ABSTRACT

Title of Document:	CLIFFORD ALGEBRA: A CASE FOR GEOMETRIC AND ONTOLOGICAL UNIFICATION
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Robert Batterman's ontological insights (2002, 2004, 2005) are apt: Nature abhors singularities. "So should we," responds the physicist. However, the *epistemic* assessments of Batterman concerning the matter prove to be less clear, for in the same vein he write that singularities play an essential role in certain classes of physical theories referring to certain types of critical phenomena. I devise a procedure ("methodological fundamentalism") which exhibits how singularities, at least in principle, may be avoided *within the same classes of formalisms* discussed by Batterman. I show that we need not accept some divergence between explanation and reduction (Batterman 2002), or between epistemological and ontological fundamentalism (Batterman 2004, 2005).

Though I remain sympathetic to the 'principle of charity' (Frisch (2005)), which appears to favor a pluralist outlook, I nevertheless call into question some of the forms such pluralist implications take in Robert Batterman's conclusions. It is

difficult to reconcile some of the pluralist assessments that he and some of his contemporaries advocate with what appears to be a countervailing trend in a burgeoning research tradition known as Clifford (or geometric) algebra.

In my critical chapters (2 and 3) I use some of the demonstrated formal unity of Clifford algebra to argue that Batterman (2002) equivocates a physical theory's ontology with its purely mathematical content. Carefully distinguishing the two, and employing Clifford algebraic methods reveals a symmetry between reduction and explanation that Batterman overlooks. I refine this point by indicating that geometric algebraic methods are an active area of research in computational fluid dynamics, and applied in modeling the behavior of droplet-formation appear to instantiate a "methodologically fundamental" approach.

I argue in my introductory and concluding chapters that the model of inter-theoretic reduction and explanation offered by Fritz Rohrlich (1988, 1994) provides the best framework for accommodating the burgeoning pluralism in philosophical studies of physics, with the presumed claims of formal unification demonstrated by physicists choices of mathematical formalisms such as Clifford algebra. I show how Batterman's insights can be reconstructed in Rohrlich's framework, preserving Batterman's important philosophical work, minus what I consider are his incorrect conclusions.

CLIFFORD ALGEBRA: A CASE FOR GEOMETRIC AND ONTOLOGICAL UNIFICATION

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Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park, in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2008

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Foreword

<u>*Statement of the Problem and Overall Summary of my Method toward solving it*</u>

Frederik Suppe (1977) details the demise of logical empiricism in the philosophy of science, (what he describes as the then-'Received View') with its associated realist and anti-realist presumptions concerning the nature of observable versus theoretical terms, and its assumptions concerning the nature of unity in the sciences, when suitably rationally reconstructed, etc. As I discuss here in §4 below (*Foreword*) this demise of a generation ago set the stage for an evolving trend in the philosophy of physics and in philosophy of science achieving its culmination in what I term 'post-Standard' accounts, based on my reading of Frisch (2005) and others (Batterman (2002-2005), Cartwright (1999), Morrison (2000*a,b*), etc.). In historical terms, such a trend could be viewed 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 in turn devote serious study to. Where the logical empiricist would rationally reconstruct a physical theory according to some logical regiment, the post-Standardist would focus on the *actual* processes the physicist implements in the development and refinement of models, laws, and mathematical formalisms. In short, where the logical empiricist saw a statically unified picture of the structure of physical theories, the post-Standard philosopher instead sees a dynamically evolving plurality.

I need not discuss herein all the obvious benefits of this legacy, much less the great amount the post-Standardist continuously offers in the way of tools for philosophical analysis of particular domains of research in the physical sciences. Yet concerns remain that I raise herein, and which others have raised elsewhere concerning their respectively critical targets (and this essay will show there is certainly no lack thereof). Basically, the concern can be narrowed down to the following questions: What is the *actual extent* of the pluralism as evinced by (or presupposed by) most contemporary post Standard accounts? Does such pluralism cut across methodological, ontological, and epistemological domains or aspects of their domains of study? If so, then, to what extent? Perhaps more pessimistically phrased: Would an excessively pluralist outlook raise the specter post-Standardist evasion of responsibility of treating philosophical analysis as a *normative* enterprise, and has opened the gates to rendering philosophy of physics (and science proper) as indistinguishable from other fundamentally descriptive enterprises of sciences, as characterized by the acronym SST (science and technology studies)?

Raising such concerns does *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, which may lead us down the slippery slope toward rendering philosophy of physics (and science) indistinguishable from SST, *or* we become reactionary and adopt some "neo" logical empiricist position. A healthy middle ground, or 'third way' is of course possible.

The 'third way' I advocate here is what I consider a fairly run-of-the mill constructive 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, my critical responses 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 a veritable 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 inevitable nature or irreducible 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 which will not hearken back to the bygone days of logical empiricism, with its associated 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).

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 the former produce some kind of a slide towards rendering philosophy of physics innocuously 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 below, 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 attitude 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. But the realist certainly does not corner the market regarding the use of such epistemic strategies and notions. Aside from that, 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. I will point out in passing, however, that just as anti-realists like van Fraasen (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 the 'truth' and reference of theoretical terms in a particular theory (while Boyd (1985), Musgrave (1985, 1989) have responded to the contrary).

If, however, I were forced to lay my metaphysical cards on the table concerning what my presumptions are concerning the question of realism, I will mention in passing here 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 Teller's (2005) and 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," 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. Worral (1989, 1994)). 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.

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 systematicity. This would include, for that matter, theories with mathematical formalisms harboring a greater degree of regularizability (as exhibited 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[1] 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,

[1] 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.

> 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 here to simply motivate my discussion toward very particular issues concerning inter-theoretic reduction and explanation vis-a-vis certain case-studies in applied and theoretical physics discussed by Robert Batterman (2002, 2004, 2005) whom I critically respond to. 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, hence my more descriptive account of 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 their generally robust *regularizability*—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.*[2]

<u>*Section 2: What Makes the Study of Such a Reformulation of Interest in Contemporary Philosophy of Physics*</u>

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 explicit 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

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[2] 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).

pluralism prevalent in the philosophy of physics and other special sciences, as well as philosophy of science in general.[3]

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.[4] 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 conjecture, by intuitive or inductive considerations, and then various intermediate supports are found and

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[3] 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), Hoefer (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).

[4] 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.

> 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 in effect proposes is a methodological rapprochement with aspects in the philosophy of science and mathematics without, however, embarking on some project of 'naturalizing' 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:[5]

> [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[6] informs the philosophy of science in a novel way concerning the choice of characterizing a physical theory with a mathematical formalism. Concerning the issue of classical versus constructive mathematics, the $P \neq NP$ conjecture[7] entails that:

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[5] 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.

[6] 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 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 φ: $\neg\neg\varphi \vdash \varphi$.) 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).

[7] 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 $N \neq NP$ conjecture is found in most textbooks of advanced discrete *classical* mathematics—hence like the continuum hypothesis such a conjecture renders

> 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.[8]

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:

> 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)

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the issue of choice of mathematical formulation in a fundamental and concretely *practical* sense peculiarly open.

[8] 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 theme of historical contingency that Burgess evinces 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).[9] Of course, it bears

[9] This 'Cartesian' intuition is shared by many contemporary esteemed mathematical physicists. See, for instance, Sir Micheal Atiyah (2001).

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 17th 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 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[10]: $W_1 : W_2 = L_2 : L_1$ was not transformed into a product $W_1 L_1 = W_2 L_2$ "because a product [WL] did not exist in [Greek] mathematics.[11]" (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.[12] *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

[10] That the ratio of the weights on a lever in equilibrium is the inverse of the ratio of the lengths of their fulcra.

[11] 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)

[12] Otherwise known as the *torque* induced by the weight force.

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[13] in the years 1878-1882 as a means to systematically develop a matrix algebra representing rotations and spin, generalized to any *n*-dimensional space: $R^n = \{(x_1,...,x_n) \mid x_k \in R, 1 \le k \le n\}$ (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 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

[13] A vector space endowed with an associative product. For further details, see Appendix below.

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 $0 \le k \le n$), 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 $0 \le k \le 3$): 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 $0 \le k \le n$), 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,[14] 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, 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[15], or apply the formalism in key instances to make some interpretative point in the foundations of quantum theory, no matter how specific[16] or general.[17]

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

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[14] 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.

[15] E.g., Finkelstein, Hestenes, Lasenby

[16] E.g., Conte, Hogreve, Snygg

[17] E.g., Hiley, Khrenikov, Vlasov

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[18]), 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.[19]

Models, however, can also be understood as structures bearing (representational) relations to *sets of phenomena*[20] characterizing "post-standard accounts."[21] 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 assumptions from…sometimes incompatible theories." (Frisch (2005)10) In this respect, high-level

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[18] 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.)

[19] In the early syntactic traditions, it was thought that such sentences could in principle be regimented in FOPL (first order predicate logic).

[20] 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)

[21] Cartwright (1982, 1999), Giere (1988), etc.

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.[22] 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.[23]

[22] As I mention in §1.4. below, the distinction is misleading in ways confirming Frisch's assessment in the case study of classical electrodynamics.

[23] 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. 43, 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).

One of Mathias Frisch's central claims is that, in the case of the theory of classical electrodynamics,[24] *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 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

[24] The prototypical field theory.

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 (2000*a*) 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.[25]

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

[25] "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)

fluid from this accurate level of description, aside 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.[26] 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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[26] 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 Hoefer's response in n. 2 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.

Acknowledgements

There are many I wish to thank for their influence, inspiration, support, and consultation on this project. Some of the central ideas herein have their origins in my days as a graduate student in the School of Physics at the Georgia Institute of Technology, nearly a decade ago. In that regard, I wish to thank my mentor and advisor from the Georgia Institute of Technology, David Finkelstein, whose ideas and ongoing research continues to influence and inspire me.

Since I joined the Committee for Philosophy and the Sciences (CPaS) at the University of Maryland to pursue graduate studies, there are many I wish to thank. First and foremost my advisor, Jeffrey Bub, for all the avenues he has shown me in his research into the foundations of physics, as well as for all his efforts to inculcate upon me effective writing and conceptualizing in the philosophy of physics. I would also like to warmly thank Michael Silberstein, for the many productive discussions we have had and continue to have—without his input, this dissertation topic would have never germinated. I also wish to thank Allen Stairs and Mathias Frisch, for the many illuminating discussions and assistance they have provided (both formal and informal), in their dedicated efforts to open my eyes more to the broader vistas of contemporary philosophy of science and physics.

I would also very much like to thank Timothy Eastman and Rajarshi Roy, both of whom I have worked with in the past at the Georgia Institute of Technology, and am delighted to work with here again at the University of Maryland. To Timothy, for his tireless dedication and assistance and avenues he has presented me in research above and beyond the scope of this dissertation project, as well as for all his assistance and consultation he has provided me in my writing, and for our many engaging and fruitful discussions—may there be many more! To Rajarshi, who continues to take an active interest and lend concrete assistance in my research, since my days as a graduate student at the Georgia Institute of Technology.

I would also like to thank Harjeet Parmar, friend and fellow graduate student, for all the many late night discussions we have had in Skinner Hall, and for kindly assisting in reading and commenting on this draft.

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