003) that such properties  $G$  can be recovered entirely via mathematical gymnastics in the most abstractly mathematical characterization of  $T'$ , then one arrives at a trivial tautology: The system  $S$  contains properties  $G$  one would *expect* it to contain after the boundary-value or initial value effects are taken into account. In short, the solutions to the differential equations are the solutions to the differential equations one would expect, in a correct calculation. Hence in this case the 'statistical' modifier is completely dispensable in the DSN model. In either case, it's hard to see how Hempel-Railton accounts shed any light in the complex interplay of structures in  $T$  and in  $T'$  in the singular asymptotic limit.

In the light of Belot's and Cohnitz's critiques, and Batterman's response to Belot, perhaps Wilson (2003) grasped Batterman's (2002) points most essentially, when he writes that:

Batterman's discussion of 'theories between theories' makes a contrary tempering moral quite plain: much real understanding within science comes in the form of appreciating the patterns in which different types and mathematical descriptions *intermingle* – through understanding the interface along which one mode of description breaks down and where some opposed mode needs to take over. Such mixing of means is quite different from the blithe 'autonomy' that those of the 'supervenience' school expect to see in their 'higher level theories'...[I]n truth, such 'levels' often mix together in very interesting patterns of mutual reinforcement. (2)

We often do not achieve descriptive success through autonomous micro- or macroscopic tools, but through utilizing a complex *intermingling* of ideas, which often obey quite complicated strategies of interdependence. These mixed techniques shall be regarded...as *signatures* of various forms of mathematical accommodation between microscopic and macroscopic behaviors (the nature of boundary conditions utilized often supplies a critical ingredient within this signature)...[W]e could do no better than...examine carefully the complex structure of revealing case [studies]...rather than continuing to languish in the crude dichotomies of 'reduction' versus 'supervenience.' (6)

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<sup>136</sup> Especially if one agrees with John Robert's (2006) claims concerning the semantic novelty of theoretical terms.

### Section 3 : Conflations of Ontological and Mathematical Content Committed by Batterman and Others

As discussed above, Belot remarks that Batterman *reifies* auxiliary mathematical structures. Batterman (2003) however may have adequately responded to Belot by calling attention to the irreducibly empirical behavior—exhibited in the case of supersonic shocks—involving a complex interplay between superseding and superseded theories ( $T'$  and  $T$ , respectively). However, in the cases of asymptotic limits, on a more fundamentally *metatheoretic* level, Belot's phrase is revealing (for reasons I shall argue that differ from what Belot had in mind). The 'reification' for *all* the authors I have surveyed appears to involve a methodological conflation of the "the actual with the ideal." All seem to assume that the most commonly-occurring instances of mathematical tactics employed by the majority in the physics community is the best or the ideal paradigm.<sup>137</sup>

Granted, none of the authors I review here state this explicitly. Yet it seems more or less assumed, based on their remarks on the mathematical standard of asymptotic analysis usually adopted in characterizing strong (or ontologically) emergent properties.

"I believe that in many instances our explanatory physical practice demands that we appeal essentially to (infinite) idealizations. But I don't believe that this involves the reification of the idealized structures." (Batterman (2003), 7). These two sentences spell out the tension(s) in the themes Batterman is advancing: On the one hand, one must *essentially* appeal to infinite idealizations when *explaining* the phenomena, on the other hand one is cautioned against reifying such explanatory structures. Aside from not providing a clear explanation of *how* one can adhere to such orthogonal purposes, Batterman here appears to be conceding to Cohnitz's Claim (II): One cannot cut the route to further (explanatory) reduction if one knows that the underlying mechanisms are different from those depicted in the explanans. (2002, 34). Perhaps this concession is satisfactory for Batterman, as he likewise seeks to sever the connection between explanation and reduction, but this is hardly satisfying

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<sup>137</sup> Consider for example the most ingeniously but notorious methods of bootstrapping and indiscriminate ontological mixing-and-matching found in the renormalization group program (RGP). In an earlier paper (2005a) I subjected the RGP—applied to the case of the IR (infrared) limit in turbulence—to Mark Steiner's (1998) illuminating semantic analysis of some contemporary physical theories, to show that the governing analogies were (hopelessly) 'doubly Pythagorean' (in Steiner's sense). This implies, among other things, that despite the apparent success of RGP at characterizing universal behavior in manifold instances, one should at best assume an instrumentalist position. RGP is stunningly empirically adequate, based on our best contemporary resolving power of measurement in some instances in renormalizable theories such as QED, but then again so was Ptolemaic astronomy at one point in history. In the latter case, aside from anomalous naked eye discrepancies involved in the observed instances of Mars' orbit, the (essentially Fourier analytic/synthetic) schemes of Ptolemaic approximation devices, denoted by artifacts 'epicycles' and 'equants', provided empirically adequate predictions for centuries. By the same token, notions like 'dressed parameters' and 'subtraction parameters' employed by RG methods, give results that are considered just as empirically adequate by contemporary researchers, and at the same time, just as unphysical.

to the practicing theoretical physicist, with other than an instrumentalist leaning. The usual reaction one would expect from the latter would be some demand for further explanatory reduction.

Batterman elsewhere remarks that one should take thermodynamics seriously, lest running the risk of “doing away with all idealizations in physics.” (2004, 12) This caveat must be qualified, however, lest it appear that the risk of an all-or-nothing fallacy is being committed here. Batterman’s claim can be countered here by pointing out that by calling the thermodynamic limit<sup>138</sup> into question, one is obviously simply aiming for a possibly more *appropriately particular* idealization. This, in and of itself, does *not* entail the questioning or possible abolishment of what constitutes the contemporary theoretical physical enterprise. The latter of course is based on the practice of idealizing in the *appropriate manner* for a particular class of phenomena under study.

Regarding the depiction of physical discontinuities, Batterman goes on to say: “The faithful representation [of physical discontinuities]...demands curves with kinks...a sense of ‘approximation’ that appeals to how similar the smooth curves are to the kinky curves is inappropriate.” (2004, 13) Batterman seems to make this claim to bolster his earlier remark (2004, 12) that physical discontinuities (like phase transitions) are “correctly represented” by mathematical singularities.

However, a simple counterexample to Batterman’s above remarks would involve for instance a Fourier series representation of a kink, in the elementary instance of the function  $f(x) = |x|$  defined on interval  $[-2,2]$ . The function of course isn’t differentiable (i.e. has a kink) at the origin ( $x = 0$ ). On the other hand the sinusoidal functions comprising the orthonormal basis  $\{(\sin^l x, \cos^m x) \mid \forall l \in \mathbb{Z} \ \forall m \in \mathbb{Z}\}$  of course are  $C^\infty((-\infty, \infty))$  or differentiable to arbitrary order (i.e. is smooth) over the domain of real numbers.<sup>139</sup> Its Fourier series  $S(x)$  representation is:

$$f(x) = |x| \equiv S(x) = \frac{1}{2} - \frac{2}{\pi^2} \sum_{k=1}^{\infty} 2^{2k+1} \cos((2k+1)\pi x) \quad (2.3.1)$$

This is obviously a typical case of regular asymptotic analysis: The sequence of partial sums:

$$S_n(x) = \frac{1}{2} - \frac{2}{\pi^2} \sum_{k=1}^n 2^{2k+1} \cos((2k+1)\pi x)$$

converge smoothly in the  $n \rightarrow \infty$  limit to the kink represented by the absolutely convergent sum  $S(x)$ .

Now any finite partial Fourier sum  $S_n$  sum, (where  $n < \infty$ ) being the superposition of smooth curves, doesn’t *exactly* model the kink  $f(x)$ , but aside from the fact that the (quantitative) error can be

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<sup>138</sup> I.e.,  $\lim_{N \rightarrow \infty, V \rightarrow \infty, \rho_N = \text{const.}}$

where:  $N$  is the number of particles,  $V$  the volume of the system and  $\rho_N = \frac{N}{V}$ , i.e. the number density, which is held fixed.

<sup>139</sup> This is obviously more succinctly and rigorously stated by invoking the analyticity of the basis elements  $\{e^{ikx} \mid \forall k \in \mathbb{Z}\}$ .

made arbitrarily small, (i.e.,  $\forall x \in [-2,2] \exists N, \epsilon_N : \forall n > N |S_n(x) - f(x)| < \epsilon_N$ ) the *qualitative* difference between sum-of-smooth curves and kinky curves washes out in the regular limit:  $n \rightarrow \infty$ .

‘Smooth’ and ‘kinky’ are topological properties. Robert Bishop (2004) discusses the interplay between a theory’s *ontology* and its *topology*. For instance, when he writes about the Born-Oppenheimer approximation in the characterization of molecular structure:

The Born-Oppenheimer ‘approximation’...is not simply a mathematical expansion in series form...It literally replaces the basic quantum mechanical descriptions with a new description, generated in the limit<sup>140</sup>  $\epsilon \rightarrow 0$ . This replacement corresponds to a change in the algebra of observables needed for the description of molecular phenomena...The Born-Oppenheimer approach amounts to a *change in topology* – i.e., a change in the mathematical elements modeling physical phenomena – as well as a *change in ontology*—including fundamental physical elements absent from quantum mechanics. (4)

What Bishop doesn’t seem to explain clearly is *how* a theory’s topology and ontology interrelate.<sup>141</sup> This produces a tension and ambiguity, resulting in what seems to be an equivocation. Like Batterman, Bishop defends the asymptotic procedure as being something more than just a heuristic approximation device, as he states:

[T]he crucial point of asymptotic reasoning...[has to do with] molecular structure presuppos[ing] both a new topology and a new ontology not given by quantum mechanics...It is definitely not the case that the sophisticated mathematics...somehow obscur[es] the metaphysical issues. Rather, the metaphysical issues and practices of science are driving the sophisticated mathematics in this example. (2004, 7).

Metaphysically speaking however, since Bishop doesn’t clarify the relationship between topology and ontology it remains unclear how “metaphysical issues...driv[e] the sophisticated mathematics.” Most practicing scientists would probably view the sophisticated mathematics of such techniques as a heuristic device, similar in kind to the semiclassical Bohr planetary model. The ontology of the approximation schemes are essentially collections of heuristic devices, guiding one’s intuitions but not opening any metaphysical black boxes.

In short, as evidenced in the above passages, Bishop seems to be reifying a sophisticated mathematical device, equivocating its topology with theoretical ontology. As in the case of Batterman

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<sup>140</sup> Here, the governing parameter  $\epsilon = (m_e/m_N)^{1/4}$  where  $m_e$  is the mass of the orbital electron, and  $m_N$  the mass of the nucleon, i.e. one forms an asymptotic series  $S(\epsilon) = \sum_k a_k \epsilon^k$ .

<sup>141</sup> Can ontology be reduced to topology? If so, in what precise manner –cross-wise or top-down? (Cartwright 1999) Does ontology supervene on topology, or is emergence a better way to understand the relation? Is ontology constitutive of topology, or vice versa? Or are topology and ontology part of the relata of relationships characterized by nomological necessity?

(2004), Bishop engages in what appears as question-begging. When considering the possibility of a future theory regularizing such asymptotically singular limits in the Born-Oppenheimer approximation, he asks rhetorically: “Why wait for the ‘final theory’ to sort things out?” But it is *never* a question of waiting for a final theory, rather *always* one of continually searching for more expressively superceding theories striking a more optimal balance between simplicity and strength.<sup>142</sup>

I bring up the topology/ontology ambiguity resulting from a reification of the mathematical device of asymptotic analysis on the part of Bishop because I argue that something similar may occur in Batterman. For example, hearkening back to the smooth curve approximation  $S_n(x)$  for a ‘kink’  $f(x) = |x|$ , Batterman would deem the smooth Fourier components ‘inappropriate’ precisely because of a similarly implicit topology/ontology conflation and equivocation: ontological discontinuity  $\leftrightarrow$  topological kink. As demonstrated above, however, a Fourier Series *is* an example of an asymptotic analysis corresponding to objective phenomena. For instance, reading (2.3.1) from left to right mathematically represents the synthesis of smooth wave forms produced in a transmitter yielding a ‘sawtooth’ wave form. Conversely, reading (2.3.1) from right to left represents a typical process of Fourier decomposition (or analysis) of a ‘sawtooth’ form, whether by a naturally-occurring system like the human ear or via a device like a spectrum analyzer.

Batterman’s further remarks appear to place undue over-reliance on the characteristically non-physical infinities comprising methods involving the application of singular limits. For example, in the case of the TDL:

One needs mathematics that will enable one to represent...genuine physical discontinuities. As a result there is something deeply right about the thermodynamic representation of the singularities –the fact that the [thermodynamic] limit is singular in critical phenomena [e.g., phase transitions] is really an indication that the idealization cannot be dismissed so easily.” (2004, 15)

On the other hand, one can argue with equal force that there is something ‘deeply inadequate’ about a theory blowing up in the face of some physical discontinuity. Though singularities obviously delimit the theory’s domain of reliability<sup>143</sup> (telling us automatically in which instances the theory fails) they fail to provide information just the same. Certainly in the case of phase transitions, for example, it was precisely why Kolmogorov and others originally applied ‘infinity-removing’ renormalization group techniques, to compensate for such singularities. Why *must*, therefore, (unless

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<sup>142</sup> Here I am referring in particular to David Lewis’s notion that laws of nature “belong to all true deductive systems with a best combination of simplicity [e.g. a minimum number of ad-hoc assumptions and axioms] and strength [i.e., predictive and expressive and explanatory power].” (Carroll (2003), 3.) Recall also my previous remarks in chapter 1 (§4.2) above, concerning Teller’s (2005) remarks on the ‘EUUE’ fallacy: invalidly inferring a “DUT” (‘desired ultimate theory’) from the plausible notion that one can always construct a refinement ( $T'$ ) from *any* theory ( $T$ ).

<sup>143</sup> Recall the discussion on a theory’s validity domain in I.3.2 above.

one equivocates topology with ontology) such asymptotically singular cases be “essential to a foundationally respectable understanding of...physical discontinuities” (2004, 22)? One could argue just the opposite: Once a theory  $T$  is reformulated in such a manner as to be *purged* of as many singularities as possible—in other words, purged of its epistemologically emergent aspects—only then can one hold out on the hope of eventually *superseding* it. This would offer the physicist the chance for a better understanding of physical discontinuities (the oftentimes ontologically emergent phenomena) in the superseded theory  $T'$ .

For example, quantum mechanics (theory  $T'$ ) removes some of the singularities found in general relativity (theory  $T$ ) depicting black hole phenomena. Vacuum fluctuations, resulting in pair production, governed by Heisenberg Uncertainty, ‘blur’ the otherwise ‘sharp’ singular edge at the black hole’s event horizon. Quantum theory offers here a depiction of the mechanism of Hawking radiation in place of the brute singularities of general relativity. This offers the physicist the opportunity to better understand such (possibly ontologically emergent) phenomena.

Writes Bishop:

If the expansion is singular, as in the case of the Born-Oppenheimer procedure, it [the series  $S(\epsilon) = \sum_k a_k \epsilon^k$ ] is not uniformly convergent in the original topology of the fundamental description as an appropriate parameter tends to some limit (e.g., as  $\epsilon \rightarrow 0$ .) *This discontinuous limiting behavior indicates the need for a change of topology.* The crucial step...*is to identify a new topology which regularizes...* such that it converges uniformly. This leads to a new contextual topology associated with novel properties not defined...under the original topology, and which is associated with ontological elements also not found at the level of the fundamental...theory. (2004, 5, italics added)

“Identifying a new topology which regularizes” is precisely what applications involving Clifford algebra succeed in doing, where functional-analytic techniques fail, as I shall show in the ensuing sections. Batterman focuses on *functional*-analytic techniques to look for singular asymptotic expansions in the *superseding* theory  $T'$  to bring it into a complex interplay with aspects of the *superseded* theory  $T$  (i.e., the behavior of  $T'$  at , but not *in* the limit).<sup>144</sup> By contrast, (Clifford)

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<sup>144</sup> Although, as perhaps suggested by my use of the phrase “behavior at the limit,” Batterman (2002) takes pains in many places to clarify that the superseded theory  $T$  is ‘contained in’ the superseding theory  $T'$ , just not in a way in which the structures of  $T$  can be straightforwardly derived in any D-N scheme. (Hence the philosophical novelty of asymptotic explanations.):

How different asymptotic reasoning is from the usual understanding of how solutions to equations are to be gotten...asymptotic analysis of singular limiting intertheoretic relations typically yield new structures that are not solutions to the fundamental, governing equations...So ‘predictable from fundamental theory’ is somewhat ambiguous. In the one sense, the solutions *are contained in the fundamental...equations.* This is the sense in which asymptotic analysis enables one to find mathematical representations of these solutions. On the other hand, the understanding of these

algebraic expansion/contraction techniques or embeddings expand out of  $T'$  (characterized algebraically) into a fully regularized version  $T'^*$  characterized by a Clifford algebra. Conversely,  $T'^*$  is fully regular in the sense that  $\lim_{\lambda \rightarrow 0} T'^* = T'$  for *any* algebraic contraction parameter or structure constant  $\lambda$ .

I summarize some of the more technical details in the ensuing sections. In closing, however, one can think of the expansion from  $T'$  to  $T'^*$  and conversely the contraction  $\lim_{\lambda \rightarrow 0} T'^* = T'$  as paradigmatic of intertheoretic reduction, insofar as the reduction here is understood *methodologically*. As I will argue below, as well as in other chapters, the algebraic regularization program can play an instrumental role and represent ontological emergence in sharper relief, precisely by clearing away in its formalism much epistemologically emergent underbrush.

#### Section 4 : Summary of Algebraic Contraction and Expansion as Applied to the Simple Case of Special Relativity

In this section I provide a conceptual summary and overview notion of algebraic contraction and expansion, which I shall cover in greater detail in the subsequent chapter, as applied in the simple example of Galilean versus Special Relativity. For further details, see Inonu and Wigner (1952). It is important to keep in mind here that none of these techniques yet involve the specific applications of Clifford algebra per se, however in the subsequent chapter I provide arguments why such structures should be chosen as a basis for characterizing the more complex phenomena that Batterman (2004) discusses and I answer to. The objective in this section is simply to convey the essential conceptual notions at work in the procedure of expansion and contraction. For more precise and explicit definitions of group, algebras, and other algebraic structures, see Appendices A.1 and A.2.

*Expansion* denotes the process extending out from algebraically characterized  $T'$  to  $T'^*$ , denoted:  $T' \xrightarrow{\lambda} T'^*$ , where  $\lambda$  is fundamental parameter characterizing the algebraic expansion (Finkelstein (2002) 4-8). The inverse procedure:  $\lim_{\lambda \rightarrow 0} T'^* = T'$  is *contraction*. The question becomes: Which  $T'^*$  should one choose to guarantee a regular limit for *any*  $\lambda$  in the greatest possible generality? Consider the case of Galilean and Special Relativity. One can characterize both theories in terms of their *relativity* groups, i.e., the group of all dynamical symmetries. In the case of Galilean relativity,  $T' = GAL$ , or the Galilean group. Such a group consists of all dynamical transformations invariant under the Galilean transformations:  $\vec{r}'(t) = \vec{r}(t) - \vec{v}t, t' = t$ . Similarly, let  $T'^* = LOR$ , the

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mathematical representations requires reference to structures foreign to the fundamental theory. In this sense, they are *unpredictable from fundamental theory*. (2002, 96) It is fruitful [therefore] to think of emergence in terms of pairs of theories and their relationships ...After all, the very notion of predictability and explainability are theory-relative. These epistemic notions depend on metaphysical or ontological relations between theories—relations that require a sufficient mathematization of the theories in order to be seen. (2002, 128)

Lorentz group, or the group of all dynamical transformations invariant under the Lorentz transformations:  $\vec{r}'(t) = \gamma(\vec{r} - \vec{v}t), t' = \gamma\left(t - \frac{\vec{v} \cdot \vec{r}}{c^2}\right)$ , where:  $\gamma = (1 - v^2/c^2)^{-\frac{1}{2}}$ . Group expansion and contraction is formally depicted by:  $GAL \xrightarrow{c} LOR$  and  $\lim_{c \rightarrow \infty} LOR = GAL$ .

*Prima facie* it appears as though all that has been done so far is to reformulate the most banal example of intertheoretic reduction into group theoretic language. However as Inonou and Wigner (1953) and Segal (1951) demonstrated, the method of algebraic expansion/contraction reveals subtleties that are concealed in the typical functional-analytical approach to taking limits. For instance, *LOR* is *simple*<sup>145</sup> while *GAL* isn't. The infinitesimal transformations spanned by the tangent spaces of Minkowski and Galilean spacetime and described in terms of Lie algebras *dLOR*, *dGAL* reveal that *dLOR* is *stable* and *dGAL* is *unstable*. That is to say, *dLOR* is less sensitive to perturbations of values of its infinitesimal contraction parameter(s) and structure constants.<sup>146</sup> Last of all, the Lorentz transformations are fully reciprocal in the sense that the couplings between space and time are fully reciprocal: the rule for transforming *r* explicitly contains *t*:  $\vec{r}'(t) = \gamma(\vec{r} - \vec{v}t)$  and vice versa for transformations of *t*:  $t'(r) = \gamma\left(t - \frac{\vec{v} \cdot \vec{r}}{c^2}\right)$ . Denote this reciprocity by the shorthand expression:  $\vec{r} \leftrightarrow_{SR} t$ . The Galilean transformations, on the other hand, are not fully reciprocal: The rule for transforming *r* explicitly contains *t*:  $\vec{r}'(t) = (\vec{r} - \vec{v}t)$  but not conversely in the case of time transformations:  $t' = t$ . Denote this 'one-way coupling' by the shorthand expression:  $\vec{r} \rightarrow_{GAL} t$ .

In summary, one expands into an algebraic structure whose relativity group is simple, whose Lie algebra depicting its infinitesimal transformations is stable, which in turn entails greater reciprocity, i.e., "reciprocal couplings in the theory...reactions for every action." (Finkelstein, 2002,10). Algebraic expansion and contraction is adopted by Baugh, et. al. (2003) and Finkelstein (2002, 2003, 2004a) as a general methodology of algebraic expansion, guided by the criteria of simplicity, stability, and reciprocity as motivated by what is described in Baugh et. al. as the *Segal Doctrine*:

Segal proposed that groups of a physical theory should be stable...[t]he Segal Doctrine suggests that any compound physical theory is a contraction of a more stable, more accurate...theory which we call its *expansion*. (2003, 1268)

### Section 5 : Concluding Remarks

<sup>145</sup> I.e., contains no non-trivial proper invariant subgroups. For further details, see Appendix A.1

<sup>146</sup> For further details, see Appendix A.1

I have critically surveyed Batterman's (2002, 2003) notions of asymptotic analysis and explanation, vis-à-vis notions of emergence, explanation, and intertheoretic reduction. I have argued that Batterman and some of his respondents appear to conflate a theory's mathematical content with its ontological content. I have suggested in response that once these notions are kept separate, instances of ontological emergence can be rendered more clearly specified by theories whose mathematical content is devoid of such instances of epistemically emergent singular behavior.

I have introduced the method of algebraic expansion and contraction as an alternative framework to singular behavior often encountered in the asymptotic analysis of theories characterized by standard analytic-functional approaches, such as, for instance, the general theory of partial differential equation as discussed by Belot (2003). The method of expansion/contraction of algebraic structures provides a regular (or singularity-free) alternative to standard 'asymptotic analysis', inasmuch as it is possible to expand to algebraic structures exhibiting simplicity and stability in their relativity groups and Lie algebras, then a regular limit in the inverse process of contraction is guaranteed. In the ensuing chapter I will focus on geometric aspects of Clifford algebraic methods of expansion and contraction, which exhibit even more robust features of regularization. This presents a compelling instance of what Bishop was referring to as 'another theoretical topology' guaranteeing regular limits when the previous scheme failed and produced a singularity.

One can appreciate Batterman's ideas involving the complex interplay of the structures inherent in the superseded and superseding theories in the singular cases. But the price one appears to pay, aside from the dangers of inadvertently placing undue emphasis on fundamentally non-physical notions of infinities in the singular limit, also include the separation of explanation from reduction: Reduction fails in singular cases, yet Batterman still wishes to preserve a sense of value in the asymptotic 'explanation' here.

Rather than rest content with such a predicament, I argue here and in the following chapter that we could select the route of algebraic expansion/contraction, to the extent that this is possible,<sup>147</sup> which bespeaks of an impetus that is quite common for the typical physicist. This impetus can be understood as a general dissatisfaction with any singularity encountered for the simple reason that a singular limit tells us that our theory has failed in a certain domain.

So rather than provisionally set up a delicate and complex admixture of infinities and patchwork ontologies in the superseding and superseded theories as Batterman seems to advocate, one could instead look for more powerful methods of regularization which would inevitably involve theories, as briefly described above, with greater algebraic and topological structure. Among other things, this latter course of action calls into question the presumed distinction of explanation versus reduction in the cases. Furthermore, in the case of algebraic expansion and contraction one would be

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<sup>147</sup> I will discuss the limits of such an approach in greater detail in the ensuing chapter.

hard pressed to claim that this approach suffers from the shortcomings of misrepresentative oversimplification that the traditional Nagelian schemes prove themselves to be.

## Chapter 3: The Case of Droplet Formation: How to Rejoin Epistemically Fundamental with Ontologically Fundamental Theories

### *Section 1: A Tale of Two Opposing Aims*

Robert Batterman (2005) distinguishes between “ontologically fundamental” and “epistemically fundamental” theories. The aim of former is to “get the metaphysical nature of the systems right,” (19) often at the expense of being explanatorily inadequate. Fundamentally explanatory issues involving the universal dynamical behavior of critical phenomena,<sup>148</sup> for instance, cannot be accounted for by the ontologically fundamental theory. The explanatory aim of epistemologically fundamental theories, on the other hand, is an account for such universal behavior at the expense of suppressing (if not outright misrepresenting) a physical system’s fundamentally ontological features.

In the case of critical phenomena such as droplet formation, even in cases of more fine-grained resolutions of the scaling similarity solution for the Navier-Stokes equations (which approximate a fluid as a continuum), “we must appeal to the non-Humean similarity solution (resulting from the singularity) of the *idealized* continuum Navier-Stokes theory.” (20) In a more general sense, though “nature abhors a singularity...without them one cannot characterize, describe, and explain the emergence of new universal phenomena at different scales.” (19)

In other words, according to Batterman (2005) we need the ontologically “false” but epistemically fundamental theory to account for the ontologically true but epistemically lacking fundamental theory. “[A] complete understanding (or at least an attempt) of the drop breakup problem requires essential use of a ‘nonfundamental’ [i.e. epistemically fundamental] theory...the continuum Navier-Stokes theory of fluid dynamics.” (18)

Batterman advocates this necessary coexistence of two kinds of fundamental theories can be viewed as a refinement of his more general themes presented in (2002). As I discussed in chapter 2 above, he argues that in the case of emergent phenomena, explanation and reduction part company. The superseded theory  $T$  can still play an essential role. The superseding theory  $T'$ , though ‘deeply containing  $T$ ’ (in some non-reductive sense) cannot adequately account for emergent and critical phenomena alone, and thus enlists  $T$  in some essential manner. This produces a rift between reduction and explanation, and one is forced to accommodate an admixture of differing ontologies characterized by the respectively superseding and superseded theories. In his later work, Batterman (2005) seems to

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<sup>148</sup>Such critical phenomena exhibiting universal dynamical properties include, but are not limited to, examples including fluids undergoing phase transitions under certain conditions favorable for modeling their behavior using Renormalization Group methods, shock-wave propagation (phonons), caustic surfaces occurring under study in the field of catastrophe optics, quantum chaotic phenomena, etc., some of which I reviewed in Chapter 2 above.

imply that epistemologically fundamental theories serve in a similarly necessary capacity in terms of what he explains the superseded theories do, in the case of emergent phenomena (2002).<sup>149</sup>

I have criticized Batterman's claims (2002, 2004) in chapter 2 above in a two-fold manner: Batterman confuses a theory's mathematical content with its ontological content. This confusion, in turn, causes him to exaggerate the importance of certain notions of singularities in the explanatory role they play in the superseded theory. I argue here that there exist methods of regularization in geometric algebraic characterizations of microphysical phenomena, which can provide a more reliable ontological account for what goes on at the microlevel level *precisely because* they bypass singularities that would otherwise occur in more conventional mathematical techniques not based on geometric algebraic expansion and contraction.

### **Subsection 1: Methodological Fundamentalism**

I characterize such a notion of 'fundamental' arising in algebraic expansion and contraction techniques as an example of a *methodological fundamentalism*, which offers a means of intertheoretic reduction overcoming the singular cases Batterman discusses in (2002, 2004). In the case of fluid dynamics, multilinear algebras like Clifford algebras have been recently applied by Gerik Scheuermann (2000), and Mann & Rockwood (2003) in their work on computational fluid dynamics (CFD). The authors show that CFD methods, involving Clifford algebraic techniques, are often applicable in the same contexts as the Navier-Stokes treatment –minus the singularities. Such results imply that methodological fundamentalism can, in the cases Batterman investigates, provisionally sort out and reconcile epistemically and ontologically fundamental theories. Hence, *pace* Batterman, they need not act at cross purposes.

Robert Batterman explains the motivation for presenting a distinction between ontological versus epistemically fundamental theories:

I have tried to show that a complete understanding (or at least an attempt...) of the drop breakup problem requires essential use of a 'nonfundamental' theory...the continuum Navier Stokes theory of fluid dynamics...[But] how can a false (because idealized) theory such as continuum fluid dynamics be *essential* for understanding the behaviors of systems that fail completely to exhibit the principal feature of that idealized theory? Such systems [after all] are discrete in nature and not continuous...*I think the term 'fundamental theory' is ambiguous...*[An ontologically fundamental theory]...gets the metaphysical nature of the system right. On the other hand...ontologically fundamental theories are often explanatorily inadequate. Certain explanatory questions...about the emergence and reproducibility of

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<sup>149</sup> Recall my mentioning in n. 99 above the distinction between superseded and superseding versus epistemically and ontologically fundamental types of theories.

patterns of behavior cannot be answered by the ontologically fundamental theory. I think that this shows...there is an epistemological notion of ‘fundamental theory’ that fails to coincide with the ontological notion. (2005, 18-19, italics added)

On the other hand, epistemically fundamental theories aim at a more comprehensive explanatory account, often, however, at the price of introducing essential singularities. For example, in the case of ‘universal classes’ of behavior of fluid-dynamical phenomena exhibiting patterns like droplet formation:

Explanation of [such] universal patterns of behavior require means for eliminating details that ontologically distinguish the different systems exhibiting the same behavior. *Such means are often provided by a blow-up or singularity in the epistemically more fundamental theory that is related to the ontologically fundamental theory by some limit.* (ibid., italics added)

Obviously, any theory relying on a continuous topology<sup>150</sup> harbors the possibility of exhibiting singular behavior, depending on its domain of application.<sup>151</sup> In the case of droplet-formation, for example, the (renormalized) solutions to the continuous Navier-Stokes Equations (NSE) exhibit singular behavior. These singularities play an essentially explanatory role insofar as such solutions in the singular limit exhibit ‘self-similar,’ or universal behavior.<sup>152</sup> Only *one* parameter essentially governs the behavior of solutions to the NSEs in such a singular limit. Specifically, only the fluid’s *thickness* parameter (neck radius  $h$ ) governs the shape of the fluid near break-up,<sup>153</sup> in the asymptotic solution to the NSE (2004, 15):

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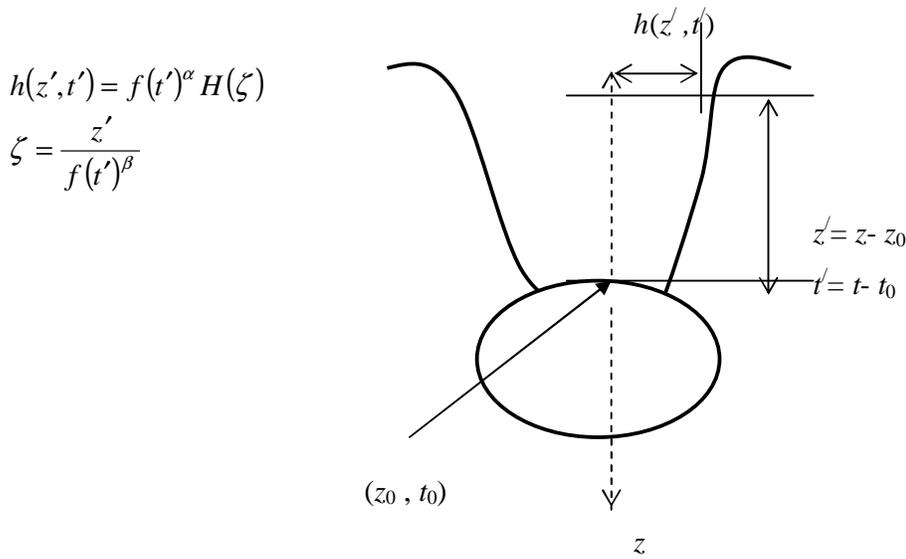
<sup>150</sup> Recall in chapter 2 above, I am borrowing from Bishop’s (2002) usage, in which he distinguishes the *ontology*, i.e. the primitive entities stipulated by a physical theory, from its *topology*, or structure of its mathematical formalism.

<sup>151</sup> This is of course due to the rich structure of continuous sets themselves admitting such effects. Consider, for example, the paradigmatic example:  $f \in (-\infty, \infty)^{(-\infty, \infty)}$  given by the rule:  $f(x) = 1/x$ . This obviously produces an essential singularity at  $x = 0$ .

<sup>152</sup> “Batterman suggests that the similarities in behavior [i.e., the universality] may be explained as a consequence of the fact that the differences in realization at the physical level are irrelevant to the higher-order behavior, in the same way that the differences between diverse systems underlying phase transitions are irrelevant to the behavior near the critical temperature.” (Strevens 2002, 655)

<sup>153</sup> For fluids of low viscosities see Batterman (2004), n 12, p.16.

**Figure 3.1 (Representation of the parameters governing droplet formation)**



where:  $f(t')$  is a continuous (dimensionless) function expressing the time-dependence of the solution ( $t' = t - t_0$  is the measured time after droplet breakup  $t_0$ ).

$\alpha, \beta$  are phenomenological constants to be determined.  $H$  is a Haenkel function.<sup>154</sup>

One could understand the epistemically and ontologically fundamental theories as playing analogous roles to Batterman's (2002, 2003, 2004) previously characterized superseded and superseding theories ( $T$  and  $T'$ , respectively). Analogous to the case of the superseded theory  $T$ , the epistemically fundamental theory offers crucial explanatory insight, at the expense of mischaracterizing the underlying ontology of the phenomena under study. Whereas, on the other hand, analogous to the case of the superseding theory  $T'$ , the ontologically fundamental theory gives a more representative metaphysical characterization, at the expense of losing its explanatory efficacy.

For instance, in the case of the breaking water droplet, the ontologically fundamental theory would be the molecular-discrete one. But aside from practical limitations posed by the sheer intractability of the computational complexity of such a quantitative account, the discrete-molecular theory, precisely *because* it lacks the singular-asymptotic aspect, cannot depict the (relatively) universal character presented in the asymptotic limit of the (renormalized) solutions to the NSE.

However, I argue here that there are theoretical characterizations whose formalisms can regularize or remove singularities from some of the fluid-dynamical behavior in a sufficiently abstract and general manner, as to call into question the presumably essential distinctions between epistemological and ontological fundamentalism. I call such formal approaches "methodologically fundamental,"<sup>155</sup>

<sup>154</sup> I.e. belonging to a class of orthonormal special functions often appearing in solutions to PDEs describing dynamics of boundary-value problems.

<sup>155</sup> Recall my specification mentioned in n. 5 above.

because of the *general* strategy such approaches introduce, in terms of offering a regularizing procedure. Adopting such methodologically fundamental procedures, whenever it is possible to do so,<sup>156</sup> suggests that Batterman's distinctions may not be different theoretical *kinds*, but function at best as different *aspects* of a unified methodological strategy. This calls into question the explanatory pluralism Batterman appears to be advocating.

## Subsection 2: Belot's Critiques Revisited

Recall from chapter 2 above that Gordon Belot's (2003) criticism consists of a more mathematically rigorous rendition of the superseding theory  $T'$  presumably eliminating the necessity of having to resort simultaneously to the superseded theory  $T$  to characterize some critical phenomenon (or class of phenomena)  $\Phi$ . Like Belot, I also claim that geometric algebraic techniques abound which can regularize the singularities appearing in formalisms of  $T$  (or  $T'$ ). Conversely, when representing such critical phenomena  $\Phi$ , singularities can occur in  $T$  (or  $T'$ ) when the latter are characterized by the more typically standard field-theoretic or phase space methods alone.

However, the mathematical content of the techniques I investigate differs significantly from those discussed by Belot (2003), who characterizes  $T'$  using the more general and abstract theory of partial differential equations on differentiable manifolds. He demonstrates that in principle, all of the necessary features of critical phenomena  $\Phi$  can be so depicted by the mathematical formalism of superseding theory  $T'$  alone (2003, 23). Because the manifold structure is continuous, this can admit the possibility of depicting such critical phenomena  $\Phi$  through complex and asymptotic singular behavior. In other words, Belot is *not* fundamentally questioning the underlying theoretical *topologies* typically associated with  $T$  and  $T'$ .<sup>157</sup> Instead, he is questioning the need to bring the two different *ontologies* of the superseded and superseding theories together, to adequately account for  $\Phi$ . Belot is questioning the presumed *ontological* pluralism that Batterman advanced in his notion of an 'asymptotic explanation'.

Batterman responds:

I suspect that one intuition behind Belot's ...objection is...I [appear to be] saying that for genuine explanation we need [to] appeal *essentially* to an idealization [i.e., the ontology of the superseded theory  $T$ .] ...In speaking of this idealization as essential for explanation, they take me to be reifying [ $T$ 's ontology]...*It is this last claim only that I reject.* I believe that in many instances our explanatory physical practice demands that we appeal essentially to (infinite)

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<sup>156</sup> The generality of the methods do *not* imply that they are a panacea, ridding *any* theory's formalism of singularities.

<sup>157</sup> I.e., differential equations on phase space, characterizable through the theory of differential manifolds.

idealizations. But I don't believe that this involves the reification of the idealized structures.”  
(2003, 7)

It is, of course, precisely the latter claim “that we appeal essentially to (infinite) idealizations” that I take issue with here, according to what the regularization procedures indicate. Batterman, however, cryptically and subsequently remarks that: “In arguing that an account that appeals to the mathematical idealization is superior to a theory that does not invoke the idealizations, I am not reifying the mathematics...I am claiming that the ‘fundamental’ theory that fails to take seriously the idealized [asymptotic] ‘boundary’ is less explanatorily adequate.” (8) In short, it seems that in his overarching emphasis in what he considers to be novel accounts of scientific explanation (namely, of the asymptotic variety) he often *blurs* the distinctions, and *shifts emphasis* between a theory's ontology and its topology. It is precisely this sort of equivocation, as I discussed in chapter 2 above, that causes him to inadvertently uphold mathematical notions like “infinite idealizations” as acting like some explanatory standard. To put it another way, since it is safe to assume that the actual critical phenomena Batterman discusses are ultimately metaphysically finite, precisely *how* can one ‘appeal essentially to (infinite) idealizations’ *without* inadvertently ‘reifying the mathematics?’

I, on the other hand, *pace* Belot (2003) and Batterman (2002-2005) present an alternative to the mathematical formalisms that both authors appeal to, which rely so centrally on continuous topological structures.<sup>158</sup> I show how discretely graded, and ultimately finite-dimensional multi-linear geometric (Clifford) algebras can provide accounts for some of the *same* critical phenomena  $\Phi$  in a *regularizable* or a singularity-free fashion.

### **Subsection 3: Disclaimer Concerning the General Applicability of Clifford Algebra in Characterizing Critical Phenomena**

Prior to describing the specific details of how to implement the strategy in the case of critical phenomena exhibited in fluid dynamics, however, I make the following disclaimer: I am definitely *not* arguing that the discrete, graded, multilinear Clifford-algebraic methods share such a degree of universal applicability that they should *supplant* the continuous, phase-space, infinite-dimensional differentiable manifold structure constituting the general formalism of the theory of differential equations, whether ordinary or partial. Nor do I *have* to make a general claim here in this chapter, but merely offer a counterexample for the case of the critical phenomenon of breaking droplets that

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<sup>158</sup> Of course, in the case of Batterman, continuous structures comprise as well the *ontology* of the epistemically fundamental theory: Navier-Stokes treats fluids as continua. In the case of Belot, the theory of partial differential equations he presents relies fundamentally on continuous, differentiable manifolds, characterizing the “formal ontology” of the theory of fluid mechanics (to use Rohrlich's notions, as discussed in I.2 above).

Batterman (2005) analyzes. Research in geometric algebra is ongoing and burgeoning, both in the fields of fundamental as well as in applied physics. (Baugh et. al. (2003), Baylis (1995), Bolinder (1987), Conte (1993-2000), Finkelstein (1999-2004), Gallier (2005), Hestenes (1984, 1986), Khrenikov (2005), Lansenby, et. al. (2000), Levine & Dannon (2000), Mann et. al. (2003), Nebe (1999, 2000), Scheuermann (2000), Sloane (2001), Snygg (1997), Van den Nest, *et. al.* (2005), Vlasov (2000)). All the authors listed above (who comprise just a miniscule sample of the enormous body of literature on the subject of applications of Clifford Algebra in physics and engineering) either describe the mathematical formalism as especially appealing, due to its providing a ‘unifying language’ in the field of mathematical physics<sup>159</sup>, or apply the formalism in key instances to make some interpretative point in the foundations of quantum theory, no matter how specific<sup>160</sup> or general.<sup>161</sup>

Certainly the empirical content of a specific problem domain determines which is the ‘best’ mathematical structure to implement in any theory of mathematical physics. By and large, such criteria are often determined essentially by practical limitations of computational complexity.

No danger of the aforementioned sort of equivocation that Batterman seems to commit, as I have argued above, is encountered so long as one can carefully distinguish the epistemological, ontological, and methodological issues vis-à-vis our choice of mathematical formalism(s) (i.e. distinguishing aspects  $\mathcal{E}, \mathcal{O}, \mathcal{M}$ . as discussed in chapter 1 above). If the choice is primarily motivated by practical issues of computational facility, we can hopefully resist the temptation to reify our mathematical maneuvering, which would confuse the ‘approximate’ with the ‘fundamental’— let alone confusing ontological, epistemological, and methodological *senses* of the latter notion.<sup>162</sup> Even Batterman admits that “nature abhors singularities.” (2005, 20) So, I argue, should we. The entire paradigm behind regularization procedures is driven by the notion that a singularity, far from being an “infinite idealization we must appeal to” (Batterman 2003, 7), is a signal that the underlying formalism of theory is the pathological cause, resulting in the theory’s failure to provide reliable information in certain critical cases.

Far from conceding to some class of “asymptotic-explanations,” lending a picture of the world of critical phenomena as somehow carved at the joints of asymptotic singularities, we must instead search for regularizable procedures. This is precisely why such an approach is methodologically

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<sup>159</sup> E.g., Finkelstein, Hestenes, Lansenby

<sup>160</sup> E.g., Conte, Hogreve, Snygg

<sup>161</sup> E.g., Hiley, Khrenikov, Vlasov

<sup>162</sup> I am, of course, *not* saying that there does not exist any connection whatsoever between a theory’s computational efficacy and its ability to represent certain fundamentally ontological features of the phenomena of interest. *What* that connection ultimately *is* (whether empirical, or some complex and indirect logical blend thereof) I remain an agnostic. I do not take simplicity as evidence of a high degree of verisimilitude, in a manner similar to van Fraassen’s (1980) “agnosticism” concerning the correct evidential consequences of a theory and its “truth.”

fundamental: regularization implies some (weak) form of intertheoretic reduction, as I shall argue below.

## Section 2: Clifford Algebraic Regularization Procedure: A Brief Overview

### **Subsection 1: Some Proposed Necessary Conditions for a Methodologically Fundamental Procedure**

In this section, I summarize aspects of methods incorporating algebraic structures frequently used in mathematical physics, leading up to and including the regularization procedures latent in applications of Clifford Algebras. Because this material involves some technical notions of varying degrees of specialty, I have provided for the interested reader an Appendix at the end of this essay supplying all the necessary definitions and brief explanations thereon.

I review here a few basic techniques involving (abstract algebraic) *expansion* and *contraction*. Recall the example concerning Special Relativity discussed in chapter 2, §4 above. The central maneuvers remain the same, save for the kinds of algebraic structures employed herein. Consider the situation in which the superceding theory  $T'$  is capable of being characterized, in principle, by an *algebra*.<sup>163</sup> Algebraic expansion denotes the process of extending out from algebraically characterized  $T'$  to some  $T'^*$  (denoted:  $T' \xrightarrow{\lambda} T'^*$ ) where  $\lambda$  is some fundamental parameter characterizing the algebraic expansion. The inverse procedure:  $\lim_{\lambda \rightarrow 0} T'^* = T'$  is *contraction*.

The question becomes: how to regularize? In other words, which  $T'^*$  should one choose to guarantee a regular (i.e., non-singular) limit for *any*  $\lambda$  in the greatest possible generality? Answer: expanding into an algebraic structure whose relativity group, i.e., the group of all its dynamical symmetries,<sup>164</sup> is *simple* implies that the Lie algebra depicting its infinitesimal transformations is *stable*.<sup>165</sup> This in turn entails greater reciprocity,<sup>166</sup> i.e., “reciprocal couplings in the theory...reactions

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<sup>163</sup> That is to say, a vector space with an associative product. For further details, see Appendix A.2 below.

<sup>164</sup> Recall the discussion in chapter 2, §4 above. In other words, the group of all actions in leaving their *form* of dynamical laws invariant (in the active view) or the group of all ‘coordinate transformations’ preserving the tensor character of the dynamical laws (in the ‘passive view.’) Also, see Defn. A.2.2 in Appendix A.2 below for a description of simple groups.

<sup>165</sup> For a brief description of stable Lie algebras, see the discussion following Defn A.2.4, section A.2, Appedix.

<sup>166</sup> For example, in the case of the Lorentz group, which is simple, it is maximally reciprocal in terms of its fundamental parameters  $x$ , and  $t$ . That is to say, the form of Lorentz transformations (simplified in one dimensional motion along the  $x$ -axes of the inertial frame  $F$  and  $F'$ ) become  $x' = x'(x, t) = \gamma(x - Vt)$  and  $t' = t'(x, t) = \gamma(t - Vx/c^2)$  (where  $\gamma = (1 - V^2/c^2)^{-1/2}$ ). Hence both space  $x$  and time  $t$  couple when transforming between inertial frames  $F, F'$ , as their respective transformations involve each other. On the other hand, the Galilean group is *not* simple, as it contains an invariant subgroup of boosts. The Galilean transformations are not maximally reciprocal, as  $x' = x'(x, t) = x - Vt$  but  $t' = t$ .  $x$  is a cyclic coordinate with respect to transformation  $t'$ .

for every action.” (Finkelstein, 2002,10, Baugh, et. al., 2003). This is an instance of a *methodologically fundamental* procedure, which I summarize by the following general necessary conditions:

- **Ansatz Ia:** If a procedure  $\mathcal{P}$  for formulating a theory  $T$  in mathematical physics is *methodologically fundamental*, then there exists some algebraically characterized *expansion*  $T'^*$  of  $T$ 's algebraic characterization (denoted by  $T'$ ) and some expansion parameter  $\lambda$  such that:  $T' \xrightarrow{\lambda} T'^*$ . Then, trivially,  $T'^*$  is regularizable with respect to  $T'$  since  $\lim_{\lambda \rightarrow 0} T'^* = T'$  is well-defined (via the inverse procedure of algebraic contraction).
- **Ansatz Ib:** If  $T'^*$  is an expansion of  $T'$ , then  $T'^*$ 's relativity group is *simple*, which results in a *stable* Lie algebra  $dT'^*$ , and whose set of observables in  $T'^*$  is maximally reciprocal.

The Segal Doctrine (Baugh, et. al. 2003) described any algebraic formalization of a theory obeying what I depict above, according to Ansatz Ib, as “fundamental.” I insert here the adjective “methodological,” since such a procedure comprises a method of regularization (viewed from the standpoint of the ‘inverse’ procedure of contraction) and so provides a formal, methodological means of reducing a superseding theory  $T'$  into its superseded theory  $T$ , when characterized by algebras.

In the following subsection, I summarize in detail how such a methodologically fundamental procedure, characterized by the Ansätze above, has been developed by Baugh (2003), Finkelstein (2001-2004a) and Shiri-Garakhani (2004b) as a means to derive continuous structures, encountered in general relativity, from this discrete geometrical algebraic basis. Because of the specificity and technicality of some of the details, the reader may skip this section without loss of any of the conceptual insights presented in this chapter. I nevertheless present the section below as part of the chapter, rather than as a separate section to the Appendix below, to illustrate to the interested reader in a concrete fashion some of the successful developments of Clifford algebraic methods in some of the most daunting areas of theoretical physics involving the complex interplay between discrete-based and continuum-based theories as constitutive of quantum topology. Such applications in my opinion reinforce the claims made by numerous researchers regarding the promise of such method, in its specifically robust regularizability which presents itself as a viable alternative to the more common continuum-based methods typically constitutive of field theory (whether quantum or classical).<sup>167</sup>

## **Subsection 2: Deriving a Continuous Space-Time Field Theory as an Asymptotic Approximation of a Finite Dimensional Clifford Algebraic Characterization of Spatiotemporal Quantum Topology.**

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Thus, when transforming between frames,  $x$  couples with respect to  $t$  but not vice versa. Recall discussion in chapter 2, §4 above.

<sup>167</sup> Of which renormalization group methods are the most notorious, as I explain in Kallfelz 2005a.

Baugh, et. al. (2003), Finkelstein (1996, 2001, 2004a-c) presents a unification of field theories (quantum and classical) and space-time theory based fundamentally on *finite* dimensional Clifford algebraic structures. The regularization procedure fundamentally involves group-theoretic simplification. The choice of the Clifford algebra<sup>168</sup> is motivated by two fundamental reasons:

1. The typically abstract (adjoint-based) algebraic characterizations of quantum dynamics (whether  $C^*$ , Heisenberg, etc.) represents how actions can be combined (in series, parallel, or reversed) but omits space-time fine structure.<sup>169</sup> On the other hand, a Clifford algebra can express a quantum space-time. (2001, 5)
2. Clifford statistics<sup>170</sup> for chronons adequately expresses the distinguishability of events as well as the existence of half-integer spin. (2001, 7)

The first reason entails that the prime variable is not the space-time field, as Einstein stipulated, but rather the dynamical law. That is to say, “the dynamical law [is] the only dependent variable, on which all others depend.” (2001, 6) The “atomic” quantum dynamical unit (represented by a generator  $\gamma^\alpha$  of a Clifford algebra) is the *chronon*  $\chi$ , with the closest classical analogue being the tangent or cotangent vector (forming an 8-dimensional manifold) and *not* the space-time point (forming a 4-dimensional manifold).

Applying Clifford statistics to dynamics is achieved via the (category) functors<sup>171</sup> ENDO, SQ which map the mode space<sup>172</sup>  $X$  of the chronon  $\chi$ , to its operator algebra (the algebra of endomorphisms<sup>173</sup>  $A$  on  $X$ ) and to its spinor space  $S$  (the statistical composite of all chronons transpiring in some experimental region.) (2001, 10). The action of ENDO, SQ producing the Clifford algebra *CLIFF*, representing the global dynamics of the chronon ensemble is depicted in the following commutative diagram:

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<sup>168</sup> The associated multiplicative groups embedded in Clifford algebras obey the simplicity criterion (Ansatz Ib, subsection 1 above). Hence Clifford algebras (or geometric algebras) remain an attractive candidate for algebraicizing any theory in mathematical physics (assuming the Clifford product and sum can be appropriately operationally interpreted in the theory  $T$ ). For definitions and further discussion thereon, see Defn A.2.5, Appendix A.2.

<sup>169</sup> The space-time structure must be supplied by classical structures, prior to the definition of the dynamical algebra. (2001, 5)

<sup>170</sup> I.e., the simplest statistics supporting a 2-valued representation of  $S_N$ , the symmetry group on  $N$  objects.

<sup>171</sup> See Defn. A.1.2, Appendix A.1

<sup>172</sup> The mode space is a kinematic notion, describing the set of all possible modes for a chronon  $\chi$ , the way a state space describes the set of all possible states for a state  $\phi$  in ordinary quantum mechanics.

<sup>173</sup> I.e., the set of surjective (onto) algebraic structure-preserving maps (those preserving the action of the algebraic ‘product’ or ‘sum’ between two algebras  $A, A'$ ). In other words,  $\Phi$  is an endomorphism on  $X$ , i.e.  $\Phi: X \rightarrow X$  iff:  $\forall x, y \in X: \Phi(x+y) = \Phi(x) + \Phi(y)$ , where  $+$  is vector addition. Furthermore  $\Phi(X) = X$ : i.e. for any  $z \in X: \exists x \in X$  such that  $\Phi(x) = z$ . For a more general discussion on the abstract algebraic notions, see A.2, Appendix.

$$\begin{array}{ccc}
& ENDO & \\
X & \longrightarrow & A = ENDO(X) \\
\downarrow SQ & & \downarrow SQ \\
S & \xrightarrow{ENDO} & CLIFF
\end{array}$$

**Fig. 3.2: Commutative diagram representing the action of deriving a statistics of quantum spacetime based on Clifford algebra**

Analogous to H.S. Green’s (2000) embedding of space-time geometry into a parafermionic algebra of qubits, Finkelstein shows that a Clifford statistical ensemble of chronons can factor as a Maxwell-Boltzmann ensemble of Clifford subalgebras. This in turn becomes a Bose-Einstein aggregate in the  $N \rightarrow \infty$  limit (where  $N$  is the number of factors). This Bose-Einstein aggregate condenses into an 8-dimensional manifold  $M$ , which is isomorphic to the tangent bundle of space-time. Moreover,  $M$  is a *Clifford manifold*, i.e. a manifold provided with a Clifford ring:  $C(M) = C_0(M) \oplus C_1(M) \oplus \dots \oplus C_N(M)$  (where:  $C_0(M)$ ,  $C_1(M), \dots, C_N(M)$  represent the scalars, vectors, ...,  $N$ -vectors on the manifold). For any tangent vectors  $\gamma^\mu(x)$ ,  $\gamma^\nu(x)$  on (Lie algebra  $dM$ ) then:

$$\gamma^\mu(x) \circ \gamma^\nu(x) = g^{\mu\nu}(x)$$

where:  $\circ$  is the scalar product. (2004a, 43) Hence the space-time manifold is a singular limit of the Clifford algebra representing the global dynamics of chronons in an experimental region.

Observable consequences of the theory are discussed in the model of the oscillator (2004c). Since the dynamical oscillator undergirds much of the framework of contemporary quantum theory, especially quantum field theory, the (generalized) model oscillator constructed via group simplification and regularization is isomorphic to a dipole rotator in the orthogonal group  $O(6N)$  (where:  $N = l(l + 1) \gg 1$ ). In other words, a *finite* quantum mechanical oscillator results, bypassing the ultraviolet and infrared divergences that occur in the case of the standard (infinite dimensional) oscillator applied to quantum field theory. In place of these divergences are “soft” and “hard” cases, respectively representing maximum potential energy unable to excite one quantum of momentum, and maximum kinetic energy being unable to excite one quantum of position. “These [cases]...resemble [and] extend the original ones by which Planck obtained a finite thermal distribution of cavity radiation. Even the 0-point energy of a similarly regularized field theory will be finite, and can therefore be physical.” (2004c, 12)

In addition, such potentially observable extreme cases modify high and low energy physics, as “the simplest regularization leads to interactions between the previously uncoupled excitation quanta of the oscillator...strongly attractive for soft or hard quanta.” (2004c, 19) Since the oscillator model

quantizes and unifies time, energy, space, and momentum, on the scale of the Planck power ( $10^{51}$  W), time and energy can be interconverted.<sup>174</sup>

### Subsection 3: (Some General Remarks) What Makes Multilinear Algebraic Expansion Methodologically Fundamental.

Before turning to the example involving applying Clifford algebraic characterization of critical phenomena in fluid mechanics, I shall give a final and brief recapitulation concerning the reasons why one should consider such methods described here as methodologically fundamental. For starters, the previous two Ansätze that I have proposed (in subsection 1 above) act as necessary conditions for what may constitute a methodologically fundamental procedure. Phrasing them in their contrapositive form (I.a\*, I.b\* below) also tell us what formalization schemes for theories in mathematical physics *cannot* be considered methodologically fundamental:

- **Ansatz (Ia\*):** If  $T'^*$  is *singular* with respect to  $T'$ , in the sense that the behavior of  $T'^*$  in the  $\lambda \rightarrow 0$  limit does *not* converge to the theory  $T'$  at the  $\lambda = 0$  limit (for any such contraction parameter  $\lambda$ ), this entails that the procedure  $\mathbb{P}$  for formulating a theory  $T$  in mathematical physics *cannot* be *methodologically fundamental*, and is therefore *methodologically approximate*.
- **Ansatz (Ib\*):** If the relativity group of  $T'^*$  is not simple, its Lie algebra is subsequently unstable. Therefore  $T'^*$  *cannot* act as an effective algebraic expansion of  $T'$  in the sense of guaranteeing that the inverse contraction procedure is non-singular.

Certainly Ansatz Ia\* is just a re-statement (in algebraic terms) of Batterman's more general discussion (2002) of critical phenomena, evincing in his case-studies a singularity or inability for the superseding theory to reduce to the superseded theory. However this need not entail that we must preserve a notion of 'asymptotic explanations,' as Batterman would invite us to do, which would somehow inextricably involve the superseded and the superseding theories. Instead, as Ansatz I.a\* states, this simply tells us that the mathematical scheme of the respective theory (or theories) is *not*

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<sup>174</sup> In such extreme cases, equipartition and Heisenberg Uncertainty is violated. The uncertainty relation for the soft and hard oscillators read, respectively:

$$(\Delta L_1)^2 (\Delta L_2)^2 \geq \frac{\hbar^2}{4} \langle L_3 \rangle^2_{|L_2=0} \approx 0 \Rightarrow \Delta p \Delta q \ll \frac{\hbar}{2}$$

$$(\Delta L_1)^2 (\Delta L_2)^2 \geq \frac{\hbar^2}{4} \langle L_3 \rangle^2_{|L_1=0} \approx 0 \Rightarrow \Delta p \Delta q \ll \frac{\hbar}{2}$$

methodologically fundamental, so we have a signal to search for methodologically fundamental procedures in the particular problem-domain, if they exist.<sup>175</sup>

Ansatz I.b\* gives us further insight into criteria filtering out methodologically fundamental procedures. Finkelstein, et. al. (2001) demonstrate that *all* field theories exhibit, at root, an underlying fiber-bundle topology<sup>176</sup> and *cannot* have any relativity groups that are simple. This excludes a vast class of mathematical formalisms: *all*-field theoretic formalisms, whether classical or quantum.

However, as informally discussed in the preceding section, if any class of mathematical formalisms is methodologically approximate, this would not in itself entail that the computational efficacy or empirical adequacy of any theory  $T$  constituted by such a class is somehow diminished. If a formalism is found to be methodologically approximate, this should simply act as a caveat against laying excessive emphasis on the theory's ontology, until such a theory can be characterized by a methodologically fundamental procedure.

A methodologically fundamental strategy does more than simply remove undesirable singularities. As discussed above in subsection 1, the finite number of degrees of freedom (represented by the maximum grade  $N$  of the particular Clifford algebra) positively informs certain ontologically fundamental notions regarding our metaphysical intuitions concerning the ultimately discrete characteristics of the entities fundamentally constituting the phenomenon of interest.<sup>177</sup> On the other hand, the regularization techniques have, *pace* Batterman, epistemically fundamental consequences that are positive.

In closing, one can ask how likely is it that methodologically fundamental multilinear algebraic strategies can be applied to any complex phenomena under study, such as critical behavior? The serious questions deal with practical limitations of computational complexity: asymptotic methods can yield simple and elegantly powerful results, which would undoubtedly otherwise prove far more laborious to establish by discrete multilinear structures, no matter how methodologically fundamental the latter turn out to be. Nevertheless, the ever-burgeoning field of computational physics gives us an extra degree of freedom to handle, to a certain extent, the risk of combinatorial explosion that such

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<sup>175</sup> In a practical sense, of course, the existence of procedures entail staying within the strict bounds determined by what is computationally feasible.

<sup>176</sup> I.e., for Hausdorff (separable) spaces  $X, B, F$ , and map  $p: X \rightarrow B$ , defined as a bundle projection (with fiber  $F$ ) if there exists a homeomorphism (topologically continuous map) defined on every neighborhood  $U$  for any point  $b \in B$  such that:  $\phi: p(\phi \langle b, f \rangle) = b$  for any  $f \in F$ . On  $p^{-1}(U) = \{x \in X \mid p(x) \in U\}$ , then  $p$  acts as a projection map on  $U \times F \rightarrow F$ . A fiber bundle consists is described by  $B \times F$ , (subject to other topological constraints (Brendon (2000), 106-107)) where  $B$  acts as the set of *base points*  $\{b \mid b \in B \subseteq X\}$  and  $F$  the associated fibres  $p^{-1}(b) = \{x \in X \mid p(x) = b\}$  at each  $b$ .

<sup>177</sup> Recall the discussion of ontological levels in I.3, I. 4 above. This is relative, of course, to the level of scale we wish to begin, in terms of characterizing the theories' ontological primitives. For instance, should one wish to begin at the level of quarks, the question of whether or not their fundamental properties are discrete or continuous becomes a murky issue. Though quantum mechanics is often understood as a fundamentally 'discrete' theory, the continuum nevertheless appears in a subtle manner, when considering entangled modes, which are based on particular superpositions of 'non-factorizable' products.

multilinear algebraic techniques may present, when applied to a given domain of complex phenomena.<sup>178</sup> I examine one case below, regarding the utilization of Clifford algebraic techniques in computational fluid dynamics (CFD), in modeling critical phenomena.

*Section 3: Clifford Algebraic Applications in CFD: An Alternative to Navier-Stokes in the Analysis of Critical Phenomena.*

Gerik Scheuermann (2000), as well as Mann & Rockwood (2003), employ Clifford algebras to develop topological vector field visualizations of critical phenomena in fluid mechanics. Visualizations and CFD simulations form a respectable and epistemically robust way of characterizing critical phenomena, down to the nanoscale. (Lenhard (2004)) “The goal is not theory-based insight as it is [typically] elaborated in the philosophical literature about scientific explanation. Rather, *the goal is* [for instance] *to find stable design-rules that might even be sufficient to build a stable nano-device.*” (2004, 99, italics added) Simulations offer potential for intervention, challenging the “received criteria for what may count as adequate quantitative understanding.” (ibid.)

Thus, Lenhard’s above remarks appear as a rather strong endorsement for an epistemically fundamental procedure: The heuristics of CFD-based phenomenological approaches lend a quasi-empirical character to this kind of research.<sup>179</sup> CFD techniques can produce robust characterizations of critical phenomena where traditional, ‘[Navier-Stokes] theory-based insights’ often cannot. Moreover, aside from their explanatory power, CFD visualizations can present more accurate depictions of what occurs at the microlevel, insofar as the numerical and modeling algorithms can

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<sup>178</sup> To be precise, so long as the algorithms implementing such multilinear algebraic procedures are ‘polytime,’ i.e. grow in polynomial complexity, over time.

<sup>179</sup> The topic of computer simulations has received recent philosophical attention. Eric Winsberg (2003) makes the case that they enjoy ‘a life of their own’ (124) between the categories of activity such as theory-articulation on the one end, and laboratory experiments on the other. “[B]y the semiautonomy of a simulation model, one refers to the fact that it starts from theory but one modifies it with extensive approximations, idealizations, falsifications, auxiliary information, and the blood, sweat, and tears of much trial and error.” (109) In other words, stated negatively, the simulation cannot be derived in any straightforward algorithmic procedure from its ‘parent’ theory. Stated positively, simulation activity inevitably involves an essential aspect of abductive reasoning. Though by the same token, argues Winsberg, to conflate computer simulation activity with standard laboratory activity would be to confuse paintings with mirrors, as being equally representative of human posture (borrowing from Wittgenstein’s analogy used in a critique of Ramsey’s theory of identity). (116)

If in our analysis of simulation we take it to be a method that essentially begins with an algorithm antecedently taken to accurately mimic the system in question, then the question has been begged as to whether and how simulations can, and often do, provide us with genuinely new, previously unknown knowledge about the system being simulated. It would be as mysterious as if we could use portraits in order to learn new facts about the postures of our bodies in the way that Wittgenstein describes. (ibid.)

A fuller account of Clifford-algebraic CFD methods in the light of some of the recent philosophical work on computer simulations is a topic clearly worthy of another study, above and beyond the scope of this essay. I briefly remark on such implications in chapter 4 below.

support a more detailed depiction of dynamical processes occurring on the microlevel. Hence there appears to be no inherent tension here: Clifford-algebraic CFD procedures are epistemically as well ontologically fundamental.<sup>180</sup> Of course, I claim that what guarantees this reconciliation is precisely the underlying *methodologically fundamental* feature of applying Clifford algebras in these instances.

### Subsection 1: An Overview of Scheuermann’s Results

Scheuermann, Mann & Rockwood are primarily motivated by the practical aim of achieving accurately representative (i.e. ontologically fundamental) CFD models of fluid singularities giving equally reliable (i.e. epistemically fundamental) predictions and visualizations covering all sorts of states of affairs.

For example, Scheuermann (2000) points out that standard topological methods in CFD, using bilinear and piecewise linear interpolation approximating solutions to the Navier-Stokes equation, fail to detect critical points or regions of higher order (i.e. order greater than 1). To spell this out, the following definitions are needed:

**Definition 1 (Vector Field):** A 2D or 3D *vector field* is a continuous function

$V: M \rightarrow \mathbf{R}^n$  where  $M$  is a manifold<sup>181</sup>  $M \subseteq \mathbf{R}^n$ , where  $n = 2$  or  $3$  (for the 2D and 3D cases, respectively) and  $\mathbf{R}^n = \mathbf{R} \times \dots \times \mathbf{R}$  ( $n$  times)..  $\times \mathbf{R} = \{(x_1, \dots, x_n) \mid x_k \in \mathbf{R}, 1 \leq k \leq n\}$ , i.e.  $n$ -dimensional Euclidean space (where  $n = 2$  or  $3$ ).<sup>182</sup>

**Definition 2 (Critical points/region):** A critical point<sup>183</sup>  $x_c \in M \subseteq \mathbf{R}^n$  or region

$U \subseteq M \subseteq \mathbf{R}^n$  for the vector field  $V$  is one in which  $\|V(x_c)\| = 0$  or  $\|V(x)\| = 0$

$\forall x \in U$ , respectively.<sup>184</sup>

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<sup>180</sup> Which is not to say, of course, that the applications of Clifford algebras in CFD contain no inherent tensions. The trade-off, or tension, however, is of a *practical* nature: that between computational complexity and accurate representation of microlevel details. Lest this appears as though playing into the hands of Batterman’s epistemically versus ontologically ‘fundamental’ distinctions, it is important to keep in mind that the trade-off is one of a practical and contingent issue involving computational resources. Indeed, in the ideal limit of unconstrained computational power and resources, the trade-off disappears: one can model the underlying microlevel phenomena to an arbitrary degree of accuracy. On the other hand, Batterman seems to be arguing that some philosophically important explanatory distinction exists between ontological and epistemic fundamentalism.

<sup>181</sup> A manifold (2D or 3D) is a Hausdorff (i.e. simply connected) space in which each neighborhood of each one of its points is homeomorphic (topologically continuous) with a region in the plane  $\mathbf{R}^2$  or space  $\mathbf{R}^3$ , respectively. For more information concerning topological spaces, see Table A.1.1, Appendix A.1.

<sup>182</sup> I retain the characterization above to indicate that higher-dimensional generalizations are applicable. In fact, one of the chief advantages of the Clifford algebraic formulations is their automatic applicability and generalization to higher-dimensional spaces. This is in contrast to notions prevalent in vector algebra, in which some notions, like the case of the cross-product, are only definable for spaces of maximum dimension 3. See A.2 for further details.

<sup>183</sup> For simplicity, as long as no ambiguity appears, a point  $x$  in an  $n$ -dimensional manifold is depicted in the same manner as that of a scalar quantity  $x$ . However, it’s important to keep in mind that  $x$  in the former case refers to an  $n$ -dimensional position vector.

<sup>184</sup> Note:  $\| \cdot \|$  is simply the Euclidean norm. In the case of a 2D vector field, for example,  $\|V(x,y)\| =$

A higher-order critical point (or family of points) may signal, for instance, the presence of a saddle point (or saddle curve) in the case of the vector field being a gradient field of a scalar potential  $\Phi(x)$  in  $\mathbf{R}^{2(\text{or } 3)}$ , i.e.  $V(x) = \nabla\Phi(x)$ . “Higher-order critical points cannot exist in piecewise linear or bilinear interpolations. This thesis presents an algorithm based on a new theoretical relation between analytical field description in Clifford Algebra and topology.” (Scheuermann (2000), 1)

The essence of Scheuermann’s approach, of which he works out in detail examples in  $\mathbf{R}^2$  and its associated Clifford Algebra  $CL(\mathbf{R}^2)$  of maximal grade  $N = \dim\mathbf{R}^2 = 2$  consisting of  $2^2 = 4$  fundamental generators,<sup>185</sup> involves constructing in  $CL(\mathbf{R}^2)$  a coordinate-independent differential operator  $\partial: \mathbf{R}^2 \rightarrow CL(\mathbf{R}^2)$ . Here:  $\partial V(x) = \sum_{k=1}^2 g^k \frac{\partial V(x)}{\partial g^k}$ , where  $g^k$  the grade-1 generators, or two (non-zero, non-collinear) vectors which hence span  $\mathbf{R}^2$ , and  $\frac{\partial V}{\partial g^k}$  are the directional derivatives of  $V$  with respect to  $g^k$ . For example, if  $g^1, g^2$  are orthonormal vectors  $(\hat{e}_1, \hat{e}_2)$ , then:  $\partial V = (\nabla \bullet V)\mathbf{1} + (\nabla \wedge V)\mathbf{i}$ , where  $\mathbf{1}$ , and  $\mathbf{i}$  are the respective identity and unit pseudoscalars of  $CL(\mathbf{R}^2)$ .<sup>186</sup> For example, in the matrix algebra  $M_2(\mathbf{R})$ , i.e. the algebra of real-valued 2x2 matrices:

$$\mathbf{1} \equiv \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix} \quad \mathbf{i} = \hat{e}_1 \hat{e}_2 \equiv \begin{pmatrix} 0 & 1 \\ -1 & 0 \end{pmatrix}$$

Armed with this analytical notion of a coordinate-free differential operator, as well as adopting conformal mappings from  $\mathbf{R}^2$  into the space of Complex numbers (which the latter form a grade-1 Clifford algebra) Scheuermann develops a topological algorithm obtaining estimates for higher-order critical points as well as determining more efficient routines:

We can simplify the structure of the vector field and simplify the analysis by the scientist and engineer...some topological features may be missed by a piecewise linear interpolation [i.e., in the standard approach]. This problem is successfully attacked by using locally higher-order polynomial approximations [of the vector field, using conformal maps]...[which] are based on the possible local topological structure of the vector field and the results of analyzing plane vector fields by Clifford algebra and analysis. (ibid (2000), 7)

## Subsection 2: An Overview of Mann and Rockwood’s Results

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$\|u(x,y)\mathbf{i} + v(x,y)\mathbf{j}\| = [u^2(x,y) + v^2(x,y)]^{1/2}$ , where  $u$  and  $v$  are  $x$  and  $y$  are the  $x,y$  components of  $V$ , described as continuous functions, and  $\mathbf{i}, \mathbf{j}$  are orthonormal vectors parallel to the  $x$  and  $y$  axis, respectively.

<sup>185</sup> For details concerning these features of Clifford algebras, see Defn A.2.5 and the brief ensuing discussions in A.2

<sup>186</sup> compare this expression with the Clifford product in Defn A.2.5, A.2

Mann and Rockwood (2003) show how adopting Clifford algebras greatly simplifies the procedure for calculating the index (or order) of critical points or curves in a 2D or 3D vector field. Normally (without Clifford algebra) the index is presented in terms of an unwieldy integral formula involving the necessity of evaluating normal curvature around a closed contour, as well the differential of an even more difficult term, known as the Gauss map, which acts as the measure of integration. In short, even obtaining a rough numerical estimate for the index using standard vector calculus and differential geometry is a computationally costly procedure.

On the other hand, the index formula takes on a far more elegant form when characterized in a Clifford algebra:

$$ind(x_c) = \frac{C}{I} \int_{B(x_c)} \frac{V \wedge dV}{\|V\|^n} \quad (\text{IV.1})$$

where:  $n = \dim \mathbf{R}^n$  (where  $n = 2$  or  $3$ )

$x_c$  : a critical point, or point in a critical region.

$C$  : a normalization constant.

$I$  : the unit pseudoscalar of  $CL(\mathbf{R}^n)$ .

$\wedge$  : the exterior (Grassmann) product.<sup>187</sup>

The authors present various relatively straightforward algorithms for calculating the index of critical points using (IV.1) above. “[W]e found the use of Clifford algebra to be a straightforward blueprint in coding the algorithm...the...computations of Geometric [Clifford] algebra automatically handle some of the geometric details...simplifying the programming job.” (ibid., 6)

The most significant geometric details here of course involve *critical surfaces* arising in droplet-formation, which produce singularities in the standard Navier-Stokes continuum-based theory. Though Mann and Rockwood (2003) do not handle the problem of modeling droplet-formation using Clifford-algebraic CFD per se, they do present an algorithm for the computation of surface singularities:

To compute a surface singularity, we essentially use the same idea as for computing curve singularities...though the test for whether a surface singularity passes through the edge [of an idealized test cube used as the basis of ‘octree’ iterative algorithm, i.e. the 3D equivalent of a dichotomization procedure using squares that tile a plane] is simpler than in the case of curve singularities. No outer products are needed—if the projected vectors along an edge [of the cube] change orientation/sign, then there is a [surface] singularity in the projected vector field. (ibid., 4)

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<sup>187</sup> For definitions and brief discussions of these terms, see DefnA.2.5, A.2

### Subsection 3: Assessment of Some Strengths and Shortcomings in the Approaches

Shortcomings, however, include the procedure's inability to determine the index for curve and surface singularities. "Our approach here should be considered a first attempt...in finding curve and surface singularities...[our] heuristics are simple, and more work remains to improve them." (7)

Nevertheless, what is of interest here is the means by which a Clifford algebraic CFD algorithm can *determine the existence* of curve and surface singularities, and track their location in  $\mathbf{R}^3$  given a vector field  $V: M \rightarrow \mathbf{R}^3$ . The authors demonstrate their results using various constructed examples. Based on the fact that every element in a Clifford algebra is invertible,<sup>188</sup> the authors ran cases such as determining the line singularities for vector fields such as:

$$V(x, y, z) = (uw^{-1})\mathbf{u} + z\hat{e}_3 \quad (\text{IV.2})$$

where:

$$u(x, y) = x\hat{e}_1 + y\hat{e}_2$$

$$w(x, y) = \sqrt{x^2 + y^2}\hat{e}_1$$

and  $(\hat{e}_1, \hat{e}_2, \hat{e}_3)$  are the unit orthonormal vectors spanning  $\mathbf{R}^3$ .

An example like this would prove impossible to construct using standard vector calculus on manifolds, since the 'inverse' or quotient operation is undefined in the case of ordinary vectors. Hence the rich geometric and algebraic structure of Clifford algebras admits constructions and cases for fields that would prove inadmissible using standard approaches. The algorithm works also for *sampled* vector fields. "Regardless of the interpolation method, our method would find the singularities within the interpolated sampled field." (ibid., 5)

The Clifford algebraic CFD algorithms developed by the authors yield some of the following results:

1. A means for determining higher-order singularities, otherwise off-limits in standard CFD topology.
2. A means for locating surface and curve singularities for computed as well as sampled vector fields. Moreover, in the former case, the invertibility of Clifford elements produces constructions of vector fields subject to analyses that would otherwise prove inadmissible in standard vector field based formalisms.
3. A far more elegant and computationally efficient means for calculating the indices of singularities.

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<sup>188</sup> See A.2, in the discussion following Defn A.2.5, for further details.

Clifford algebraic CFD procedures that would refine Mann and Rockwood’s algorithms (described in §2 in this chapter) by determining for instance the indices of surface singularities, as well as being computationally more efficient, are precisely the cases that will serve as effective responses against Batterman’s claims. For there would exist formalisms rivaling, in their expressive power, the standard Navier-Stokes approach. But such CFD research relies exclusively on finite-dimensional Clifford algebraic techniques, and would not appeal to the asymptotic singularities in the standard Navier-Stokes formulation in any meaningful way. Certainly the “first attempt” by Mann and Rockwood in characterizing surface singularities is an impressive one, in what appears to be the onset of a very promising and compelling research program.

I have furthermore argued in this section that such Clifford algebraic CFD algorithms are both epistemically and ontologically fundamental. It remains to show how these CFD algorithms are, in principle, methodologically fundamental. I sketch this in the conclusion.

Section 4: Concluding Remarks

To show how Clifford algebraic CFD algorithms in principle conform to a methodologically fundamental procedure, as defined in described in III in this essay, recall the (Category theoretic) commutative diagram (Fig. 3.2):

$$\begin{array}{ccc}
 X & \xrightarrow{\text{ENDO}} & A = \text{ENDO}(X) \\
 \downarrow \text{SQ} & & \downarrow \text{SQ} \\
 S & \xrightarrow{\text{ENDO}} & CL
 \end{array}$$

**Fig. 3.2: Commutative diagram representing the action of deriving a statistics of quantum spacetime based on Clifford algebra**

Now, let  $X$  be the mode space of the eigenvectors of one particular fluid molecule. Then, the SQ functor acts on  $X$  to produce  $S$ : the statistical composite of the fluid’s molecules. The ENDO functor acts on  $X$  to produce  $A$ : the algebra of endomorphism (operators) on the mode space, which represents intervention/transformations of the observables of the molecule’s observables.

Acting on  $X$  either first with SQ and then with ENDO, or vice versa, will produce CL: the Clifford algebra representing the global dynamics of the fluid’s molecules for some experimental region. Though the grade  $N$  of this algebra is obviously vast,  $N$  is still finite. Hence a Clifford algebraic characterization of fluid dynamics is, in principle, methodologically fundamental for the same formal reasons as exhibited in the case of deriving the space-time manifold limit of fundamental quantum processes as characterized by Clifford algebras and Clifford statistics. (Finkelstein (2001, 2004a-c)).

Robert Batterman is quite correct. Nature abhors singularities. So should we. The above procedure denoted as ‘methodological fundamentalism’ shows us how singularities, at least in principle, may be avoided. We need not accept some divergence between explanation and reduction (Batterman 2002), or between epistemological and ontological fundamentalism (Batterman 2004, 2005).

## Chapter 4: Concluding Remarks

### Section 1: Ontological Pluralism Without Methodological Pluralism: Is Asymptotic Explanation and Reduction a Form of Transformation Reduction?

The unifying character of Clifford algebra is an instance of characterizing a theory with a mathematical formalism that strengthens its methodological unity: Both internally-by unifying geometric content, and externally, via increasing its intra-theoretic systematic connections via the Methodologically Fundamental (ch 3) procedure characterized by algebraic expansion and contraction. However, this does not come at the expense of diminishing ontological pluralism. So we have an instance of a mathematical system that extends and strengthens Rohrlich's framework, and conversely deflates the claims of Batterman and others who advocate a methodological pluralism.

I have subjected Batterman's work to a rigorous critique: both in an external sense (chapter 1 above) where I pointed out in several stages how Fritz Rohrlich's systematic program of reduction and explanation can go a long way to make up for deficits in Batterman's work, and conversely how Batterman's schemas could be characterized and reconstructed in Rohrlich's framework. This set the stage for my internal critique (chapters 2 and 3) in which I introduced aspects of geometric algebra by way of counterexample to Batterman's singular schema. Expressed in Rohrlich's terms, I presented a mathematical formalism  $\mathcal{M}^*$  (the Clifford) such that for theories  $T, T'$ :  $\lim_{p \rightarrow 0} \mathcal{M}(T') \neq \mathcal{M}(T)$  for the non-Clifford formalism or mathematical aspect  $\mathcal{M}$ , however:  $\lim_{p \rightarrow 0} \mathcal{M}^*(T') = \mathcal{M}^*(T)$ . This further adds the required 'symmetry' to seat Batterman's schema in the inter-theoretic reduction framework of Rohrlich, as mentioned in chapter 1, §3.2

However one can stretch the association between Batterman and Rohrlich too far, and certainly I do not wish to gloss over the philosophical merit of Batterman's case studies into diverse critical phenomena with its association of the mathematically intricate amalgamation of techniques, whether in the case of modeling supersonic shocks vis-à-vis non-linear Navier-Stokes CFD procedure, caustic surfaces vis-à-vis catastrophe optics, Gutzweiler scarring vis-a-vis quantum chaos, etc.

Hence I submit the claim here that even in the event that all the aforementioned domains will be eventually replaced with a Clifford algebraic characterization in the future,<sup>189</sup> Robert Batterman's work represents a noteworthy instance of a *transformation reduction* (TR) (Jeffrey Ramsey (1995)). Ramsey describes TRs as a species of *reduction by construction*: they share similarities with traditionally philosophical notions insofar as: a.) using fewer explanatory factors, b.) containing one structure within another. Dissimilarities however include:

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<sup>189</sup> Recall I only constructed a sketch of one concrete counterexample, in Chapter 3 above involving droplet formation.

1. Reduction usually doesn't take place between *theories*:

[P]rosaically [in TR] one theory is 'shrink-wrapped' so as to make it applicable to a particular range of phenomena. [However] liberalizing the notion of 'theory' to that of 'theoretical structure' and thus includ[ing] laws and models, then there is little if any difficulty with the claim of containment. (Ramsey (1995), 7-8)

2. In TR models are usually 'desorbed' rather than 'absorbed' into their underlying theories. In other words, though *generally* satisfying the relation of containment, structural characteristics seem to range from consolidation to elimination of variables, as well as from the construction of concepts and functional relations. (8)

3. TRs are a *discovery-oriented*, not a justification-oriented, enterprise. They aim to provide new testable consequences from theories with calculational and analytical structures that are insolvable as they stand: "One has to go in and 'muck around' with the calculational or analytical structure to produce a claim that can be compared to existing phenomena." (12)

Hence given these brief remarks above, one could argue that the asymptotic mathematical techniques satisfy the above *if* a (justificatory) reduction has already taken place in terms of Clifford-algebraic characterization. As mentioned in chapter 3, the fact that multilinear algebraic methods abound obviously does not eliminate the value of non-Clifford asymptotic methods. To name one instance: in the case of critical phenomena (turbulent cell formation) even if Clifford CFD procedures could model such behavior in their associated singularity-free manner, Batterman still presents a case for the legitimacy of adopting continuum-asymptotic methods for their *heuristic value* in depicting universal behavior. To name another example: though Clifford-algebraic characterizations of quantum theory abound, semi-classical approaches still have their heuristic value insofar as physicists have analogical representations of the surfaces of the Wigner function serving a useful discovery role akin to Winsberg's (2003) study of computer simulations.

Moreover, if Ramsey is correct in his assessment that inter-theoretic reduction should be conceived of as a spectrum spanned by the extremes of logical/semantic on one end (Nagel) and constructive-transformational (TR) on the other, then Rohrlich's (like Nickels) schema would sit squarely in the center, sharing a constructive character based on structuralist underpinnings (as discussed in chapter 1) as well as justification features based on its domain-preserving aspect as applied to mature theories.

## Section 2: Ramifications for Future Research

Clearly, I have only scratched the surface here in my study. For starters, I suggest three avenues below:

1. Further empirical-analytical work in applying geometric algebraic techniques to the case studies that Batterman investigates.
2. Adopting Burgess's (1992) suggestion (mentioned in the *Foreword*) to expand on a study of the *application* of mathematical formalisms in a rapprochement of philosophy of science and mathematics. I hope I have presented the case here that Clifford algebras, in their robust regularizability and generality, are an ideal candidate as a subject for such a study.
3. Extending some of the claims of contemporary structuralist philosophers of science and mathematics (Scheibe, Ehler, etc.) to the case of Clifford algebraic characterizations of the theories they they investigate.<sup>190</sup>

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<sup>190</sup> I began such an exercise in embedding aspects of Sneed (1971) into Category Theory (Kallfelz (2006c)).

# Appendices

## Appendix A.: A Brief Synopsis of the Relevant Algebraic Structures

### A.1: Category Algebra and Category Theory

As authors like Hestenes (1984, 1986), Snugg (1997), Lasenby, et. al. (2000) promote Clifford Algebra as a unified mathematical language for physics, so Adamek (1990), Mikhalev & Pilz (2000) and many others similarly claim that Category Theory likewise forms a unifying basis for all branches of mathematics. There are also mathematical physicists like Robert Geroch (1985) who seem to bridge these two presumably unifying languages by building up a mathematical toolchest comprising most of the salient algebraic and topological structures for the workaday mathematical physicist from a Category-theoretic basis.

A *category* is defined as follows:

- **Defn. A1.1:** A category  $C = \langle \Omega, \text{MOR}(\Omega), \circ \rangle$  is the ordered triple where:

a.)  $\Omega$  is the class of  $C$ 's *objects*.

b.)  $\text{MOR}(\Omega)$  is the set of *morphisms* defined on  $\Omega$ . Graphically, this can be depicted (where  $\varphi \in \text{MOR}(\Omega), A \in \Omega, B \in \Omega$ ):  $A \xrightarrow{\varphi} B$

c.) The elements of  $\text{MOR}(\Omega)$  are connected by the *product*  $\circ$  which obeys the law of composition: For  $A \in \Omega, B \in \Omega, C \in \Omega$ : if  $\varphi$  is the morphism from  $A$  to  $B$ , and if  $\psi$  is a morphism from  $B$  to  $C$ , then  $\psi \circ \varphi$  is a morphism from  $A$  to  $C$ , denoted graphically:  $A \xrightarrow{\varphi} B \circ B \xrightarrow{\psi} C = A \xrightarrow{\psi \circ \varphi} C$ . Furthermore:

c.1)  $\circ$  is *associative*: For any morphisms  $\phi, \varphi, \psi$  with product defined in as in c.) above, then:  $(\psi \circ \phi) \circ \varphi = \psi \circ (\phi \circ \varphi) \equiv \psi \circ \phi \circ \varphi$ .

c.2) Every morphism is equipped with a left and a right identity. That is, if  $\psi$  is any morphism from  $A$  to  $B$ , (where  $A$  and  $B$  are any two objects) then there exists the (right) *identity* morphism on  $A$  (denoted  $\iota_A$ ) such that:  $\psi \circ \iota_A = \psi$ . Furthermore, for any object  $C$ , if  $\varphi$  is any morphism from  $C$  to  $A$ , then there exists the (left) *identity* morphism on  $A$  ( $\iota_A$ ) such that:  $\iota_A \circ \varphi = \varphi$ . Graphically, the left (or right) identity morphisms can be depicted as *loops*.

A simpler way to define a category is in terms of a special kind of *semigroup* (i.e. a set  $S$  closed under an associative product). Since identities are defined for every object, one can in principle identify each object with its associated (left/right) identity. That is to say, for any morphism  $\varphi$  from  $A$

to  $B$ , with associated left/right identities  $\iota_B, \iota_A$ , identify:  $\iota_B = \lambda, \iota_A = \rho$ . Hence condition c2) above can be re-stated as c2'): "For every  $\varphi$  there exist  $(\lambda, \rho)$  such that:  $\lambda \circ \varphi = \varphi$ , and  $\varphi \circ \rho = \varphi$ ." With this apparent identification, DefnI.1 is coextensive with that of a "semigroup with enough identities.

Category theory provides a unique insight into the general nature, or universal features of the construction process that practically all mathematical systems share, in one way or another. Set theory can be embedded into category theory, but not vice versa. Such basic universal features involved in the construction of mathematical systems, which category theory generalizes and systematizes, include, at base, the following:

Feature	Underlying Notion
Objects	The collection of primitive, or stipulated, entities of the mathematical system.
Product	How to 'concatenate and combine,' in a natural manner, to form new objects or entities in the mathematical system respecting the properties of what are characterized by the system's stipulated objects.
Morphsim	How to 'morph' from one object to another.
Isomorphism (structural equivalence)	How all such objects, relative to the system, are understood to be equivalent.

**Table A.1.1**

For an informal demonstration of how such general aspects are abstracted from three different mathematical systems (sets, groups, and topological spaces<sup>191</sup>), for instance, see Table A.1.2 below.

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<sup>191</sup> Such systems, of course, are not conceptually disjunct: topological spaces and groups are of course defined in terms of sets. The additional element of structure comprising the concept of group includes the notion of a binary operation (which itself can be defined set-theoretically in terms of a *mapping*) sharing the algebraic property of associativity. The structural element distinguishing a topological space is also described set-theoretically by use of notions of 'open' sets. Moreover, groups and topological spaces can conceptually overlap as well in the notion of a *topological group*. So in an obvious sense, set theory remains a general classification language for mathematical systems as well. However, the *expressive power* of set theory pales in comparison to that of category theory. To put it another way, if category theory and set theory are conceived of as deductive systems (Lewis), it could be argued that category theory exhibits a better combination of "strength and simplicity" than does naïve set theory. Admittedly, however, this is not a point which can be easily resolved as far as the simplicity issue goes because the very concept of a category is usually cashed out in terms of three fundamental notions (objects, morphisms, associative composition), whereas, at least in the case of 'naïve' set theory (NST), we have fundamentally two notions: a) of membership  $\in$  defined by extension, and b) the hierarchy of *types* (i.e., for any set  $X, X \subseteq X$ , but  $X \notin X$ . Or to put more generally,  $Z \in W$  is a meaningful expression, though it may be false, provided, for any set,  $X: Z \in \wp^{(k)}(X)$  and  $W \in \wp^{(k+1)}(X)$ , where  $k$  is any non-negative integer, and  $\wp^{(k)}(X)$  defines the  $k$ th-level power-set operation, i.e.:  $\wp^{(m)}(X) = \wp(\wp(\dots k \text{ times} \dots(X))$ .)

<b>I.a) Set</b>	(by Principle of Extension) $S_{\Phi} = \{x \mid \Phi(x)\}$ for some property $\Phi$
<b>I.b) Cartesian Product</b>	For any two sets $X, Y : X \times Y = \{(x,y) \mid x \in X, y \in Y\}$
<b>I.c) Mapping</b>	For any two sets $X, Y$ , where $f \subseteq X \times Y$ , $f$ is a <i>mapping</i> from $X$ to $Y$ (denoted $f : X \rightarrow Y$ ) iff for $x_1 \in X, y_1 \in Y, y_2 \in Y$ , if $(x_1, y_1) \in f$ (denoted: $y_1 = f(x_1)$ ) $(x_1, y_2) \in f$ then: $y_1 = y_2$ .
<b>I.d) Bijection (set equivalence)</b>	For any two sets $X, Y$ , where $f : X \rightarrow Y$ is a mapping, then $f$ is a bijection iff: a) $f$ is onto (surjective), i.e. $f(X) = Y$ (i.e., for any $y \in Y$ there exists a $x \in X$ such that: $f(x) = y$ ), b) $f$ is 1-1 (injective) iff for $x_1 \in X, y_1 \in Y, y_2 \in Y$ , if $(x_1, y_1) \in f$ (denoted: $y_1 = f(x_1)$ ) $(x_1, y_2) \in f$ then: $y_1 = y_2$ .
<b>II.a) Group</b>	I.e., a group $\langle G, \circ \rangle$ is a set $G$ with a binary operation $\circ$ on $G$ such that: a.) $\circ$ is <i>closed</i> with respect to $G$ , i.e.: $\forall (x, y) \in G : (x \circ y) \equiv z \in G$ (i.e., $\circ$ is a <i>mapping into</i> $G$ or $\circ : G \times G \rightarrow G$ , or $\circ(G \times G) \subseteq G$ ). b.) $\circ$ is <i>associative</i> with respect to $G$ ,: $\forall (x, y, z) \in G : (x \circ y) \circ z = x \circ (y \circ z) \equiv x \circ y \circ z$ , c.) There (uniquely) exists a (left/right) identity element $e \in G : \forall (x \in G) \exists ! (e \in G) : x \circ e = x = e \circ x$ . d.) For every $x$ there exists an <i>inverse element</i> of $x$ , i.e.: $\forall (x \in G) \exists (x' \in G) : x \circ x' = e = x' \circ x$ .
<b>II.b) Direct product</b>	For any two groups $G, H$ , their <i>direct product</i> (denoted $G \otimes H$ ) is a group, with underlying set is $G \times H$ and whose binary operation $*$ is defined as, for any $(g_1, h_1) \in G \times H, (g_2, h_2) \in G \times H$ : $(g_1, h_1) * (g_2, h_2) = ((g_1 \circ h_1), (g_2 \bullet h_2))$ , where $\circ, \bullet$ are the respective binary operations for $G, H$ .
<b>II.c) Group homomorphism</b>	Any <i>structure-preserving mapping</i> $\phi$ from two groups $G$ and $H$ . I.e. $\phi : G \rightarrow H$ is a homomorphism iff for any $g_1 \in G, g_2 \in G : \phi(g_1 \circ g_2) = \phi(g_1) \bullet \phi(g_2)$ where $\circ, \bullet$ are the respective binary operations for $G, H$ .
<b>II.d) Group Isomorphism (group equivalence)</b>	Any <i>structure-preserving bijection</i> $\psi$ from two groups $G$ and $H$ . I.e. $\psi : G \rightarrow H$ is an isomorphism iff for any $g_1 \in G, g_2 \in G : \psi(g_1 \circ g_2) = \psi(g_1) \bullet \psi(g_2)$ (where $\circ, \bullet$ are the respective binary operations for $G, H$ ) and $\psi$ is a <i>bijection</i> (see I.d above) between group-elements $G$ and $H$ . Two groups are <i>isomorphic</i> (algebraically

	equivalent, denoted: $G \cong H$ ) <i>iff</i> there exists an isomorphism connecting them $\psi: G \rightarrow H$ .)
<b>III.a) Topological Space</b>	<p>Any set <math>X</math> endowed with a collection <math>\tau_X</math> of its subsets (i.e. <math>\tau_X \subseteq \wp(X)</math>, where <math>\wp(X)</math> is <math>X</math>'s power-set, such that: 1) <math>\emptyset \in \tau_X</math> , <math>X \in \tau_X</math> 2) For any <math>U, U' \in \tau_X</math> , then: <math>U \cap U' \in \tau_X</math> . 3) For any index (discrete or continuous) <math>\gamma</math> belonging to index-set <math>\Gamma</math>: if <math>U_\gamma \in \tau_X</math>, then: <math>\bigcup_{\gamma \in \Delta \subseteq \Gamma} U_\gamma \in \tau_X</math> . <math>X</math> is then denoted as a <i>topological space</i>, and <math>\tau_X</math> is its <i>topology</i>. Elements <math>U</math> belonging to <math>\tau_X</math> are denoted as <i>open sets</i>. Hence 1), 2), 3) say that the empty set and all of <math>X</math> are always open, and finite intersections of open sets are open, while arbitrary unions of open sets are always open. Moreover: 1) Any collection of subsets <math>\mathfrak{S}</math> of <math>X</math> is a <i>basis</i> for <math>X</math>'s topology <i>iff</i> for any <math>U \in \tau_X</math>, then for any index (discrete or continuous) <math>\gamma</math> belonging to index-set <math>\Gamma</math>: if <math>B_\gamma \in \mathfrak{S}</math>, then: <math>\bigcup_{\gamma \in \Delta \subseteq \Gamma} B_\gamma = U \in \tau_X</math> (i.e., arbitrary unions of basis elements are open sets.) 2) Any collection of subsets <math>\Sigma</math> of <math>X</math> is a <i>subbasis</i> if for any <math>\{S_1, \dots, S_N\} \subseteq \Sigma</math>, then <math>\bigcap_{k=1}^N S_k = B \in \mathfrak{S}</math> (I.e. finite intersections of sub-basis elements are basis elements for <math>X</math>'s topology.)</p>
<b>III.b) Topological product</b>	<p>For any two topological spaces <math>X, Y</math>, their <i>topological product</i> (denoted <math>\tau_X \otimes \tau_Y</math> ) is defined by taking, <i>as a sub-basis</i>, the collection: <math>\{(U,V)   U \in \tau_X, V \in \tau_Y\}</math>. I.e., <math>\tau_X \times \tau_Y</math> is a subbasis for <math>\tau_X \otimes \tau_Y</math>. This is immediately apparent since, for <math>U_1</math> and <math>U_2</math> open in <math>X</math>, and <math>V_1</math> and <math>V_2</math> open in <math>Y</math> : since: <math>U_1 \times U_2 \cap V_1 \times V_2 = (U_1 \cap V_1) \times (U_2 \cap V_2)</math> this indeed forms a basis.</p>
<b>III.c) Continuous mapping</b>	<p>Any mapping from two topological spaces <math>X</math> and <math>Y</math>, preserving openness. I.e. <math>f: X \rightarrow Y</math> is continuous <i>iff</i> for any <math>U \in \tau_X</math>: <math>f(U) = V \in \tau_Y</math></p>
<b>III.d) Homeomorphism (topological space equivalence)</b>	<p>Any <i>continous bijection</i> <math>h</math> from two topological spaces <math>X</math> and <math>Y</math>. I.e. <math>h: X \rightarrow Y</math> is a homeomorphsim <i>iff</i> : a) <math>h</math> is continuous (see III.c), b) <math>h</math> is a bijection (See I.d). Two spaces <math>X</math> and <math>Y</math> are <i>topologically equivalent</i> (i.e., homeomorphic, denoted: <math>X \cong Y</math>) <i>iff</i> there exists a homeomorphism connecting them, i.e. <math>h: X \rightarrow Y</math></p>

### Table A.1.2

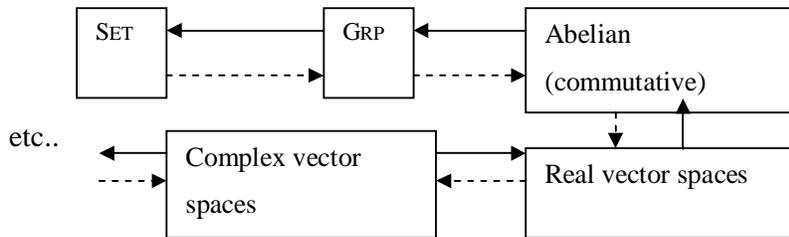
Now the classes of mathematical objects exhibited in Table A.1.2 comprising sets, groups, and topological spaces, all exhibit certain common features:

- The concept of *product* (I.b, II.b, III.b) (or concatenating, in ‘natural manner’ property-preserving structures.) For instance, the Cartesian (I.b) product preserves the ‘set-ness’ property for chains of objects formed from the class of sets, the direct product (II.b) preserves the ‘group-ness’ property under concatenation, etc.
- The concept of ‘morphing’ (I.c, II.c, III.c) from one class of objects to another, in a property-preserving manner. For instance, the continuous map (III.c) respects what makes spaces  $X$  and  $Y$  ‘topological,’ when morphing from one to another. The homomorphism respects the group properties shared by  $G$  and  $H$ , when ‘morphing’ from one to another, etc.
- The concept of ‘equivalence in form’ (isomorphism) (I.d, II.d, III.d) defined via conditions placed on ‘how’ one should ‘morph,’ which fundamentally should be in an *invertible* manner. One universally necessary condition for this to hold, is that such a manner is modeled as a bijection. The other necessary conditions of course involve the particular property structure-respecting conditions placed on such morphisms.

Similar to naïve set theory (NST) Category theory also preserves its form and structure on any level or category ‘type.’ That is to say, any two (or more) categories  $C, D$  can be part of the set of structured objects of a *meta-category*  $\mathbf{X}$  whose morphisms (functors) respect the categorical structure of its arguments  $C, D$ . That is to say:

- **Defn A1.2.** Given two categories  $C = \langle \Omega, \text{MOR}(\Omega), \circ \rangle, D = \langle \Omega', \text{MOR}(\Omega'), \bullet \rangle$ , a *categorical functor*  $\Phi$  is a morphism in the *meta-category*  $\mathbf{X}$  from objects  $C$  to  $D$  assigning each  $C$ -object (in  $\Omega$ ) a  $D$ -object (in  $\Omega'$ ) and each  $C$ -morphism (in  $\text{MOR}(\Omega)$ ) a  $D$ -morphism (in  $\text{MOR}(\Omega')$ ) such that:
  - a.)  $\Phi$  preserves the ‘product’ (compositional) structure of the two categories, i.e., for any  $\varphi \in \text{MOR}(\Omega), \psi \in \text{MOR}(\Omega): \Phi(\varphi \circ \psi) = \Phi(\varphi) \bullet \Phi(\psi) \equiv \varphi' \bullet \psi'$  (where  $\varphi', \psi'$  are the  $\Phi$ -images in  $D$  of the functors  $\varphi, \psi$  in  $C$ ).
  - b.)  $\Phi$  preserves identity structure across all categories. That is to say, for any  $A \in \Omega, \iota_A \in \text{MOR}(\Omega), \Phi(\iota_A) = \iota_{\Phi(A)} = \iota_{A'}$  where  $A'$  is the  $D$ -object (in  $\Omega'$ ) assigned by  $\Phi$ . (I.e.,  $A' = \Phi(A)$ )

Examples of functors include the ‘forgetful functor’  $\mathbf{FOR}: \mathbf{C} \rightarrow \mathbf{SET}$  (where  $\mathbf{SET}$  is the category of all sets) which has the effect of ‘stripping off’ any extra structure in a mathematical system  $\mathbf{C}$  down to its ‘bare-bones’ set-structure only. That is to say, for any  $\mathbf{C}$ -object  $A \in \Omega$ ,  $\mathbf{FOR}(A) = S_A$  (where  $S_A$  is  $A$ ’s underlying set), and for any  $\psi \in \mathbf{MOR}(\Omega)$ :  $\mathbf{FOR}(\psi) = f$  is just the mapping (or functional) property of  $\psi$ . Robert Geroch (1985, p. 132, p. 248), for example, builds up the toolchest of the most important mathematical structures applied in physics, via a combination of (partially forgetful<sup>192</sup>) and (free construction functors.) Part of this toolchest, for example, is suggested in the diagram below. The boxed items represent the categories (of sets, groups, Abelian or commutative groups, etc.), the solid arrows are the (partially) forgetful functors, and the dashed arrows represent the free construction functors.



**Figure A1.1: Hierarchy of Categories Bound by Free Construction Functors and Forgetful Functors**

## A.2 Clifford Algebras and Other Algebraic Structures

I proceed here by simply defining the necessary algebraic structures in an increasing hierarchy of complexity:

**Defn A2.1: (Group)** A group  $\langle G, \circ \rangle$  is a set  $G$  with a binary operation  $\circ$  on  $G$  such that:

- a.)  $\circ$  is *closed* with respect to  $G$ , i.e.:  $\forall (x, y) \in G : (x \circ y) \equiv z \in G$  (i.e.,  $\circ$  is a *mapping into*  $G$  or  $\circ : G \times G \rightarrow G$ , or  $\circ(G \times G) \subseteq G$ ).
- b.)  $\circ$  is *associative* with respect to  $G$ ,:  $\forall (x, y, z) \in G: (x \circ y) \circ z = x \circ (y \circ z) \equiv x \circ y \circ z$ ,
- c.) There (uniquely) exists a (left/right) identity element  $e \in G : \forall (x \in G) \exists! (e \in G) : x \circ e = x = e \circ x$ .
- d.) For every  $x$  there exists an *inverse element* of  $x$ , i.e.:  $\forall (x \in G) \exists (x' \in G): x \circ x' = e = x' \circ x$ .

<sup>192</sup> ‘Partially forgetful’ in the sense that the action of such functors does not collapse the structure entirely back to its set-base, just to the ‘nearest’ (simpler) structure.

In terms of categories, Defn A2.1 is coextensive with that of a monoid endowed with property A.2.1.d.). A monoid is a category in which all of its left and right identities coincide to one unique element. For example, the integers  $\mathbb{Z}$  form a monoid under integer multiplication (since,  $\forall n \in \mathbb{Z} \exists! 1 \in \mathbb{Z}$  such that  $n \cdot 1 = n = 1 \cdot n$ ), but not a group, since their multiplicative inverse can violate closure. Whereas, the non-zero rational numbers  $\mathbb{Q}^* = \{n/m \mid n \neq 0, m \neq 0\}$  form an Abelian (i.e. commutative) group under multiplication.

**Defn A2.2: (Subgroups, Normal Subgroups, Simple Groups)**

- i.) Let  $\langle G, \circ \rangle$  be a group. Then, for any  $H \subseteq G$ ,  $H$  is a *subgroup* of  $G$  (denoted:  $H \leq G$ ) if for any  $x, y \in H$ , then  $x \circ y \in H$ . In other words,  $H$  is closed under  $\circ$ ,  $e \in H$ , and if  $x \in H$  then  $x^{-1} \in H$ . If  $H \leq G$ , and  $H \subset G$ , then  $H$  is a *proper subgroup*, denoted:  $H \subsetneq G$ . Moreover, if denoted:  $\emptyset \subset H$ , then  $H$  is *non-trivial*.
- ii.)  $H$  is a *normal* (or invariant) subgroup of  $G$  (denoted:  $H \triangleleft G$ ) if its left and right cosets agree, for any  $g \in G$ . That is to say,  $H \triangleleft G$  iff  $\forall g \in G$ :  
 $gH = \{gh \mid h \in H\} = Hg = \{kg \mid k \in H\}$ .
- iii.)  $G$  is *simple* if  $G$  contains no proper, non-trivial, normal subgroups.

**Defn A2.3: (Vector Space)** A *vector space* is to a structure  $\langle V, F, *, \cdot \rangle$  endowed with a (commutative) operation (i.e.  $\forall (x,y) \in V : x * y = y * x$ , denoted, by convention, by the “+” symbol, though not necessarily to be understood as addition on the real numbers) such that:

- i)  $\langle V, * \rangle$  is a commutative (or Abelian) group.
- ii) Given a field<sup>193</sup> of scalars  $F$  the *scalar multiplication* mapping into  $V \cdot : F \times V \rightarrow V$  obeys distributivity (in the following two senses):
- iii)  $\forall (\alpha, \beta) \in F \forall \phi \in V : (\alpha + \beta) \cdot \phi = (\alpha \cdot \phi) + (\beta \cdot \phi)$
- iv)  $\forall (\phi, \psi) \in V \forall \gamma \in F : \gamma \cdot (\phi + \psi) = (\gamma \cdot \phi) + (\gamma \cdot \psi)$ .

**Defn A2.4: (Algebra)** An *algebra*  $A$ , then, is defined as a *vector space*  $\langle V, F, *, \cdot, \bullet \rangle$  endowed with an associative binary mapping  $\bullet$  into  $A$  (i.e.,  $\bullet : A \times A \rightarrow A$ , such that  $\forall (\psi, \phi, \phi) \in G$ :  $(\psi \bullet \phi) \bullet \phi = \psi \bullet (\phi \bullet \phi) \equiv \psi \bullet \phi \bullet \phi$  denoted, by convention, by the “ $\times$ ” symbol, though not necessarily to be understood as ordinary multiplication on the real numbers) This can be re-stated by saying that  $\langle A, \bullet \rangle$  forms a *semigroup* (i.e. a set  $A$  closed under the binary associative product  $\bullet$ ), while  $\langle A, * \rangle$  forms an Abelian group.

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<sup>193</sup> I.e. a an algebraic structure  $\langle F, +, \times \rangle$  endowed with two binary operations such that  $\langle F, + \rangle$  and  $\langle F, \times \rangle$  form commutative groups and  $+, \times$  are connected by left (and right, because of commutativity) distributivity, i.e.,  $\forall (\alpha, \beta, \gamma) \in F : \alpha \times (\beta + \gamma) = (\alpha \times \beta) + (\alpha \times \gamma)$ .

Examples of algebras include the class of *Lie* algebras, i.e. an *algebra*  $dA$  whose ‘product’  $\bullet$  is defined by an (associative) Lie product (denoted  $[ , ]$ ) obeying the Jacobi Identity:  $\forall(\zeta, \xi, \zeta) \in dA : [[\zeta, \xi], \zeta] + [[\xi, \zeta], \zeta] + [[\zeta, \zeta], \xi] = 0$ . The structure of classes of infinitesimal generators in many applications often form a Lie algebra. Lie algebras, in addition, are often characterized by the behavior of their *structure constants*  $C$ . For any elements of a Lie algebra  $\zeta_\mu, \xi_\nu$  characterized by their covariant (or contravariant –if placed above) indices  $(\mu, \nu)$ , then a *structure constant* is the indicial function  $C(\lambda)^\sigma_{\mu\nu}$  such that, for any  $\zeta_\rho \in dA : [\zeta_\mu, \xi_\nu] = \sum_{\sigma=1}^N C^\sigma_{\mu\nu}(\lambda) \zeta_\sigma$ , where  $N$  is the dimension of  $dA$ , and  $\lambda$  is the Lie Algebra’s *contraction parameter*. A Lie algebra is *stable* whenever:

$\lim_{\lambda \rightarrow \infty, \nu \lambda \rightarrow 0} C(\lambda)^\sigma_{\mu\nu}$  is well-defined for any structure constant  $C(\lambda)^\sigma_{\mu\nu}$  and contraction parameter  $\lambda$ .

**Defn A2.5: (Clifford Algebra)** . A *Clifford Algebra* is a *graded* algebra endowed with the (non-commutative) Clifford product. That is to say:

- i.) For any two elements  $A, B$  in a Clifford algebra  $CL$ , their Clifford product is defined by:  $AB = A \bullet B + A \wedge B$ , where  $A \bullet B$  is their (commutative and associative) *inner* product, and  $A \wedge B$  is their anti-commutative, i.e.  $A \wedge B = -B \wedge A$ , and associative *exterior* (or Grassmann) product. This naturally makes the Clifford product associative:  $A(BC) = (AB)C \equiv ABC$ . Less obviously, however, for reasons that will be discussed below, is how the *existence of an inverse*  $A^{-1}$  for every (nonzero) Clifford element  $A$  arises from the Clifford product, i.e.:  $A^{-1}A = I = AA^{-1}$ , where  $I$  is the *unit pseudoscalar* of  $CL$ .
- ii.)  $CL$  is equipped with an adjoint  $\hat{\phantom{A}}$  and grade operator  $\langle \phantom{A} \rangle_r$  (where  $\langle \phantom{A} \rangle_r$  is defined as isolating the  $r$ th grade of a Clifford element  $A$ ) such that, for any Clifford elements  $A, B$ :  $\langle AB \rangle_r^\hat{\phantom{A}} = (-1)^{C(r,2)} \langle B^\hat{\phantom{A}} A^\hat{\phantom{A}} \rangle_r$  (where:  $C(r,2) = \frac{r!}{(2!(r-2)!)} = \frac{r(r-1)}{2}$ .)

Hence a general Clifford element (or multivector)  $A$  of Clifford algebra  $CL$  of maximal grade  $N = \dim V$  (i.e the dimension of the underlying vector space structure of the Clifford algebra) is expressed by the linear combination:

$$A = \alpha^{(0)}A_0 + \alpha^{(1)}A_1 + \alpha^{(2)}A_2 + \dots + \alpha^{(N)}A_N \quad (\text{A.3.1})$$

where:  $\{ \alpha^{(k)} \mid 1 \leq k \leq N \}$  are the elements of the scalar field (expansion coefficients) while  $\{ A_k \mid 1 \leq k \leq N \}$  are the *pure* Clifford elements, i.e.  $\langle A_k \rangle_l = A_k$  whenever  $k = l$ , and  $\langle A_k \rangle_l = 0$  otherwise, while for a general multivector (A.3.1),  $\langle A \rangle_l = \alpha^{(l)}A_l$ , for  $1 \leq l \leq N$

Hence, the pure Clifford elements live in their associated closed Clifford subspaces  $CL_{(k)}$  of grade  $k$ , i.e.  $CL = CL_{(0)} \oplus CL_{(1)} \oplus \dots \oplus CL_{(N)}$ .

Consider the following example: Let  $V = \mathbf{R}^3$ , i.e. the underlying vector space for  $CL$  is a 3 dimensional Euclidean space  $\mathbf{R}^3 = \{ \vec{r} = (x,y,z) \mid x \in \mathbf{R}, y \in \mathbf{R}, z \in \mathbf{R} \}$ . Then the maximum grade for Clifford Algebra over  $\mathbf{R}^3$ , i.e.  $CL(\mathbf{R}^3)$  is  $N = \dim \mathbf{R}^3 = 3$ . Hence:

$CL(\mathbf{R}^3) = CL_{(0)} \oplus CL_{(1)} \oplus CL_{(2)} \oplus CL_{(3)}$  where:  $CL_{(0)}$  (the Clifford subspace of grade 0) is (algebraically) isomorphic to the real numbers  $\mathbf{R}$ .<sup>194</sup>  $CL_{(1)}$  (the Clifford subspace of grade 1) is algebraically isomorphic to the Complex numbers  $\mathbf{C}$ .  $CL_{(2)}$  (the Clifford subspace of grade 2) is algebraically isomorphic the Quaternions  $\mathbf{H}$ .  $CL_{(3)}$  (the Clifford subspace of grade 3) is algebraically isomorphic to the Octonions  $\mathbf{O}$ .

To understand *why* the Clifford algebra over  $\mathbf{R}^3$  would invariably involve closed subspaces with elements related to the unit imaginary  $i = \sqrt{-1}$  (and some of its derivative notions thereon, in the case of the Quaternions and Octonions) entails a closer study of the nature of the Clifford product.

Defn. A.2.4 i) deliberately leaves the Grassman product under-specified. I now fill in the details here.

First, it is important to note that  $\wedge$  is a *grade-raising* operation: for any pure Clifford element  $A_k$  (where  $k < N = \dim V$ ) and  $B_1$ , then  $\langle A_k B_1 \rangle = k + 1$ . It is for this reason that pure Clifford elements of grade  $k$  are often called *multivectors*. Conversely, the inner product  $\bullet$  is a *grade-lowering* operation: for any pure Clifford element  $A_k$  (where  $k < N = \dim V$ ) and  $B_1$ , then  $\langle A_k \bullet B_1 \rangle = k - 1$ . (Hence the inner product is often referred to as a *contraction*).

The reason for the grade-raising, anti-commutative nature of the Grassman product is historically attributed to Grassman's geometric notions of (directed) line segments, (rays) areas, volumes, hypervolumes, etc. For example, in the case of two vectors  $\vec{A}, \vec{B}$ , their associated directed area segments  $\vec{A} \wedge \vec{B}, \vec{B} \wedge \vec{A}$  are illustrated below:



**Fig. A.2.1: Directed Areas**

The notion of directed area, volume, hypervolume segments indeed survives, to a certain limited sense, in the vector-algebraic notion of ‘cross-product.’ For example, the magnitude of the cross-product  $\vec{A} \times \vec{B}$  is precisely the area of the parallelogram spanned by  $\vec{A}, \vec{B}$  as depicted in Fig. A.2.1.

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<sup>194</sup> Since the real numbers are a *field*, they're obviously describable as an algebra, in which their underlying ‘vector space’ structure is identical to their field of scalars. In other words, scalar multiplication is the same as the ‘vector’ product  $\bullet$ .

The difference, however, lies in the fixity of grade in the case of  $\vec{A} \times \vec{B}$ , in the sense that the anti-commutativity is geometrically attributed to the directionality of the *vector*  $\vec{A} \times \vec{B}$  (of positive sign in the case of right-handed coordinate system) perpendicular to the plane spanned by  $\vec{A}, \vec{B}$ . This limits the notion of the vector cross-product, as it can only be defined for spaces of maximum dimensionality 3.<sup>195</sup> On the other hand, the Grassmann product of multivectors interpreted as directed areas, volumes, and hypervolumes is unrestricted by the dimensionality of the vector space.

The connection with the algebraic behavior of  $i = \sqrt{-1}$  lies in the inherently anti-commutative aspect (i.e. the Grassmann component) of the Clifford product, as discussed above. To see this, consider the even simpler case of  $V = \mathbf{R}^2$  (as discussed, for example, in Lasenby, et. al. (2000), 26-29). Then;  $N = \dim \mathbf{R}^2 = 2$ . Moreover,  $\mathbf{R}^2 = \langle (\hat{e}_1, \hat{e}_2) \rangle$ , where  $\langle \dots \rangle$  denotes the *span* and  $(\hat{e}_1, \hat{e}_2)$  are the ordered pair of orthonormal vectors (parallel, for example, to the  $x$  and  $y$  axes.) Hence:  $\hat{e}_1^2 = \hat{e}_2^2 = 1$ , and  $\hat{e}_1 \cdot \hat{e}_2 = \hat{e}_2 \cdot \hat{e}_1 = 0$ . So:  $\hat{e}_1 \hat{e}_2 = \hat{e}_2 \cdot \hat{e}_1 + \hat{e}_1 \wedge \hat{e}_2 = \hat{e}_1 \wedge \hat{e}_2 = -\hat{e}_2 \wedge \hat{e}_1 = -\hat{e}_2 \hat{e}_1$ . Hence:  $(\hat{e}_1 \hat{e}_2)^2 = (\hat{e}_1 \hat{e}_2)(\hat{e}_1 \hat{e}_2) = \hat{e}_1(\hat{e}_2 \hat{e}_1)\hat{e}_2 = -\hat{e}_1(\hat{e}_1 \hat{e}_2)\hat{e}_2 = -(\hat{e}_1 \hat{e}_1)(\hat{e}_2 \hat{e}_2) = -(\hat{e}_1^2)(\hat{e}_2^2) = -1$  (using the anti-commutativity and associativity of the Clifford product.) Hence, the multivector  $\hat{e}_1 \hat{e}_2$  is algebraically isomorphic to  $i = \sqrt{-1}$ . Moreover,  $(\hat{e}_1 \hat{e}_2)\hat{e}_1 = -\hat{e}_2$  and  $(\hat{e}_1 \hat{e}_2)\hat{e}_2 = \hat{e}_1$ , by the same simple algebraic maneuvering. Geometrically, then, the multivector  $\hat{e}_1 \hat{e}_2$  when multiplying on the left has the effect of a *clockwise*  $\pi/2$ -rotation. Represented then in the matrix algebra  $M_2(\mathbf{R})$  (the algebra of real-valued 2x2 matrices):

$$\hat{e}_1 \hat{e}_2 \equiv \begin{pmatrix} 0 & 1 \\ -1 & 0 \end{pmatrix}, \quad \text{where: } \hat{e}_1 \equiv \begin{pmatrix} 1 \\ 0 \end{pmatrix}, \hat{e}_2 \equiv \begin{pmatrix} 0 \\ 1 \end{pmatrix}$$

Moreover, for  $CL(\mathbf{R}^2)$  the multivector  $\hat{e}_1 \hat{e}_2$  is the *unit pseudoscalar*, i.e. the element of maximal grade. In general, for any Clifford Algebra  $CL(V)$ , where  $\dim V = N$ , and  $V = \langle (\gamma_1, \gamma_2, \dots, \gamma_N) \rangle$ , where the basis elements aren't necessarily orthonormal, the unit pseudoscalar  $I$  of  $CL(V)$  is:  $I = \gamma_1 \gamma_2 \dots \gamma_N$ . In general, for grade  $k$  (where  $1 \leq k \leq N$ ) the closed subspaces  $CL_{(k)}$  of grade  $k$  in  $CL(V) = CL_{(0)} \oplus CL_{(1)} \oplus \dots \oplus CL_{(N)}$  have dimensionality  $C(N, k) = \binom{N}{[k(N - k)]}$ , i.e are spanned by  $C(N, k) = \binom{N}{[k(N - k)]}$  multivectors of degree  $k$ . Hence the total number of Clifford basis elements generated by the Clifford product acting on the basis elements of the underlying vector space is:  $2^N = \sum_{k=0}^N C(N, k)$ . The unit pseudoscalar is therefore the (one) multivector (only one there are  $C(N, N) = 1$  of them, modulo sign or order of multiplication) spanning the closed Clifford subspace of maximal grade  $N$ .

For example, in the case of  $CL(\mathbf{R}^3) = CL_{(0)} \oplus CL_{(1)} \oplus CL_{(2)} \oplus CL_{(3)}$ , where:

<sup>195</sup> “[T]he *vector algebra* of Gibbs...was effectively the end of the search for a unifying mathematical language and the beginning of a proliferation of novel algebraic systems, created as and when they were needed; for example, spinor algebra, matrix and tensor algebra, differential forms, etc.” (Lanseny, et. al. (2000), 21)

$$\mathbf{R}^3 = \langle \langle \hat{e}_1, \hat{e}_2, \hat{e}_3 \rangle \rangle : CL_{(0)} = \langle 1 \rangle \cong R, CL_{(1)} = \langle \langle e_1, e_2, e_3 \rangle \rangle, CL_{(2)} = \langle \langle e_{12}, e_{13}, e_{23} \rangle \rangle, CL_{(3)} = \langle I \rangle = \langle e_{123} \rangle$$

(where the abbreviation  $e_{i\dots k} = \hat{e}_i \dots \hat{e}_k$  is adopted). As demonstrated in the case of  $CL(\mathbf{R}^2)$  the multivector, the unit pseudoscalar  $I$  should *not* be interpreted as a multiplicative identity, i.e. it is certainly *not* the case that for any  $A \in CL(V)$ ,  $AI = A = IA$ . Rather, the unit pseudoscalar is adopted to define an element of dual grade  $A^*$  : for any pure Clifford element  $A_k$  (where  $0 \leq k < N$ ) : the grade of  $AI$  (or  $A^*$ ) is  $N - k$ , and vice versa. Thus an inverse element  $A^{-1}$  can in principle be constructed, for every nonzero  $A \in CL(V)$ . So the linear equation  $AX = B$  has the formal solution  $X = A^{-1}B$  in  $CL(V)$ . “Much of the power of geometric (Clifford) algebra lies in this property of invertibility.” (Lasenby, et. al. (2000), 25)

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