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Asymptotics, reduction and emergence

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Abstract

All the major inter-theoretic relations of fundamental science are asymptotic ones, e.g. quantum theory as Planck's constant h → 0 yielding (roughly) Newtonian mechanic. Thus asymptotics ultimately grounds claims about inter-theoretic explanation, reduction and emergence. This paper examines four recent, central claims by Batterman concerning asymptotics and reduction. While these claims are criticised, the discussion is used to develop an enriched dynamically based account of reduction and emergence, show its capacity to illuminate the complex variety of inter-theory relationships in physics, and provide a principled resolution to such persistent philosophical problems as multiple realisability and the nature of the special sciences.

1 *Introduction*

Asymptotics is the study of what happens in some mathematical domain as some parameter tends to zero, e.g. quantum theory as Planck's constant h → 0, yielding (roughly) Newtonian mechanics. Sometimes there are behaviours unique to the asymptotic domain near the zero limit, e.g. so-called quantum chaos, and sometimes the behaviour in the limit at zero is suddenly different, e.g. at h = 0 quantum theory becomes (roughly) Newtonian. These kinds of behaviours are grounded in singular asymptotics, marked by discontinuity and non-analytic infinities. By contrast under $1/c \rightarrow 0$ special relativity is said to exhibit regular asymptotics because it passes continuously and analytically into Newtonian mechanics in the limit.

Although it is not usual for philosophers to discuss asymptotics, once it is realised that all the major inter-theoretic relations of fundamental science are asymptotic ones, and that reduction and emergence are typically posed in terms of inter-theoretic explanatory relations, the connection becomes clearer: asymptotics provides the ground on which claims about inter-theoretic explanation, reduction and emergence must ultimately rest. But rest how? Are the concepts, criteria and evidence to be primarily formal, in terms of logical relations among theoretical propositions, or are they to be primarily dynamical, in terms of kinematical structure, constraint formation and perturbation dissipation? While the former is the standard approach of analytic philosophy, in this paper by contrast I develop a dynamically based account of reduction and emergence and show its capacity to illuminate the complex variety of inter-model relationships in physics and how it leads to a natural resolution of the multiple realisability problem.

Commencing from an earlier essay on employing dynamical criteria in the theory of reduction (Hooker [1979], [1981] Parts I-III), the present account improves upon and extends that position, stimulated by, and employing as foil, important new work by Batterman on the role of asymptotics, summarised in his new book (Batterman [2002]). Batterman reviews asymptotics and discusses leading cases in detail, deriving many interesting mathematical and scientific consequences. This is in the service of formulating and arguing four primary and closely interrelated philosophical claims about asymptotics: (1) asymptotic and causal explanation are quite distinct, (2) asymptotic analysis requires irreducible reference to the entities of the limit domain as well to those of the non-asymptotic domain, (3) failure of reduction (to the non-asymptotic theory) coincides with the occurrence of singular asymptotics, and (4) singular asymptotic patterns constitute a new basis for emergence and provide new forms of emergence. These claims are discussed generally in the introduction, chapter 1, and chapter 2 on asymptotic reasoning, and thereafter throughout the presentation of various case studies (see e.g. pp.96, 120 for claims (1) and (2)), but roughly in the order listed: chapters 3 and 4 focus on explanation, 5 on reduction and multiple realisability, and 8 on emergence, with chapters 6 and 7 providing more detailed illustrative case studies of wave/ray optics and classical/quantum mechanics respectively.

In what follows these claims are critically assessed and rejected as they stand. While Batterman too champions dynamically based approaches in key places, I try to show how he remains too bound by formal considerations. More constructively, the process of critical assessment provides the framework for further developing my own contrasting dynamically-based account and exhibiting its merits, including the incorporation of Batterman's dynamical insights. In what follows all unattributed page references are to Batterman's book.

Because of the arcane, but important, nature of the subject matter I next enter into some brief, simple exposition, before moving to critical discussion.[1] However the exposition

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[1] Batterman's own exposition is largely clear and careful. However I found a few of Batterman's diagrams too obscure to follow easily. Fig. 4.6, p.53, e.g., is there to illuminate a fluid flow example but contains no explanation or definition of the $t_{i,j}$ symbols (there, or in the text), the structuring of the dynamics around a 'stationary point' r_0 (that actually changes) is left obscure, as are the choices of liquid incidence angles. Similarly, Fig. 4.3, p.41, the crucial piece in making the renormalisation process intelligible, is left with virtually no interpretation and is quite opaque. A little more attention to (inexpert) reader information would have corrected these flaws. Similarly for a few poorly worded sentences: at p.19, l.21 the reference of "former" is obscure (it refers to limiting cases which are not singular); at

will also provide an essential framework of concepts and principles used in the later discussion, and introduce some further worthwhile issues that cannot be pursued in detail here.

2 *Exposition*

An asymptote in mathematics is a limiting value of a function that is approached indefinitely closely but never quite reached, except by carrying some approximation process literally to infinity. Consider, for instance, the function y =1/x, x a real number; 1/x is always finite for x > 0, its limit as x becomes indefinitely large (x → ∞, 'infinity') is zero, $\text{Lim}_{x \to \infty}$ (1/x) = 0, and $\text{Lim}_{x \to 0}$ (1/x) = ∞. So there are two asymptotes for the hyperbolic curve y = 1/x: y = 0 (the x axis) and x = 0 (y axis). An asymptotic domain refers to the region 'near' an asymptote and asymptotic theory is the (mathematical) theory of what happens in the asymptotic domain and 'in the limit' at the asymptote. In the case of 1/x, e.g., the asymptotic domains are the domains of very small and very large numbers and asymptotic theory would be the theory of the y-curve properties for these numbers. For y = 1/x, no new arithmetic phenomena emerge within the asymptotic domains, but there is a discontinuity at each of its asymptotes since there the laws of arithmetic break down.

In physics we find that the most famous theory pairs are all asymptotically related, e.g. (and roughly) $\text{Lim}_{1/c \to 0}$ (special relativity) → Newtonian mechanics, $\text{Lim}_{\lambda \to 0}$ (wave optics) → ray optics, $\text{Lim}_{h \to 0}$ (quantum mechanics) → Newtonian mechanics. In the last two cases the asymptotic domains are filled with interesting phenomena, like rainbows in wave optics.

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p.23, l.6 from bottom the sense of "instance" in "explain an instance of universality" is ambiguous as between the detailed motion of a system, which would contradict the remainder of what is said, and a case of a universal law (evidently intended). Finally, equation 7.6, p.103, strictly needs $K^{1/2}$ (not K) to square it with substituting 7.4 into 7.3, and at p.107, l.9 insert "space" after "phase".

(However, as Batterman notes at p.78, exact retrieval of a predecessor theory in the asymptotic limit rarely happens, there are typically formulae on both sides that don't 'match up' asymptotically; one of the best fits is the Special Relativity-Newtonian mechanics pair, one of the most uneven fits is the statistical mechanics-thermodynamics pair, cf. Hooker [1981], Part I.) More abstractly, consider a "more fundamental theory" T_f and its coarser predecessor T_c, with $\mathrm{Lim}_{\mathbf{p} \to 0} (T_f) = T_c$, for appropriate **p** (1/c, λ, h, etc.). There are three kinds of properties and relations, found in three separate domains, connected with this situation. In progression as $\mathbf{p} \to 0$, these are: the properties and relations of T_f in the non-asymptotic domain (**p** not small), those of the T_f asymptotic domain (**p** small), and those of T_c, the limit domain (**p** zero).

For the wave/ray optics example ($\mathbf{p} = \lambda$), the non-asymptotic domain is characterised by wave phenomena like diffraction and interference through superposition, the limit domain is characterised solely by linear rays, and the asymptotic domain by tiny wave patterns concentrated along curves constructed as the tangents to rays, called caustics. Large wavelengths swamp the linear ray relationships, but as the wavelength becomes small these ray relationships emerge from the interference patterns among waves, decorated by small wave fringes, until at zero wavelength there is nothing left but the rays and their caustics. The caustics are singularities in the wave pattern because, strictly, along them the wave intensity becomes infinite in the limit. They are "diffraction catastrophes" (a mathematical term) and "constitute a hierarchy of nonanalyticities" (Berry, quoted at p.116). This singular asymptotics lies in contrast to the regular asymptotics of special relativity (cf. p.79) where, setting $\mathbf{p} = 1/c$ we observe that the non-Newtonian factor in relativity theory, $1 + v^2(1/c)^2$, $\to 1$ smoothly as $1/c \to 0$ (finite v), yielding the Newtonian condition, yet relativistic phenomena are strongly non-Newtonian. Note that even for regular asymptotics the properties and relations of the limit (T_c) domain may still be markedly different from those of the fundamental theory (T_f) domain, as in this case.

Asymptotic phenomena, whether in the asymptotic domain or the limit domain, have an interesting and important property: universality. All classic pendulums (light, inelastic arms with heavier 'bobs' on the end, swinging frictionlessly) swinging through only an asymptotically small angle θ ($\theta \to 0$) perform simple harmonic oscillations, their oscillation frequency (f) determined solely by their arm length (l) and the common force of gravity (g, per unit mass), $f = (g/l)^{1/2}$. This result applies no matter what they are made of, where they are swinging, and so on. This is why Batterman calls it a universal law and its realizations a universal phenomenon. Note that this example shows that not all asymptotic phenomena are concerned with important theory pair interrelations, though there will always be a relevant pair of asymptotically related theoretical models, in this case a non-linear pendula model applicable to all swings and the small-swing linear simple harmonic model, both within Newtonian mechanics.

The link with asymptotics is this: as $\mathbf{p} \to 0$ terms involving **p** grow small and may be neglected, vanishing in the limit, and in this process information about this parameter is systematically deleted or 'thrown away'. For instance, the relativistic information encapsulated in $v^2(1/c)^2$ is deleted in the limit. Functions that differ only in such information will now coincide. Thus all **p**-differentiated laws will coincide yielding a law universally applicable across the erstwhile **p**-differentiated spectrum. For instance, all detailed non-linear dynamical state-sequence formulae for small-angle pendula movements will converge to the simple harmonic law, and the relativistic laws will converge to the Newtonian laws. But note, what Batterman does not, that not all demonstrations of irrelevance of information require asymptotics; e.g. the conclusion that energy is conserved, so all states lie on an iso-energetic sphere, immediately entails the irrelevance of all information to specifying trajectories but total energy and equality of initial momenta.

As illustrated in the $v^2(1/c)^2$ parameter of relativity, all asymptotic processes generate dimensionless constants, viz. those reflecting the discarded aspects. In the case of the small-swing pendula, e.g., the key dimensionless constant is $f(l/g)^{1/2}$. As this example shows, however, these constants need not refer to single basic physical dimensions. And while asymptotic phenomena may be captured by the right kind of dimensional analysis—in the simplest cases, like pendula, some simple assumptions and the requirement of dimensional consistency alone suffice to derive the universal law—in more complex cases a straightforward analysis may not be available (cf. p.16).

Though it is never quite made explicit, Batterman (e.g. pp.17, 125-9) creates the presumption (with considerable plausibility) that the main, and possibly the only, way to obtain principled universal phenomena and laws is in this asymptotic manner. This raises two interesting, interrelated issues.

(A—looking back.) Since Newtonian mechanics is an asymptotic limit of relativistic mechanics, Newtonian mechanics systematically excludes certain information, viz. that concerning 1/c-dependent phenomena. This exclusion process is irreversible; there is no set of assumptions that can be added to Newtonian mechanics that will then deliver relativistic theory again. Contrast, e.g., the small-swing pendulum motion as an idealisation of the more realistic non-linear motion, where adding the assumptions of non-negligible arm weight and angular displacement back into the same force analysis that produced the asymptotic law will restore the non-linear law, or the perfect gas (Boyle) law as an idealisation of the more realistic van der Waal's law where adding the assumption of finite gas molecule size to the gas axioms used to derive Boyle's law will again deliver the van der Waal's law. However, in the limit of $1/c \rightarrow 0$ the light cone edges collapse into the space axis thus irreversibly removing a key part of the structure of relativistic space-time and there is no set of assumptions that can be added to Newtonian space-time that will deliver relativistic space-time again. Hooker ([1994], section 4) coined the term 'degenerate idealisation' to refer to this irreversible relationship (degenerate, in the mathematician's sense, because structure is collapsed together, idealisation, physicist's sense, because the result is simplified without omitting what remains central and valid). Similarly, Newtonian mechanics is a degenerate idealisation of quantum mechanics. This degeneracy relationship will later play a useful role in the analysis of reduction under claim (3).

It is a fact, as Batterman's discussion shows, that the core history of modern physics is the successive revealing of then-best theories as in fact degenerate idealisations of still deeper theories (cf. also Hooker [1991]). Something like this might perhaps also be claimed for the recent history of chemistry as first classical atomic theory then quantum theory, especially electron orbital theory, have been worked through it. (Those histories may also involve removing non-degenerate idealisations—reversible, convenient simplifications—but this does not change anything fundamental, it is properly regarded as a process of refinement, to be carried out as useful.) It is cognitively apparent why this progression might apply: we notice only the crudest features of a new domain at first and only later learn to deepen and/or extend our examination techniques sufficiently to notice asymptotic-level discrepancies. If this line proves supportable it offers a more systematic conception of the history of mature sciences than merely empirical improvement.

(B—looking forward.) What of relativistic and non-relativistic quantum theories in turn? The Newtonian mechanical fundamental laws of motion are universal, applying irrespective of the nature and arrangement of the moving matter, and are indeed obtained as an asymptotic limit, in this case both from relativistic and non-relativistic quantum theories. Can then these latter theories also be obtained as the asymptotic limit of some still more fundamental theory by 'collapsing dimensions'? This is a perplexing and controversial issue—not least because, while they share a common limit (Newton's laws), there is as yet no fully satisfactory way to unite them. Whether this explanatory regress ever terminates in a principled way lies behind the ideas of a categorical theory (Petersen [1968]) and Batterman's speculation that asymptotics is the proper and only explanation of universality. Again Batterman does not discuss these issues, but they complement his account and deserve philosophical attention.

There is at least one further interesting feature shared by some universal phenomena in the asymptotic domain, self-similarity. In every case of so-called 'critical phenomena', e.g. near the 'critical point' beyond which there is no vapour phase between liquid and gas, the asymptotic domain shows a universally self-similar spectrum of fluctuations. (A pattern P is self-similar if there is a sub-scale on which P is repeated, and so on indefinitely.) This is indicative of chaos and occurs when behaviours are super-complexly, but still systematically, interrelated: "... the range of [interaction] correlations ... diverges to infinity ... [so that] correlations at every length scale (between near as well as extremely distant components) contribute to the physics [dynamics] of the system as it undergoes a phase transition." (p.40) The result is that the physics is essentially the same at all scales and is thus invariant under microscopic perturbation; this both explains its universality (p.44) and permits coarse-grained or more macroscopic re-scaling without loss of overall form, a fact exploited by Renormalisation Group methods (pp.40-4). These same self-similar fluctuation patterns turn up in the quantum asymptotic domain $h \rightarrow 0$ (p.110.)

However, clearly not all asymptotic phenomena are of this kind, as witness small-swing pendula. Whence, since some of the main methods of asymptotic analysis, e.g. the methods of intermediate asymptotics and the Renormalisation Group, search explicitly for self-similar solutions, they are not suited to all asymptotic phenomena—a conclusion acknowledged by Batterman (private correspondence) but not made explicit in the book. Thus while all asymptotic processes generate universal phenomena characterised by dimensionless constants (because what ensues is independent of the details captured in the discarded terms), and possibly all universal phenomena are generated as outcomes of asymptotic processes (that depends on the controversial issues canvassed above), not all asymptotic processes lead to self-similarity (chaos occurring only in rather special circumstances).

3 *Examination I: claims (1) and (2), asymptotic explanation and reference*

(1). *Asymptotic and causal explanation are quite distinct*. Batterman argues that 'causal-mechanical' (in particular, read 'microphysical') explanations adequately answer questions about why and how specific dynamical processes occur (his "type (i)" questions) but cannot adequately answer questions about why universal phenomena occur (his "type (ii)" questions). For instance, consider explaining why a particular pendulum motion has exactly the state (position-momentum) sequence it does and explaining why, given that it has only a small swing, it is exhibiting the universal small-swing pendulum simple harmonic oscillation. Though each proceeds in a principled manner, these two explanations are argued to be quite different from one another since the former explanation requires all the microphysical details for its completion but the latter explanation precisely requires their exclusion for its completion.

The claims after "since" are true. This certainly shows that the two explanations differ in aim, as between detailed, multi-feature, system-specific prediction and single-feature, cross-system robust generalisation. And while derivation of robust generalisations (e.g. through induction) is a universally acknowledged aim of scientific reasoning, Batterman is right to underline the existence, peculiarity and importance of this sub-class of cases, the dynamical asymptotics-based derivations of universal phenomena. But do they differ in nature? *For*: the two explanations differ in aim, and in formal structure since type (ii), but not type (i), explanations necessarily require premises from mathematical asymptotics and employ asymptotic methods for extracting commonalities (universal phenomena). *Against*: in both cases the explained conclusions are deduced from common dynamical and empirical premises and differ only in their formal premises, their use of mathematical asymptotic analysis.[2] What shall we take as constitutive of explanatory nature? I think it of little consequence on which side one comes down. Of more consequence is the temptation to illegitimately move from adopting the 'different aims and formal premises' side to supposing that the addition of asymptotic analysis to the premises makes some substantive difference (as I suppose Batterman might have done, see claim (2) below).

In any case, though this is how Batterman typically presents the explanatory issue, I think his concern is really focused on a different, independent claim, namely that it is impossible to reconstruct the explanation of asymptotic phenomena from causal-mechanical explanations. The idea is that the causal-mechanical explanations of various instances of a universal phenomenon will be infinitely various in detail and this will block any reconstruction of what is universal about them. This is closely related to the multiple realisation argument against reduction, namely that putative realisations of some property or object are endlessly diverse, thereby preventing reduction of the property or object to what is common to them, or to any one of them, or to their disjunction. Unsurprisingly, the fates of these two arguments are closely connected (see claim (4) below).

In response, a preliminary challenge to Batterman: universal phenomena are not magic, they are simply dynamical features robustly common to many detailed, albeit

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[2] Despite contrary appearances they can even both be placed in universally quantified form (that of the explanation of laws rather than particulars) by quantifying over the empirical system details of the type (i) explanation ('All systems with initial conditions IC(s) ...'). Then adjoining asymptotics premises simply alters the quantificational scope ('All systems with initial conditions IC(s) and dynamical form F(s) ...').

otherwise diverse, instances, so the common relationships must be at least implicitly present in the detail characterising each case. Then why not just extract these common relations from the detailed causal-mechanical data used to explain detailed dynamical state sequences using formal asymptotic methods and, generalising (note 2), deduce the universal phenomenon? Consider, in analogy, a set of triples of numbers generated from a single triple by some simple algorithm, e.g. adding randomly selected multiples of some number **d** by some parametrised procedure P to each of the original numbers, and then collecting together the set of all such sets of numbers formed by varying P and repeating random generation. As a numerical set it will have certain statistical properties, and that is all; but add to it the apparatus of Cartesian geometry and the result is seen to be a collection of values of semi-random variables on a single three-dimensional cubic lattice with sides of length **d**. In consequence, it can be deduced that any subset of triples will exhibit certain simple lattice relations among them, and so on. It requires embedding the sequence in the mathematical apparatus to reveal the geometrical order underlying it, but the substantive order is in the data, not the mathematics.

But Batterman's thought seems to be this: unlike for the lattice example, the simple data + mathematics approach is blocked in the specific case of universality because its asymptotic origins show that it only emerges when much of the data is eliminated (via $\mathbf{p} \to 0$). However this need not be so. Consider the class of all detailed classical small-swing pendula models. Each model is used to provide a causal-mechanical explanation of one instance of pendulum behaviour. None of these detailed dynamical descriptions will specify exactly simple harmonic oscillation [SHO]. With each model given as just a set of number pairs (the pendulum states) the class of models will have certain statistical properties, and that is all. But add to it the mathematical apparatus of general dynamics and of asymptotic analysis (in fact just its barest beginnings in parametric expansion) and what these models do specify in the asymptotic domain can be re-described as SHO + small correcting terms, terms that vanish in the limit of small oscillations. So we can after all extract the universal claim that they all share SHO in common, their diversity occurring in their differing correcting terms. QED.

Though Batterman does not explicitly tackle this obvious challenge, this may be because he takes himself to have disposed of it in an early, preliminary discussion of how to explain commonalities among physical manipulations that solve some given puzzle, e.g. releasing scissors from a cord wound around them. The appropriate explanations are topological (about pure connectedness): commencing with a relevant topological characterisation of the initial (puzzle) and final (solved) spatial arrangements, they deduce the class of successive transformations (manipulations) that take the former into the latter and then explain the manipulative commonalities as features shared by all solving transformation sequences. Batterman then argues that the topological principles involved in this explanation could not have been derived from commonalities among specific causal-mechanical explanations of detailed puzzle resolutions (pp.34-5). The equivalent objection in the case of the pendula would be that the derivation of the SHO commonality required general principles drawn from the twin mathematical apparatus of general dynamics and of asymptotic analysis and these could not have been derived from commonalities among specific causal-mechanical explanations of detailed motions (presumably of pendula or not).

But arguing in this way invites confusion. Appeal to mathematical principles is involved in every dynamical explanation, specific causal-mechanical or not. General dynamics cannot be formulated except within such a mathematical framework. If all that asymptotic explanations of commonality appeal to are mathematical principles of the same status as those appealed to in any dynamical explanation then, whatever that status, Batterman's objection to extraction from causal-mechanical explanations via these principles must fail on pain of declaring nothing derivable from any dynamical theory. Yes, asymptotic principles tell us how to systematically take relational structure out of the same numerical dynamical models as dynamical principles had told us how to insert their relational structure into them, but this difference is irrelevant to the issue of whether numerical data sets plus mathematical principles can do both jobs—they evidently can. And in each case the relationships they make explicit and systematise must substantively subsist in the empirical numerical data. To suggest otherwise is to endow mathematics with a substantive empirical contribution that goes beyond its formal expression of logical possibility relations. Batterman certainly makes no such argument. The issue of the status and role of mathematics in science is an important and difficult one, but it is not differentially germane to Batterman's problem. This unresolved ambiguity about the role of mathematics will also dog his argument in claim (2) below.

Thus, Batterman's first claim, as he presents it, is rejected. Even so, there is more to be said. Nothing dynamically constrains the SHO pattern when the 'classical pendula' conditions are disturbed; alter pendula a bit (e.g. put them in a stiff, variable breeze) and the motion changes, the SHO pattern falls apart. While the small-swing SHO commonality is dynamical in origin and robust across classical pendula, it is not dynamically robust in the further sense that pendulum systems dynamically resist its disturbance with real energy-dissipating restoring forces. The SHO pattern has no 'top-down' constraining power. But consider instead the claim that all Newtonian rigid solid objects behave dynamically as if they were concentrated at their centres of mass. This conclusion can't be so easily disturbed because the molecular binding forces holding each solid object together in a rigid manner actively resist deformation, they respond to externally applied force with restoring forces and dissipate the energy of external blows in thermal molecular lattice vibrations. This gives them top-down constraining power. These two cases indicate an important distinction among kinds of robustness, one that Batterman ignores but which will be central to assessment of claims (3) and (4) below. Call the wider class of cases of universality to which pendula belong the asymptotically robust phenomena and the proper subset to which rigid solids belong the dynamically stabilised asymptotically robust phenomena.

The present issue is whether dynamically stabilised robustness might pose a greater challenge to extracting the shared commonality from the detailed microphysical explanations, e.g. because it may not be possible to follow the dynamical behaviours in detail through the macro-level dissipation processes. What Batterman would have to show is that in these cases of dynamically stabilised universal patterns, there was some principled barrier (e.g. deriving from in-principle computationally inaccessible dissipation processes) to the extraction of common patterns from the detailed dynamical explanations. While he has not shown, or even discussed, this, there are hints of such a case in scattered remarks about the limits of analytic solvability. Referring to the correlational divergence characterising phase transitions (see quote from p.40 above), Batterman says: "This is a highly singular mathematical problem and is, in effect, completely intractable. It is relatively easy to deal with correlations between pairs of particles, but when we must consider correlations between three or more—even more than 10^{23}—particles, the situation is hopeless." (p.40), and there is the (p.116) remark about wave optics non-analyticities also noted above.

I propose to accept a systematic deductive barrier of a kind implicit in these remarks: when the asymptotics are singular it is not possible in principle to deduce the detailed behaviour of the limit domain (T_c) systems from models, however detailed, of the corresponding T_f domain systems.[3] But the relevant point here is that these analytic barriers

[3] Here the 'not possible in principle' is to be understood in the sense of Cherniak [1986] as 'computationally inaccessible to any finite computation process of this universe'. See also Hooker [1994] for further discussion.

cannot do what Batterman requires to block extraction of common patterns. To see this suppose (counter-factually) that the non-linear pendulum models were similarly analytically unsolvable. We can write down equations of motion for them but cannot obtain their exact analytic solutions. (Some pendula, e.g. compound pendula, will be like this.) Does this prevent us extracting analytic commonalities among the models? Well if it did, if they resist even asymptotic analysis, then there will be no asymptotic universalities for Batterman to hold up. Contrariwise, so long as a general asymptotic analysis can be given of the equations in the classical small-swing condition this must show how, in that condition, they all display SHO + correcting terms which vanish in the limit of small oscillations. So we can again extract the universal claim that they all share SHO in common, their diversity confined to their differing correcting terms. Indeed, Batterman makes the point that it is the job of asymptotic analyses, such as Renormalisation Group methods, to isolate exactly those properties that will persist (are invariant) under change of micro details.

In sum, while Batterman has succeeded in drawing attention to the distinctive structure-removing aim and formal methods of asymptotic analysis, and while important issues about the nature and status of mathematics, especially asymptotic analysis, and of dynamical non-analyticities have been raised, I conclude that Batterman has not made out his case for a distinctive asymptotic explanatory form significantly divorced from general dynamical explanation. On the other hand, this examination of claim (1) has brought us a useful distance. For the analytic barrier above, when applied to cases of dynamically stabilised asymptotically robust phenomena, will play a central role when discussing the nature and diversity of reduction and emergence under claims (3) and (4) below.

(2) *Asymptotic analysis requires irreducible reference to the entities of the limit domain as well to those of the non-asymptotic domain*. Batterman shows how rainbows, e.g., are asymptotic objects within wave optics and argues that nonetheless their explanation

cannot be constructed without referring essentially to caustics which, being ray tangent curves, are ray optical objects. Batterman's forms of expression make it easy to read him as claiming that we are thereby ontologically committed to caustics. Suggesting this reading are passages like this:

> "Furthermore, if I'm right and there is a genuine, distinct, third theory (catastrophe optics) of the asymptotic borderland between wave and ray theories—a theory that of necessity makes reference to both ray theoretic and wave theoretic structures in characterising its "ontology"—then, since it is this ontology that we take to be emergent, those phenomena are not predictable from the wave theory. They are "contained" in the wave theory but aren't predictable from it." (p.119).

Or consider: "It seems reasonable to consider these asymptotically emergent structures to constitute the ontology of an explanatory "theory"..." (p.96) or the many remarks like "The phenomena are not explainable through derivation—that is, through straightforward solutions to the differential equation—from the fundamental wave theory alone." (pp.118-9, cf. the quote from p.120 below).

But how much ontological weight can "referring essentially" carry? The mere formal use of a term like "caustic", even essentially, is no guide. It is also not possible to discuss a particular cloud formation as like a flying saucer, in explanation of reports of flying saucers, without using the term "flying saucer"—and in the sense a UFOlogist would—but, as Plato noted, this had better not ontologically commit us to flying saucers. So we need an ontologically committed use of the term.

Passages like that quoted above from p.119 are not of much help in deciding the issue, since the argument is predicated on the assumption that the ontology of the asymptotic domain is emergent, thereby begging our question here. Equally unhappily, the last quoted remark (from p.118-9) is crucially ambiguous about whether the premises for the derivation include or not mathematical asymptotics. The omission is systematic (cf. the same ambiguity in the quote from p.120 below). Yet there is no question that the mathematics is contributing something of some import, and that Batterman thinks it is, that is the whole point behind his denial of any straightforward derivability. His clearest discussion is at pp.96-97 where, while agreeing that in "one sense, the solutions are contained in the fundamental wave equation" he argues that "the understanding of those [asymptotic] mathematical representations requires reference to structures foreign to the fundamental theory" and concludes again that "it is entirely reasonable to think of catastrophe optics as a genuinely new theory, a "third theory", describing the asymptotic [domain] ... between wave and ray theories." (cf. e.g. the earlier pp.96 and 119 quotes). Here we re-encounter the view discussed under claim (1) that asymptotic features cannot be derived from dynamical explanations. However, here the scare quotes at crucial places throughout these passages leave the intention crucially vague (and their inconsistent use the more so), so that it is impossible to determine their real import. Further, Batterman has subsequently denied (in private correspondence, see below) that there is any third theory, since there are no rays.

While I sympathise with his evident uncertainty about what to say in the face of the mathematical subtleties, the main points as I see them are (I) the use of mathematical principles in analysis should not count as a relevant appeal to extra-dynamical laws (this was argued under claim (1) above), but then (II) the inclusion of asymptotics now opens the way for considering caustics to be construed as just mathematical constructions within wave asymptotics. Compare here, from the cubic lattice example (see claim (1) above), the construction of regular lattice invariants (e.g. diagonal distance across a lattice cube) through use of mathematical principles of Cartesian geometry. But if caustics need be no more than constructions of (asymptotically) idealised invariance conditions within wave theory, even their essential occurrence within the product of wave theory and mathematical asymptotics will not suffice for ontological commitment to them. (Perhaps this is why "ontology" and "theory" are so often in scare quotes, despite the interpretive uncertainty that brings.) For ontological commitment we need, with Quine, to be shown an unavoidable piece of reasoning where quantification over the objects appealed to is logically ineliminable. Neither mere use, nor abstract construction, will provide this.

However, appearances notwithstanding, providing Quinean satisfaction perhaps turns out not to be quite Batterman's aim (private correspondence): "I do not believe that there is any new ontology in the asymptotic catastrophe optics. The wave theory has replaced the ray theory and there simply are no caustics (as characterized by the ray theory). Asymptotic analysis of the wave equation yields terms (think syntax here) which require for their interpretation (semantics) that we make reference to ray theoretic structures. In effect, it is the understanding/interpretation of these terms in the asymptotic expansions that requires 'appeal' or reference to structures that 'exist' only in the ray theory." So there are after all no new basic asymptotic entities. (And while limit objects, rays, are different from waves, they have been superseded.) This makes sense of Batterman's denial of novel causal powers to them (e.g. p. 126). The status claimed for caustics based on essential reference is not after all that of ontological commitment but one of ineliminable semantic role, without ontological commitment. This occurs, argues Batterman, whenever (though only) where the asymptotics is singular (private correspondence): "Caustics are not abstract constructions from wave features since caustics have no wave features. Had the limit been regular and not singular, then one might be able to talk this way, but because the ray theory is qualitatively distinct from the wave theory, it is not really possible to think either of rays as constructions of wave features or of waves as constructions from ray features." This is evidently how to read ambiguous remarks like that from pp.118-9 quoted earlier.[4]

But though this helps to explain Batterman's ambiguous or 'half-way' sense of the status of asymptotic features, it equally leaves open the question of what we are to say about this semantically necessary/ ontologically otiose position. This approach evidently violates the widely adopted principle that semantics ought to clearly reflect ontology. However one responds to that problem, I note that the issue of how to treat formal constructions is not peculiar to physics asymptotics but is the problem of mathematical limit constructions generally. What is the status of frictionless planes, of two-dimensional geometric surfaces, and so on? (This width, incidentally, voids Batterman's suggestion above of distinguishing between cases of singular and non-singular asymptotics, the problem applies to both.) The status of limit constructs is a perennial debate. It was, e.g., at the centre of nineteenth century Scottish education (Davie [1961], Part 2) where its resolution was contested as between abstractionists (one version is roughly Batterman's approach) and empiricists, with theoretical realism a continuing option. Versions of this space of possibilities continue to be debated today.

Rather than attempting to resolve this debate here, especially along Batterman's complicated lines, I simply note that Batterman has not convincingly argued his position. His argument that caustics are not abstract constructions from wave features since caustics have no wave features, invites the response that waves provide the only real resources for constructing descriptions in this situation (similarly, the dynamical laws of the microphysical

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[4] Compare here Kim's less subtle claim (Kim [1998], p.97) that Nagelian reduction requires the augmentation of the ontology of the reducing theory simply as a result of the fact that the laws of the reduced theory can't be formally deduced from those of the reducing theory without employing the vocabulary of the reduced theory (as it figures in bridge laws).

constituents of a Newtonian rigid solid determine the character of the top-down rigidity constraint they support) and caustics merely show none of the features that characterise wave relations generally in the non-asymptotic domain but are how certain special relations among waves appear when their specific wavelengths are abstracted away. Nor will his supporting treatment of asymptotic relations (see below) withstand examination. I conclude that Batterman has not made out his case for any substantive sense of essential reference to asymptotic limit entities.

4 *Examination II: claim (3), reduction and singular asymptotics*

(3) *Failure of reduction (to the non-asymptotic theory) coincides with the occurrence of singular asymptotics*. We enter discussion of this claim bringing the degenerate/non-degenerate idealisation distinction from section 2 and from the discussion of claim (1) the notion of an analytic barrier in cases of singular asymptotics plus the distinction between asymptotic robustness simpliciter and dynamically stabilised asymptotic robustness. (The related connection of universality to multiple realisability carries over to the claim (4) analysis.)

A: *Context*. Since keeping the issues straight here can be difficult, it is useful to begin with some context-setting remarks. (I) The general framework within which Batterman discusses reduction and emergence is that of a superior successor theory T_f and an earlier, coarser theory T_c where T_c is some asymptotic limit of T_f: $\mathrm{Lim}_{\mathbf{p} \to 0} (T_f) = T_c$, for appropriate **p**, which expression Batterman numbers (2.6), see p.18 (= expression (6.1), p.78), and refers to sometimes as an equation (e.g. p.65) and sometimes as a schema (e.g. p.95, see further below). (ii) Recall that there are three kinds of properties and relations involved, found in three distinguishable domains, those of T_f in the non-asymptotic domain, those of the T_f asymptotic domain and those of T_c in the limit domain. (iii) It is assumed that there is no reduction/emergence issue concerning the non-asymptotic T_f domain. (iv) Traditional philosophical discussions of reduction focus on whether the T_c entities and properties are reducible to those of non-asymptotic T_f; e.g. whether Newtonian theory and its objects reduce to relativistic theory and its objects. However there is equally an issue concerning the reductive status of the entities and properties of the T_f asymptotic domain, both in relation to those of non-asymptotic T_f and, as we have seen, in relation to those of T_c. (v) Emergence is typically taken as the opposite of reduction in philosophical discussion, so there will similarly be two separate issues of emergence, for the T_f asymptotic domain and for the T_c domain.

Batterman accepts this basic framework. Within it he identifies the success or failure of reduction with expression (2.6) holding or failing to hold (pp.18-9, 124, etc.), and essentially equates that with emergence succeeding (p.124). (He says that (2.6) failing is a necessary condition for emergence but I say "essentially equates" because while, strictly, there are several conditions to be met for emergence—see below, on Batterman's version they all co-occur with (2.6) failing.) He supposes that reduction fails with (2.6) failing in cases of singular asymptotics because when the theory in the limit is discontinuously different from the asymptotic theory (e.g. for wave optics and quantum theories) the equality in (2.6) is no longer justified and the objects of the two theories involved will be quite different from each other. Conversely, he supposes that reduction succeeds with (2.6) holding in cases of regular asymptotics, when the asymptotic theory passes smoothly into the theory in the limit (e.g. for pendula and relativity theories) because the equality in (2.6) is thus justified and ensures similarity in objects. And also perhaps because here the asymptotic models are constructively analytic and so 'transparent'—he refers to them as "aggregates" (p.124).

One matter is left ambiguous. Batterman rightly follows Nickles in distinguishing common philosophical and scientific senses of the term "reduction" related to expression

(2.6). For scientists T_f reduces$_2$ to T_c when $\mathrm{Lim}_{p\to 0}(T_f) = T_c$, while philosophers may think that T_c is reduced$_1$ to T_f. Batterman speaks (p.78) of reduction$_2$ as “satisfying ... [the] schema (equation 2.6)”. Read most literally reduction$_2$ holds or fails with (2.6), i.e. reduction$_1$ ≡ reduction$_2$. Here if you thus derive T_c from T_f you mean deduce T_c from T_f (plus ‘bridge laws’ or identities) and treat (2.6) as an equation expressing this. On the other hand, it is possible to read the expression (2.6) as indeed a formal schema specifying the structural deformation of one set of mathematical relations into another, whether ‘smoothly’ (regular asymptotics) or not. In that case schema (2.6), and so also reduction$_2$, always holds. This is how all mathematicians and most physicists would read it, again using ‘derivation’ to describe the process—but now a process logically very different from deduction. Batterman (p.79) clearly thinks the physicist Rohrlich reads it this way because he chastises him for supposing that reduction$_2$ could be a criterion for reduction$_1$, i.e. chastises him for holding reduction$_1$ ≡ reduction$_2$, on the grounds that reduction$_1$ holds only for regular asymptotics. But he then repeats that his schema (2.6) fails for the singular cases, which treats it as a literal equation, while the contradictory assumption behind his criticism of Rohrlich is that it is a schema and always holds. On balance, I shall drop the literal reading for the formal reading of reduction$_2$ (i.e. I will treat (2.6) as a derivation schema, not an equation—see also Hooker [1979]). Moreover, it seems to be on the latter basis that Batterman concludes that philosophers and physicists have fundamentally different conceptions of the explanation and evaluation of intertheoretic relations (e.g. pp.17-18) Later I shall have something to say about how the differing concerns of philosophers and physicists interrelate, and in a way that picks up the status of 2.6 and connects it to my critique of his account. (Meanwhile, I shall nonetheless hereafter drop the subscripts on “reduction”, unless needed to avoid ambiguity.)

Batterman’s discussion shifts between reduction/emergence for the T_f asymptotic domain and for the T_c domain without always clearly distinguishing them. For example, the discussion of reduction in chapter 5 is preoccupied with the T_c domain (as contrasted to T_f) and the challenge of multiple realisability (see claim (4) below); with the latter associated to the occurrence of universality the discussion passes to chapter 6, which focuses instead on the T_f asymptotic domain and its singularities, illustrated through the case of wave optics, concluding (p.95) that equation (2.6) fails and "the various philosophical accounts of reduction discussed in chapter 5 will fail to account" for them. The discussion of emergence in chapter 8 equates emergence with the failure of T_c reduction$_1$ to T_f but passes immediately to the emergence of rainbows, entities instead in the wave optics asymptotic T_f domain, only to re-introduce 'essential reference' to caustics, entities of the T_c ray optics domain. Though mathematically connected, this sliding between domains can cause problems. For instance, Batterman spends some time (p.118-9) seriously fending off the implausible view that, since emergence is about the arising of a new entity from the coming together of its components, for rainbows and other wave phenomena to be emergent must mean that they arise from the coming together of rays.[5] That view is arrived at by considering emergence as anti-reduction$_1$ of T_c to non-asymptotic T_f whereas the issue with rainbows concerns emergence as anti-reduction$_1$ of asymptotic T_f to non-asymptotic T_f (hardly to T_c—even if it is referred to essentially).

In the light of the foregoing views, and related remarks, I suggest that Batterman's overall position can be summed up in what I shall label his dichotomy principle [DP]: both reductions, from asymptotic T_f and from T_c, to non-asymptotic T_f succeed for regular asymptotics and fail for singular asymptotics, and in each case the same reasons apply for

[5] It is true that there is an approximation to asymptotic wave phenomena constructible from rays, but this cannot do the appropriate job because (i) it breaks down near and on a caustic and (ii) it anyway requires assigning a phase to a ray, so it is not a pure ray construction—see pp. 86-89.

both domains. (That he holds to DP perhaps explains why he feels free to shift between domains.) Given DP, one then obtains a conception of the scientific landscape along the lines of Table 1.

INSERT TABLE 1 ABOUT HERE

Keeping all this in mind, we are ready to discuss Batterman's treatment of reduction.

B: Reduction. The dominance of the abstract relation $\text{Lim}_{p \to 0} (T_f) = T_c$ in Batterman's formulation of the reduction/ emergence relation requires further clarification, the need being made clear by Batterman's misplaced concerns with constructing waves out of rays (above). First, many of the central cases of reduction/ emergence do not arise as relations among successor theories. The relation of solids to their molecular components, e.g., is a classic focus of the reduction/ emergence issue but, as the earlier discussion made clear, this issue persists even when both the components and the solid are described in terms of the same theory, whether that be Newtonian, relativistic or quantum mechanical. There will always be a relevant pair of asymptotically related theoretical models, but they may be constructed within the same overall theory. In the case of classical pendula, e.g., there is a non-linear model applicable to all swings and there is the small-swing linear simple harmonic model, both formulated within Newtonian mechanics. Second, as the case of forming waves by combining rays makes clear, not all cases of asymptotic inter-theory relations are candidates for emergence from components. Indeed, virtually none of them are; rather they concern instead the reduction/emergence of properties, but typically properties of the same objects in some sense rather than the simultaneous emergence (or not) of both an object and its properties from its components. Batterman knows this in some sense, for he draws attention to the failure of component/whole formulations of emergence to apply to wave/ray asymptotics and like cases (pp. 114-6). But nor is the intra-theoretic case of forming rainbows out of waves a case of forming a new object, for since waves superpose linearly there can be no cohesion among them. However he also refers to systems with regular asymptotics as “aggregates” when these refer to composites from components, even though in its usual sense an aggregate is restricted to an especially simple case of the dynamical analyticity that would underlie the case for property reduction. Cases of phase change within the thermodynamics/ statistical mechanics relationship clearly offer outcomes where components form non-aggregates, where there does emerge a new part/whole relation, as do related cases of n-body stabilised interaction modelled within a single dynamics (see below). This suggests that he has not got this distinction clear. As a crude first approximation we have something like Table 2.

INSERT TABLE 2 ABOUT HERE

Batterman is surely right to emphasise the centrality of asymptotic relations connecting successor theories in mature sciences and right to regard the asymptotic successor (T_f) domain as raising interesting new phenomena, neglected by philosophy. But there is reason to doubt that the asymptotics-reduction relationship is as simple as DP makes out. To properly capture the dynamical diversity in Table 2 three nested distinctions need to be introduced.

First, regular asymptotics includes both cases of non-degenerate idealisation, e.g. the small-swing pendula, and cases of degenerate idealisation, e.g. the relativistic/Newtonian pair (see section 2 above). In the former cases there is sufficient similarity throughout (asymptotic and limit domains) that reduction goes through. But while in the latter cases there will be no new asymptotic T_f domain phenomena, the limit domains T_c are still sufficiently different from the T_f domains that reduction there cannot proceed undisturbed. For example, relativistic theory has regularly been argued not to straightforwardly reduce Newtonian theory because of their structural differences: relativistic mass varies with reference frame while Newtonian mass does not, etc. (See e.g. Sklar [1967] and Yoshida [1977] for the classic debates.) The dynamical categories remain essentially the same (force, mass, acceleration, etc.) as do the essential dynamical principles, e.g. the generalised principle that acceleration = force/mass, the regular asymptotics ensures that. It does not, however, ensure such basic Newtonian relational principles as that distance and time intervals are non-locally unique. Here this mixed result arises because the essential difference between the two theories is not immediately focussed on the dynamical acceleration/force relation but on the kinematical framework of possibilities (space-time geometry) within which that relation is expressed. Because of this there is no simple way to allocate reduction; the regular asymptotics, expressed in the common dynamical force/mass relation, speaks for ontological retention and hence reduction while the deeply changed space-time (possibility) relationships urge elimination and hence reductive failure. We can worry ourselves trying to force a resolution, e.g. argue that kinematics is not of the essence of object constitution so reduction holds, or argue that whenever there is a significant change in overall dynamical form reduction fails, or we could try to finesse a neat resolution somewhere along the retentive/eliminative reduction spectrum (see below). But much more important is to grasp the nature of the issue and that the degenerate/non-degenerate distinction represents a first departure from DP. Perhaps it is Batterman's neglect of the retention/elimination continuum that allows him to adopt the more simplistic DP position.

Second, degenerate idealisation includes two importantly different cases, those where the distinctive properties of the T_f asymptotic or T_c limit domains are constituted in dynamical constraints on components, i.e. where they top-down constrain the dynamical behaviour of their components and those where they do not—as in the rigid objects versus the SHO pendula discussed under claim (1) above. With specific reference to singular asymptotics consider, e.g., the wave-fringed quasi-rays of the wave optics asymptotic T_f. Batterman agrees that these latter structures have no "causal powers", = top-down dynamical constraint capacity, of their own (pp.126-7), they are just constructed patterns. But not all singular asymptotic phenomena are like this; there are also asymptotic processes that do result in the formation of genuinely new top-down dynamical constraint capacity. Consider, e.g., the formation of an iron bar from molten iron. In this phase transition a macroscopic pattern of inter-molecular relations is formed, the iron crystal, which does thereafter have the power to constrain the movements of its molecular components through the formation of a new macro-scale force constituted in the chemical bonds formed. Its formation alters not only individual component behaviour but also the specific dynamics under which they move. (Its formation alters the force form of the dynamical equations that govern them.) The new macro-scale force is able to retain the constraint relationship invariant under component fluctuations and exogenous perturbations, through dissipation of the energy. This is how Collier/Hooker ([1999]) characterise all genuine self-organisation, for it is what must happen if self-organisation is to constitute a new dynamical existence, since it is the only thermodynamic condition under which new work can be performed.

All this has an asymptotic characterisation. The formation of such a top-down constraint, as in the rigid iron macroscopic object, is equivalently the formation of a dynamically stabilised asymptotically robust or universal property (see claim (1) above). And the analytic inaccessibility of the resulting supra-component behaviour to bottom-up component dynamical analysis associated with the singular asymptotics of formation is a corresponding expression of the stabilising capacity of the new macro-scale force. The iron molten/solid phase transition will be asymptotically characterised by inter-molecular crystalline-like clustering fluctuations on every scale, a critical phenomenon governed by singular asymptotics. In the asymptotic domain these fluctuations, while they last, have local top-down constraining power. And when, under cooling, a single fluctuation is amplified to dominate all components, forming a new top-down constraint on them, their altered behaviour is an expression of their altered dynamical force form. And the singular asymptotic limit of infinitely rapid crystalline propagation of inter-molecular perturbations (the iron T_c) characterises the treatment of the now-permanent iron crystal as a new dynamical existent, a Newtonian rigid object. This shift in constraint formation, characteristic of phase transition, also characterises critical phenomena such as the case of merging solid/liquid/gaseous phases described by Batterman (pp.37-42).[6]

Dynamical interaction leading to top-down constraint formation is in this respect very different from dynamical construction leading to inter-component patterning, as when one

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[6] In a more traditional philosophical language, the iron bar is supervenient on its molecules, nothing about the bar can change without the change being dynamically grounded in appropriate molecular changes. But, as the foregoing discussion shows, dynamical analysis provides a much richer language in which to discuss the possibilities. First, it specifies top-down behavioural constraint formation in terms of change in dynamical form, the change in form describing the causal power this novel constraint possesses. (This also distinguishes such effects as non-epiphenomenal.) Second, the dynamics itself shows how the constraint, a (relatively) macro level state/property, is determined by the states/properties of its micro constituents and so is supervenient on them, yet can nonetheless also constitute a constraint on them. Here dynamics gives the constraint a subtle status that eludes conventional formal analysis, combining what common philosophical assumption opposes. (Much of this is prefigured in Collier [1988].) Thus dynamical determination, = there being only one dynamical possibility for the collective dynamical state/property, cannot be equated with logical determination, = the collective dynamical state/property is logically derivable from, can be expressed as a logical sum of, its constituent states/properties. The former is specified as the constituents fixing all space-time trajectories so as to allow only one macro possibility, but these trajectories may be computationally strongly inaccessible (note 3).

constructs a macroscopic wave pattern—e.g. a diffraction pattern, or a ray trajectory—from interference among superposing wavelets. In the former case there have to be physical components that, “coming together” (p.116), interactively ground the new constraint, i.e. whose ongoing interactions continue to form the constraint. In the latter solely constructive cases this is not so; in the superposition of waves to construct rays, e.g., the linearity of superposition means that component waves have no dynamical form-changing interaction (they are confined to vector addition) and no determinate ongoing identity within the resultant pattern because it may be infinitely variously decomposed into wave components. (If non-linear optics becomes an established theory then this case will likely revert to that of the iron bar.) This contrast between constraint formation and not represents the second departure from DP.

Third, degenerate idealisations with no top-down constraint formation now clearly include two importantly different cases, those that arise from dynamical construction, e.g. the rainbow in wave optics, and those that arise from kinematical shift, e.g. the relativistic/Newtonian shift. Both kinds of cases share the feature that there are no components to “come together” through dynamical interaction to form the emergent structure. However the reasons for this seem very different in the two kinds of cases. In the optics case the reason is dynamical, the electromagnetic field is treated as superposing linearly and this precludes the possibility of forming self-stabilising supra-component constraints like those in iron bar formation. In the case of relativity, however, dynamical issues of formation, whether particle or field like, are not relevant, rather it is the transformation of the kinematical possibilities of motion that is involved. And without attempting to unravel the persisting mysteries of quantum theory, suffice it to say that the shift from Hilbert to Cartesian geometry of phase space plus Batterman’s nice treatment of the WKB approximation which connects the two in the asymptotic quantum domain, suggests that this case is as much like that of relativity (despite the singularity of the quantum asymptotics) as it is like the optics case.[7] This contrast within the no-constraint category represents the third departure from DP.

If all this is roughly right we obtain a picture of the landscape of relations something like that of Table 3.

INSERT TABLE 3 ABOUT HERE

By contrast, Batterman's DP principle, based on a single formal criterion, removes all the structure from this table except the regular/singular distinction and identifies reduction failing with emergence and both with singular asymptotics. The upshot is that *there are two importantly different ways to characterise the success and failure of reduction, formally and dynamically, and these are not equivalent*. (a) The formal characterisation is the success or failure of T_f to formally reductively explain T_c, which we might equate to the success or failure of $\text{Lim}_{\mathbf{p} \to 0} (T_f) = T_c$, Batterman's equation (2.6), and identify with DP. I have argued that this misses much important structure, and misses the dynamical essences of reduction

[7] However, the focus of the interpretive literature on substantival/dynamical issues, combined with the continuing absence of clear metaphysical options and the dynamical differences between standard quantum theory and quantum field theory, renders any such proposal speculative and tentative. Moreover, all these cases may ultimately prove more deeply linked through the ideas of space-time as dynamically identified with matter fields and of asymptotic limits representing dimensional elimination (see section 2 above), though this too remains speculative at this time. Similarly, while in what follows I have assumed the traditional position that space-time itself is a pure framework with no dynamical constraint capacity of its own, should the preceding view prevail and space-time be attributed dynamical constraint capacity it would also be appropriate to speak of emergence for all kinematical cases where reduction fails.

and emergence. (There is irony here, for Batterman is also a substantial ally in this cause: he criticises analytic philosophy of explanation and reduction for being overly formalist and ignoring asymptotic universality, rightly in my view.) (b) The dynamical characterisation of reduction not clearly holding is that of degeneracy of relationship in the limit domain, while the characterisation of emergence is of the formation of a top-down dynamical constraint—thereby also ensuring degeneracy of relationship. (Phase changes in the asymptotic domain constitute a space-time half-way-house; they are patchily—space-time locally—like the limit domain case.) If reduction failing is given this widest interpretation of dynamical form change then reduction and emergence fail to coincide. Only if reduction failing is identified with top-down or macro constraint formation do the two coincide. (Again Batterman is close to an ally, for on my account also emergence is restricted to singular asymptotics, and he too emphasises the need for a dynamical understanding of emergence; but ironically he chastises others for not seeing the common universality to all the singular asymptotic cases of emergence.) These conceptions and the distinctions that underlie them respect the relations and phenomena Batterman is rightly concerned to emphasise, while providing a finer delineation of reduction and emergence.

The proper way to capture the full explanatory role of equation (2.6) as a formal schema is by including both explaining X and explaining X away. (Batterman ignores the latter, though at p.64, point 4 he cites it and its historic expression in Sellars, showing that he knows about it.) In every case, whether the asymptotics is regular or singular, T_f explains both why T_c is as accurate as it is when it works, and why it fails, when it does. I.e. T_f partly explains T_c and partly explains T_c away, the mix varying according to the details. Explaining is most complete for non-degenerate idealisation under regular asymptotics, becomes less so for degenerate idealisation under regular asymptotics, and is minimal under singular asymptotics (where all idealisations are degenerate). Whence it also becomes clear that to capture the corresponding ontological range we need to include both retention, = reduction$_1$, = explaining T_c, and elimination, = failure of reduction$_1$, = explaining T_c away. Batterman gives the impression that the rentention/ elimination asymmetry is a recent discovery of asymptotics but, as his citation of Sellars shows, it has long been recognised—and long championed by Churchland (e.g. [1979]). The extremes really define a continuum, with the imperfections of the thermodynamics-statistical mechanics reduction relation one of the intermediate inspirations (Hooker [1981], Part I)—pace Batterman—who, despite citing Sklar ([1967]), considers that it is virtually a "dogma in the philosophical literature" that that relation is a paradigm of reduction (p.63, n.1). Confusing the formal reduction$_2$ role of schema (2.6) for reduction$_1$ is also not a new mistake. While Batterman, citing singular asymptotics, criticises Rohrlich for supposing that reduction$_2$ always suffices for reduction$_1$ (cf. above), Hooker ([1979]) offers essentially the same criticism of Yoshida's similar ([1977]) proposal.

There is no mystery about the difference in focus between reduction$_1$ and reduction$_2$: scientists have to be concerned with continuing practical reliance on T_c and hence with the errors involved in doing so, which asymptotic analysis reveals, while philosophers are concerned with accurately specifying ontological commitment in the light of T_f's explanatory superiority. Putting it this way shows how, pace Batterman, both physicists and philosophers are concerned with how things "fit together" and how both can pose this in terms of relations between successor and predecessor theories. But the difference is important. Formal reduction$_2$ always holds, whether the asymptotics is regular or singular, but this is from the scientist's point of view where 2.6 is treated as a formal mathematical transform schema. It is important that it always thus hold since scientists must use it to derive the kinds and magnitudes of the errors made in using the typically simpler T_c models instead of the typically more complicated T_f models. But reduction$_1$ does not always hold. However this is from the philosopher's point of view where (2.6) is treated as a schema still, not an equation, but now one providing the dynamical basis for asserting ontological identity or distinctness. Thus we have a universal explanatory role for the formal schema, i.e for reduction_2, coupled with an asymmetric failure of the ontological schema, i.e. of reduction_1. For the same reason the formal distinction between regular and singular asymptotics does not coincide with the ontological reduction_1 relation, or with emergence; asymptotic regularity specifies smoothness of error increase while reduction_1 specifies ontological identification.

5 *Examination III: claim (4), emergence and multiple realisability*

(4): *Singular asymptotic patterns constitute a new basis for emergence and provide new forms of emergence.*

A. *Batterman on emergence*. Batterman is convinced that the distinctive emergent phenomena occur in the T_f asymptotic domain, e.g. rainbows, and fixes his attention on these. He proposes to test emergent status against 5 criteria he takes from Kim ([1999]): **a**: higher-level entities emerge from the "coming together of lower-level entities in new structural configurations", **b**: higher-level properties, whether emergent or resultant, arise out of "properties and relations that characterise their constituent parts", **c, d**: emergent higher-level properties are not predictable or explainable from, or reducible to, these "basal conditions", and **e**: they have novel causal powers not reducible to those of their "basal conditions" (p.115). Batterman notes immediately the component/whole bias of **a** and **b** and concludes (pace Kim) that it will be inappropriate to wave/ray optics—and, I add, similarly for like continuous field-theoretic applications, and for all kinematical cases like Newtonian/relativistic asymptotics.

Ignoring the domain shift, Batterman tests for emergence from T_f against **c**, **d**, **e**.[8] He

[8] The domain shift arises because the wave/ray relation is one between limit T_c and asymptotic T_f domains, but T_f asymptotic properties do not arise from combining entities or

comes to an affirmative conclusion on **c** and **d**, e.g. for optics:

> "... the emergent rainbow phenomena—the nature of the fringe spacings near the caustic—have the following features: (i) They are not reducible to either theory involved—neither to the more fundamental wave theory nor to the coarser ray theory. (ii) They are not explainable in terms of the more fundamental theory (though there are asymptotic explanations that are "grounded" in that theory). (iii) They are not predictable from the properties of the more fundamental theory (though they are in a well-defined sense asymptotically "contained in" that theory)." (p.120).

Batterman's unparenthesised formulation here expresses his tendency to talk of a "third theory" and of a new "ontology" in these cases, while his parenthesised remarks (notwithstanding their scare-quoted crucial terms) equally express denial of any such ontological reading. These forms of words were discussed earlier as central to Batterman's claim (2), especially the quote from pp.118-9. There we saw that they harbour a crucial ambiguity concerning whether or not mathematical asymptotics is a premise to the explaining, predicting or reducing. Without that premise claims of non-derivability are true

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properties from the limit T_c domain at all, they cannot be constructed from those resources. Rather, they arise from those of the T_f domain itself (p.118). So with respect to Batterman's focus on the asymptotic T_f domain, the issue is instead the emergence or not of asymptotic entities and properties from the basic entities and properties of T_f. But there he needs, but does not provide, a reformulated version of "basal conditions". Substituting broadened notions of constituent and entity so as to include waves as constituents of superpositioned entities and the like would preserve the spirit of Kim's conditions (including **a** and **b**) while including superpositions among wave components as the "basal conditions" for asymptotic optics, and the like. (The kinematical cases will remain exceptions, as they should.)

but uninteresting, while with the premise included they are false since the asymptotic phenomena are then derivable from wave optics conjoined to mathematical asymptotics—as the parenthesised passages quoted imply. In short, Batterman's treatment of **c** and **d** does not provide any well-defined, defensible notion of emergence.[9]

This conclusion is supported by Batterman's treatment of requirement **e**, where he concedes that the asymptotic domain does not exhibit any novel causal powers in the optics case (pp.126-7). Substituted for **e** is **e**′: Emergent properties figure in novel explanatory stories. These stories involve "novel asymptotic theories irreducible to the fundamental theories of the phenomena." (p.127). Again, this sense of irreducibility may refer to no more than the need for mathematical asymptotic approximations to be added. These "novel explanatory stories" could be just the usual explanations with asymptotic mathematics as an additional premise, the novelty being no more than the ontologically innocent constructed descriptions of caustics (etc.) referred to under claim (2) and irrelevant to emergence.

Batterman does discuss a sense of emergence in which asymptotic features clearly do emerge, and in ways associated with singular asymptotics, viz. "emerge from concealment" (p.125), the sense in which a figure emerges from the background once one approaches closely enough to resolve the detail. In this sense, asymptotic features emerge in the asymptotic domain under the use of asymptotic methods; these methods 'bring out' or make explicit patterns that were heretofore implicit in the domain. Alas, it is an unhelpfully liberal and imprecise analogy for its present purpose: in this sense emergence does not discriminate

[9] What they do reflect is the two aspects of the dynamical status of emergent constraints, as both determined by constituents and yet possessing distinctive causal power, discussed in note 6. Batterman wavers between them, I suppose, because he tacitly accepts the usually assumed dichotomy between these, and hence wrongly equates dynamical determination with logical derivability.

merely making explicit from constituting a new or novel existence.

However, an alternative, more adequate analysis of genuine emergence is implicit in the preceding analysis of reduction under claim (3), namely the formation of a new top-down dynamical constraint. For just this is characteristic of the emergence of a new existent: the iron top-down constraint formation constitutes the coming into being of an new, individuated capacity to do work, expressed both endogenously in dissipation of perturbations and exogenously in rigid-body action. It is the arrival of a new dynamical individual characterised by a new dynamical form. The dynamical cohesion created by such constraints is the ultimate foundation of all physical system identity (Collier [2002]) for it determines the substantive system boundary and with properties also dynamically individuated (Hooker [1981], Part II), the character of the new individual is constituted by its capacity to do new work. Otherwise we simply have the formation of a new pattern among dynamically unchanged components.

It follows that emergence is dynamically different from merely the formation of new inter-component patterns, it also requires change in dynamical form due to constraint formation.[10] Batterman's DP, while neat, results in using the term to refer to the formation of any supra-component pattern. Since this includes both interaction and superposed construction it conflates genuine interactive emergence with the mere emerging in time of a

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[10] If there are unique unchanging spatio-temporally local fundamental dynamical entities (i.e. a-toms) then there is no fundamental emergence, only existential emergents having these atoms as ultimate components in various dynamical combinations. But self-organisation of itself does not require this. Fundamental fields would yield the same emergent result, while mutant spatio-temporally local fundamental components, or cases where they or fields do not preserve their identity through the process of top-down constraint formation, would issue in fundamental kind emergence.

pattern ("from concealment"). Batterman's argument to the contrary has been examined above (under Kim's **c**, **d**, **e**) and rejected. Nevertheless Batterman also recognises that emergent constraint cases have differences from the wave optics asymptotic features (pp.123-4, 125 bottom), but does not grasp their import for his concerns. As noted, an immediate consequence of the self-organisation conception of emergence is that reduction not clearly succeeding and emergence occurring are not the same thing.

Batterman misses capturing this characterisation of emergence, I suggest, for the same reason that he mistakes absence of a premise (mathematical asymptotics) for failure of reduction, because he is (as is most analytic philosophy) too focussed on the formal logical structure of relations and too little focussed on their dynamical relations, relying on schema (2.6) to define emergence and reduction. The point is well illustrated by Batterman's remarks on levels. He entertains, seriously enough, metaphorical talk about upper and lower level theories, meaning simply more and less fundamental theories (p.116); this is a purely formal notion. At least Batterman notes that this sense of the term is quite inappropriate for discussing relations between material entities in emergence, which is a dynamical matter. However he fails to note any genuine role for dynamical levels in emergence, taking merely spatial (and perhaps temporal) scale as the key relation instead. But genuine level formation is formation of a new top-down dynamical constraint and hence is the dynamical fact of emergence. The iron bar is a new macro-scale level with respect to its molecular constituents because it has its own characteristic dynamical interaction form. All other talk of levels is either quantitative (liquid level), gravitational (level surface), or metaphorical (semantic, social, abstraction, theory ... levels) and can thus be paraphrased away—or is confused. Since level formation through top-down constraint formation involves singular asymptotics, Batterman turns out to be right to single out this category as to do with emergence, but wrong to suggest the formal position of singular asymptotics being coincident with emergence.

Batterman himself recognises that in the dynamical sense, talk of levels is inappropriate for most singular asymptotic relations, but does not discern the implications for discriminating among these relations.

Batterman is right to claim that the universality characterising the asymptotic process of forming such top-down constraints is central to explaining why new constraints yield the law-likeness necessary to support supra-component laws at all. (But, crucially, it is not sufficient, requiring also dynamical stabilisation.) And he is right to point out that this is a dynamical, not a formal, underpinning of law-likeness. But it does this job equally for explaining the universality of constructive pattern formation, as in the rainbow, where no existential emergence and no stabilised robustness is involved. Thus he misses the full significance of his asymptotic processes for emergence (and unity, see below).

B. *Multiple realisability*. But he does understand its dynamical significance for approaching the vexed problem of multiple realisability (see his clear exposition in Batterman [2000]). The dynamical grounding of universality Batterman describes has as a non-trivial consequence the proper framework for treating multiple realisability in relation to emergence. However, before it is properly able to neutralise multiple realisability as an objection to reduction, it needs to be refined by incorporating the preceding treatment of emergence, fortified with an extended suite of diagnostic tools, and extended to encompass an account of the nature of the special sciences. To all this I next turn.

The origins of the problem of multiple realisability lie in Nagel's formal account of reduction: T_2 reduces to T_1 if (T_1 and L_B) → T_2, where L_B states 'bridge laws'. L_B needs to state at least law-like connections between T_1 and T_2 if T_2 -law-like regularities are again to be assigned law-like status on the basis of T_1. I say "at least" because identities are still stronger assertions and so will also suffice, and are clearly what is to be desired if reduction is to effect, not just formal explanation, but a genuine reduction in ontology (cf. Hooker

[1981], Part I). But then it cannot be the case that there are radically heterogeneous multiple T_1 realisations of T_2 entities or properties, that is T_1 realisations whose descriptions have nothing in common across them all, because then L_B could not state law-like or identity relationships connecting entities and properties of T_1 with those of T_2 so that the patterns of T_2 can be fully explained and its terminology removed in favour of that of T_1 (cf. claim (2) above). Jade, for example, has been claimed to have two distinct chemical forms (jadeite and nephrite) with no commonality and hence no natural chemical kind corresponding to jade; whether or not this claim is correct, if it were correct no jade property or law could be retentively reduced to chemistry. In particular, the special sciences, like biology, psychology, economics and sociology, all involve theories about entities and properties that appear to have radically heterogeneous multiple realisations when described in physical terms. Money, e.g., can be anything from shells to shillings while still satisfying economic principles. So, it is concluded, the special sciences are irreducible to the basic natural sciences of physics and chemistry.

Batterman again structures his discussion of multiple realisation around Kim's work ([1992], [1998], [1999]). Kim, following Fodor, Putnam and others (e.g. Fodor [1974]), frames the issue in terms of natural kinds: laws and identities relate natural kinds, but radically heterogeneous disjunctions of descriptions are what is forced on L_B and these do not describe natural kinds, so reduction fails. Alternatively, Kim proposes two natural kind principles, the first stating that natural kinds are individuated by their causal powers (cf. the same position argued in Hooker [1981], Part II) and the second stating that the causal powers of a T_2 property are, on each realisation occasion, identical with the T_1 powers of the realisation (his 'causal inheritance principle'). But then it follows that radically heterogeneous T_1 descriptions cannot be realisations of the same T_2 property. Whence reduction fails. In particular, the would-be kinds of the special sciences must either be counted as new emergent causal kinds or denied the status of proper scientific kinds. Kim opts for the latter (Kim [1992], p.18, quoted at Batterman p.67), whence the special sciences, like theories of jade, turn out not to be sciences at all. Next note with Batterman (pp.71-3) that another version of the same problem appears as the 'metaphysical mystery' (Fodor [1997]) of how it is possible both that T_2 theories, e.g. psychology, can state laws and yet have radically heterogeneous T_1 realisers. Both claims seem right, but what, as it were, binds all of the diverse T_1 realisations into participating in the same T_2 lawful regularity?

To resolve this mystery Kim ([1998], [1999]) adopts a 'functionalising' approach that, as Batterman notes, is designed to ensure that all well-founded special sciences will reduce to the basic natural sciences. The root idea is to generalise the following kind of example: 'Water soluble' is the (second-order functional) property of having some (first order substantival, dynamical) basis in virtue of which if an entity exhibiting that basis is placed in water (input condition) it will dissolve (output condition). Generalising, the approach construes the terms of the special sciences functionally, that is as the (second-order) property of having some (first order) basis that realises a given input-output relation (the function). Such input-output conditions can be satisfied by radically heterogeneous bases. In that case for each basis they can be re-described in those terms, producing a restricted homogeneous class of basis instances; the functional property can then be reduced, on each occasion, to its realising basis on that occasion. This approach grows out of a long functionalist tradition in theory of mind and is essentially a generalisation of Armstrong's analysis of a mental state (Armstrong [1972]) as that condition apt to be caused by specified stimuli (input condition) and to give rise to specific behaviours (output condition). Hooker ([1981], Part III) had developed essentially the same functional analysis as Kim but applied it to complex systems properties generally, proposing to reduce the special sciences in the same way as Kim by construing them as theories of functional properties of complex systems. Both note that there will then be natural laws involving functional properties only to the extent that there are unifiable treatments of realising bases. Further, both discuss genetic reduction as an example, Hooker setting out schematically the complex dynamical conditions for demonstrating that transmission genetics (functional genes) is reducible to DNA bio-chemical genetics (chemical genes) and Kim ([1999]) noting that the hard part scientifically is establishing that DNA-based biosynthetic pathways are (imperfect) realisers of functional genes.

Batterman finds Kim's approach inadequate. Batterman's position is that it is the nomic status of universal phenomena that prevents reduction but also opens the only way to explaining multiple realisability, but Kim's functionalising misses all this.

> "The reduction afforded by Kim's functionalist model is a species—or structure—specific reduction. We have, for instance, reductions of human psychology to human physiology (and ultimately to physics) and reductions of reptilian "psychology" to reptilian physiology (and ultimately to physics). We do not, however, have a reduction of psychology to physics." (p. 70)
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> "... [Kim's] model fails to account for the important—universal—nature of the phenomenon ... We want to know whether ... law-like patterns of light in the neighbourhood of caustics are reducible to law-like relations in the wave theory. At best ... we can say the following ... If the phenomena are functionalisable, we will have accounts of why individual instances of the patterns are displayed on given occasions. But this ... in no way provides a reductive account of the law-like universal pattern itself." (p. 120)

About this three things need to be said, in the light of the preceding account of emergence. (1) Batterman is right that functionalising does not provide a basis for deriving universal phenomena. And its not doing so runs deeper than the need to adjoin asymptotic extraction theory as an additional premise (recall the criticism of claim (1)). Functionalising provides only symbols empty of substantival content; while this ensures they will admit diverse substantival realisers, indeed that they place no limits on the degree of radical heterogeneity of their realisers, it does not provide those realisers and so cannot provide any dynamical basis for extracting universal phenomena.

(2) Even so, functional specification will not go away, and in particular areas it may be most or all of the story. It will not go away because the dynamics itself supports such abstractions. For instance, for each dynamically stabilised (emergent) property P of an emergent system S_e there will be an associated class of true functional claims of the sort 'For variations to (perturbations in) the basis conditions within some domain D_{var} as input there is a constant emergent output E_P' and a further class or classes of true functional claims of the sort 'If S_e has input I then it generates output O(P, I)' deriving from P's nomic roles in S_e's dynamics (cf. rigidity in rigid body dynamics). Functional claims may be most or all of the story in some areas since, to the extent that information about the underlying dynamics is absent in an area, to that extent functional properties and laws are all that are epistemicly available. This was substantially true in genetics until molecular genetics was established, before that functional transmission genetics prevailed. It still is largely true for psychology, reinforced by ancient and recent anthropocentric assumptions about the independence of psyche from matter, though the neurological sciences are now progressing apace. And use of functional laws will persist as a matter of justified convenience in many areas, for the same reasons that engineers build bridges using near-rigid body mechanics and not quantum mechanics. That said, functional theory is necessarily limited, e.g. it cannot understand its own formation and breakdowns; for that it requires a dynamical foundation.

(3) The dynamics underlying function undoes Kim's position. While Batterman correctly rejects the disunified outcome of Kim's position he doesn't pursue its origins to the

dynamical principle at stake. But, contrary to what both Kim and Batterman suppose, functionalising does not preclude the unity of universal phenomena. The issue is the extent to which there are unifiable treatments of realising bases. Here there is a crucial ambiguity in 'unifiable', as between substantival and dynamical unification. As noted, Kim ([1992]) adopts a 'causal inheritance principle' to the effect that the causal powers of an emergent property in some situation S are identical with those of its realiser in S. But then diverse realisers across differing situations will lead to diverse emergent causal powers for P. Whence the above disunity in reduction. But we know from the empirical reality of asymptotic universality that the very same property, with its distinctive causal powers, can in fact be formed from diverse realisers (e.g. across diverse thermal fluctuations, some properties even realised in both magnetic and fluid domains). This comes about because universal phenomena are *dynamical* emergents, so that substantivally diverse composites can in fact bring about the same emergent dynamical state.

At the least this shows that the statement of the causal inheritance principle is ambiguous as between meaning that, whatever powers the composite has, it only has because it *formed from* the particular components of that composite and meaning instead that the powers of a composite are logically *derivable from* the powers of its substantival components. While the former version is true but trivial, the latter version (which would serve to prevent cross-diverse-realiser common powers) is false (note 6). More importantly, the applicability of this dichotomy is refuted by the occurrence of robustly stabilised universal dynamical phenomena, i.e. by the occurrence of emergent properties with their own top-down causal power, for such emergents are not logically derivable from those of the constituents yet are dynamically determined by them.[11]

[11] See note 6, cf. note 9. The same status indicates an equivalent coarseness to Nagel's use of logic to formulate a reducibility condition. Since logical derivability of the new constraint

While Kim's functionalising indirectly acknowledges, but cannot adequately introduce, the dynamical foundations for multiple realisability, a preceding account (Hooker [1981], Part III) does offer dynamical conditions for reduction that explicitly introduces the dynamical underpinnings of function. The simple core idea is to represent functions as input-output maps and for each map to construct a matching dynamical process connecting input to output. Consider a system S with input I, output O and dynamics D. Then S will have functional properties $f_{S, m} = m: I_m \rightarrow O_m$, for every map m of some input feature into some output feature. For the case of dissolving in water, e.g., we have properties $f_{S, m}$ = 'dissolving', I_m = S is placed in water, O_m = S is dissolved in the water and m = the map that carries the former condition into the latter. Underlying the operation of m is a dynamical process $P_{D, m}$ that physically converts S from undissolved to dissolved form. This process will not be the same in every detail from one instance of dissolving to the next, or from one kind of soluble substance to the next, but there will be successively more generic dynamical specifications of the process that will capture what is common across these various classes, e.g. in terms of ion formation. We can then identify a particular dissolving with its most specific dynamical process $P_{D, m}$ on that occasion, and the generic dissolving process for some class of cases with the corresponding generically specified $P_{D, m}$. Roughly, the dynamical coherence of the determinate/determinable hierarchy of all such interrelated specifications then provides the warrant for asserting the reduction of each $f_{S, m}$ to its $P_{D, m}$, and the structure of $P_{D, m}$ at different degrees of genericness provides the relational structure for a functional

(whether described in T_2, or in T_1) from descriptions of constituent-constituent interactions (in T_1) fails there is emergence in Nagel's terms, but this criterion cannot further distinguish cases of top-down constraint from others, or distinguish cases where the dynamical state of the constituents (described in T_1) dynamically determines the constraint (the T_2 outcome) from those where it does not.

decomposition of $f_{S, m}$ (as per Cummins [1983], cf. Hahlweg/Hooker [1989], section IV.3).

But while this may seem conceptually straightforward, as noted the difficult part is the empirically valid construction of the $P_{D, m}$ processes. Even in this simple case, we must discover how to characterise the input and output conditions dynamically to match their functional characterisation, and then construct a model of the dynamical process $P_{D, m}$ with sufficient dynamical power to model differences across different chemical elements in a way consonant with basic chemical principles. All of this in fact requires developing the atomic-molecular theory of chemistry; prior to that it could not be done. But in this example the route from molecular chemical activity to macroscopic result is relatively straightforward, it simply goes by ion or molecular density. Genes, however, play out complex roles grounding still more complex biosynthetic pathways spanning many layers of system organisation before their presence can be appreciated in the often distantly removed phenotypic features. In this case there are a great many dynamical conditions to meet before seemingly simple functional ascriptions like ‘the gene for black hair dominates that for brown’ can be provided a principled dynamical basis and the conditions for reduction shown to be met. The basic explanatory strategies are, however, the same in the two cases.

The dynamically structured collection of $P_{D, m}$ processes provides the dynamical underpinnings of $f_{S, m}$, including the top-down constraint conditions that allow functions to be well specified—e.g. dynamically well specified system boundaries that permit well specified inputs and outputs and directed energy interactions combined with top-down constraints in a way that permits well specified processes. Think here of complex intra-cellular chemical organisation. Note in particular that there is generally no reduction in complex systems without underlying emergence, for top-down constraints are needed to order system processes. Nonetheless those constraints can at least be reduced to ionic bonded lattices of various kinds. Reduction is intricately interwoven with emergence. Only the full dynamical foundations can reveal these interrelations and the consequent functional organisation. It is these conditions that are required, e.g., to capture the very complex reduction and emergence conditions for genetics.

The basic reduction requirement, that functional maps are mirrored by dynamical maps, is in fact just the application of Nagel's deductive reduction conception, rightly understood. Nagel shows how scientists arrive at reduction of a law L_2 or property P_2 of theory T_2 respectively to a law L_1 or property P_1 of theory T_1 by first showing how to choose conditions (real or idealised) under which it is possible to construct in T_1 a law L_1 or property P_1 that will mirror (be a relevantly isomorphic *dynamical* image of) the dynamical behaviour of L_2 or P_2. From that the reduction is shown possible through the identification of L_2 or P_2 with the mirroring L_1 or P_1. Indeed, the requisite 'bridging' conditions can be deduced from the mirroring condition, and then asserted as identities on the basis that doing so will achieve a reduction, supported in that light by claims of spatio-temporal coincidence or appeal to Occam's razor. This is the position taken in Hooker ([1981]). It has recently been re-explained clearly by Marras ([2002]), using Nagel's discussion of the Boyle-Charles law reduction. Marras remarks that the so-called 'bridge laws' are not central to the achievement of reduction but merely formally required to form the deduction of T_2 laws and properties from T_1 laws and properties. Marras shows how this understanding both invalidates Kim's earlier criticisms of Nagelian reduction and demonstrates that Kim's most recent functionalising approach is after all essentially equivalent to Nagel's.[12] Marras' most pointed criticism of Kim's functionalising position is its inability to coherently resolve the problem of multiple realisability (cf. Batterman above). I shall now try to show that, with the

[12] Marras [2002] also adds some further formal semantic problems unresolved in Kim's version, but these are resolved, I think, through the dynamical analysis of multiple realisability below.

dynamical mirroring condition backed by the dynamical account of emergence in hand, a dynamically based analysis will allow us to do decisively better.

To properly understand the dynamical conditions under which multiple realisability may occur, it is essential to distinguish three different kinds of cases: (I) multiple realisability internal to a determinate/determinable hierarchy, (II) multiple realisability arising from dynamical emergence and (III) multiple realisability that violates the relevant determinate/determinable hierarchy. I shall argue that (a) each of these three cases exhibit a heterogeneous multiple realisation relation, but none exhibit the same relation, (b) the case (I) relation is non-radical, in the sense that it is indifferent to reduction and emergence, being compatible with them both, (c) the case (II) relation is the only proper underpinning of multiple realisability, but is also non-radical because it is maximally naturalisable, its interleaved reduction and emergence being fully dynamically integrated, and (d) the case (III) relation is the only candidate for being a radical heterogeneity of the sort that raises a problem for the account of reduction and emergence provided here, but there is as yet little or no reason to suppose that it actually occurs.

Case (I): multiple realisability internal to a determinate/determinable hierarchy. We too facilely attribute highly generic functions without thought to that fact, saying e.g. that heating water has the function of boiling it. This may seem an innocuous physical process but we face the problem that at the molecular level there are very many specific ways to inject heat into water and for water to boil, so that the functional property apparently has radically heterogeneous realisations and thus 'floats free' of its molecular base. The proper response to this is to note, as with dissolving (above), the hierarchy of increasingly generic or determinable descriptions possible for *both* functional *and* dynamical processes and that these can be matched up across the two determinate/determinable hierarchies, so that reductive identifications can proceed for each of them (see Hooker [1981], Part III for discussion).

However, some may object that this still leaves heterogeneous realisations since there are still many different specific dissolving or boiling processes that realise a generic or determinable dissolving or boiling process, irrespective of whether these are described functionally or dynamically. This is true, but is not an instance of radical heterogeneous realisation. Every instance of every determinate/determinable hierarchy inherently shows this same diversity of realisation. For instance, sky blue, ultramarine and many other qualitatively distinct specific shades of blue all instantiate blue. The multiple realisability lies in the nature of the determinate/determinable relationship, not in the reduction or otherwise of the states it is used to characterise. Every state is described by a multi-dimensional determinate/determinable lattice of descriptions generated by logically conjoining (taking the Boolean product of) all the determinate/determinable descriptive hierarchies that it instantiates. I call this internal multiple realisation (or 'internal cross classification' in the language of Hooker [1981], Part III). The metaphysics of determinable properties is certainly contestable across the nominalism to Platonism spectrum but, as we shall see, this is a quite different issue from radical heterogeneous realisation.

Case (II): multiple realisability arising from dynamical emergence. This is the class of cases that show heterogeneous realisations of an emergent property by its constituents. The reason is immediate: an emergent property is a robustly stabilised supra-constituent (or relatively 'macro') constraint, hence it must be capable of dynamically 'filtering out' constituent (or 'micro') fluctuations so that the macro constraint is preserved; thus many different collective constituent or micro states will satisfy the same macro constraint and each of these will count as an alternative realisation of the macro constraint property. Since constraint formation is characterised asymptotically, this stability under micro fluctuations is picked out by (robustly stabilised) asymptotic universality (see claim (3) above).

For example, a Geiger counter registers the presence of radiation by making use of the fact that a sufficiently fast moving charged particle will split a gas molecule into charged parts (ionise it). The counter has a gas chamber with a strong electric field in it so that when a charged particle entering the chamber ionises gas molecules the field accelerates the resulting charged parts causing a cascade of ionisations that is registered as an electrical pulse (and derivatively, a sound). Whence it follows that Geiger counter triggerings can occur in widely heterogeneous manners; counters can be triggered by an entering radiation of various sorts (photon, electron, alpha particle, etc.), but also by an internal gas molecular collision creating ions, by poking a hot wire into the chamber, and so on. Any physical process initiating gas ionisation suffices to generate Geiger counter triggering and such causes can be very diverse. Dually, the physical amplification and cascade detection processes insure that none of the particular or distinctive properties of the initiating causes appear in the effect, the electrical output pulse; that pulse is the same for all of them, filtering out their particularities. Without this dynamical filtering the heterogeneity of initiating causes would be removed.[13]

Thus the property of being in the triggerable condition (chamber gassed, correctly wired up, field applied, etc.) can be quite variously molecularly realised, the chamber gas molecules at different temperatures and with different local fluctuations, the chamber electric field varying spatially in modest ways, and so on. Nonetheless none of this variation appears at the macro counter level, its particularities have also been similarly filtered out.

The filtering process was deliberately designed into the Geiger counter and is central to its useful, reliable operation.[14] But while the commonness of such processes may lead to

[13] Note that it need not have been this way; the basic dynamical properties of the ionising process are carried forward into the immediately resulting ions and it is a particular dynamical feature of cascade formation and electrical detection registration that it filters out this information.

[14] Such processes are the central feature of current human engineering, for they simplify and

our overlooking them, it does not make them any less real or functionally radical in their consequences. They are in fact top-down dynamically stabilised constraints—the dynamical equations governing cascading in a Geiger counter issue in these consequences. In the same way, diverse kinds of perturbations to the molecular constituents of an iron bar nonetheless all leave the macroscopic iron shape and rigidity unaltered. The Geiger counter constraints have the same top-down character and dynamically stabilised status as those formed under phase changes (because they were) and likewise constitute a distinctive supra-component, emergent dynamical level with respect to the component processes occurring in the chamber. In particular it is the dynamically stabilised character of these constraints that explains: (i) why they can act as the basis for lawful generalisations about counter triggering readiness and triggered pulse outputs and (ii) why all successful inputs must share certain dynamical features in common, namely those sufficing to dynamically trigger the counter and so bring about an output. These are the key dual nomic and functional properties of the input-output relation. It is the basis of the apparently independent laws of the macroscopic realm that is so useful in a complex world.

Universality, while a decisive step toward providing dynamical underpinnings to functions, does not suffice in itself to provide these key conditions. Asymptotic extraction of invariant pattern only tells us that there is some principled reason that physical details can be ignored but it does not in itself make that reason explicit; since universality covers both cases with and without top-down constraint power, we require further dynamical grounding for the former cases yielding genuinely dynamically grounded heterogeneous realisations. We can confidently generalise about shared Geiger counter triggering conditions and causes (inputs)

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render reliable what would otherwise be extremely complex regulatory problems. Rigidity is the commonest filtering device; if buildings were bodies their engineering would be as complex and risky as medicine—but they would be a lot more adaptable.

because the dynamical process of cascade amplification and electrical conversion is stabilised against molecular level variations in the counter and initial ionising process, and this insures that only (but all) those processes capable of causing ionisation in the chamber will trigger the counter. Dually, we can confidently generalise about Geiger counter detection events (outputs) because of the same active dynamical insensitivities. And the resulting electrical pulses, in virtue of their shared physical properties, will also share various other law-like roles, among them causing sound wave bursts in audio speakers. (The remaining cases of universal pattern extraction without top-down causal power, like the rainbow in wave optics, do not provide genuine cases of realisation, just of sub-patterns of a pattern.) This not only provides a more precise dynamical treatment than Batterman's universality alone permits, we no longer need to join Batterman in avoiding reduction of the Geiger counter level (cf. special sciences) by weak appeal to the negative proposition that asymptotics does "not provide sufficient conditions for the universal behaviour" (p.75). While true, at least in all the vast range of cases of the Geiger counter sort the requisite dynamical underpinnings can be provided and show precisely where and how dynamically based emergence occurs.

While real emergent dynamical filtering insures that macroscopic counter properties like 'is in the triggerable condition' have heterogeneous microscopic molecular realisations, it also insures that counter properties will be coextensive with suitable generic molecular descriptions, e.g. 'is a counter trigger' with 'has the capacity to produce ions in the chamber' and similarly for processes and functions, e.g. 'is triggering' with 'is producing an ionisation cascade in the chamber'. In so doing the dynamics provides the grounds for reductively identifying these pairs. This includes the emergent constraints themselves, e.g. 'chamber'; while they cannot be further reduced to just aggregate constituent patterns, they can be reductively identified in constituent terms, e.g. a chamber as a bonded molecular lattice of particular dynamical character. This both grounds chamber electrical functioning, and reveals the sameness of fundamental kinds. In these ways emergence dynamically underpins reduction. But it is no physical mystery at all when the correct dynamical model of emergence is to hand since it is precisely what the filtering consequent upon formation of a new dynamical constraint provides.[15] And it is precisely on that general basis, and only on that basis, that we can at all track causal paths 'up' and 'down' through the component/supra-component levels (e.g. from entering radiation to counter triggering). In this way we are able to understand the principle behind the Geiger counter, and so design and build one—without removing its dynamical filtering. But equally we are enabled to understand (up to current ignorance) what is behind the similar operation of the nervous system and other bodily organs, passing from sub-cellular biochemical to cellular to multi-cellular and organ levels and back, and plausibly similarly for economic and social processes (see also special sciences and unity of science, below).

However, loose semantic analysis can still create the illusion of a problem here. In the present case, it would be a mistake to simply assume that 'being a Geiger counter trigger' was a 'logically simple' property and so treat it as a dynamically unified property, semantically construing "X is a Geiger counter trigger" as "There is a kind GT ("Geiger triggers") such that X is a member of GT". This will remove any molecularly specified natural kind shared across counter triggering conditions, ensuring that Geiger counter triggers even appear to be radically heterogeneously realised, and ensuring that GT laws will form a 'special Geiger counter science' that is independent of molecular laws. But if instead we set the truth-making conditions of "X is a Geiger counter trigger" to be those of "There is some causal process within the Geiger counter such that X causes triggering" (or refinements

[15] Block's insistence that there could not be heterogeneous realisations so radical as to share no physical properties at all across them (the Disney Principle, Block [1997], p.120), was a step toward this analysis.

thereof providing more of counter internal organisation) then this appearance of radically heterogeneous realisations disappears, for all instances of that process share the dynamical capacity to ionise the gas in a counter and, the dynamics itself informs us, this suffices. This provides a dynamical warrant for this form of semantic syncategorematic reconstrual of sentences referring to Geiger counter triggers.[16]

The syncategorematic reconstrual then allows us to construct suitable determinate/determinable hierarchies for describing the phenomena. For instance, corresponding to the large number of more specific molecular ways that the generic Geiger counter trigger process can be realised there are something like the hierarchy of properties 'triggers the counter with a pulse that builds in manner P_j', where P_j varies in degree of dynamically generic specification. In this way the heterogeneous realisations are internalised to a dynamically principled determinate/determinable hierarchy (one grounded in the dynamics of the counter processes). This consistently extends to reduction of macro counter to micro molecular processes, since as before the counter triggering determinate/determinable hierarchy reduces to the gas molecular process determinate/determinable hierarchy. (And as before the counter constraints remain emergent.)

The many molecularly specific Geiger counter triggering (input) conditions that cause the same firing (output) condition provides a paradigm for the analysis of all many-one causal relations. Each such relation is understood by revealing some common dynamical condition (some generic property) that dynamically suffices to generate the output condition and that filters out the remaining heterogenous conditions from affecting the output state. But

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[16] See Hooker ([1981], Part III), which also discusses Dewan's similar example of electrical governors, relating it to the functional analysis of genetic ascriptions ('has a dominant gene', etc.) and Armstrong's functional analyses of mental ascriptions.

while there is genuine physical diversity of cause yielding the same effect in these cases, it is important not to mistake that relation itself for a case of heterogenous realisation, radical or not. Effects don't realise their causes in any relevant sense. The multiple realisability lies in the input itself, and precisely because of the filtering. This is normally obscured by purely formal analyses.

In sum, case (II) presents a dynamically based heterogenous multiple realisation (plus a many-one cause-effect relationship that is not a relevant form of realisation) that can be fully identified at, though where emergent not reduced to, the micro level; in consequence it can be equipped with a dynamically grounded semantic syncategorematic reconstrual of terms under which the heterogenous multiple realisation is internalised to a dynamically principled determinate/determinable hierarchy. We may say that the multiple realisations have been maximally naturalised, for the irreducible component that grounds their occurrence has been integrated into a single principled dynamical account of their domain. Thus understood, they can act as an effective model for approaching the analysis of phenomena containing multiple emergent levels, particularly those found in biology, psychology and the social sciences. [17]

Case (III): multiple realisability that violates the relevant determinate/determinable

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[17] This provides a more principled and more satisfying solution to incorporating multiple realisability into a theory of reduction, I suggest, than the proposal of Marras [2002] to simply give up reduction for a one-way, many-one homomorphic mapping from T_1 to T_2. Marras' proposal unnecessarily sacrifices identities and so both the ontological goals of reduction and the dynamical structure of scientific unity (see below) and fails to distinguish between naturalisable and radical heterogeneous realisations. However, were the latter forced upon us as the so-called special sciences matured then we would be driven back to something like his position.

hierarchy. These are the unnaturalisable cases, not simply because they display emergent, irreducible components, but because the realisations cannot be integrated into a single principled dynamical account of their domain. Davidson's anomalous properties, ones that do not appear in integratable dynamical laws, are examples, as is the earlier jade example where the jadeite/nephrite differences are supposed to defy dynamical unification within chemistry. If both jadeite and nephrite shared a common chemical constitution that was causally responsible for the common jade properties and each differed only in further determinations of this constitution then, as with Geiger counter triggers, they would stand to jade in a genuine determinate/determinable relationship and the reduction would proceed by identifying jade with the determinable constitution and jadeite and nephrite with the more determinate constitutions. It is only when the multiple realisation violates or stands outside this relationship, is in that strong sense external multiple realisation (or cross classification), as it is hypothesised to do in the jade case, that one obtains radical heterogeneous realisation. This radical structure defeats systematic scientific treatment, and is the sort of anomaly often attributed to mental, social and other 'personal' properties. But consider that if the above analysis of emergence and emergent filtering stands then (i) science has evidently not needed to recognise any cases of radical heterogeneous realisation within its most mature disciplines (physics, chemistry) and (ii) the naturalising analysis above must raise reasonable expectations that this will extend, albeit more complicatedly, to biology, social theory and the like as well where our capacity to maturely understand the multiple emergent levels involved is only in its infancy.[18]

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[18] This has not, however, stopped some philosophers and others from making strong claims about these domains. An important case in point for reduction is the (usually tacit) assumption of a synonymy criterion of property identity: $P_1 = P_2$ if and only if P_1 is synonymous with P_2. For example Kim ([1998], p.98) says: "If *M* and *P* are both intrinsic

C. *Unity of science and special sciences*. The emergence of new existents governed by their own distinctive dynamics may seem tantamount to generating a thoroughgoing disunity to science (cf. e.g. Dupré [1993]). But careful dynamically-based analysis shows that, while irreducibility and emergence is a real feature of science, the threat of radical disunity is greatly exaggerated. Rather, an elegant and powerful variety-within-unity is generated by dynamical emergence. Here the paradigm is the fully naturalised Geiger counter whose inter-woven emergence and reduction provides an integrated dynamical unity encompassing both the special laws characterising the Geiger counter level and the grounding molecular-level dynamics. Generalising, first note that whenever there are fundamental unchanging dynamical kinds there will be no fundamental kind emergence for it is in terms of such ultimate component-level kinds that the asymptotic processes yielding universality are formulated (cf. note 10). Second, while there may be a wide variety of dynamically emergent existences, each instantiating its natural kind and forming the basis for a 'special science' of it, these will be dynamically determined by, and identified in terms of, their dynamical constituents and governed by laws which themselves are thus grounded in the underlying dynamics. All this provides a shared dynamical framework interconnecting

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properties and the bridge law connecting them is contingent, there is no hope of identifying them. Distinct properties are just distinct, and we can't pretend they are the same…" Kim relies on this centrally in his argument against the reduction of mind. But Hooker ([1981], Part III) argued that on even a general realist approach to language (like Armstrong's) there is every reason to abandon this criterion. Moreover, a biologically grounded approach to cognition provides a supporting conception of semantics (see Christensen and Hooker [2001]) quite different from any that might encourage the ignoring of dynamical grounding of property semantics evidenced both here and in the ignoring of dynamically compound properties discussed under syncategorematic reconstrual above.

emergent variety in intimate ways that make it possible to successfully model complex dynamics and navigate through our complex world of emergent but interconnected levels and laws.[19]

However, detailed computational navigation is defeated, and the resulting scientific unity is made more complex, by another dynamical feature: the formation of supra-component constraints, like the iron bar crystal, is computationally inaccessible to component-level analysis (note 3 and text). In consequence the sciences of the emergent entities are not therefore wholly logically determined as simply particular cases of general scientific (physics and chemistry) laws (note 6). This explanatory restriction propagates. The emergence of a new constraint with new dynamics may lead to the subsequent dynamical formation of still further top-down constraints, and so new entities, that would not have been dynamically possible without that preceding formation event. Moreover, this cascade of dynamical consequences is marked by its initiating formation event and thus exhibits dynamical fixation of (these) historical constraints. If the emergent dynamical forms all lie within a single branch of the same scientific domain, as all rigid body mechanics lie within the domain of simple mechanics, then related general laws of the same type will again apply, e.g. that rigid bodies behave dynamically as if concentrated at their centres of mass. But there will be vast ranges of phenomena where this last simplification does not hold, e.g. throughout biochemical formation of biological entities and no doubt throughout the 'special sciences'.

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[19] see further note 7. Kim's general rejection of the special sciences (Kim [1992]) no longer follows because in these cases the diverse realisations that make up a special science kind all do share something in common and, on the assumption that the special science has genuine, dynamically grounded laws, the shared condition will suffice to dynamically determine the special science laws involved. However, with Kim, generalisations formed on the basis of radical heterogeneity of realisation (if any) will not form naturalisable special sciences.

Here we find very complex patterns of constraint-dependent dynamics generating complex histories of emergent phenomena marked by strong dynamical fixation of historical constraints where dynamical form may change as a system—including even just its initial conditions—changes. This means that it is impossible to form simple first order laws about such domains, as is the norm in basic physics and chemistry; rather all such laws become strongly constraint-, and so historically-, dependent. In addition, because of their internal dynamical diversity these systems will often display diverse responses to the same situations, increasing the complexity.[20] These twin heterogeneities provide the core reason why special sciences occur, are irreducible, distinctive and analytically complex. Yet these conditions too are ultimately explained by fundamental dynamics. The upshot is that dynamics grounds a unity to scientific laws but this unity is internally very complex (see also Hooker [2000]).

6 Conclusion

As an old physicist I have long approved Batterman's philosophical attention to the neglected study of asymptotics and this book is replete with rich explorations of its important results, e.g. the wonderful discussion of rainbows and catastrophe optics. But from a philosophical point of view what is important about Batterman's discussion of asymptotics is the way it moves the analysis of reduction and emergence away from purely formal foundations and toward more dynamical foundations. Yet not far enough. What I have striven to show here is how a more thoroughly dynamical approach can bring clarity, precision and a rich order to this field, and resolve some significant philosophical problems along the way.

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[20] But this latter variety should not be mistaken for the former kind. And we may reasonably hope that it too will provide only non-radical, naturalisable heterogeneities.

helpful quality control.

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References

Armstrong, D.M. [1972]: *A materialist theory of mind*, New York: Humanities Press.

Batterman, R.W. [2000]: 'Multiple realisability and universality', *The British Journal for the Philosophy of Science*, **51**, 115-45.

Batterman, R.W. [2002]: *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction and Emergence*, Boston: MIT Press.

Block, N. [1997]: 'Anti-reductionism slaps back', *Philosophical Perspectives*, **11**, 107-32.

Cherniak, C. [1986]: *Minimal Rationality*, Boston: MIT Press.

Christensen, W.D. and Hooker, C.A. [2001]: 'Self-directed agents', in J.S. MacIntosh (*ed*.), 2001, *Naturalism, Evolution and Intentionality*, Ottawa: *Canadian Journal of Philosophy*, Special Supplementary Volume 27, pp.19-52.

Churchland, P. [1979]: *Scientific realism and the plasticity of mind*, London: Cambridge University Press.

Collier, J. D. [1988]: 'Supervenience and reduction in biological hierarchies', in M. Matthen and B. Linsky (*eds*.), 1988, *Philosophy and Biology,* Ottawa: Canadian Journal of Philosophy, Supplementary Volume 14, pp. 209-234.

Collier, J.D. [2002]: 'What is Autonomy?' *Select Proceedings, CASYS'01: Fifth International Conference on Computing Anticipatory Systems, International Journal of Computing Anticipatory Systems*: **12**.

Collier, J.D. and Hooker, C.A. [1999]: ‘Complexly Organised Dynamical Systems’, *Open Systems & Information Dynamics*, **36**, 1-62.

Cummins, R. [1983]: *The nature of psychological explanation,* Boston: Bradford/MIT Press.

Davie, G.E. [1961]: *The Democratic Intellect*, Edinburgh: Edinburgh University Press.

Dupré, J. [1993]: *The disorder of things: metaphysical foundations of the disunity of science*, Cambridge, Mass.: Harvard University Press.

Fodor, J. [1974]: ‘Special sciences, or the disunity of science as a working hypothesis’, *Synthese*, **28**, 97-115.

Fodor, J. [1997]: ‘Special sciences, still autonomous after all these years’, *Philosophical Perspectives*, **11**, 149-63.

Hahlweg, K. And Hooker, C.A. [1989]: ‘Evolutionary epistemology and philosophy of science’, in K. Hahlweg and C.A. Hooker (*eds*.), 1989, *Issues in evolutionary epistemology*, Albany, N.Y.: State University of New York Press, pp. 21-150.

Hooker, C.A. [1979]: ‘Critical notice of R. Yoshida, *Reduction in the physical sciences*’, *Dialogue* **XVIII**, 81-99.

Hooker, C. A. [1981]: ‘Towards a general theory of reduction’, *Dialogue* **XX**, ‘Part I, Historical framework’, pp.38-59, ‘Part II, Identity and reduction’, pp.201-36, ‘Part III, Cross-categorial reduction’ pp. 496-529.

Hooker, C.A. [1991]: ‘Between formalism and anarchism: a reasonable middle way’, in G. Munevar (*ed*.), 1991, *Beyond reason: essays on the philosophy of Paul Feyerabend*, Boston: Kluwer, pp. 41-107.

Hooker, C.A. [1994]: ‘Idealism, naturalism and rationality: some lessons from minimal rationality’, *Synthese*, **99**, 181-231.

Hooker, C.A. [2000]: ‘Unity of Science’ in W. H. Newton-Smith (*ed*.), 2000, *A Companion to the Philosophy of Science*, Oxford: Blackwell, pp. 540-9.

Kim, J. [1992]: 'Multiple realisation and the metaphysics of reduction', *Philosophy and Phenomenological Research*, **52**, 1-26.

Kim, J. [1998]: *Mind in a physical world: an essay on the mind-body problem and mental causation*, Boston: MIT Press.

Kim, J. [1999]: 'Making sense of emergence', *Philosophical Studies*, **95**, 3-36.

Marras, A. [2002]: 'Kim on reduction', *Erkenntnis*, **57**, 231-57.

Petersen, A. [1968]: *Quantum physics and the philosophical tradition*, Boston: MIT Press.

Sklar, L. [1967]: 'Types of inter-theoretic reduction', *The British Journal for the Philosophy of Science*, **18**, 109-24.

Yoshida, R. [1977]: *Reduction in the physical sciences*, Dalhousie: Dalhousie University Press.

TABLE 1

ASYMPTOTIC RELATIONS AND REDUCTION

Reduction failing is indicated by cells with grey backgrounds.

Emergence is indicated by bold face type.

Regular asymptotics T_f asymptotic and T_c limit domains	Singular asymptotics T_f asymptotic and T_c limit domains
pendula gas laws Relativistic/Newtonian	**Quantum/Newtonian** **Wave/Ray Optics** **phase changes** **critical phenomena**

TABLE 2

ASYMPTOTIC, COMPONENT/WHOLE AND INTER/INTRA THEORY RELATIONS

	Regular asymptotics		Singular asymptotics	
	Properties and no component/ whole relation	Properties and component/ whole relation	Properties and no component/ whole relation	Properties and component/ whole relation
Inter-theory asymptotic relation	Newtonian/ relativistic mechanics		Newtonian/ quantum mechanics wave/ray optics	Thermodynamics/ statistical mechanics
Intra-theory asymptotic relation	Pendula, gas laws	Aggregates		Solid/n-body molecular lattice

TABLE 3

ASYMPTOTIC RELATIONS AND REDUCTION/EMERGENCE

Asymp. = asymptotic(s), dom. = domain

DI = degenerate idealisation

TCF = top-down constraint formation

Kin = kinematical

DynC = dynamical construction

DynI = dynamical interaction

Rel/Newt = Relativistic/Newtonian

Qu/Newt = Quantum/Newtonian

W/R Op = Wave/Ray Optics

Phase chs = Phase changes

Reduction not clearly holding is indicated by cells with grey backgrounds.

Emergence is indicated by bold face type.

			Regular asymp.		Singular asymp.	
			T_f asymp. dom.	T_c limit dom.	T_f asymp. dom.	T_c limit dom.
Non-DI			pendula gas laws	pendula gas laws	-	-
DI	No TCF	Kin	Rel/Newt	Rel/Newt	Qu/Newt	Qu/Newt
		DynC	-	-	W/R Op	W/R Op
	TCF	Dy^{nI}	-	-	Phase chs	**Phase chs**

FootNotes: Contents

Batterman's own exposition is largely clear and careful. However I found a few of Batterman's diagrams too obscure to follow easily. Fig. 4.6, p.53, e.g., is there to illuminate a fluid flow example but contains no explanation or definition of the $t_{i,j}$ symbols (there, or in the text), the structuring of the dynamics around a 'stationary point' r_0 (that actually changes) is left obscure, as are the choices of liquid incidence angles. Similarly, Fig. 4.3, p.41, the crucial piece in making the renormalisation process intelligible, is left with virtually no interpretation and is quite opaque. A little more attention to (inexpert) reader information would have corrected these flaws. Similarly for a few poorly worded sentences: at p.19, l.21 the reference of "former" is obscure (it refers to limiting cases which are not singular); at p.23, l.6 from bottom the sense of "instance" in "explain an instance of universality" is ambiguous as between the detailed motion of a system, which would contradict the remainder of what is said, and a case of a universal law (evidently intended). Finally, equation 7.6, p.103, strictly needs $K^{1/2}$ (not K) to square it with substituting 7.4 into 7.3, and at p.107, l.9 insert "space" after "phase".

[2] Despite contrary appearances they can even both be placed in universally quantified form (that of the explanation of laws rather than particulars) by quantifying over the empirical system details of the type (i) explanation ('All systems with initial conditions IC(s) ...'). Then adjoining asymptotics premises simply alters the quantificational scope ('All systems with initial conditions IC(s) and dynamical form F(s) ...').

[3] Here the 'not possible in principle' is to be understood in the sense of Cherniak [1986] as 'computationally inaccessible to any finite computation process of this universe'. See also Hooker [1994] for further discussion.

[4] Compare here Kim's less subtle claim (Kim [1998], p.97) that Nagelian reduction requires the augmentation of the ontology of the reducing theory simply as a result of the fact that the

laws of the reduced theory can't be formally deduced from those of the reducing theory without employing the vocabulary of the reduced theory (as it figures in bridge laws).

[5] It is true that there is an approximation to asymptotic wave phenomena constructible from rays, but this cannot do the appropriate job because (i) it breaks down near and on a caustic and (ii) it anyway requires assigning a phase to a ray, so it is not a pure ray construction—see pp. 86-89.

[6] In a more traditional philosophical language, the iron bar is supervenient on its molecules, nothing about the bar can change without the change being dynamically grounded in appropriate molecular changes. But, as the foregoing discussion shows, dynamical analysis provides a much richer language in which to discuss the possibilities. First, it specifies top-down behavioural constraint formation in terms of change in dynamical form, the change in form describing the causal power this novel constraint possesses. (This also distinguishes such effects as non-epiphenomenal.) Second, the dynamics itself shows how the constraint, a (relatively) macro level state/property, is determined by the states/properties of its micro constituents and so is supervenient on them, yet can nonetheless also constitute a constraint on them. Here dynamics gives the constraint a subtle status that eludes conventional formal analysis, combining what common philosophical assumption opposes. (Much of this is prefigured in Collier [1988].) Thus dynamical determination, = there being only one dynamical possibility for the collective dynamical state/property, cannot be equated with logical determination, = the collective dynamical state/property is logically derivable from, can be expressed as a logical sum of, its constituent states/properties. The former is specified as the constituents fixing all space-time trajectories so as to allow only one macro possibility, but these trajectories may be computationally strongly inaccessible (note 3).

[7] However, the focus of the interpretive literature on substantival/dynamical issues, combined with the continuing absence of clear metaphysical options and the dynamical differences

between standard quantum theory and quantum field theory, renders any such proposal speculative and tentative. Moreover, all these cases may ultimately prove more deeply linked through the ideas of space-time as dynamically identified with matter fields and of asymptotic limits representing dimensional elimination (see section 2 above), though this too remains speculative at this time. Similarly, while in what follows I have assumed the traditional position that space-time itself is a pure framework with no dynamical constraint capacity of its own, should the preceding view prevail and space-time be attributed dynamical constraint capacity it would also be appropriate to speak of emergence for all kinematical cases where reduction fails.

[8] The domain shift arises because the wave/ray relation is one between limit T_c and asymptotic T_f domains, but T_f asymptotic properties do not arise from combining entities or properties from the limit T_c domain at all, they cannot be constructed from those resources. Rather, they arise from those of the T_f domain itself (p.118). So with respect to Batterman's focus on the asymptotic T_f domain, the issue is instead the emergence or not of asymptotic entities and properties from the basic entities and properties of T_f. But there he needs, but does not provide, a reformulated version of "basal conditions". Substituting broadened notions of constituent and entity so as to include waves as constituents of superpositioned entities and the like would preserve the spirit of Kim's conditions (including **a** and **b**) while including superpositions among wave components as the "basal conditions" for asymptotic optics, and the like. (The kinematical cases will remain exceptions, as they should.)

[9] What they do reflect is the two aspects of the dynamical status of emergent constraints, as both determined by constituents and yet possessing distinctive causal power, discussed in note 6. Batterman wavers between them, I suppose, because he tacitly accepts the usually assumed dichotomy between these, and hence wrongly equates dynamical determination with logical derivability.

[10] If there are unique unchanging spatio-temporally local fundamental dynamical entities (i.e. a-toms) then there is no fundamental emergence, only existential emergents having these atoms as ultimate components in various dynamical combinations. But self-organisation of itself does not require this. Fundamental fields would yield the same emergent result, while mutant spatio-temporally local fundamental components, or cases where they or fields do not preserve their identity through the process of top-down constraint formation, would issue in fundamental kind emergence.

[11] See note 6, cf. note 9. The same status indicates an equivalent coarseness to Nagel's use of logic to formulate a reducibility condition. Since logical derivability of the new constraint (whether described in T_2, or in T_1) from descriptions of constituent-constituent interactions (in T_1) fails there is emergence in Nagel's terms, but this criterion cannot further distinguish cases of top-down constraint from others, or distinguish cases where the dynamical state of the constituents (described in T_1) dynamically determines the constraint (the T_2 outcome) from those where it does not.

[12] Marras [2002] also adds some further formal semantic problems unresolved in Kim's version, but these are resolved, I think, through the dynamical analysis of multiple realisability below.

[13] Note that it need not have been this way; the basic dynamical properties of the ionising process are carried forward into the immediately resulting ions and it is a particular dynamical feature of cascade formation and electrical detection registration that it filters out this information.

[14] Such processes are the central feature of current human engineering, for they simplify and render reliable what would otherwise be extremely complex regulatory problems. Rigidity is the commonest filtering device; if buildings were bodies their engineering would be as complex and risky as medicine—but they would be a lot more adaptable.

[15] Block's insistence that there could not be heterogeneous realisations so radical as to share no physical properties at all across them (the Disney Principle, Block [1997], p.120), was a step toward this analysis.

[16] See Hooker ([1981], Part III), which also discusses Dewan's similar example of electrical governors, relating it to the functional analysis of genetic ascriptions ('has a dominant gene', etc.) and Armstrong's functional analyses of mental ascriptions.

[17] This provides a more principled and more satisfying solution to incorporating multiple realisability into a theory of reduction, I suggest, than the proposal of Marras [2002] to simply give up reduction for a one-way, many-one homomorphic mapping from T_1 to T_2. Marras' proposal unnecessarily sacrifices identities and so both the ontological goals of reduction and the dynamical structure of scientific unity (see below) and fails to distinguish between naturalisable and radical heterogeneous realisations. However, were the latter forced upon us as the so-called special sciences matured then we would be driven back to something like his position.

[18] This has not, however, stopped some philosophers and others from making strong claims about these domains. An important case in point for reduction is the (usually tacit) assumption of a synonymy criterion of property identity: $P_1 = P_2$ if and only if P_1 is synonymous with P_2. For example Kim ([1998], p.98) says: "If *M* and *P* are both intrinsic properties and the bridge law connecting them is contingent, there is no hope of identifying them. Distinct properties are just distinct, and we can't pretend they are the same…" Kim relies on this centrally in his argument against the reduction of mind. But Hooker ([1981], Part III) argued that on even a general realist approach to language (like Armstrong's) there is every reason to abandon this criterion. Moreover, a biologically grounded approach to cognition provides a supporting conception of semantics (see Christensen and Hooker [2001]) quite different from any that might encourage the ignoring of dynamical grounding

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[20] But this latter variety should not be mistaken for the former kind. And we may reasonably hope that it too will provide only non-radical, naturalisable heterogeneities.