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# MOVING BEYOND THE INFLUENCE OF MOLECULAR GENETICS ON THE DEBATE ABOUT REDUCTIONISM IN PHILOSOPHY OF BIOLOGY

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**ABSTRACT:** The rise of molecular genetics has had a fundamental influence on the development of philosophy of biology because of its central role in the debate concerning reductionism. I begin by describing notable attempts to reduce Mendelian genetics to molecular genetics, the lightning rod of reductionist attempts in philosophy of biology. I then suggest that the syntactic reductionist may wish to focus on neglected biological cases (e.g. ecosystem evolution) that *may* eventually yield laws that could provide easier reduction than the attempted reduction in genetics.

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## INTRODUCTION

“A belief common among philosophers and biologists alike is that Mendelian genetics has been or is in the process of being reduced to molecular genetics, in the sense of formal theory reduction current in the literature.” (Hull 1972, p.491)

“The debate about reductionism in genetics was joined by a number of philosophers (...) Unlike so many debates in philosophy, this one has yielded a near consensus. The dominant view is that classical genetics has not, is not, and will not be reduced.” (Waters, 2000, p.539)

What happened in the intervening thirty years between these two accurate depictions of the state of play in philosophy of biology shows the influence the study of molecular genetics has had on philosophy of biology: it has shaped in radically different ways its consensus views about reductionism. In this paper, I will trace some of the impact the rise of molecular genetics has had on philosophical debates, mostly on the issue of reductionism.

The problem of reductionism has taken many forms in the philosophy of biology literature. As Waters (2000) points out, most debates concerning reductionism in philosophy of biology fall into variants of one of three debates. Are genes, whatever they are, the main or sole unit of natural selection?; Is Mendelian genetics reducible to molecular genetics?; If so, can molecular genetics lead to a reduction of biology to chemistry and physics –if not in practice at least in theory-? In this article I will focus on the second question, for it might be the best way to understand just how much the development of molecular genetics has influenced debates in

philosophy of biology<sup>1</sup>. I will also focus mainly on reductionist approaches that wish to use the second question as a way to bolster claims about the third question. In other words, the reduction of Mendelian genetics to molecular genetics is often (but not always<sup>2</sup>) used as the first step to a wide reduction of biology to chemistry and physics.

I will describe how the rise of molecular genetics first motivated syntactic reductionists and their attempt to reduce Mendelian genetics to molecular genetics (Schaffner, Ruse), and then motivated anti-reductionists to reject positivist accounts of biology altogether (Hull). I also briefly address non-positivist accounts of reduction that arose from this debate (Wimsatt, Sarkar). After this examination of some of the foundational aspects of contemporary philosophy of biology, I will offer a possible way out for syntactic reductionists; reduction and syntactic approaches to biology might be available outside of molecular genetics. The last section sketches this possibility with the case of potential ecosystem evolution (Van Valen) and the laws it may inspire. This programmatic point is intended as an exhortation for reductionists to move beyond molecular genetics and examine other types of biological explanations.

### **General state of play**

Arguably, the received view in philosophy of science for the first half of the twentieth century was to endorse some variety of positivism concerning the nature of scientific theories. Physics was seen by many as the paradigmatic example of a scientific discipline where an axiomatic structure girded the theoretical apparatus and correspondence rules linked the logical structure to the body of empirical observations. Although there have been notable arguments to show that physics itself was a poor exemplar for a positivist view (e.g. Cartwright 1983), most philosophers of science saw physics as following the positivist programme somewhat faithfully.

Biology did not seem to fit the hypothetico-deductivism mold. Contra rare but notable attempts to axiomatise certain aspects biology (see Woodger 1937, 1952, Ruse 1973, Williams 1970 and Rosenberg 1985), most philosophers of science argued that biology was better understood as a special science whose object was too context dependent to lead to the uncovering of universal laws that could lead to the axiomatisation of biology.

Not surprisingly many biologists –who after all had vested interests in the issue- were vocal in their take on the rise of molecular genetics (e.g. Crick 1966, Monod 1970, Dobzhansky 1966, Mayr 1961, Simpson 1964; see Beatty 1990 for a useful analysis of many biologists' anti-reductionist response). But aside from the obvious repercussions the development of molecular genetics had on biological sciences, the molecular discoveries of the 1940s, 1950s and 1960s had lasting consequences in the humanities as well. In this article, the focus is on the influence the rise of molecular genetics had on philosophical reductionist and anti-reductionist stances, and not on biologists' take on the significance of this scientific discipline.

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<sup>1</sup>All the varieties of reductionism (and counter-moves) that have arisen in philosophy of biology cannot be adequately addressed in this format. For an excellent survey of all the positions see Wimsatt 1979, Schaffner 1993 and Sarkar 1998. What I intend to do here is describe the reductionist impetus molecular genetics has provided to some philosophers of biology.

<sup>2</sup>Waters 2000 for instance argues that, epistemically, Mendelian genetics is reduced to molecular genetics but he does not believe that such a reduction is but the first step in a metaphysical reduction of biological phenomena to organic chemistry, chemistry and ultimately physics.

The influence of molecular genetics on philosophical discourse is striking not only for its scope but for its dialectical nature: first, with the development of molecular genetics, reductionist approaches for biology seemed intuitively vindicated in part because of the provisional pre-analytic reductionism defended by Crick 1966, Monod 1970 and other architects of the molecular genetics revolution<sup>3</sup>.

The reductionist hope in philosophy of science was that Mendelian genetics could be reduced to molecular genetics: it was assumed pre-analytically that the Mendelian generalizations concerning the similarity between parents and their offspring could be explained by molecular mechanisms. The problem of reductionism in philosophy of science being usually articulated as a problem concerning relations between theories (not entities), the question is whether Mendelian genetics *theory* could be reduced to molecular genetics *theory*. Most philosophers of biology may well grant that biological ‘stuff’ is ultimately reducible to physical ‘stuff’; metaphysically the commitment of the vast majority of philosophers of biology is to physicalism. The disagreement is over whether or not this commitment entails that our theories should all be reducible to the basic physicalist theories (i.e. physics)<sup>4</sup>. The anti-reductionist claim is that biological theories are not reducible to physical theories<sup>5</sup>.

The development of molecular biology has proven to be the blessing of various reductionist projects. It has been a blessing for it seemed to warrant both a metaphysical reductionism (all biological processes could in theory be reduced to processes at the macro-molecular level and then to chemical and physical processes) and a methodological reductionism (science should focus on macro-molecules to make the largest strides in explanation of the biological world). The near omnipresence of DNA as the inheritance mechanism in nature, its high fidelity and its role in the presence of variation in populations seemed to give biology strong chemical and physical underpinnings making the reduction, replacement or elimination of biological theories in favor of chemical or physical theories only a matter of time (this enthusiasm is evident for example in Schaffner 1969, Ruse 1971 and Rosenberg 1985). Although reduction, replacement and elimination are not identical, proponents often share the goal of exchanging a higher-level of organization explanation for a more fundamental level of organization explanation<sup>6</sup>. When reduction fails, we can either assume that replacement or elimination is the way to go or assume that we have genuine emergence (or, we can, obviously, question both theories’ adequacy). Pre-analytically, molecular genetics seemed to provide the foundation for reduction in biology.

This initial reductionist enthusiasm was quickly dashed: opponents of reductionism in

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<sup>3</sup> I’m including Monod as a pre-analytic reductionist because of his public advocacy for the overwhelming explanatory force of molecular genetics, even though some of his remarks would clearly disappoint reductionist philosophers. A clearly reductionist manifesto “*Ainsi définie, la théorie du code génétique constitue la base fondamentale de la biologie*” (Monod, 1970, p.12) is immediately followed by the qualification that “*Ce qui ne signifie pas, bien entendu, que les structures et fonctions complexes des organismes puissent être déduites de la théorie, ni même qu’elles soient toujours analysables directement à l’échelle moléculaire.*” (ibid).

<sup>4</sup> See Rosenberg, 1985, chap. 4 for more on this issue.

<sup>5</sup> An example of this weak physicalist position can be found in Kitcher 1984.

<sup>6</sup> Wimsatt gives us a helpful general criterion for identifying a reductionist: “A reductionist conceptual scheme (or world) is at least one in which when explanations are not forthcoming in terms of other same-level entities and phenomena, one is more likely to look for (or find) an explanation in terms of lower-level phenomena and entities than in terms of higher level phenomena and entities.” (Wimsatt, 1976, p.689)

philosophy biology quickly pointed out that the canonical model of reduction (Nagel 1961) did not apply in the case of the purported reduction of Mendelian genetics to molecular genetics. The most lasting argument was provided by Hull (1976, more on his argument below), with an influential addition provided by Kitcher (1984). What seemed as a blessing turned out to be a curse for the seemingly promising reduction of Mendelian genetics to molecular genetics has in fact not occurred and various attempts have shown the seams of theories of reduction. The apparent failure of this reduction prompted many (if not most) philosophers of biology to reject the approach that fueled the desire for reduction in the first place namely post-positivism, *at least* about biology but probably about other scientific disciplines as well.

In some respects, reductionist and anti-reductionist arguments were both inspired by developments in molecular genetics and as such Watson and Crick's discovery and other aspects of the development of molecular biology shaped much of the discussions in philosophy of biology from the 60s on. The attempt to reduce Mendelian genetics to molecular genetics has therefore had the strange dual role of encouraging post-positivists reductionist projects for biology (e.g. Schaffner 1969, Ruse 1971, Waters 1990, Rosenberg 1985 or in a more intuitively vein Crick 1966 and Monod 1970) while providing the theoretical foundation to reject positivist projects altogether (Hull 1976 Beatty 1980, Kitcher 1984, Lloyd 1994, Thompson 1989 and many others). Ironically molecular genetics is seen by both provincialists and autonomists as the best reason to adopt their respective position<sup>7</sup>.

Last but not least, the philosophical study of molecular genetics has lead to new research programmes to better understand reduction outside of post-positivist strictures (Wimsatt, Sarkar). As such, the influence of molecular genetics is almost omnipresent in philosophy of biology. Let us now see in more details how the reductionism debate was shaped by molecular genetics.

### **The Syntactic Hope**

The standard bearer of syntactic reductionism in biology has been Schaffner who with two seminal papers (Schaffner 1967, 1969, and an amended version in 1993) attempted to show that a variety of the Nagelian reduction model obtained (or *could* in theory obtain) with the reduction of Mendelian genetics to molecular genetics.

Reduction as described by positivists is a type of explanation: the theory being reduced is explained by the reducing theory by virtue of it being deduced from the reducing theory. In the classic Nagelian account theory T2 is reduced to theory T1 when all the generalisations made by T2 can be logically deduced from the generalisations made by T1. In cases where such deduction is not available because the theories don't share a common language in which the deduction could make 'sense', T1 can be supplemented by 'translating' principles (or bridge laws) that can make sense of claims made by T2.

In Schaffner a significant accommodation is introduced, for such 'straight-forward' deductions would be very rare. He recognizes (Schaffner 1967, 1969, 1993) that Mendelian genetics (the T2 in this case) as is cannot be reduced to molecular genetics (T1 here), but he suggests that we still have a relatively 'Nagel-like' reduction (hereafter nomological reduction)

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<sup>7</sup> Rosenberg (1985, chap. 2) tags 'provincialists' as those who see biology as merely a province of physics (i.e. biological theories can ultimately be reduced to Physical theories), whereas 'autonomists' see biology consistent with but independent from theories in physics.

if we allow for T2 to be ‘slightly’ modified prior to reduction. T2 will not be reducible to T1 without the intermediate introduction of new terms and concepts. In other words, although Mendelian genetics (T2) is not deducible from Molecular Genetics (T1), a slightly modified version of Mendelian genetics (now T2\*) that takes into account evidence marshaled by T1 will ultimately reduce T2, or T2\* (see Schaffner 1969 pp.332-333, 1976 and 1993, p.429). Of course, not all possible modifications of T2 will be acceptable; if that were the case, reduction would be as trivial as adding ad-hoc modification to any theory to permit deducibility. T2 and T2\* need to be ‘strongly analogous’ for the reduction to be acceptable. More will be said later about this concept of analogy.

The translation problem comes in part from the fact that a radically different concept of gene is involved in T1 and T2. Mendelian genes are identified via function whereas molecular genes are identified structurally<sup>8</sup>. Since function does not have a necessary connection to specific structures, it isn’t clear how Mendelian genes could be made to correspond to molecular genes. A Mendelian gene can only correspond to a disjunction of molecular genes and as such, no deduction will be straightforward. Reductionists (following Benzer 1957) suggested that T2 be modified into T2\* so as to replace the ‘vague’ term of Mendelian gene into one of three categories: mutons correspond to the smallest unit of mutation, recons are the smallest unit of recombination and cistrons are the smallest unit of phenotypic expressions. Cistron is in some case analogous to specific Mendelian genes. By replacing a functional term of Mendelian genetics by terms that can be translated into structural terms (e.g. the cistron represents the minimum number of nucleotides that codes for a given phenotype.), the reduction of now a somewhat structural theory (T2\*) to structural theory (T1) seems more feasible. The problem remains of providing a criterion for distinguishing strong analogy and weak analogy: how are we to know whether there are too many differences between T2 and T2\* to really claim that a Nagel-like reduction can be effected? The necessary introduction of the concept of analogy might be the hint that a syntactic nomological conception of reduction is not appropriate, minimally in the case of genetics, but probably in cases other than genetics as well.

Ruse offers a related but somewhat different analysis of the relationship between Mendelian genetics and molecular genetics (1971). The position slightly changed over the years, but the original argument was in favor of ‘strong replacement’ of Mendelian genetics by molecular genetics, as opposed to ‘weak replacement’ more in line with the reduction discussed above. This replacement is justified for various reasons: among them are the fact, Ruse argues, that molecular biology explains everything that Mendelian genetics explained, explains things that Mendelian genetics cannot explain, and explains things that contradict explanations made by Mendelian genetics. For our purposes the salient point is that the relation is one of replacement because it isn’t *exactly* fitting with Nagel’s model of reduction. Ruse argues in 1971 that the conditions for Nagelian reduction are almost met but not exactly –the introduction of the concept of analogy makes this evident. Ruse argues for an *informal* reduction of Mendelian genetics to molecular genetics, one that strictly speaking is actually a relation of replacement but that is close enough to reduction to be considered such in everyday practice. Ruse later (1976) adopted an account closer to Schaffner’s, explaining that the informal nature of the reduction is merely the result of the uncompleted status of the enterprise. In some sense, Schaffner and Ruse

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<sup>8</sup> Molecular genes as Waters (1994, 2000) points out are also defined functionally, but in a way that is constrained by the chemical-physical structure of the gene.

while not *yet* having provided the full nomological reduction, argue that, based on partial reductions, optimism concerning the eventual reduction of Mendelian genetics to molecular genetics is warranted. But this optimism needs to be justified, especially in light of the constraints that a realistic appraisal of the mechanisms involved provides.

The relationship between Mendelian genetics and molecular genetics is what has been called a many to many relation. A single Mendelian gene does not correspond to a single molecular gene (and the same is true for loci and alleles) Hull describes how this type of relationship dooms reductionist projects:

Phenomena characterized by a single Mendelian predicate term can be produced by several different types of molecular mechanisms. Hence, any possible reduction will be complex. Conversely the same type of molecular mechanism can produce phenomena that must be characterized by different Mendelian predicate terms. Hence reduction is impossible (Hull, 1976, p.39).

Moreover, Hull points out that the lack of search of derivations in the actual practice of molecular biologists -coupled with the multiple realizability problem described above- puts into question the optimism displayed by the would-be reductionists.

Hull's pointing out of the many-many problem is not the only problem for the reductionist. As Hull also pointed out (1972, 1974, 1976) Schaffner and Ruse did not in fact provide a full description of the theories involved in the reduction. Even the axiomatic nature of existing 'laws' in biology needs profound qualification: Kitcher (1984) pointed out how it was recognized early on that Mendel's 'laws' of heredity were not as universal as one could hope and so there wasn't a unified coherent Mendelian theory of genetics<sup>9</sup>. A related problem with reduction is that it isn't even clear that there is a unified molecular gene concept that could be the reducing concept for the Mendelian gene concept. Many have concluded that 'gene' in molecular genetics "is a place holder or dummy term designating a motley collection of different kinds of molecular entities" (Waters, 2000, p.544). This has motivated Waters to suggest a unified concept of molecular gene (see Waters 1994 and a revised argument in 2000) that according to their account accurately describe most biologists' usage of the term. Waters argues that a coherent structural concept of gene is available<sup>10</sup>.

So the first step to obtain a nomological reduction is still missing in action: what are the theories involved in the reduction? Although attempts to provide axiomatic descriptions of certain aspects of biology have been made (e.g. populations genetics and evolutionary theory: Ruse 1973, Williams 1970, Rosenberg 1985 ) they remain more promissory than complete and as such, the propedeutical step of actually identifying the full theories involved in reduction in biology has not been successfully completed.

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<sup>9</sup> Many have since argued that while Kitcher may be right about the *initial* laws he is wrong about the present 'anomalous' status of Mendelian theory. As Sarkar (1992) puts it: "it is apparent that classical genetics does have a theory: the laws of classical genetics are simply Mendel's laws corrected by a host of discoveries, mainly in the 1920s, long before the advent of molecular biology." (Sarkar 1992 p.185). Sarkar goes to argue that actually it is molecular genetics that might still lacking unified theory (not Mendelian genetics).

<sup>10</sup> Waters provides what is arguably the most convincing molecular gene concept that may enable eventual reduction: "Despite the appearances, however, there is a clear and uniform way of understanding genes at the molecular level, which (...) can be summarized as follows: a gene *g* for linear sequence *l* in product *p* synthesized in cellular context *c* is a potentially replicating nucleotide sequence, *n*, usually contained in DNA, that determines the linear sequence *l* in product *p* at some stage of DNA expression." (Waters, 2000, p.544)

This problem is compounded if we adopt Schaffner peripherality view concerning reductionism (Schaffner, 1974). He acknowledges that the reduction problem is a problem concerning mainly philosophers not biologists: since initially offering his reduction model, he observed that deducibility or logical coherence is nowhere apparent in the strategy deployed by molecular geneticists. So, not only is nomological reduction of Mendelian genetics to molecular genetics ‘irrelevant’ to biologists as Schaffner concedes, but as Hull (1972, 1974, 1976, 1979) points out it cannot satisfy philosophers for it remains incomplete. Since both Schaffner and Hull agree that a reductionist approach is not an adequate descriptive approach concerning biology, the question then becomes whether to take a reductive normative approach seriously. But with Schaffner’s account, not only reductionism is not normative for biologists, but because of its promissory nature it is not evident why it should be a normative account satisfactory for philosophers either<sup>11</sup>.

The Schaffner model would not satisfy biologists, cannot satisfy anti-positivists, and will leave post-positivists waiting for the *actual* reduction to be provided. This being said, Schaffner’s reduction model remains a significant contribution to the debate. Not only it provided the first thorough attempt to reduce Mendelian genetics to molecular genetics but it did so in a way very close to Nagel’s reduction model, hinting that a post-positivist approach to biological knowledge, if not easy, was more than a pipe dream. By introducing the role of analogy, Schaffner might have weakened the positivist credentials of his account, but he showed that limited accommodations might keep the syntactic account of theories relevant for biology. Analogy does not fit nicely in the syntactic view, but it provides a way to accommodate some discontinuity within scientific theorizing while not giving up on the enterprise of rational reconstruction and of nomological reduction.

Ruse’s later endorsing of Schaffner’s model has the advantage of minimizing the peripherality point. Ruse wishes to show how adopting such a view may in time provide real discoveries (Ruse 1976). By doing so, Ruse may convince some to adopt the normative claim even though the reduced and reducing theories have not been fully described. The difficulty of providing complete theories of both Mendelian genetics and molecular genetics as convinced some to give up on nomological reductionism. Even initially enthusiast reductionists (e.g. Rosenberg 1985) later bit the many-many bullet: “A more reasonable response is to accept that the systematic deduction of theory that characterizes physics and chemistry is not to be had in biology, to explain why not, and to show that the absence of reduction is no threat either to materialism or to the unity of science.” (Rosenberg 1994 p.22)<sup>12</sup> If, as Rosenberg (1994) concedes, selection for function is blind to structure, maybe one should expect the supervenience of Mendelian genetics on Molecular genetics without the type of identity that would enable Nagel-like reduction. But Waters (2000) gives reasons to weaken the problem of

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11 Many philosophers of biology do not think that a reductive approach is a good normative approach for biology in general: “Perhaps a completed science would be able to unite biology and physics but this claim about some hypothetical future says nothing about we should conduct our investigations in the present” Sober 1993 p. 25. As Rosenberg points out (1985, p.88), the reductionists often adopt a normative stance because their position is steeped in metaphysical commitments whereas most anti-reductionist’s favoring of descriptive claims is grounded in epistemological presuppositions.

12 The apparent failure of straight-forward reductionism has pushed Rosenberg to now embrace a larger role for ecology in evolutionary biology. See Bouchard and Rosenberg 2004 and Rosenberg and Bouchard 2005 for examples.

multiple-realizability: he argues that in genetics we are not faced with the ‘extreme’ functionalism that is found in psychology. As Sarkar also points out (1992, p.186), the acceptance of molecular genetics supervenience is premature. Basically they both show that the apparent immense disjunction of mechanisms involved in any genotype-phenotype relation is more limited than it is believed.

But Ruse (1976) had already offered hope that we weren’t in fact faced with many-many problem in the first place:

What seems to follow from the genetical case is not the conclusion that a deduction is in principle impossible, but that more information is required at the molecular level showing why the various phenotypic effects occur. Then we can get away from such correspondences as  $\beta = (B1 \vee B2)$ , replace them by such as  $\beta_1 = B1$  and  $\beta_2 = B2$ , and hence deduction is once again possible (Ruse 1976, pp.635-636)

The move is to show how we are not in fact faced with one molecule related to multiple phenotypes but rather different molecules involved with different phenotypes. The disjunction of phenotype is replaced by various identities making nomological reduction possible.

Such a complete molecular story was famously provided for the case of sickle-cell anemia (see Rosenberg, 1985, section 4.2 for what is, in his own words, a ‘triumph of reductionism’). But such complete descriptions are relatively rare (even today). Aside from the practical difficulty of providing such fine-grain molecular story to permit reduction to obtain, Hull, pointing out the same possibility –albeit with irony (1976, p.20 and p.42)- warns us that such a move makes any reductive claim uninformative: we wish to relate kinds, not particulars. If the only way to get rid of the many-many problem is to over-specify the individual molecular mechanism in order to show that it can only lead to one phenotype, we will lose much of the explanatory appeal that laws had in the first place. Ruse (1976, p.638) argues that Hull’s point while valid may not be as extreme as Hull believes the kinds might be less ample in scope but we will not be relating solely particulars.

The worry remains that post-positivist approaches to reduction in the case of molecular genetics will be either impossible or at least horribly complex. Or that fundamental changes to the syntactic view may have to be made (e.g. introduction of analogy) to make reduction possible.

These difficulties motivated others with reductionist inclinations to offer a novel account of reduction not dependant on theories (or for that matter, not fully dependant on a syntactic account of scientific explanation). This departure from Nagel’s account of reduction is in some sense the second phase of the attempt to reduce Mendelian genetics to molecular genetics and one example of it will now be briefly examined.

### **Explanatory reductionism**

What the normative-descriptive question as well as the peripherality issue highlight is the tension between various forms of reductionism at play in this debate: Sarkar (1998) offers a thorough classification of types of reductionism. He offered a more basic classification in 1992 that will adequately serve our purposes here. Sarkar distinguishes between theory reductionism, explanatory reductionism and constitutive reductionism. Schaffner and Ruse’s attempts to reduce Mendelian genetics to molecular genetics are clearly of the first kind (although because

reduction is a form of explanation, it also belongs to the second kind<sup>13</sup>). Following post-positivist strictures, Schaffner and Ruse aim to deduce the laws of heredity provided by Mendelian genetics from the purported laws of molecular genetics (derived from laws of chemistry and physics). This ‘gamble’ fails in part because of the difficulty of identifying coherent theories and universal laws both in the reducing and reduced ‘theories’.

But there remains a certain intuitive reductionism at play in the relationship between Mendelian genetics and molecular genetics, and Sarkar argues that some varieties of explanatory reductionism can account for it. Following a related argument provided earlier by Wimsatt (1976), Sarkar argues that there is a reduction involved here but a reduction of explanations (not theories). Wimsatt stressed that the Nagelian model (and its refinements with Schaffner) do not reflect how scientific progress is often achieved via the successful explanation of systems by their lower-level parts. In explanatory reductionism, reduced entities are explained by the reducing one. Most important to that approach is that those entities are not necessarily theories and laws but could also include empirical generalizations and even as Sarkar points out (1992, p.170) individual observation reports. Since, as anti-reductionists of all kind gleefully point out, molecular genetics does not seem to have the unifying reducing theory that enable nomological reduction in the first place, reductionists might be better served giving up on theory reduction in molecular genetics and instead focus on explanatory reductionism advocated by Wimsatt and developed by Sarkar. What Schaffner’s model made apparent is that modification of the theories involved may be necessary to effectuate the reduction. Wimsatt argues that this modification only shows how much theories co-evolve (Wimsatt, 1976, p.682). If the theories are not atemporal unchangeable constructs, the reductionist may be better served focusing on more ‘stable’ entities such as explanations where piecemeal reductions can be obtained.

The general framework of explanatory reductionism is to begin with an explanation of a phenomenon and identify all the causal interactions at a given level of organizations. Then we take the same explained phenomena and attempt to describe how it could be the result of interactions at a lower more fundamental level of organization. Maybe more importantly we analyze the causal chains in light of the higher level description (i.e. we recognize that the higher level explanation might still be useful to evaluate the claims made the lower level characterization). In the case of genetics this means that although we might reduce a trait’s ‘origin’ to molecular factors, we may maintain the trait’s identification thanks to the functional analysis provided at the level of classical genetics.

One obvious advantage of this approach is that deflates some of Hull’s (1974, 1976) original reproach concerning reduction: by widening the potential membership of reduction relation to include non-universal or non-perfectly axiomatised theories, explanatory reductionism can now better reflect actual biological practice, where some intuitive form of reductionism is practiced (mostly in molecular biology) outside of an explicit positivist framework. It also weakens Schaffner’s peripherality claim concerning reductionism. Some

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13 Most reductionist projects fall into more than one camp: constitutive reductionisms are interested in making metaphysical claims, which is obviously what Schaffner is after. For our purposes the interesting comparison is between reductionist projects where only reduction between theories is allowed, and reductionist project where other theorizing ‘tools’ can be used as well. The latter is interesting for it introduces a type of reductionism that while sometimes compatible with positivism (when the reduction entities are theories) often does not have to be compatible (when the reduction entities are observations or imperfect generalizations).

explanatory reductionism might lead to new empirical claims therefore granting reductive explanation much instrumental value to scientists as well as philosophers.

But adopting this account of reduction may constitute a Pyrrhic victory. By adopting explanatory reductionism, the reduction of Mendelian genetics to molecular genetics is replaced by the project of reducing *some* explanations of Mendelian genetics to *some* explanations of molecular genetics, thereby jettisoning the reduction *en masse* developed in post-positivist projects.

### **Nomological Reductionism beyond molecular genetics**

Where is a reductionist to go? If genetics does not seem to lend itself easily to axiomatisation, or to law-like description, or rather if one of the best candidates for nomological reduction faces seemingly insurmountable problems, is the lesson to be drawn, as it claimed by anti-positivists, that biological knowledge does not lend itself to a positivist account of scientific explanations? Maybe we should simply adopt, as Wimsatt, Sarkar, Waters and others suggest, a more pragmatic view of reduction more in line with biologists' actual practice, an explanation-based reduction. Although Schaffner, Ruse and others' approach faces many problems, it maybe too early to toll the bell for a post-positivist approach to biology.

What I would now like to suggest is that the nomological reductionist may wish to shift the focus of reduction away from molecular genetics onto other biological processes that might lend themselves to easier reduction to chemical or physical processes. Although some successes in piecemeal reduction to molecular genetics can be achieved if one endorses explanatory reductionism, Nagel-like reduction may still be available in other areas of biology. Admittedly, like other nomological reduction arguments examined here, this argument is more promissory than conclusive; it will be sketched merely as an invitation for further inquiry.

In the same way that many anti-reductionists decry the over emphasis on molecular genetics by reductionists, the reductionist should examine how the anti-reductionist consensus holds in face of attempts to axiomatise other fields of biology. Few areas of biology have taken the question of reductionism as seriously as ecology.

As Hagen (1989) explains, two diverging traditions have driven its development in the last century. This distinction stems from the ecologist Hutchinson (1978) who distinguished two theoretical camps in ecology: the *merological* approach is more demographic and examines populations of independent organisms (Simberloff is a notable 'merologist'). *Holological* approaches "focus upon the flow of materials and energy through ecosystems without considering the organisms that are constituents of the system" (Hagen, 1989, p. 434). The idea of ecosystems as super-organisms stems from that approach. On the population ecology side of ecology (Simberloff and others) a very sharp Ockam's razor is used to dismiss most apparent higher-level community interactions that holological minded ecologists (e.g. E. Odum) find interesting. Ironically, holological thinking may lead us to nomological reduction: the holological tradition has inspired some biologists to posit general trends if not laws about evolutionary processes<sup>14</sup>. I wish now to briefly examine Leigh Van Valen's interest in ecology and energy, only to suggest that the post-positivist may want to examine the possibility that axiomatisation and reduction may lie beyond molecular genetics.

If as Van Valen (1973) famously suggested, evolution is the control of development by

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<sup>14</sup> See McShea 1998 and Bouchard 2004 for some analysis of the usage of energy as a trend in evolution.

ecology, the possibility that energy transfers could explain much of ecosystem interaction would have interesting consequences beyond ecology. In some respect this is exactly what Van Valen hypothesized in subsequent papers (Van Valen (1989, 1991). Van Valen ultimately advocates an energetic paradigm of evolution, according to which *all* evolution is ultimately an attempt to increase control over available energy. This is compatible with the holological approach which as a 'physiological' approach is sympathetic to the reduction of many ecosystem interactions to chemical and physical processes, albeit at a very high level of organization. Ecology has been interested in energy transfers since its birth (Hagen 1989) and it has also been interested in how this could explain evolutionary phenomena (e.g. Tansley 1935). The choice of Van Valen is justified in part because he has thought about the nature and role of laws in scientific explanations (Van Valen 1972) and he has had, in large part because the Red Queen, a lasting impact in evolutionary theory work. For Van Valen, the data demanding explanation are the apparent constant extinction rates for given groups<sup>15</sup>. Van Valen suggests the Red Queen's Hypothesis to explain these patterns: in ecological terms, the environment for a given group is stochastically deteriorating at a constant rate. One should understand deterioration of the environment in adaptive terms: the environment at t2 (i.e. later time) has degraded from the environment you were adapted to at t1 (i.e. original time). If the environment is constantly degrading, one has to adapt simply to stay at the same 'coordinate' in a fluctuating adaptive landscape (defined in resource space), or as Van Valen quotes Lewis Carroll's *Through the looking Glass*: "Now here you see, it takes all the running you can do, to keep in the same place" (Van Valen 1973, p.25, n.32). This hypothesis is intended to explain the apparent linearity of extinction curves across taxa<sup>16</sup>.

The problem is that 'perfect' linearity should not be expected: concave survivorship curves should be expected. Taxa occupying larger spatial area –in real space *not* in resource space- are harder to stamp out and therefore, older taxa are 'probably' harder stamp to out since they have had more time to increase the area they occupy. In other words, the linearity observed in the survivorship graphs, does not match ecological assumptions concerning the difficulty of younger, less spread out, taxa to persist: in fact the whole project makes the strong Markovian assumption that present survivorship probability is independent from previous probabilities. As Van Valen points out, his data implies that "The probability of extinction of a taxon is then effectively independent of its age" (Van Valen 1973, p.17)

After examining (and ultimately rejecting) various explanations to explain the linearity as merely data artifacts, Van Valen concludes that "extinction in any adaptive zone occurs at a stochastically constant rate" (Van Valen 1973, p.16). This is actually a reformulation of what he sees as the new 'law' namely that "The effective environment of the members of any homogeneous group of organisms deteriorates at a stochastically constant rate" (ibid). Since he does not believe his law is exceptionless, it is doubtful that his law is as robust as laws found in physics, but his principle can be seen as an attempt to provide a principle more general than any

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15"The method is an application of the survivorship curve of the population ecology (including demography). It is a simple plot of the proportion of the original sample that survives for various intervals. (...) A logarithmic ordinate, standard in ecology, gives the property that the slope of the curve at any age is proportional to the probability of extinction at that age. "(Van Valen 1973, p.1)

16As Van Valen notes however, even though extinction rates are constant over the majority of temporal scales, they are not constant over geological time (see Van Valen 1973, pp. 10-12).

previous attempt. As such it is somewhat surprising that Van Valen's Red Queen Hypothesis, well known by philosophers of biology, has not been seen as the foundations for a syntactic argument.

In two subsequent articles, this resource-control space morphs into an energy-control space which is the salient point for our purpose here. The first paper "Three paradigms of evolution" (Van Valen 1989) describes the superiority of an energetic paradigm over a reproductive paradigm (the current consensus) for understanding evolution. In "Biotal evolution: a manifesto" (1991) Van Valen describes a possible application of this energetic paradigm to a 'new' level of selection: Van Valen suggests that we could theoretically observe evolution of complete biotas. Although Van Valen argues that one should look at biotas (all the living entities in a given area), not at whole ecosystems (which comprise the biota and all the non-living substances in an area), other comments he has made concerning the potential evolution of non-living systems (see Van Valen, 1989) warrants an interpretation of the 'biotat evolution manifesto' as an 'ecosystem evolution manifesto'. One could see biotas as super-organisms but the moniker as Van Valen points out himself is not appropriate. He is more comfortable with the concept of a community, but a community of different members (a motley crew different species, non-organic material etc.). He takes this biotal evolution view to argue for a new understanding of evolutionary theory in terms of energy control. These biotas, he argues, evolve but do not really reproduce. How can we make sense of their evolutionary success *sans* reproductive success? Van Valen argues that any system will try to control more free energy than its competitor. This attempt to control more energy might translate in having offspring (or it might not). But in the end, the currency of evolutionary success will *always be* energy commandeering. A sketch of this would be to say that ecosystem A is fitter than B if A controls more energy (Van Valen does want to use joules or some other unit here). Survival of the fittest if you will.

Part of Van Valen's motivation for proposing an energetic paradigm is his dissatisfaction with the lack of interaction between molecular genetics and ecology (Van Valen 1989, p.1). Van Valen argues that energy control is the only thing being maximized in nature by all species and that sometimes it translates into higher reproductive numbers, whereas sometimes it translates into higher growth (e.g. in asexual clonal species).

Although Van Valen has not joined this hypothesis to this Red Queen argument, one can see the possibility of finding a law of evolution, based on ecological considerations (holological considerations) and that could breathe some life into post-positivistic accounts in biology.

### **Post-molecular reductionism: examining ecology and evolution**

Taking seriously such bridges between evolutionary explanations and holological approaches may solve some of the outstanding issues reducing the plausibility of reductionism in philosophy biology. Looking back at our discussion concerning the first round of the reductionism fight in philosophy of biology, we identified a few issues that now have some hope of being solved.

First, an energetic paradigm weakens the autonomist view of biology in favor a kind of provincialism. This is not a novel result of course: the holological approach in ecology has from the start been an effort to bring physiology, physics and chemistry in ecological research (see Hutchinson 1978). But the novelty with Van Valen's extension of this approach is that we now have a vindication of provincialism that flows from ecology to evolutionary biology. If, as is

often argued, evolution is the strongest source of unification in biology, the reductionist should look seriously at any attempts to reduce the assumed autonomy of biology: if some ecological processes can potentially be reduced to energy transfers, physical and chemical reactions, if some evolutionary explanations concerning ecosystems and their evolution can possibly be described in terms of energy transfers as well, and finally if evolution is to be the unifying framework for biology, one might wish to reconsider the autonomy of biological explanations.

Second, the criticism that reduction cannot obtain because the theories involved are fully fleshed out is not eliminated but weakened. As we have noted, a problem bedeviling the reduction of Mendelian genetics was that T1 and T2, the ‘theories’ in Nagelian reduction, have not been fully specified. This is still the case for an energetic paradigm of evolution but notice that Van Valen was explicitly searching for a new law of evolution and that the energetic paradigm is likely to be applicable in a very general fashion. This is not to say that the theory is completely specified but rather that there is hope that it could be. Using Van Valen’s ideas concerning the Red Queen and biotal manifesto, one could describe a general theory evolution that could then be eventually reduced to a theory about energy (i.e. a physical explanation). This is very promissory, but I hope I have given enough reasons to justify further inquiry.

Third, Ruse’s case for strong replacement may apply in this case as well; an energetic paradigm might be able to explain everything that current evolutionary accounts explain while explaining phenomena that cannot be accounted for by these same models (e.g. ecosystem evolution), thereby warranting a strong replacement of accepted explanations and theories.

Fourth, even if we do not flesh out T1 and T2 in the case of a reduction of biotal manifesto, we would probably obtain explanatory reductionism as it is described by Sarkar. Such reduction is, as he point out, already obtained in the case of molecular genetics, but it strengthens the appeal of a shift of focus to ecological cases: in the eventuality that we do not establish laws in an energetic paradigm of evolution, one may still provide many explanatory reductions focused on specific energy interactions in ecosystems and how they affect or are affected by evolutionary processes. Evolutionary processes that seemed non-reducible might be explained solely in energetic terms, bringing biological processes under the mantle of chemical and physical explanations.

Finally, the peripherality fear (Schaffner, 1974) is unwarranted in the case of ecology and evolution. Van Valen and other ecologists’ approach allows for the explanation of previously unexplained or unrecognized phenomena such as biotal evolution. This type explanation will require partial reduction to physical and chemical explanations and is a true contribution to biology, and not just a philosophers’ quarrel.

## **Conclusion**

Much more would need to be said to be convinced of the appeal of these accounts to explain evolutionary processes, but the fact remains that these suggestions among others may provide the foundations for what could constitute biological laws. My suggestion here is nothing but programmatic: from the energetic camp in ecology and its possible application in evolutionary theory, we may see law-like statements that could lend themselves to eventual reduction in the spirit of Nagel or Schaffner’s accommodation. Ironically, this suggestion was indirectly hinted very early in the molecular genetics reductionism debate. Ruse’s 1971 article ‘Reduction, Replacement, and Molecular Biology’ is an oft-quoted article concerning reductionism, but in

the same *Dialectica* issue, Ruse pens another reductionist manifesto ('Two Biological Revolutions') that also deserves our attention. In it, Ruse points to some of the dividends molecular genetics has had on other areas in biology.

These advances in molecular genetics have spurred and illuminated other branches of biology, outstanding amongst which is perhaps that which is normally called bioenergetics, the part of biology which concerns itself with the flow of energy through living systems. (Ruse 1971, p.22)

Here Ruse is interested in energy flows at the level of mitochondria and chloroplasts but the same reason that make bio-energetics appealing to a reductionist may make the large scale ecological study of energy transfers at the ecosystem level appealing as well: energy is a physical quantity transferred via chemical reactions in biological processes

The immediate problem with such a suggestion is that, apart from the difficulty of identifying and measuring energy transfers for all biotas, the level of abstractness of the claims offered by such an account of biotal evolution only make the hypothetical reduction to physical theories even less useful to every-day biology, possibly condemning the reductionist achievement to the peripherality described by Schaffner. Although this worry is justified, it should not cloud the philosophical issue. If some aspect of biological knowledge does lend itself to axiomatisation, if such axiomatisation is in term compatible with theories in physics (in this case energy), then reductions closer to the Nagelian concept might be attainable. If that is the case, the positivists and their intellectual heirs might have a toehold in biology.

It is not that surprising that most philosophers of biology have not looked to holological approaches to bolster nomological reductionism: between the failure of the apparent best candidate for reduction (i.e. Mendelian genetics to molecular genetics), the almost complete control evolutionary biology has had over debates in philosophy of biology since the 1970s, the ever growing popularity of the merological approach in ecology, ecosystem evolution is not exactly on the philosopher's (or the biologist's) radar. But the recent reappraisal of group selection (Sober and Wilson 1998), artificial selection experiments for whole ecosystems (Swenson et al. 2000a, 2000b) a reappraisal of the holological approach by philosophers is urgently needed. If such reconsideration leads to entertaining Van Valen's ideas about evolution, we may get laws in biology where it was assumed there where none.

Crick, Monod, and many others inspired some philosophers to adopt some form of reductionist outlook on biological processes. Although this molecular outlook has yielded positive outcomes, it hasn't vindicated the positivist hope for nomological reduction. I suggest here that reductionists might wish to look at other areas of biology as inspiration as well: I am not arguing that reductionists get over molecular genetics but rather that they stop ignoring the vast expanses of biological knowledge left out from that debate.

Molecular genetics inspired many philosophers to think about the very small to understand biological processes, what I am suggesting here is that it may be time to think very big in order to bring biology closer to chemistry and physics.

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