Biological narrativism

Historical narrativism and the science of biology

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# Contents

1 **Introduction** ......................................................................................................................... 1

2 **Evolutionary explanations** ....................................................................................................... 6

2.1 Laws, regularities, and models in biology .................................................................................. 6
   Laws of nature and the received view ......................................................................................... 7
   Strict biological laws .................................................................................................................. 10
   Non-strict laws in biology ......................................................................................................... 18
   The apriori nature of the Hardy-Weinberg principle .................................................................. 21

2.2 Natural selection and the tautology problem ............................................................................. 25
   Natural selection as the law of evolution .................................................................................... 25
   The ecological interpretation of fitness ..................................................................................... 27
   The propensity interpretation of fitness ..................................................................................... 28

2.3 Explaining in biology ............................................................................................................... 33
   Biology as a pseudo-science ....................................................................................................... 34
   Alternatives to the received view of science ............................................................................ 34
   Biology as history ....................................................................................................................... 36

3 **Narrativism in the philosophy of history** ................................................................................. 37

3.1 The narrative fact distinction .................................................................................................... 38
   The nature of historical texts .................................................................................................. 40
   Narrative as a conjunction of statements ............................................................................... 41

3.2 The roots of narrativism .......................................................................................................... 42
   Temporal change and Historical Ideas .................................................................................... 42
   The problematic nature of Historical Ideas ............................................................................ 44

3.3 The analytical philosophy of the historical narrative ............................................................... 45
   Danto’s narrative sentences ....................................................................................................... 47
   Colligation, temporal wholes and intentionalism ....................................................................... 48
   Colligation as interpretation ...................................................................................................... 52
   Louis Mink and the three comprehensions .............................................................................. 53
   Time, truth, and the narrative .................................................................................................. 55
   The transcendental turn of Baumgartner .................................................................................. 57

3.4 From Historical Idea to linguistic narrative ............................................................................. 60

4 **Biological knowledge as narrative knowledge** ....................................................................... 62

4.1 Narrative sentences in biology .................................................................................................. 62
   Explicit narrative sentences in biology ..................................................................................... 62
   Species and implicit narrative sentences ................................................................................. 63
   Implications of narrative sentences in biology ......................................................................... 67

4.2 Configurational comprehension in biology ............................................................................... 70
   Seeing species as individuals ................................................................................................... 70
   Individuals and biological explanations ..................................................................................... 72
   Natural selection implies natural history ................................................................................. 76
   Biological narratives ................................................................................................................. 80
5 Conclusion

Post-positivism and the causal theory of reference ............................................................. 83
Reference in biological narratives ....................................................................................... 89

Conclusion ......................................................................................................................... 95
For my dear friend Robert,
A teacher of biology and life
1 Introduction

The last few decades there was an invasion of evolutionary inspired thinking in many fields of research. Psychology, cognitive sciences, sociology, and, of course, also the study of human society. Ever since Darwin posited his great idea, it has conquered more and more territory in the academic world. While Richard Dawkins could write in 1976 that “[p]hilosophy and the subjects known as ‘humanities’ are still taught almost as if Darwin had never lived”, in 2011 this statement would be thought absurd. Some academics do resist evolutionary explanations in their field. But the fact that they resist it means that Darwin’s ideas have indeed penetrated the academic world. It must be acknowledged that is nearly impossible to ignore evolutionary thinking. But getting clear what evolutionary thinking exactly means remains a difficult task, and is subjected to much debate.

This thesis is an attempt to show how the philosophy of narrativism can be used to get more insight in evolutionary biological explanations. Philosophers of history like Louis Mink, Michael Baumgartner, and Frank Ankersmit believe that historical texts are wholes that cannot be reduced to their parts. The individual statements are connected to reality, but the text as a whole cannot be reduced to any single thing what happened in the past. The structure of a narrative lays in the text alone, and not in the past. Hence the name narrative idealism that is sometimes given to these ideas. While narrativism as I will present it here is not the only philosophy of history, it is acknowledged by most philosophers of history that the writing of history has a distinct narrative nature.

A large part of this thesis is devoted to the philosophy of biology. In the philosophy of biology there is an ongoing debate about the nature of biological explanations. Especially explanations that involve natural selection are difficult to align with the received view of science – a view that is the legacy of the logical-positivist philosophy of science. Somehow explanations that evoke natural selection cannot be accounted for in this traditional philosophical manner of the hypothetical-deductive method. Since biology is about the past, it seems natural to look for a solution to this problem in the philosophy of a field of study that is also about the past, namely the philosophy of history. In this thesis I will, therefore, try to show that explanations in biology that invoke the principle of natural selection can be given a philosophical justification using the narrative philosophy of history.

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There are a few reasons why the present thesis’ arguments are useful. The first is that the thesis will show that the narrative philosophy of history comprises of a viable and important set of ideas that extends beyond the interest of philosophers of history, and into the general philosophy of science. The insights about textual wholes and the way in which these can be analysed are useful in many other situations in which historical processes are studied. As such, one implicit conclusion in this thesis will be that narrativism can be a valuable addition to fields outside the philosophy of history. Philosophy of history can teach other fields as much as it can learn from them.

Connected with this is the concern with the contemporary efforts in the philosophy of history to learn from the philosophy of biology. Many philosophers of history draw inspiration out of the findings of the philosophy of the natural historical sciences – biology, geology, etc.. The ways in which biological explanations are justified and analysed are often seen as the basis for the analysis of explanations in history. Showing why, and how the direction of this analysis can be reversed is a second reason for the current thesis’ subject-matter.

That this flow of ideas can be reversed also throws some light on another project in the study of society. This is the project that tries to use the mechanisms of biological evolution to explain the different developments in human society, i.e. the idea of cultural evolution. This idea proposes that the changes in culture and society are best explained by invoking a form of natural selection as a force inside culture. Within this view, the whole of culture itself is viewed upon as an evolutionary process. Showing how an explanation is a narrative when it uses natural selection has the consequence that it cannot be claimed that cultural evolutionary explanations about a certain topic are always better explanations compared to the traditional explanations of the cultural anthropologists, sociologists, and especially historians. There is no epistemologically magical property in evolutionary explanations that makes them a priori better than the traditional forms of explaining. Many others have already pointed out the difficulties with the cultural evolutionary program, but these criticisms were mainly based on the differences between the biological world and the cultural world. The conclusions of this thesis implies a reversal of this. It implies a criticism of the idea that evolutionary explanations of culture are always better explanations than traditional ones because they are alike.

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However, the other side of this coin is that this also opens the door to evolutionary explanations in the cultural realm. For if biological explanations that invoke natural selection are narrative in nature, then using natural selection in the domain of culture does not mean the crossing of an impassable gap that separates the domain of culture from the domain of nature. The shared basis of narratives in evolutionary biological explanations and historical explanations shows that Johann Gottfried Herder’s ideas about a human history in which no shift between natural history and cultural history is present, is possible. The so called big histories that are become fashionable lately have a similar outlook. This is not a vindication of these big history approaches. These approaches should be judged on their respective merits. Yet it does show that these histories are to be treated with equal scientific scrutiny as any other historical approach. These are the four issues on which the present thesis hopes to shed some light. These issues are, however, not addressed directly, but rather show the importance of getting more insight in the workings of explanations that use natural selection.

What The structure of the text is as follows. In the second chapter I will give a short overview of the received view of science, and how evolutionary biology does not fit with this model. These arguments are about the presence, and especially absence of law-like regularities in evolutionary thinking, and about some of the solutions that have been proposed to save biology from the exclusion out of the logical-positivists’ paradigm. Connected with this is the putative tautological character that is often associated with the principle of natural selection. Again, the problematic nature and some of the proposed solutions are treated. In the end of this chapter it will be clear that the received view of science cannot account for evolutionary biological explanations.

Instead of looking to the post-positivistic philosophy of science for a solution to the problems set forth in the second chapter, the third chapter focuses on the narrative philosophy of history. Via the origins of narrativism, the German Historist tradition, the analytical philosophy of history is introduced. The four key figures in this section are Arthur Danto, William Walsh, Louis Mink, and Michael Baumgartner. They represent the different steps in the development of the current ideas about narrativism. Danto shows how it is the kind of language that makes the difference between the historical sciences and the physical sciences. He shows that the linguistic devices he calls narrative sentences implies a narrative history. With Walsh the idea of colligation enters the scene. He shows how historians interpret a diversity of facts and bring them under one concept. Mink combines the ideas of Danto and

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Walsh. He logically analyses narratives, and draws surprising conclusions about the narrative’s reference and truthfulness. But these conclusions also lead him to concerns about the historical narrative’s sceptical nature. With Baumgartner we see how Mink’s concerns about narratives are overcome by showing that the narrative is the condition for historical knowledge. Knowing and thinking about the past is only possible within a narrative. The sceptical concerns are downplayed with Baumgartner. With these four figures the stage is set to look into evolutionary biological explanations.

The last chapter begins with an analysis of the species concept. The logic behind the species concept in biology leads to the conclusion that using species always implies the usage of narrative sentences, and thus, that using species means thinking historical in the sense of Danto’s analysis.

The next step is a critical review of Michael Ghiselin’s and David Hull’s species as individuals thesis. They believe that biologists treat species as individuals. Since the specifics of individuals cannot be subsumed under a law, Hull draws on Mink’s narrativism to get a better philosophical grip on the concept of an explanation concerning such an individual. His views of Mink’s narrativism are, however, distorted. His reluctance to accept all the consequences that Mink makes in light of the narrative, draws Hull into position in which he is in contradiction with himself.

Following these arguments on species the principle of natural selection is analysed using the work of Wim van der Steen. This philosopher denies that the principle of natural selection is a tautology. Yet the consequences of this denial are that every explanation that invokes this principle always implies natural history. The result is that the principle of natural selection does not explain in itself, but always needs, or implies, a historical narrative.

With these steps the argument has been made that evolutionary biology always implies narratives. The last parts of the thesis are about the problem of reference. First a specific critique on narrativism by some post-positivist philosophers of history is retorted. These philosophers claim that narrativism ignores advances in the field of the philosophy of language. The presumptions that narratives do not refer are, according to the critics, based on a description theory of reference that has been refuted. With the causal theory of reference it is possible, so they claim, that narratives do refer. However, the qua-problem of reference aborts this possibility, because there are not narratives of a certain kind, i.e. qua kind, yet this is a requirement for the causal theory of reference to work properly.

In the last part some preliminary examples are given that indicate that evolutionary biological explanations, being implicit narratives, also do not refer. The conceptual
difficulties that vex the idea of natural selection, constraint, and drift show how biologists come to different conclusions based on the same evidence. This situation is not unlike that in the field of history, and show once more how related the two are.

While all this might seem to be a plea for scepticism, the concluding remarks bring us back to the arguments of Baumgartner. Only when we use narratives can we speak about the past. The sceptical sting is removed with this argument. Something that is as true for narratives in history, as it is for those in biology.
2 Evolutionary explanations

Since the sixties, the received view of science is the contrast fluid for the philosophy of science. The failure of the logical positivist philosophy of science to use logical as the basis of knowledge led to a search for alternatives. Together with the demise of the received view, the king of the sciences – physics – also lost its status as the prime subject-matter of philosophers. The search for the single justifying scheme of scientific knowledge, the holy grail of the philosophy of science, was replaced with a search for a diversity of justifying schemes. Because of this the science of biology could rise to become one of the more respectable subject-matters for philosophers of science.

Within the field of biology, the problems of the received view come to the fore in a forceful manner. Biology is considered, by most at least, a respectable science. Many debates about the teaching of evolutionary theory in schools in the United States ended in the conclusion that evolutionary biology is sound science. And biological articles appear regularly, if not predominantly, in renown interdisciplinary magazines like nature.

What then are the problems with biology in respect to the received view? The two most common problems in the philosophy of biology concern the status of theories and laws on the one hand, and the putative tautological character of the principle of natural selection on the other.

2.1 Laws, regularities, and models in biology

What are the laws in biology? The first thing to do when inquiring whether there are laws of nature in biology is to get clear what we mean by laws of nature. This, however, is quite an undertaking since there are more things philosophers generally disagree about concerning laws of nature, then there are things they agree about. In the end this thesis is an attempt to analyse how evolutionary biological explanations work. And to speak of laws in evolutionary biology is generally understood to mean that within biology there are law-like statements that are used in the same way as law-like statements in the physical sciences. Mostly this latter way is referred to as the “received view of science”, and it is often associated with the logical

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4 An overview of these debates can be found in Larry A. Witham, Where Darwin Meets the Bible: Creationists and Evolutionists in America (New York, 2002), 220-232.
positivists and Carl Hempel’s Deductive-Nomological model of explanation, or covering law model.\(^5\)

Hempel’s model is well known and discussed, therefore I will only give short summary of it.\(^6\) According to Hempel an explanation of a singular event \(e\) (the explanandum) is validly explained if and only if the description of that event \(e\) is the conclusion of a deductive argument of a specific form. The antecedents of that conclusion (the explanans) consist of one or more laws \(L\) and certain antecedent conditions \(c\). Since giving an explanation means making a deductive argument, the model’s first name is Deductive. The second part, Nomological, refers to the need that an argument is only valid if there is a law in the explanans. Hence the name Deductive-Nomological modal. An explanation of an event, according to this model, thus consists of an argument that shows that the explanandum “was to be expected” given the particular circumstances.\(^7\)

It is with this notion of law where things become interesting. These laws have certain characteristics, most of which are generally uncontroversial. The first more or less uncontroversial characteristics is that laws should be true.\(^8\) One of the conditions for a valid explanation is that the explanans is true. Since the law is within the explanans, it should be true as well, or at least, approach truth.

Another characteristic of laws is connected with the putative division between empirical laws and theoretical laws. Empirical laws only hold within a limited range. They describe observational established regularities. Theoretical laws, on the other hand, deepen our understanding of these empirical laws by explaining them, and predicting other empirical laws.\(^9\) So Galileo’s law about free falling objects explains instances of falling objects over short distances. Newton’s law of gravitation explains this observational law, but also explains other instances in a more diverse number of situations. The point of science, according to many, is to find theoretical laws that are as universal as is possible – so called fundamental

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\(^5\) The first academic publication in which the Deductive-Nomological model is presented is about the model’s validity with respect to historical explanations. Carl Gustav Hempel, “The Function of General Laws in History”, The journal of philosophy 2 (1942) 35-48. This is quite ironic since historical explanations are as remote from Deductive-Nomological explanations as is possible.

\(^6\) This debate is the famous “bickering over the covering law model” that dominated the philosophy of history until Hayden White’s Metahistory relegated the bickering to the background.


\(^8\) There are some anti-realists that believe that laws of nature are neither true, nor false. For example Bas C. van Fraassen, Laws and symmetry (Oxford, 1989).

laws of physics. Scientists strive to find laws that are not spatially or temporally restricted, i.e. they have the working hypotheses, to use Bertrand Russell’s phrase, that there is some form of uniformity in nature that can be discovered.\(^{10}\) Whether this hypotheses is valid is a matter of scientific inquiry, but up until now the natural sciences have a great track record in generating theories that deal with ever more phenomena, and, consequently, become more uniform. Einsteinian physics subsumed more phenomena than Newtonian physics since, for example, the special theory of relativity can explain the behaviour of very fast moving objects, something classical Newtonian mechanics fails to explain. The former is thus more fundamental, i.e. more universal, than the latter. Science strives to find ever more universal laws. A law that has a limited range of application and that cannot be subsumed under a more universal theoretical law is, according many philosophers and scientists, not a law, but a mere accidental contingent fact.

Laws are also believed to be exceptionless (with respect to one or more variables within that law). This character is captured in the formal requirements of the model that laws need to be generalized sentences that contain one or more quantifiers. The deductive nature of the model, combined with the quantifiers \(\forall\) and \(\exists\), make sure that the laws are exceptionless regularities.\(^{11}\) These quantifiers also make it possible that the laws can be empirically tested. Important in all this is that these quantifiers are real quantifiers. The difference between a real and a false quantifier is the following. The sentence “Every element of the class consisting of the objects \(a, b, c\) has the property \(P\)” is not a regularity that contains a quantifier since it is logically equivalent to the conjunction “\(Pa \land Pb \land Pc\)”\(^2\). It is, consequently, a logical tautology that cannot support counterfactual conditionals. The sentence “Every element in class \(k\) has the property \(P\)” (in which being in class \(k\) means having the property \(K\)) will lead to “\(\forall x (xK \rightarrow xP)\)”\(^3\). And this \(can\) be empirically tested. So a valid law requires a quantifier that makes sure that a counterfactual conditional can be stated with which the law can be empirically tested.

\(^{10}\) Bertrand Russell, *The problems of philosophy* (New York, 2004 (1912)), 46.

\(^{11}\) This requirement of exceptionless is dropped in the inductive statistical model of explanation. This model tries to account for the many statistical explanations in the special sciences and in quantum mechanics. In this model explanation is grounded on an inductive argument that includes a statistical law with a high probability. Exceptionless is, therefore, replaced with near exceptionless. Hempel, “Aspects of scientific explanation”, citing 381ff. This latter requirement of high probability leads to problems since for the mentioned sciences, the special sciences and quantum mechanics, this is not a necessary condition for statistical explanations. A short overview of the different attempts to overcome this problem can be found in Stathis Psillos, “Past and Contemporary Perspectives on Explanation” in: Theo A.F. Kuipers ed., *General Philosophy of Science* (Amsterdam, 2007) 97-173, citing 151-157.
confirmed. And since the model is based on deductive arguments, the quantifier cannot but be exceptionless.

The requirement of generalized sentences is also related to another element of laws and science in general. In physics there are a lot of mathematical models that are not in the form of a general conditional statement. Instead of defining a straight general conditional in the form of “All $F$’s are $G$’s”, physical laws state variables that are only mathematically related to each other. Within the received view these statements nevertheless imply a set of generalized sentences that conform to the requirements of laws. If these physical laws did not imply valid regularities, explanation would, according to the received view, fail. Therefore, the generalities involved in mathematically stated physical statements are not explicitly mentioned. They are rather stated in an “elliptical manner”. In the received view the implicit regularities are, thus, the more fundamental laws underlying the more practical laws that physicists use.

Up until now we have the following characteristics of a law within the “received view”: valid laws should be true, exceptionless and the result of a striving for more universality. The real trouble comes when we try to specify the difference between a mere accidental generalization and a law of nature. It is so problematic that Hempel admitted “giving a clear characterization of lawlike sentences[…] has proved to be highly recalcitrant.”

The logical positivists followed Hume in the claim that laws of nature are mere exceptionless generalizations describing observed regularities. But we say, to use Hempel’s example, that the statement “All members of the Greensbury School Board for 1964 are bald” is not a law, while the statement “All gases expand when heated under constant pressure” is a law. We intuitively know the difference between the two. The former we deem not necessary, while the latter we do think to be, in some way necessary. Yet both are exceptionless generalizations that describe regularities, i.e. both are formally valid. Since the introduction of the Deductive-Nomological model a lot of philosophical effort has been put into solving this problem of justifying this intuition in philosophical valid manner. But each solution seems to be a turn into a new dead end, and a decisive conclusion is far from reached. However, there is a lesson to be learned from all this. For it does seem that laws of nature are descriptions of regularities plus something more. What this something more is, remains unclear. But we do

14 Hempel, “Aspects of scientific explanation”, citing 338.
differentiate between the generalizations about bald School Board members and about heating gas.

In general there are two sides in this debate. Empiricists claim that the something more is to be found in the description of the laws and/or their usage. Their opponents claim that these attempts fail to be objective, or implicitly refer to causality, which defies the empiricists’ fundamental distrust of causal terms. These opponents, however, believe that the laws of nature imply that the world has some form of natural necessity. These attempts to explicate the something more as a necessity are criticized by the empiricists for their unaccountable reliance on notions like universals, cause or modality.

For our present purpose we can say there is a “something more” to laws of nature, and call this something more necessity. This necessity could, however, be an empirical necessity, only relying on how the world is perceived and described, or, as the other group would have it, a nomic necessity, relying on the idea that there is something in or above the world that imposes constraints on all (other) things in that world.\textsuperscript{15}

To sum up, laws should be statements that are true, exceptionless, universal, and capture some form of necessity – with or without parenthesis. Whether the received view is accurate and complete is open to debate. For the current discussion the important thing is to see whether there could be laws within biology that are like these laws.

There are statements in the field of biology that look quite like laws. Examples are: “Humans have 23 pair of chromosomes”, or more general: “Mammals have four-chambered hearts”. Even more general is the following law: “All genes consist of DNA”. While biologists do not always call all these examples laws, they sure look a lot like laws. And what to think of Mendel’s laws? Are these not true laws of nature?

Yet there are quite some problems when we want to call these statements laws in the received view kind of way. There are, for example, so many exceptions to Mendel’s laws – these exceptions became known the moment the laws were rediscovery in the early 1900’s – that learning genetics is all about learning these exceptions. Speaking about the laws of Mendel is quite different from speaking about the laws of gravity. Being exceptionless is seen as one of the characteristics of laws. So at first sight, Mendel’s laws cannot be called laws. But what about the other mentioned laws? Are these not exceptionless? The answer is no, since there are all kinds of exceptions to these laws too. There are humans who do not have 23 chromosomes, mammals which do not have four-chambered hearts, and it is not clear what

\textsuperscript{15} Psillos, “Past and Contemporary Perspectives on Explanation”, citing 132ff.
constitutes an individual gene, for there are viruses that do not have DNA. So these statements are not exceptionless.

From a philosophical point of view this is not a convincing argument against the usage of laws in biology. The fact that we currently only perceive biological statements that have all kinds of exceptions says nothing about the possibilities of genuine laws in the future. Just as Galileo’s law of constant acceleration in a vacuum only worked under specific – read short distance – conditions, so too could the biological laws still be ceteris paribus laws that await refinement. Scientists could find exceptionless biological laws in the future. So if we think there are problems with biological laws, then we need an argument that does more than just take notice that this or that is the case in contemporary biological practice. We need an argument that shows that this search for exceptionless biological laws will fail in principle. But before we take a look at the problem with ceteris paribus laws, we first examine some other arguments.

Arguments that deny biological laws mostly rely on the fact that laws in biology cannot be necessary valid. The main problem within the field of biology is seen to be that the systems under study, i.e. biological entities, posses “nomic inhibitors”. One philosopher describes these as “features of systems that preclude there being laws of those systems, or preclude our access to whatever laws there may be.”\textsuperscript{16} That is, biology is about things that have certain qualities that makes them unfit for laws. These qualities are due to the nature of evolution.

The law of gravity is believed to apply, for example, to all particles, everywhere and in all times. A putative law of biology seems not to be able to make the same claim. Such laws are, as we can see in the above examples, about contingently formed species, the human species; they are about a contingently formed class of species, mammals; or they are about certain contingently formed information carrying structures, genes. It is for example clear that laws about humans are only valid during a very short time period and in a very limited area: earth, roughly during the last 200,000 years. If things would have been slightly different, other laws would have emerged. This feature of evolutionary biology is called the evolutionary contingency thesis. The palaeontologist Stephen Jay Gould popularized this view with his famous thought experiment called “replaying life’s tape”.\textsuperscript{17} Gould claims that when we could rewind this tape up until the starting point of life and then erase everything from that point up until the present, we would get a completely different world if the tape would be played again.

This idea is philosophically refined by John Beatty.\textsuperscript{18} He claims that the regularities that are found in biology are all evolved regularities. Given certain initial conditions and the “rule-making capabilities” of the agents of evolutionary change, new generalizations will keep up appearing in the biological world. New systems will bring forth new regularities. But these systems are due to earlier systems and the associated regularities. In the end all regularities are the result of the initial conditions. Change these, and the regularities would have been different as well, so is the idea.

The nomic inhibitor in this case is the fact that within an environment, the mechanism of evolution generates new generalities, and since this environment is the result of the contingent starting conditions, all these agents and generalities are contingent. Hence biological laws are, according to Beatty, not necessary regularities.

Again, what necessity exactly means remains inconclusive. Most philosophers do agree that a law can be identified in terms of a counter-factual conditional. If we take a look at Hempel’s example of an accidental generalization, “All members of the Greensbury School Board for 1964 are bald”, we can construct a counter-factual conditional that would falsify this generalization: “If one of the members of the pop group the Beatles would have joined the Greensbury School Board for 1964, this Beatle would be bald.” For everybody knows that in 1964 the Beatles had much hair, and that this hair would not just disappear. Genuine laws have the mark that they do support counter-factual conditionals. This does not mean that the ability to state a counter-factual conditional defines the necessity of a law. Such a conditional is rather an intuitive indicator of the necessary nature of a law.\textsuperscript{19} But, thus Gould and Beatty, since the regularities in biology are contingent, they can never support such conditionals. Every conditional could have been true if things would have been just a little bit different at an earlier time. Of course there surely are certain counter-factual conditionals about biological systems that could falsify a biological regularity: those that are falsified by some laws of physics. But if such conditionals would be constructed, they would not be a mark of a biological law, but rather be the mark of a law of physics. These counter-factual conditionals would show how the laws of physics restrain the biological world. And however informative


\textsuperscript{19} David Lewis famously but unsuccessfully did tried to define causality, and thus laws, in an objective manner using counter-factual conditionals. David Lewis, "Causation", \textit{The journal of philosophy} (1973, Issue 17) 556-567.
such a statement might be, it is not a biological regularity. For it applies to non-biological entities as well.

This latter point relates to the much discussed issue of reductionism in biology. For it could be argued that the previous point about the laws of nature is not a weakness at all. According to those who uphold a strong form of reductionism it is a desirable thing to reduce biological explanations to more fundamental physical ones. They claim that biological explanations should be grounded in molecular biology and, ultimately, in physical science. Just as the reduction of physics led to better explanations, so biology should eventually be all about macro molecular phenomenon. The problems with reductionism are, however, not so easily solved.

The first problem with reductionism is that the realization of higher level features of the biological world by macro molecular phenomenon depends on the context in which the molecules find themselves. A single gene might result in a different phenotype when it is placed in a different context. So a gene that results in one phenotypic expression in genotype A might result in a totally different phenotypic expression when it is present in genotype B. This prevents the reduction of the explanation of those phenotypic expressions to explanations of the chemical processes of the genes alone. The bigger picture is needed.

The second problem is that higher level features are multiple realizable. David Hull, among others, stresses that higher level biological phenomenon can be realized by different kinds of molecular structures. Wings, for example, are realized in many different ways. They are all wings nonetheless. Alexander Rosenberg shows how this problem relates to the status of laws as well.

Rosenberg states that it is “in the nature of a domain governed by natural selection over blind variation that no […] laws will arise.” Instead of noticing problems with whole laws, Rosenberg comes to this conclusion because he focuses on the kind of types biology is about, and how these types, or taxa, are identified.

21 Ibidem.
23 Alexander Rosenberg, *Darwinian reductionism, or, How to stop worrying and love molecular biology* (Chicago, Ill., etc., 2006), 137.
In contrast to physics, the individuation of different types in biology is (almost) always done by means of their function. In physics phenomenon are individuated in terms of “physical composition and spatial relations”. We individuate atoms, the stock example of philosophers, by means of the number of sub-atomic particles and their respective “positions” within the system. In biology things are different. A “wing” is identified by its ability to let the organism fly; An “amphibian” is identified as an organism that starts it lifecycle in water, and ends it breathing air; And “sexual reproduction” is identified with the ability to procreate with a member of the same species, but of a different sex.24 These functions are constituted by the properties of biological systems. A bird has certain properties that makes it to have wings, a frog has properties that makes it amphibian, and a sexually reproducing organism has properties that makes it reproduce sexually. Remember that this also applies to the individuation of a single species, for describing a species ultimately leads to describing the different parts of this species, and thus to functional descriptions. The effects of all these functional properties are selected for by the mechanism of natural selection. So the properties that together form the wings in birds and let it fly are, according to the theory of evolution, selected for in the past because of their function of aerial locomotion. Other effects of the properties, and there are many, are disregarded. As such, functional notions in biology are notions that refer to the broader process of evolutionary development, i.e. macro-evolution.25

The thing Rosenberg stressed about this selection mechanism is that it is blind to the way these functions are structurally achieved. Natural selection filters the effects of properties, it does not differentiate between different structures with similar effects. To take an example: a wing that is blue could be just as good in bringing about flight as a white wing. Functional equivalent, but differently structured biological systems will pass the same environmental “filters”, as Rosenberg puts it.

What is more, natural selection does not select in a detailed manner, but in a loose way: it is about functional equivalence. The wingspan of the Wandering Albatrosses is between 2,5 and 3,5 meters. But both a 2,5 meter and 3,5 meters long wing make that these Albatrosses can fly and, therefore, survive in their environment. It is well known that evolution does not select the best, or optimal structure within a given environment. It rather filters those that make the biological system survive. Since environments are constantly changing – a single adaptation

25 In this I follow the etiological view of functionality, and not the causal-role alternative set forth in Robert Cummings, "Functional Analysis", Journal of Philosophy 72 (1975) 741-765. This latter view regards functional descriptions of systems as being about the causal role this system plays in a larger system’s "programmed manifestation".
of an organism is in itself already a change of environment for all other organisms – new ways of filtering for functionality are quickly replaced by older ones. Combining this changing environment with the constant input of new variations within biological organisms will lead to the result that structural different, but functionally similar biological structures will persist and thrive within the biological world. All functionalities in biology are realized in different structural ways.\textsuperscript{26}

But laws do require such structural individuations. Take a simple generalization like \textit{All genes are composed of DNA}. In this generalization the biological entities called genes are individuated. We formalize this generalization like

\[(x)(Fx \Rightarrow Gx)\]

\(Fx\), in the example, is the functional property of being a gene. It is functional because this is an individuation of a biological system – being a gene means something like ‘having the function of storing information about phenotypic characters of an organism’. \(Gx\) is the non-biological predicate of being composed of certain molecules. If this is to be a biological law similar to a law of physics, then \(G\) should be a structural kind. This means that \(G\) should be a property that all items that are \(F\) posses.\textsuperscript{27}

But, Rosenberg asks, “could there be a (biological significant) physical feature common to all items that have property \(F\) or are \(F\)’s”?\textsuperscript{28} Are there, in our example, any structural properties that \textit{all} genes posses? The answer is no. The process of natural selection does not know the difference between structures with the same, or similar effects. Two sibling blackbirds posses wings with similar functions, yet their wings are not the same. So \(F\) might be realized by \(G\), \(G’\), \(G’’\) and so on. Because two creatures will have a similarity of function and a related similar survival value, but a lack of structural equality, Rosenberg concludes that “\(Fx\) will have to be a physically heterogeneous class, since its members have been selected for their effects.”\textsuperscript{29} Again we see that there is the process of natural selection that makes that there are nomic inhibitor: functionally individuated biological systems. And this makes laws, at least in the sense of the physical sciences, about these kind of functional biological entities impossible.

\textsuperscript{26} Rosenberg, \textit{Darwinian reductionism}, 137-140.
\textsuperscript{27} \(Gx\) could also be another functional property that all \(Fx\)’s share but that is different from \(Fx\) itself. This option is even more difficult than the option that \(Gx\) is a structural kind. Since the members of the extension of \(Fx\) are physically different, they will also have different sets of effects. Ibidem, 139-140.
\textsuperscript{28} Ibidem, 139.
\textsuperscript{29} Ibidem.
If we look at our example on genes, we can say that there is no single structure that we can use to individuate genes. Remember that this is about what the biological term gene means. In daily use we often think genes is a chemical term, and while most people know what is meant, gene is a biological term. The question is if the term “gene” can be related to a single chemical description in a non-functional manner. The discovery that the genes of viruses are made up of RNA instead of DNA already falsifies the claim that genes can be individuated by means of a single structure in a rather dramatic way. But even identifying just one single gene by its physical structure is tremendously difficult, if not impossible. We can see this if we look at how DNA, the putative G property, works. DNA codes for proteins, which are considered the building blocks of phenotypes. These proteins are coded by any of 20 amino acids. But the way DNA code for these 20 amino acids makes clear how evolution selects for function instead of structure. For every amino acid is coded by a triplet of nucleic acids, of which there are four. With four nucleic acids ordered in triplets we can code for 64 amino acids, while only 20 are needed. What about the other possible codes? First some triplets code for regulating codes like start here, stop here. But they also redundantly code for some of the 20 amino acids. So one amino acid can be coded in more than one way. The code for just one protein can therefore be realized in many ways. And if we would try to find the structural code for a specific phenotype, like the colours of human eyes, we run into even more problems since many different proteins are used to code for this single phenotypic structure.

But the problems are even worse. Genes are build up out of structural and regulatory types. The structural type codes for proteins that are used to make the phenotype, while the regulatory type is used to regulate the “genetic machine”. These genes code for proteins that turn on and off the different structural genes. The question is if we want to include these regulatory genes when we individuate a single gene or not. The simple answer would be to say that we should not include them if they are also used in the realization of other genes then the one we are individuating. So if structural type gene A uses regulatory type gene K, and structural type gene B uses K as well, then K is not part of the physical structure of gene A. But this will quickly lead to a slippery slope. There are regulatory genes that are only used for two structural genes. Do we have to include this regulatory gene or not? And if so, why not a regulatory gene that is used with only three, or some more structural genes? Deciding which
parts to include when speaking about a specific gene with this or that function is near impossible.\textsuperscript{30}

In conclusion we can say that there are no structural (physical) properties that are together sufficient, and individually necessary which we can use to individuate the biological function of being a gene.

Of course we can say that there are certain structural properties that are shared among all $F$'s. We can, to use Rosenberg’s example, say that “all mammals are composed of confined quarks”. This is true. But while it might be a law, it is surely not a law that is biological interesting. Being composed of confined quarks is also true of chairs, the moon, or the paper of this print. Being composed of DNA molecules \textit{is} biological interesting. But giving a structural property that all genes posses is, due to the ever changing circumstances and the process of evolution, not possible.

We have seen that for Rosenberg the nomic inhibitor are functionally individuated biological systems. It is due to the fact that natural selection only selects for functional effects and disregards any structural differences with the same effect that we cannot turn biological regularities into strict ones. Gould and Beard’s nomic inhibitor is due to the contingent nature of natural selection. Given a certain initial state, all kinds of biological regularities emerge in the ensuing process. All these problems show that laws in biology cannot be the same kind of laws as we find in physics or chemistry, because there are certain nomic inhibitors that prevent that reliable laws are found by biologists.\textsuperscript{31}

But, as one could reply, can the above examples not be made more precise? Are the examples not laws which are valid within certain situational constraints? One can pursue this tactic, and some have done so.\textsuperscript{32} The idea is that we can evade the problems of the contingency of the biological entities when we expand the explanans with certain situational constraints. We could, for example, formulate the following law: “Humans, creatures that evolved in a situation like on the African peninsula two hundredth thousand years ago, have 23 chromosomes.” In this way the previous law could become exceptionless and universally

\textsuperscript{30} Even more problems arise with the different RNA types, introns, exons, and start and stop codons that are discovered to operate on the macromolecular level. See Alexander Rosenberg and Daniel W. MacShea, \textit{Philosophy of biology : a contemporary introduction}, Routledge contemporary introductions to philosophy (New York, N.Y. etc., 2008), 106ff.


\textsuperscript{32} In history this idea is advanced in Nicholas Rescher and Carey B. Joyn, "The Problem of Uniqueness in History", \textit{History and Theory} 1 (1961, Issue 2) 150-162.
valid. It, then, is the job of the biologist to search for just that precise law, the one that will be a good explanation. Include precisely so many elements to the generalization for it to work, but do not include so many that it will become cumbersome. This is a tactic that has also been proposed, among others, in the philosophy of history by Arthur Danto. Finding the laws of biological history is just like finding the laws of history: the laws must not be too detailed, but not too general either.\textsuperscript{33}

While the example statement might be a true statement, it does not live up to the expectations we have of a law. Laws are normally considered to be informative because of their generality. They tell us something about a lot of occurrences, known and unknown. When, however, a law is broadened in its details, it loses explanatory force. As the philosopher of history William Dray has shown, the more information you include in a law’s \textit{explanans}, the less applicable and universal this law becomes. But the point of laws is that they are universally applicable. Making a law more specific, renders testing that law nearly impossible. A law can never pass scrutiny if there is only one instance to confirm it with. The biologist thus should choose between a law that is general and reliable but uninformative, or a law that is detailed and specific but unreliable.\textsuperscript{34} And that seems to be a choice that cannot be made.

All the above is true for strict nomological laws. But it could be the case that laws should not be seen as strict laws without exceptions. In this view the contemporary biological laws are laws that, in the future, need some sort of refinement, but are fine for now. So all things being equal we can say that “Humans have 23 pair of chromosomes” \textit{ceteris paribus}.\textsuperscript{35} If this fails to be an exceptionless law, this only means, according to this view, that in the future the biologists will have to narrow down the \textit{ceteris paribus} clause.\textsuperscript{36}

This argument starts with the claim that the fundamental laws in physics are in fact also \textit{ceteris paribus} laws.\textsuperscript{37} Most philosophers would say that an exceptionless law a scientist discovers is a law at the level of fundamental physics. No more fundamental explanation is possible, so goes the idea. But the philosopher Nancy Cartwright believes that the idea that

\begin{itemize}
\item \textsuperscript{35} Michael Ruse, for example, compares Newton’s first law of motion with the Hardy-Weinberg principle. A principle we will come to shortly, Michael Ruse, \textit{Philosophy of biology today}, \textit{SUNY series in philosophy and biology} (Albany, NY, 1988), 19.
\item \textsuperscript{36} I will mostly follow Rosenberg’s argument against laws in biology in his Rosenberg, \textit{Darwinian reductionism}, 140ff.
\end{itemize}
even at this fundamental level these laws are exceptionless regularities misses an important point. For the explanatory and descriptive aspects of laws are constantly in conflict when such a law is applied to explain a specific individual event. She writes that laws “[r]endered as descriptions of fact, […] are false; amended to be true, they lose their fundamental explanatory force”. Therefore she believes that biological laws, with respect to factuality, are better laws than physical laws. Biological laws, as she claims, actually show what is going on in a specific situation. Physical laws, on the other hand, miss this property of showing what goes on because they only work ceteris paribus, i.e. when all other things remain the same. But things will never remain the same in physics. If, for example, we take the law of gravity \( F = \frac{Gmm'}{r^2} \), we forget that electricity also, and always, exerts a force on a body:

For bodies which are both massive and charged, the law of universal gravitation and Coulomb's law (the law that gives the force between two charges) interact to determine the final force. But neither law by itself truly describes how the bodies behave. No charged objects will behave just as the law of universal gravitation says; and any massive objects will constitute a counterexample to Coulomb's law.

The law of gravity thus actually is: “If there are no other forces than gravity at work, then \( F = \frac{Gmm'}{r^2} \).” But although this regularity is true, it is, according to Cartwright, not very informative when used to explain a single event: we want to know how and why things happen when other things remain equal, and, if they do not remain equal, how to include the interference into the explanation. So to explain a single event other forces are normally added to the ceteris paribus physical law that is used so that the exceptions and interference is narrowed down. If the number of these forces is finite, we can come to a pretty precise “law” that can be used in the explanation and prediction of the single events. Since the history of the physical sciences shows that the number of fundamental forces that are posited within physical laws is constantly narrowed down to an ever increasing number of fundamental forces, it is not so strange to expect biology to go down the same path.

The question is, however, if we can specify how the ceteris paribus clauses of biological regularities influence a specific event under consideration. Cartwright does not argue that the

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39 Ibidem.
40 Ibidem, citing 57.
41 Note that Cartwright does not believe there is such a thing as a composite force.
42 Rosenberg, Darwinian reductionism, 140-141.
laws of physics are useless in singular events, she argues that we cannot use them without a refinement that means adding a ceteris paribus clause to adjust it to a specific situation. And this refinement eventually comes down to, or so it appears, a finite number of influencing forces – in light of this think about Russell’s remark that the scientist’s working hypotheses is that the world is a uniform place. In the end we know how to work with these laws because there are only a few fundamental laws, and we know, more or less, how to combine them to get to reasonable results. So in physics we do have, so claims Cartwright, perhaps not completely true and exceptionless laws, but we can get around with them quite well. Is this also true for biology? It seems there is a difference. The refinement of the ceteris paribus clause of a biological regularity is different since there are an infinite number of influencing “forces” that act upon a single biological event.

This infinity of possible influences is due to the process of natural selection. Since the selection is so bound up with the environment, every adaption a biological system makes creates a new environment for all the other biological systems and, thus, new adaptive challenges for those other biological systems within that environment. This is meant with the “arms-race character of evolution”. An adaption of a prey species S will lead to a counter-adaption of predator species W, which will lead to a counter-counter-adaption of species S, etc. All this comes down to the conclusion that any biological established regularity will, given enough time, acquire more and more exceptions. Such a biological established regularity is the effect of an adaptation of a biological system. But at the moment a biological system adapts, it changes the environment of all other biological systems. These other systems will again adapt to this new environment, and thereby create exceptions to the original established regularity. These exceptions can be accounted for within the specification of the ceteris paribus clause of the regularity. The problem with this counter-strategy is that the number of exceptions is infinite. With natural selection new innovative ways to survive within an environment always pop up. In other words, “to the extend that general laws must be timeless truths to which empirical generalizations approximate as we fill in their ceteris

43 This insight is turned into a law with the famous “Red Queen hypothesis” in L. Van Valen, "A New Evolutionary Law", Evolutionary Theory 1 (1973) 1-30.
44 But it is not limited to predator-prey situations. For example, it also happens within embryonic phenotypes, between genes that code for the adult’s traits and genes that code for fetus’ traits. In short, the argument is about all biological systems. Rosenberg, Darwinian reductionism, 141.
45 Note that some would not view this as a problem. For example: within the Marxist theory of historical development there is a constant reaction to the previous state as well. The problem with these theories is their speculative character. The most famous criticism on speculative philosophies is Karl R. Popper, The poverty of historicism (London, 1957).
paribus clauses, no such laws are attainable in biology, because we can never fill in these clauses.\(^\text{46}\)

We can see this in the example of the biological generalization \(L\): “Zebras have black and white vertical stripes.” The explanation for why Zebras have this feature is that the stripes make it more difficult for colour-blind lions to catch them. These are the conditions that should remain the same, i.e. the ceteris paribus clause of \(L\). But in the long run lions could develop improved eye-sight, say by developing sight of colours. Natural selection could very well select for lions that easily spot Zebras because they can see colours. With this adaption of the lions the Zebras’ black and white stripes adaptation becomes less effective. The Zebras will, under pressure of the new environment, either die out, or adapt. But this new adaptation, for example having green stripes, has a high chance of not being in line with generalization \(L\).\(^\text{47}\) Because of natural selection there is no hope in establishing a biological generalization that has ceteris paribus clauses that could be specified in a further satisfying manner.

True as this all is, one can reply that the periods under discussion in biology are very long. While not all Zebras might have black and white stripes in the future, it did hold for quite some years, and will probably hold for many more to come. So compared to a human lifetime the ceteris paribus clause of the latter generalization is very stable. And this is also true for many other generalizations in biology. Why all the philosophical fuss if the exception is one that will not take place for the coming thousands or so years? The problem with this reply is that we know from other sciences and our daily experience that generalizations with long lists of exceptions often lack explanatory power. Furthermore, such exception-rich generalizations are often accidental generalizations rather than universal ones.\(^\text{48}\) Admitting that the biological regularities are exception-rich, but that the exceptions are not bound to happen quickly only means that there is a difference in degree between accidental generalities and biological generalities. If this line is chosen, there has to be an argument that shows why common accidental regularities do not explain, while biological ones do. And this is, as is shown above, not possible.

Philosophers of biology did not avail. Elliot Sober, for one, thinks there are laws in biology. These laws are rather \emph{a priori} in nature. And this, he claims, is not a problem at all. He

\(^{46}\) Rosenberg, \textit{Darwinian reductionism}, 142.

\(^{47}\) This argument is intricately bound up with arguments that deny the existence of biological species. But it is more general than the there are no species arguments for it applies to all biological systems, not only to species. See note 44.

\(^{48}\) Again, the exact meaning of a “universal law” remains debated, but that there is some kind of universality in the laws of nature, and that scientists are in search of these laws, seems clear. See page 9.
discusses the Hardy-Weinberg principle to show how these a priori laws are used by biologists.  

The Hardy-Weinberg principle is a formal model which states how, in an ideal situation, the frequencies of genes remain constant between two generations of a population. With this formal statement population genetics can “uncover the patterns of [genetic] dynamics via the causes of evolution, namely, mutation, migration, natural selection, and random drift.”  

The principle assumes that these variables remain the same. There are further some conditions that should be met for the principle to work: the population under investigation must interbreed completely random, it must be infinitely large, it must not undergo mutation, and it must consist of diploid organisms with only one genetic locus and two alleles.  

This, of course, is a highly idealized situation, for in reality these kind of populations cannot exist. But if these background conditions are present, the frequency of all the genes in a population will remain stable between generations. This means that the model can be seen as a (set of) general conditional statement(s), just as models in the physical sciences are considered to be general conditional statements. If the conditions apply in a specific situation, the model can explain that situation, or predict what will happen.

The Hardy-Weinberg laws works as follows. Given a set of initial allele frequencies and the above mentioned conditions, the genotypic frequencies remain constant between generations. So if we have a certain distribution $p$ and $q$ for the alleles $a$ and $A$ in generation 1, the frequencies of all the different combinations, $AA$, $aA$, $Aa$ and $aa$, for generation 2 can be calculated, and the result is:

$$AA : p^2$$  
$$Aa : 2pq$$  
$$aa : q^2$$

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51 A diploid organism is an organism which has two copies of each chromosome, mostly one of each parent. A genetic locus is the location of a gene on a chromosome, i.e. the location on which the genetic code for a certain trait is coded. Stating that there is only one locus means that one gene, and one corresponding trait, has only one physical location on the chromosome. An allele is, in case of a diploid organism, a member of two or more different possible pairs of genes occupying a specific locus. So an animal can have an allele for blue eyes, brown eyes, and green eyes.
Here \( p^2 \) \( 2pq \) and \( q^2 \) are the frequencies of the possible combinations of the genotypes \( AA, \) \( aa \) and \( Aa \) taken together. In this second generation the proportions \( p \) and \( q \) are the same as in the first generation. So if there would be an ideal situation, the gene frequencies of these alleles \( a \) and \( A \) would never change. In reality changes actually do occur. Looking how real situations differ from the principle can, according to population geneticists, tell us how an external factor influenced the process. A much used external factor is natural selection, included in the formula using the \( \text{fitness}(w) \) variable. By embedding fitness into the formula – measured as a relative value between different genotypes – the effect of \( w \) upon the frequencies of the alleles \( A \) and \( a \) can be measured. The result will be:

\[
\begin{align*}
AA &: p^2w \\
Aa &: 2pqw \\
aa &: q^2w
\end{align*}
\]

With this knowledge we can calculate the respective frequencies of \( A \) and \( a \) after natural selection took place.\(^52\) "Starting from a mathematical statement about the distribution of allele frequencies in the absence of evolutionary causes, one may understand the ways in which those causes change that distribution by modifying the mathematical statement with parameters measuring the influence of those causes."\(^53\) So the putative causes of evolution – mutation, migration, multiple modes of selection, and random drift – can be uncovered by means of the Hardy-Weinberg principle. Or at least, that is the idea behind population genetics.

But this Hardy-Weinberg principle is nothing more than a mathematical truth. We can replace alleles with coins and the Hardy-Weinberg principle would still be valid. Sober shows this by rewriting the Hardy-Weinberg principle into the following:

If two coins are tossed independently, where each has a probability \( p \) of landing heads and \( q \) of landing tails, then the probabilities of getting two heads, one head and one tail, and two tails are \( p^2, 2pq, \) and \( q^2, \) respectively.\(^54\)

The formula is the same, the results are the same, but it is about coins instead of genes. Sober does not judge this \textit{a priori} character of biological laws as something negative.\(^55\) He believes that this just shows the interesting way in which biology differs from the physical sciences. Biological models explain because, when a species satisfies the assumptions of the model, a certain outcome can be expected. These models are general conditionals. Sober now

\(^52\) We can calculate the average fitness by adding the frequencies of all the genotypes. We then calculate the frequency of allele \( A \) by dividing \( p^2 + 2pq \) by the average fitness. The same can then be done for allele \( a \) (dividing \( q^2 + 2pq \) by the average fitness.) The resulting difference shows the difference in fitness for \( A \) and \( a \).

\(^53\) Millstein and Skipper Jr., "Population Genetics", citing 28.

\(^54\) Sober, \textit{Philosophy of biology}, 73.

\(^55\) Ibidem.
stresses this logical form of the conditional since this form is also predominant in the physical sciences. If certain ideal conditions apply, for example the assumptions of the billiard ball of gas model, we can expect that gas behaves nearly in the way the ideal gas law predicts. The difference between biology and the physical sciences, or so Sober claims, is that in the physical sciences the models are empirical, while those in biology are a priori.

It seems that Sober is right in pointing out this difference. Still, the models in biology do not differ that much. A physical model is as much a mathematical construction as a biological model. The mathematically a prior form of the model cannot be the biggest difference. There is another more pressing difference. Both in biology and in physics there are models that have highly idealized assumptions and, as such, are limited in their domain of application. But the limits of a model in physics, applied to a specific situation, can be explained by more fundamental, and thus more exceptionless laws. Using a more fundamental law to explain why a model in biology is or is not applicable to a specific situation cannot be given since there are no more fundamental laws. So the explanation why the idealized gas law does not apply to situations in which a space is under a very high pressures is that we know that molecules do not behave as the billiard ball model states they do. In a gas under high pressure the molecules are also influenced by other forces like gravity and electromagnetism. Molecules do not just bounce around like billiard balls. We know this because we have more fundamental laws (or models) about how molecules behave in respect to each other. These fundamental laws are more universal and more exceptionless generalizations about the world than is actually assumed in the former model. Physicists start with models that are full with highly idealized assumptions and, therefore, have a limited domain of application. Later on these highly idealized assumptions are loosened and the domain of application is broadened. In biology, as shown above, we do not have the more fundamental laws. Why a biological model does or does not apply cannot be answered with some other regularity or model. It is more the other way around. Biologists identify a certain phenomena after which they try out which model applies. As the biologist Levin writes: “The validation of a model is not that it is "true" but that it generates good testable hypotheses relevant to important problems.”

Another way to put this distinction between models in physics and biology is that in physics the world is the model of the theory, while in biology the theory is the model of the world.

57 This is quite similar to the way economists use mathematical models. For this comparison see for example Kuipers, "Laws, Theories, and Research Programs", citing 80-81. Notice that Kuipers points out that theoretical improvements in scientific fields that use these kind of models function as truth approximations. The recent economic crisis indicates that the theoretical improvements of economics are still not very successful.
Sober’s proposal to just accept the *a priori* state of laws in biology, i.e. see biological models as laws, does not lead us anywhere. While biological models do have certain similarities with those of the physical sciences, they are used in a different manner. Biologists need to explain why their mathematical models apply to specific situations, physicists create mathematical models that are then applied to a specific case to explain that case. This latter kind of explanation is something the biological models lack.

To sum up, we can say that laws are not present in biology, or at least not in evolutionary biology. If, however, we stick to the idea that laws are necessary to explain and predict events, and that laws are constant conjunctions of the kind described above, than the question is how evolutionary biology explains. If it does not use such laws, how do we justify the knowledge that is produced in this field? Before this problem is explored we need to take a look at the last, and according to some, best candidate for a biological law.

### 2.2 Natural selection and the tautology problem

One of the most important components in evolutionary biology, if not *the* most important, is the principle of natural selection. Most biologists see this principle as the driving force behind the success of the explanatory value of the theory of evolution. The ascent of the modern synthesis in the 1940’s and 1950’s brought natural selection to the fore. Since then it has been denied, played down, or redressed, but it never left the scene completely. Some think that this principle is *the* law that can be used to derive, or constitute all other laws in biology, or that it can fundamentally explain all biological phenomena. Others think it is not a law, but still the most important aspect of evolutionary thinking. Nevertheless, this principle of natural selection (PNS) remains as opaque as the status of regular laws in biology.

The above mentioned problems regarding laws within biology are all due to the nature of natural selection. The biological world seems to evade the straightjacket of laws because the nature of the PNS constantly changes the rules of the game. This nature results in all kinds of nomic inhibitors. In the biological world, today’s winners are tomorrow’s losers. There is a constant arms race to gain the upper hand in the struggle of life. Consequently, the players change the rules while they play. But the PNS itself does not seem to fall under this struggle. It is the PNS that lays at the basis of this arms race. The PNS implies, or leads to the problems

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58 If the biologist Dobzhansky is right in believing that all biology is evolutionary biology, and it does seem that he was right, then there are no laws in the whole field of biology. Theodosius Dobzhansky, "Nothing in biology makes sense except in the light of evolution", *The American Biology Teacher* 35 (1973, Issue March) 125-129.

of the non-universality of biological laws, which prevents strict-laws from being formulated. And the PNS causes, if that word can be used in this context at all, the inability to further specify the conditional ceteris paribus clauses of non-strict laws. So the most likely candidate to actually be a law within biology is the PNS itself. The question is not if the PNS is true or false, with few exceptions biologists and philosophers accept the PNS as being true in one sense or the other. The question rather is, if the PNS is a law or set of laws, and whether this is the law that confers explanatory power upon natural selection. To get an idea of what this law might be, we will take a look at the different manners in which this principle is defined, and what problems arise with these definitions.

The PNS is, of course, derived from Charles Darwin’s *On the Origin of Species*. Darwin gave the most concise formulation of the principle of natural selection in the introduction:

> As many more individuals of each species are born than can possibly survive; and as, consequently, there is a frequently recurring struggle for existence, it follows that any being, if it vary however slightly in any manner profitable to itself, under the complex and sometimes varying conditions of life, will have a better chance of surviving, and thus be naturally selected. From the strong principle of inheritance, any selected variety will tend to propagate its new and modified form.

Central concepts in this definition are the ensuing struggle, which with the constant occurring of variation leads to differentiating survival, which, because of the “strong principle of inheritance” leads to selection. While he saw variation and inheritance happen, the mechanisms or reason for these phenomenon were unknown to Darwin and his contemporaries. It is not surprising that, up until the great synthesis, many disagreements were related to these two concepts. In the twentieth century the advent of macromolecular biology, especially since the Watson and Crick discovery of DNA, made these two concepts much more clear. Variation and inheritance are now uncontroversial terms that are understood in principle, if not in practise. Ironically, the scientific community, with the advent of the

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60 There is, however, much disagreement about the uniqueness it has in explaining the whole process of evolution. Especially the famous “spandrels” article of Lewontin and Gould resulted in much discussion about the sometimes dogmatic reliance on the PNS. Stephen Jay Gould and Richard C. Lewontin, "The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme", *Proceedings of the Royal Society of London. Series B, Containing papers of a Biological character* 205 (1979, Issue 1161) 581-598.


modern synthesis, blindly accepted the other concepts, struggle, survival and selection for a long time, while there was, and is no consensus about the precise meaning of these concepts at all. The biologists of the past quibbled over those concepts we now know best, while uncritically accepting those that are problematic to say the least.

The controversy over the PNS shows this problematic nature. For biologists which treat evolution as being about populations, the PNS – somewhat derived from Darwin’s definition – can be interpreted as follows:

If \( x \) and \( y \) are competing populations and \( x \) is fitter than \( y \) in a certain environment \( E \) at generation \( n \), then probably \( x \) has more members than \( y \) in environment \( E \) at some later generation \( n' \).

The question now revolves around the meanings of \( fitter \) and \( probably \). What do biologists mean when they use the relational property \( x \) is fitter than \( y \)? And what does probably mean?

The question of the definition of fitness can be answered by referring to the way in which a creature or population can solve the survival problems it faces. This is called the ecological or vernacular fitness interpretation. A fit population is able to cope with a certain environment better than another less fit population. The philosopher Daniel Dennett for example has defined fitness as:

\[ x \text{ is fitter than } y \text{ if and only if } x\text{'s traits enable it to solve the ‘design-problems’ set by the environment more fully than } y\text{'s traits do}.\]

Although this definition is the most common way by laymen to talk about the theory of evolution, it makes viewing the PNS as a law problematic. What exactly are design-problems? And how do we objectively measure the relative fitness between \( x \) and \( y \)? Giving answers to these questions is hard. Is a design-problem a vague metaphorical notion? Then it has no place in a strict scientific law. If design-problem has literally meaning, it is required to define what the design problems mentioned exactly are. And this, in evolution, seems eventually to be about getting as much offspring as is possible within the situation. If we now fill in the definition of fitness into our original definition of the PNS we get:

\[ x \text{ is fitter than } y \text{ if and only if } x\text{'s traits enable it to solve the ‘design-problems’ set by the environment more fully than } y\text{'s traits do}.\]

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63 For the latter characterization see Mohan Matthen and André Ariew, "Two Ways of Thinking About Fitness and Natural Selection", *Journal of Philosophy* 99 (2002, Issue 2) 55-84.
65 I would, by the way, not dear to claim that Daniel Dennett is a layman.
66 I disregard the distinction between fitness as viability and fitness as fertility. While Darwin made this distinction quite explicit when he writes about sexual selection and natural selection, modern biologists seem to regard them both as parts of one fitness.
If \( x \) and \( y \) are competing populations and \( x \)'s traits enable it to solve the ‘design-problems’ set by the environment \( E \) more fully than \( y \)'s traits do in environment \( E \) at generation \( n \), then probably \( x \) has more members than \( y \) in environment \( E \) at some later generation \( n' \)

But since “solving design-problems” means getting as much offspring as possible – what else is the problem of a creature in an evolutionary context than to procreate? – we get a circular argument: Population \( x \) has more offspring than \( y \) because it is better at solving the design-problems in their environment, which means, \( x \) has more offspring than \( y \). And this tells us nothing new.\(^{67}\) Using this definition of fitness as a law cannot work. It results in a tautological definition in which the definition is equated with the method of measurement. It has been a constant philosophical challenge to deal with this circular character of the PNS.

A related issue with the ecological fitness interpretation is that it is principally not possible to get a finite set of design-problems. As mentioned above, finding laws in biology is impossible because the evolutionary process keeps changing the players and the playing field. This changing character of evolution also applies to the notion of a design-problem. Therefore, there cannot be any law that offers a quantitative measurement of fitness which can be used to predict and explain a difference in rates of reproduction. In other words, with the ever changing nature of the design-problems no law can be devised that has a single operational mode of measurement. Measuring a difference in fitness values between two populations, if fitness is seen as a design-problem, is not possible.

For now, we can conclude that the ecological interpretation of fitness does not yield the kind of justification that a scientific law requires. The notion of a design-problem leads to a definition of the PNS that is tautological and not measurable.

The other popular interpretation of fitness, one that is more generally accepted by biologists and most philosophers of biology, is the propensity interpretation of fitness. This interpretation is stated by Robert Brandon, and, in a famous article, by Susan Mills and John Beatty.\(^{68}\) They proposed to interpret fitness as a probabilistic disposition to have a certain number of offspring. The most common example of a propensity property is being magnetic. A piece of metal can be magnetic while never attracting any actual iron filling. Therefore we say that a magnet has a disposition to be magnetic. We see that the dispositional property

\(^{67}\) This argument also lays at the basis for Karl Popper’s famous claim that the theory of evolution cannot be falsified. See Karl R. Popper, Unended quest : an intellectual autobiography, Repr. with updated bibliography editie. (London etc., 1992).

“magnetic” is distinct from the behaviour it might cause. But being magnetic is also distinct from its cause: two different pieces of metal can be magnet. This means that dispositional properties supervene on the physical structure that bring them about.

In the same way, an organism or group of organisms can have the disposition to have more offspring than another organism or group. Nevertheless, because of bad luck – say because it is struck by deadly lightening – this fitter organism cannot realize this higher number of offspring. So fitness is a disposition. Furthermore, a zebra can, given a certain environment, have the same fitness value as a fox. While a zebra and a fox differ in structure, they can have the same fitness-value. So fitness is also a supervening property, being fit is distinct from the specific physical structure that brings about this fitness. Therefore, Brandon, Mills and Beatty define fitness as a disposition for having a certain difference in number of offspring.

They did noticed a problem when seeing fitness as a disposition. We do not know which particular disposition to use when a biologist speaks about a certain fitness value. For an organism (or group) can have more than one disposition for producing a number of offspring. For example, it can have the dispositions to leave one offspring, two offspring, and three. Which of these dispositions does the biologist use when talking about the organism’s fitness? The solution is to view fitness as the expected number of offspring, i.e. what is probably going to be the number of offspring. With this probabilistic character added to the propensity part we get the following definition of fitness:

\[ x \text{ is fitter than } y \text{ in } E = x \text{ has a probabilistic propensity } >.5 \text{ to leave more offspring than } y \]

Since fitness is now defined as differing from its effect, the problems with circular arguments that plagued the ecological fitness interpretation are evaded. For with the probabilistic propensity interpretation, the measurement of fitness is not equated with its definition. And that was just the problem with other interpretations.

According to the proponents of the probabilistic interpretation, the claim that fitness leads to a tautological definition rests on the mistaken assumption that an explanatory term must always be defined in terms that are distinct from its causes and effects. They argue that this mistaken view, an inheritance of the logical-positivists, was proven wrong when it became clear that the strict reduction of theoretical terms to empirical observations was illusory. We cannot reduce a theory to observation without mentioning the correct explanatory context. So, according to Brandon et. al., it is also not required to define an explanatory term like fitness

\[ ^{69} \text{Mills and Beatty, "The Propensity Interpretation of Fitness".} \]
without any reference to the causes and effects of this term. In the case of fitness, the causes are the physical relations between the organism(s) and the environment, while the effects are the expected number of offspring. Fitness is exactly that which intervenes between these two elements of cause and effect.

Since Mills and Beatty’s publication, most philosophers and biologists embraced this interpretation. Nevertheless, there are some serious problems with the propensity interpretation that are insurmountable. The first problem with the propensity interpretation, due to Elliott Sober, is that it does not work well with a lot of the probabilistic statements in biology. A propensity property is characterized as an *if then* statement. *If* a lump of sugar is immersed in water *then* it melts, is a statement that identifies the propensity of sugar to melt in water. And it is clear that the immersing causes the melting. However, biologist use more than only *if then* statements. The probabilistic statement that a heterozygote organism has heterozygote parents is not a causal relation between the antecedent and the consequent of the conditional. It is a statement about the chance that a heterozygote’s parents are also heterozygote. The cause and effect are turned upside down, so to speak. Such a statement cannot be stated in the “if then” form because a child cannot cause such a thing in its parents.\(^70\)

Another more pressing problem is that this definition of fitness is plainly false. In the biological world there are cases in which it is not at all desirable to maximize the number of offspring. Variance in the number of offspring of an organism that is due to temporal or spatial differences occur all the time in the biological world. So in certain periods, or at certain locations it is more favourable to have less offspring instead of more. A simplified example is the following thought experiment by Robert Brandon.\(^71\) One can have two organisms, \(z\) and \(x\) that differ in number of offspring per year. Organism \(z\) has 2 offspring each year, while organism \(x\) has 3 offspring each odd year, and 1 offspring in even years. The average number of offspring appears to be equal. Nevertheless, after one generation \(x\) will be more fitter than \(z\). But after another year, \(z\) will be fitter again. The next year, \(x\) will have the upper hand again. But after nine generations \(z\) will have 512 descendants while \(x\) will only have 253. This argument about temporal variance holds for populations as much as for

\(^70\) Sober, *Philosophy of biology*, 64. Danto shows that effects can precede their causes in statements like “Aristarchus anticipated in 270 B.C. the theory which Copernicus had published in A.D. 1543”. But these statements do not lead to regularities; they are thoroughly historical. The whole idea of clearing up the definition of fitness is to find out if the PNS can be defined in a non-historic manner. For Danto’s argument see Danto, *Analytical philosophy of history*, 153ff. Sober’s argument thus already indicates the intrinsic historical nature of biological language.

\(^71\) This example is from Brandon, *Adaptation and environment*. 

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individuals. Now one could be tempted to state that this is not a problem. Getting the right fitness value just depends on which generation you choose. The question what fitness means is thus answered with: “it depends on what you want to know.” But that is an answer that has to do with the measurement of fitness, it has noting to do with its meaning. And the whole point of the propensity interpretation was to get rid of the connection between measurement and definition.

Next to this problem of temporal differences of offspring, there is a very similar problem of a spatial influence on the number of offspring. When being in a certain place a, it could be less profitable to have many offspring than at another place b. Certain species reduce their young because of a regional lack of food sources. Raising one strong child is sometimes better than raising two weaker ones. Getting fitness right is a perilous job. And again, closing the matter by saying that it all depends on the question asked is not helpful. Such a strategy confuses the meaning of a term with its measurement.

A putative solution to this problem is to add a *ceteris paribus* clause to the PNS definition:

\[ x \text{ is fitter than } y = \begin{cases} \text{probably } x \text{ will have more offspring than } y, \text{ unless their average numbers of offspring are equal and the temporal and/or spatial variance in } y \text{'s offspring numbers is greater than the variance in } x \text{'s, or the average numbers of } x \text{'s offspring are lower than } y \text{'s, but the difference in offspring variance is large enough to counterbalance } y \text{'s greater number of offspring} \end{cases} \]

But now the question is how many of these clauses are there so that all exceptions can be accommodate for. This is a legitimate question for there are more exceptions. For example, in certain circumstances in which the mean fitness is low, a higher variance is sometimes selected for. While the higher variance might well result in less fitter offspring in the next generation, the chance that some offspring of a later generation has a higher fitness is increased, resulting in more descendants for the original organism. It seems that there is no finite set of clauses that can be used to buttress the PNS.

The general problem seems to be that only counting the offspring of the following generation is not enough to define fitness. A reply could be that more than one generation should be counted. But then the question is at which generation to stop counting? At the second generation? Or the third? Or should the time-frame be taken very broad, like $10^8$?

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72 Rosenberg, *Darwinian reductionism*, 163.
73 For example Ibidem.
74 See for example John Beatty and Susan Finsen, "Rethinking the propensity interpretation -- a peek inside Pandora’s box" in: Michael Ruse and David L. Hull ed., *What the philosophy of biology is: essays dedicated to*
years, as was once proposed. No objective answer can be given. The number of offspring alone is just not enough to define fitness. Different cases lead to all kinds of different variances in this variable.

To accommodate these different variances Brandon introduces an additional function into his propensity definition of the PNS. This function represents the variance on the number of offspring. It is defined as “some function of the variance in offspring numbers for a give type[…]and of the pattern of variation”. When the PNS is used in a particular case this function modifies the fitness value according to the variance that has to be applied. The function thus accounts for the differences between expected fitness and the actual number of offspring that is observed. The point is thus that we can only determine this function after examination of the phenomena. Things are actually more complicated since there are more factors that influence fitness that also influence each other. It could be the case that one factor, a temporal variance like the example above, is cancelled out by a spatial variance. The question is if these functions add to the conceptual clearness of fitness. As Rosenberg asks, “how many such factors are there, and when do they play a nonzero role in fitness?” He asks if there is ever an ideal situation in which these exceptions do not play a role. Without such an ideal situation this fitness definition becomes unusable since the function is always determined after the examination of the phenomena. But the answer to the question is no, there is no ideal situation. There are indefinitely many factors because “every strategy for enhancing reproductive fitness (including how many offspring to have in a given environment) calls forth a counterstrategy among competing organisms”. And this counterstrategy again undercuts the initial strategy. The problem that troubled “ordinary” biological laws also seems to trouble the PNS: the arms-race nature of evolution makes the class of operational measures of fitness infinitely large. Consequently we do not know how to measure fitness in a single and uniform way.

A third problem of the propensity interpretation of fitness is the part that the word “probably” plays in this definition. For the term probabilistic in the antecedent of the whole

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Sober points to this interpretation, due to Thoday. Thoday proposed this large number to show how evolution is a progressive process. Sober criticizes this view for its unscientific nature. Sober, "The Two Faces of Fitness".

Brandon, Adaptation and environment, 20.

Rosenberg, Darwinian reductionism, 164.

Ibidem.
fitness definition needs to be different from the probability of the consequent of in the PNS
definition. If embed the propensity definition into the PNS we get the following:

If \( x \) and \( y \) are competing populations and \( x \) is fitter than \( y \), i.e. \( x \) has a \textbf{probabilistic propensity} \( >.5 \) to leave more offspring than \( y \), in a certain environment \( E \) at generation \( n \), then \textbf{probably} \( x \) has more members than \( y \) in environment \( E \) at some later generation \( n' \).

The first probable has to be different in meaning from the second probable, for else the
definition is again circular, and thus not falsifiable. We can see this better if we reformulate
the definition in a more informal way: “If \( x \) has the disposition to probably have more
offspring than \( y \), then it is probable that \( x \) will have more offspring than \( y \).”

Giving two meanings to one term needs a justification. A possibility is to claim that the first
propensity interpretation of probability is a brute fact of the world while the second is some
other interpretation of probability, mostly a frequency interpretation. But up until now, there
have only been observations of brute probabilities in quantum mechanics. It can be doubted
that there are any similar stochastic facts in the biological world, and no conclusive proof of
such processes has been given.\(^79\) Yet to be viable, the meaning of the propensity interpretation
needs to include a difference between the two probabilities, for else it will not work.\(^80\)

To sum up, we can say that defining \textit{fitness} and \textit{probability} is a difficult undertaking. The
current proposals all fail. The conclusions that can be drawn from this apparent failure
resonates throughout the different fields of the philosophy of biology. Questions about the
possibility of reductionism, the central role of the gene, and ideas about adaptationism are all
related to the working of the PNS.

\section*{2.3 Explaining in biology}

The lack of laws and the ambiguity of the principle of natural selection raise important
questions about the nature of explanations in biology. The received view of science does not
seem to apply to the biological sciences, or at least, not to the evolutionary variant of this
science. And since Theodosius Dobzhansky declared that “nothing in biology makes sense
except in the light of evolution”, these problems seem to concern the whole field of biology.\(^81\)

What next?

\(^79\) Alexander Rosenberg and Frederic Bouchard, \textit{Fitness} (Summer 2009, 2010); available from
\(^80\) In general the definition of probably is a difficulty that philosophy has yet to overcome.
\(^81\) Dobzhansky, "Nothing in biology makes sense except in the light of evolution".

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The first reaction could be to accept that there are no laws in biology and that the PNS is not a law or law-like generalization either. The conclusion would then be that biology is not a real science, or that evolutionary theory is flawed. This view is sometimes associated with the earlier Karl Popper and is used extensively in the school curriculum debates in the United States about creationism, intelligent design, and evolution. The earlier Popper mainly stressed the inability to falsify the fundamental hypotheses of evolution. From the point of view of his falsificationism the PNS and all related theses look rather a priori. Every single biological property can, eventually, be explained away as being an adaptation. Ergo, explanations always work. This is similar to the same kind of reasoning of Gould and Lewontin when they charged many biologists in the seventieths of thinking like pangloss. The biologists seemed to think: there is no biological property that the PNS cannot explain, so the PNS automatically explains everything.

To the extent that one takes an extreme position regarding how science is to be justified, this is a firm but just conclusion. If you think science is only about strict laws that need to be falsified or justified in controlled environments, biology is no science. Only physics can claim that position, is the thought. But as the history of the philosophy of science has shown, only prescribing what science needs to do leads to justificatory philosophies that never match the reality of scientific practice. Prescription is not the task of the philosopher of science, still those that find biology unscientific are unjustly prescribing what scientists should do. Because of the unnecessary high standards, this position will lead to scepticism about many sciences, not only biology. Other sciences, like history, will be ruled out as well. But since most people tend to think that biology, and most other sciences, are bona fide sciences, the extreme position does not hold.

Three alternatives now remain that do not downplay the scientific status of evolutionary biology. The first one is to accept the received view of science – the axiomatic view of explanation – notwithstanding the above mentioned problems. In this view, fitness should be

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82 For Karl Popper’s earlier views on evolution see Popper, *Unended quest: an intellectual autobiography*. The view that evolution is an unscientific view is brought forward many times in the evolution-creationism debates. For a history about these debates in the United States see Witham, *Where Darwin Meets the Bible*.

83 Gould and Lewontin, “The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme”.

84 Note that this is not Gould and Lewontin’s conclusion. They wanted biologists to pay more attention to drift, the process of random non-detrimental gene dispersion in populations. Since drift is now part of the mainstream thinking about evolution, it seems that their quest was a success.

85 This is one of the lessons that Thomas Khun and the more radical strong programme learned: look at what scientists actually did and do. The strict division between the context of justification and the context of discovery leads to unrealistic views about science. What scientists do resides predominantly in the latter context. Keeping that context out of scope in the philosophical analysis leads to dead ends.
seen as a primitive term on a highly axiomatic level. In the axiomatic model there are two kinds of terms, theoretical terms and primitive terms. The theoretical terms need correspondence laws because, in the end, they are to be about real world phenomena: theories need to be reduced to observational regularities. Primitive terms, on the other hand, are *a priori*. As a highly axiomized primitive term fitness is supposed to be self-evident. But the above mentioned problems with respect to laws, and the argument against Sober’s *a priori* laws deflect the hope to build a coherent axiomatic theory that does justice to the intricacies of biological explanations. In other words, saying that fitness is *a priori* ignores the problem of defining fitness, and how the mechanism of the PNS works. Biology needs correspondence rules if it is to explain as an axiomatic system. This model is unable to cope with the complexity and questions that are implied in evolutionary biology.

The other view is the semantic theory of explanation. In this view the evolutionary theory does not consist of one system, but of many different formal models of explanation. Different models can be used in different situations, and not all models work in all the different situations. So a specific variation on the Hardy-Weinberg principle might be in line with some real population $p^1$, while with respect to population $p^2$, it is not applicable. The model view of explanation has the advantage over the received view that laws are not a necessity. Nor does this view need the correspondence rules (that connect theoretical terms with direct observation terms) that frustrate the axiomatic model. It also fits well with the diversity of sub-disciplines in biology. But the semantic theory of explanation has its own problems. Among others, there is the question of how many models there are, and what the common denominator is between all these models. There has to be such a common denominator, because without it, there would be no explanation for how the world became such that all these models apply to it at these specific times and places. We cannot say that biology is a collection of models without answering why we think these models are all part of the evolutionary theory. This common denominator is the PNS.\textsuperscript{86} For implicitly, all models do rely on the PNS in one way or another. The PNS is what binds them in being about the evolution of all biological life on earth. But with this reintroduction of the PNS the problems of dealing with it are back on the agenda. We cannot avoid giving an explanation of how and why the PNS works.

Both alternatives, the axiomatic approach and the semantic approach try to ignore the philosophical problems of the PNS by claiming that these philosophical problems are of no

real concern. The former because it axiomizes the concept of fitness, the latter by chopping up all the different explanatory methods and parts of biology into models. But both still fail to do justice to the PNS.

In this thesis a different approach is taken. Biology will be seen as a predominantly historical science. Since evolution is a claim about history, it seems logical to take a look at the philosophy of history for insights into how historical explanations work. For the different problems with fitness and the PNS can be solved with these insights.

The discussion about laws in history took a different direction in history than it did in biology. Many philosophers have tried to “inject” laws into historical explanations. If we look at the debate about the covering law model, we see that it was mostly about how laws, at least on the surface, were lacking in the field of history, and how this hiatus could be justified on a philosophical level. In biology, this trend happened the other way around. Until Mendel’s ideas were incorporated into Darwinism, evolution was often believed to be a force that was similar to the law of gravity. Only later did philosophical issues concerning the status of the biological laws start to trouble biologists, and, after the downfall of the logical-positivist programme, also philosophers of science.

Perhaps these opposite directions of interest is the reason for the limited cross-fertilization between the philosophy of biology and history. The historians questioned the necessity of laws in their field, while the philosophers of history could first not envisage history function without it. The biologists could not envisage their field without laws, while philosophers of biology started to question the validity of those laws. The consequence has been that for a long time the philosophies of both fields travelled in opposite directions. A few attempts have been made to view the one in light of the other. But a radical change, such as after the publication of Hayden White’s Metahistory, did not occur in the philosophy of biology. Narrativism did not receive the full attention in the philosophy of biology as it did in the philosophy of history. Deeper investigations of the narrative nature of evolutionary thinking seems more than welcome in light of the ever growing presence that the theory of evolution acquires in, and outside the academic world.

87 The locus classicus that most concisely presents the covering law model is Hempel, “The Function of General Laws in History”. Apt examples of the “forceful” infusion of laws into the field of history, one older, the other more contemporary, are Henry Thomas Buckle, “History and the Operation of Universal Laws” in: Patrick Gardiner ed., Theories of history (Glencoe, IL, The Free Press, 1959) 106-124, Morton White, Foundations of historical knowledge (New York etc., 1965), especially 27-29.
3 Narrativism in the philosophy of history

If biology is a historical science, and if this science is narrative in nature, then taking a look at the nature of historical narratives seems prudent. Historical narrativism is a philosophy about what historians do and produce. It is not a speculative view about the content of the historical process, but rather a critical philosophy about the procedures and products of historians, and how these procedures and processes are justified. Historical narrativism developed out of a criticism of two other critical philosophies of history. Both of these philosophies are obsolete in their most radical form. The first grew out of the extensive philosophical oeuvre on the natural sciences. The other focuses and elaborates on the transformation of the old methodology of text interpretation and became a philosophy about the interpretation of all human symbolic communication.

Out of the logical positivist’s philosophy grew the idea of the application of the covering law model to historical knowledge. During the fifties and sixties there was a vehement debate between philosophers of history about the validity of Carl Hempel’s covering law model of explanation in history, and about the different emendations to this model. In the covering law model an empirical observation and a general law are combined to explain a particular fact of history. This explanation is valid because the particular fact to be explained follows logically from the empirical observation(s) and a general law(s), hence the name covering law model. Mainstream philosophy (of history) has all but abandoned the strictest variant of this approach. Generally one can say that laws are neither sufficient nor necessary for historical explanations. In the end, it seems that historians are just not interested in generalities. The specifics of the past are the things that make the historian’s hearth beat faster. The covering law model, however, presupposes the exact opposite. While attempts have been made to find a middle road between the generality of the law and the specificity of description, satisfying result are yet to be found.

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89 For this distinction between speculative and critical philosophy of history see, among many others, William H. Walsh, An Introduction to Philosophy of History, 3th editie. (London etc., 1967), 13-17.
91 Within the philosophy of science it is recognized that the covering law model, being a syntactic justification of explanation, fails to do justice to the actual scientific practices. The best known example of a syntactic valid, but practical invalid explanation is the shadow of the flagpole that explains the flagpole’s height.
92 The most fundamental criticism of the covering law model can be found in Dray, Laws and explanation in history.
93 An example of an attempt to find this middle road is Danto, Analytical philosophy of history, 220-232.
A second general approach is hermeneutics. In this view, writing history is all about interpreting the intentions and actions of past actors. The best known proponent in the English speaking world is Robin C. Collingwood. The historical hermeneutical approach originates out of the works of Friedrich Schleiermacher and Wilhelm Dilthey.\textsuperscript{94} Dilthey saw historical research as understanding of past human experiences out of the cultural remains of those humans. Similarly, Collingwood thought that the main objective of the historian is a form of re-enactment of past thoughts. The ideas and intentions of historical actors should be re-enacted in the mind of the historian so that a good interpretation of the past can be made. The difference between Dilthey and Collingwoos is that the former is concerned with the whole range of human experience, while the latter, seeing all kind of problems with this broad strategy, only aimed at thought patterns as grounds for explaining human action.\textsuperscript{95}

The biggest problem with these approaches is the lack of attention they have for the unintended consequences of intentional action. No sane person ever intended the First World War to become the stalemate battle of unprecedented destruction. Notwithstanding the intentions of the different sides to quickly make do with the enemy, finding out how the stalemate war became possible is a legitimate goal nonetheless. Much historical research has been devoted to answer this question. Qualifying this kind of historiography as non-history because it is not about re-enactment is absurd. Because of this deficiency, the hermeneutical view as a legitimization of historical knowledge falls short.

3.1 The narrative fact distinction

The approach that is most in line with actual historical practise is that of the narrativist philosophy of history. Narrativism originates out of hermeneutical, analytical, and historicist philosophies. This philosophy underlines that the search for models of explanation within the narratives of historians, as the above mentioned philosophies do, ignores the fact that historical narratives are explanatory in themselves.

The starting point of this approach is the two-step analysis of history in a) the historical facts and b) the narrative whole. These historical facts are given in statements that make up the narrative. But the important point is that this narrative whole is not just the concatenation


\textsuperscript{95} Walsh, \textit{An Introduction to Philosophy of History}, 49-50.
of these statements. A narrative has a different epistemological status, i.e. narratives are not only the conjunction of their consecutive statements. And because of this different epistemological status, narratives are to be analysed separately from the facts.

This distinction is similar to the nineteenth century German Historist movement’s distinction between the gathering of data, Geschichtsforschung, and the interpretation and ordering of that data, Geschichtsschreibung. The former is the important and extensive task of searching in the archives, tracking and interpreting sources, and their critical analysis. The latter is the equally important task of combining all this information into a coherent whole, i.e. writing a narrative. The narrativists now claim that analysis of historical knowledge must, in accordance with the Historists, be conducted with this distinction in mind. The historical facts are an important part that should be scrutinized with the utmost care – and much important work has been done at that end. But the narrative whole should be analysed with a similar philosophical rigour if we want to understand the writing of history. Philosophical inquiry in the nature of historical knowledge is, for the narrativists, always a two-step rocket: the analysis of usage and justification of the individual facts, and the similar task with respect to the narrative whole.

Why, now, do the narrativists believe that this distinction between the narrative and the fact should be so decisive in our analysis of history? Philosophers have acknowledged the importance of the narrative form of history without analysing this narrative form as a whole. The traditional analytical philosophical response towards historical knowledge is to view histories as the conjunction of individual statements. In a logical system we can make the simple conjunction \((a \land b \land c)\). If all three variables are true, the whole statement is true. The order of the variables in this statement is of no consequence to the truth-value of the whole statement. Statement \((b \land c \land a)\) has the same truth-table as the former statement. For a narrative, a similar thing could be proposed. If a narrative is the concatenation of all the different statements, we could – save some grammatical adjustments to facilitate the readability of the text – place all the statements in a different order, while the truth of the total narrative would be retained. According to those who think historical narratives are mere conjunctions, this new narrative might be a selling failure in the book-store. But regarding the

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97 Examples are Danto, Analytical philosophy of history, White, Foundations of historical knowledge.
truth it is similar to the original. The specific order of the statements, the way in which the statements are related to each other, is seen as a mere ornamentation which is of no consequence to the truth of the narrative. This latter claim, the narrativists deny.

Before we look into the arguments of the narrativists, we need to be aware of an important aspect of justifying scientific knowledge. This is that there is a difference between an empirical claim and a philosophical claim about the status of historical writing and knowledge. The latter philosophical claim will be extensively dealt with below. In the case of the field of history, the former claim is about the way in which historians write history, and the things they produce. And this, something nobody denies, is most of the time done in some kind of narrative mode of writing or another. It is important to remember that this narrative mode encompasses more than is generally understood when we speak about “narrative”. First we take a look at what this narrative mode is not. Historians do not conduct the kind of research in which they posit hypotheses that are consequentially falsified or validated. As Louis Mink once noted, "the historian seems to use the hypotheses as suggestive rather than deductively fertile. [...] For the scientist, the hypothesis is the target; for the historian, [it is] a signpost". A historical hypothesis is a signpost in that it shows how the past could, or should be viewed according to the historian. As such it should be considered much more like a thesis. More specifically, it should be looked upon as a thesis that cannot be deduced from the available evidence.

 Furthermore, histories are not the same as annals, chronicles, or source books. All these kinds of text lack the coherence of the narrative mode. The sentences are not connected, and there is no beginning, middle, and ending. Every part is disconnected to the rest, and arbitrarily chosen. The chronicler just writes down what he thinks is interesting at the that moment, never coming to a real end, while the source book is a plain presentation of historical sources.

This leads to a positive qualification of the histories as narrative. Narratives have a beginning, a middle, and an end. Historians write a plot in which the events about characters, social structures or any other historically interesting entity are discussed and described. This might imply that the narrated events are explicitly placed in a temporal succession, but this is

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99 To be more specific, annales are written without any clear decision about what is significant. Chronicles lack closure, and therefore miss the capacity to relate all earlier events with a specific ending. Hayden V. White, "The Value of Narrative in the Representation of Reality" *The content of form : narrative discourse and historical representation* (London, Johns Hopkins University Press, 1987) 1-25, citing 9-20.
not always the case. Jacob Burckhardt’s *Die Kultur der Renaissance in Italien* is a synchronic history of Renaissance Italy. This book does not seem to posses a beginning, a middle, and an ending. In Burckhardt’s analysis the Italian culture is, however, contrasted against the cultures of the earlier Middle Ages, while it is seen as the starting point of the Modern period idea of individuality.\(^{100}\) So these synchronic histories do imply a temporal order. Another problem might be historians who explicitly refute the story-telling character of history. They believe stories are unscientific. The French structuralists, for example, criticized those historians who use the narrative form. But, as Paul Ricoeur has extensively shown, they still implicitly write a narrative.\(^{101}\)

Historians do not produce hypotheses in the way that natural scientists do. Nor do they write chronicles. They write narratives with a beginning, a middle, and an ending. How to justify this narrative is the more important philosophical question.

How do the narrativists counter the claim that narratives can be analysed as the conjunction of individual statements? Recall that the claim that narratives are mere conjunctions implies that the order of the individual statements of a narrative can be randomly reordered. The truth of the narrative does not change, because the truth is the conjunction of the individual sentences. Everybody will nevertheless prefer the original narrative above the reordered narrative. What then, is it that the original narrative possesses which the reordered narrative lacks? The answer is that there is some kind of ordering principle in the original narrative that makes us prefer it above the unordered narrative. Acknowledging this ordering principle makes it impossible to say that the historical narrative should be analysed only as a conjunction of individual sentences, because as soon as the narrative has an ordering principle, it is more than those individual sentences. Viewing a narrative as the conjunction of individual statements would demote the narrative to the status of a chronicle, for an ordering principle that is more than a mere chronological sequence is what sets the narrative apart from the chronicle.\(^{102}\) For Narrativism, the logical structure of written histories consists of the

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\(^{101}\) Paul Ricoeur, *Time and narrative*, vert. Kathleen McLaughlin and David Pellauer, 3 vols., Volume 1 (Chicago etc., 1984), 214-217. This is not to say that Ricoeur’s notions of a quasi-plot and a quasi-character are valid. The connection between these concepts and experienced time he finds (by phenomenological derivation) is untenable. As Louis Mink forcefully showed: stories are told, not lived. The quasi-plot and quasi-characters seems to indicate that Ricoeur believes that narratives are, in the end, more or less abbreviated copies of the way the historical actors experienced the world. He fails to differentiate between the presence of a narrative order in Braudel’s work, and the history this narrative is about.

\(^{102}\) Mink, “Narrative Form”. For a more elaborated critique on the thesis that narratives are mere conjunctions of individual statements see Frank R. Ankersmit, *Narrative logic : a semantic analysis of the historian's language* (The Hague and London, 1983), 58-61.
combination of the individual sentences and the ordering structure of those sentences within the narrative whole.

### 3.2 The roots of narrativism

As mentioned above, narrativism’s core distinction between fact and narrative whole has its origins within the German Historist tradition. First it should be noted that this philosophy must not to be confused with the Historicism that Karl Popper famously attacked in his *The poverty of Historicism*. Popper’s Historicism is a category of believes we associate with, for example, certain aspects of the philosophies of Giambattista Vico, Georg Wilhelm Friedrich Hegel or Karl Marx. These philosophies advance the speculative idea of an order in history that is not only visible in the past, but that can also be projected onto the future. Historism is to be differentiated from this view, for it does not project anything on the future at all. The Historists insist on the openness of the future. Such an insistence is antithetical to the historicism that Popper criticized.

What, then, is this Historism? There are many definitions and descriptions on what Historism is, or what the Historists believed. In this thesis, Historism will be treated as a philosophical system that prescribes how a certain part of the world can be known. According to the Historists, knowing the human past is a form of inquiry that is incompatible with the natural scientific way of reasoning. Not that this natural scientific way is fraud. It is just not suited to acquire knowledge about the human past. Historists are, thus, explanatory pluralists. The core believe of Historism is the view that the essence of a thing is its history. This does, however, lead to the age old problem of accountability of temporal change. Classical thinkers like Parmenides, Zeno, and Melissus already denied the possibility of change. The topic survived well into our time, exemplified by Mc Taggert’s denial of time, and the consequential denial of temporal change. We will not elaborate on this discussion in a direct way, but rather focus on the specific problem the historian faces when dealing with historical change, and on the way the philosophy of Historism solved it.

The problem is the following. If the essence of a thing is its history, how can we account for radical change? For change needs something that remains the same. If no part of a thing...

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104 It is not surprising that Louis Mink, one of the most outstanding narrative philosophers of history, started his academic career with a refutation of Mc Taggert’s denial of time and change. See the editors introduction in Louis O. Mink, *Historical understanding*, ed. Brian Fay, Eugene O. Golob and Richard T. Vann (Ithaca etc., 1987).
remains the same, we cannot speak of something that changes. The first thing simply stops existing, while something new comes in its place. How can, for example, historians speak about the history of the “thing” France while pre-revolutionary France is in many respects radically different from post-revolutionary France? What arguments can a historian give in his claim that the two Frances are the same thing? The problem Historism had to deal with is the problem of identity through time.

The roots of the Historist’s answer can be found in Leibniz’ philosophy of the Monads. In the metaphysical system of the Monads individual undividable elements develop from within, unfolding their own natural development, never interfering with each other. The Historists use this idea of elements that follow a unique individual path and introduce it within history. Individuals, institutions, and above all nations are the Historist’s counterpart of Leibniz’ Monads. Just as the latter’s Monads, these Historical Ideas have an internal development that is the origin of all social change. All the social phenomenon in the world, all the historical facts, are properties of Historical Ideas.

The question is how we can know these Historical Ideas. Because of the never ending process of creativity that drives the development of these elements – a creativity that has its source in man’s creativity – each element’s development is unique. We cannot recognize Historical Ideas by applying generalities to the phenomena in the world. The uniqueness of every element in space and time prohibits any such attempt. So the methods of the natural sciences – the subsuming of phenomena under a generality – are of no use. The historian is left with an overwhelming number of phenomena (or the evidences of these phenomena), i.e. the historical sources, on the one hand, and the elements that are at the basis of all these phenomenon, but which he cannot perceive using the method of generalization, on the other.

As the Historist Wilhelm von Humboldt (1767-1835) wrote: “For observations can perceive circumstances which either accompany or follow one another, but not their inner causal nexus, on which, after all, their inner truth is solely dependent.”


107 Iggers and Moltke, “Introduction of The theory and practice of history”, citing xlv-l.

individual element historians use “intuition, inference, and guesswork.” This intuition, this *Ahnen*, is the only way of acquiring knowledge about the Historical Ideas. With this intuition the unseen connection of the different phenomena belonging to a Historical Idea become visible. And only with the Historical Idea can we, according to the Historists, come to approach the essence of the (historical) world.\textsuperscript{109}

Yet there is a problem with the concept Historical Idea. According to the Historists, the task of the historian is to find the “inner causal nexus” of the Historical Ideas. This does not mean that the historian searches for some mechanically explainable causal link, but rather that the historian tries to understands how the Historical Idea works from within. Historians try to interpret the unique development of these Ideas. This seems to indicate that the Historical Idea is similar to the Aristotelian idea of an entelechy, with the exception that the Historists did not believe in any final cause. The creative character of the Ideas prevents any predictions or determined final goal. The problem is, however, that the Historists assume that this entelechy exists in historical reality.\textsuperscript{110} This idea of an ontological real entelechy has lost much of its force. The Historists view the world as consisting of several discrete elements that have an inner force responsible for the development of these elements. This, however, leads to problems. The biggest of these is that these elements are difficult to verify, and seem to be nothing more than metaphysical speculation. There is no single objective (or inter-subjective) method that can verify or falsify the existence of such elements. Two historians that both interpret a similar set of phenomena can, and in practice almost always will, come to quite different conclusions. Therefore, the belief that there are real Historical Ideas seems grounded in speculation.

The Historical Idea resides on the same metaphysical plain as the old idea of an entelechy in biology. The Historists believe that the Historical Idea is ontologically real. The difference is that in biology, entelechies were presumed to reside within organisms, while the Historical Idea resides within society. But when these ideas are considered to be ontologically true entities, such believes are metaphysical speculation.

The origin of this problematic nature of the Historical Idea seems to lay within the Historist focus on the creativity of the human mind. The Historists claim that history is not knowable by means of general laws because of the infinitely unique process that is based on human creativity. As Igers and Moltke put it: "At the core of the historicist orientation was the

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\textsuperscript{109} Ibidem.
\textsuperscript{110} Igers and Moltke, "Introduction of The theory and practice of history", citing xvii-xlviii.
\textsuperscript{111} As can be seen in Humboldt, "On the historian's task", citing 15-22.
\end{flushright}
insistence that man can be understood only in terms of his history, that the sciences dealing with man's cultural creativity are historical sciences and that the methods of the historical sciences are fundamentally different from those of the natural sciences or traditional philosophy.” The consequence is that the validity of their philosophical system depends on the validity of this claim about man’s creativity. But this claim is a claim about the world. And as such, this view about human creativity is vulnerable to the criticism that it is not a philosophical, but an empirical claim. An opponent of Historism could claim that while no interesting, reliable, and universal valid sociological laws have been found in society, the question if such a law will be found is an empirical question. The philosophical system of the Historists could be falsified by the discovery of a single useable law.

This criticism is not to be confused with the criticism that the believe that human creativity prevents law-like knowledge about humans is a contradiction. Thinking that human creativity prevents the application of laws to society is, so goes this criticism, a generality about that society. But that generality is in contradiction with the claim that generalities are impossible within the social domain. But the Historist can counter the argument by pointing out that the claim about the creativity of humans is a claim about the nature of man, and as such, is not located within the domain of historical knowledge. Nature is knowable by laws. Only the human world defies the application of such laws.

We conclude that the Historist solution to the problem of change, the Historical Idea, does not give an adequate answer. For them, the problem of change is a problem out there, in the real world. Their mistake is to search for an answer in that world. The rise of linguistic analysis within the philosophy of science, the idea that the task of the philosopher is the analysis of the language use and production, comes, however, to the rescue. Where the Historists thought that the Historical Idea can be found in reality, the narrativists transpose this Historical Idea to the linguistic level. Historical Ideas are not things in the world, they are things in language. This transposition is the topic of the next section.

3.3 The analytical philosophy of the historical narrative

The road towards the narrative philosophy of history starts with a philosopher who resided within the camp of those philosophers of history who believed that laws are important for explanation. His concept of explanatum is an attempt to fuse explanation by law with the

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112 Iggers and Moltke, "Introduction of The theory and practice of history", citing xliii. An example of this belief can be found in Humboldt, "On the historian's task", citing 16-17.
narrative mode. But even though his allegiance is more within the camp of “the law-abiding philosophers of history”, Danto is an important for bearer of the narrativist philosophy of history. He is important because of his ideas on the distinction between the historical sciences and the natural sciences.

The debate about the application of the covering law model in history was part of the more general unificationist program originating out of logical positivist philosophy. With their method of rational reconstruction, using the rigor of the new mathematical-logical tools, and a thorough anti-metaphysical stance, the logical positivists tried to find the foundations of all rational knowledge. Quickly many difficulties appeared when the logical-positivist’s ideals were applied to the field of history. At that time, two responses were possible. The first stance was to say that history, if it is to be taken serious, needs a philosophical justification that is in line with the logical-positivists ideas. The failure to do so means that the field of history loses its status as a science. The downfall of the unificationist program and the greater attention paid to description instead of prescription within the philosophy of science deems this response unjustified. Philosophers of science do not endeavour to tell the practitioners how to do their jobs: the practice of science is only to be analysed by its philosophy.

The second response relied on the traditional neo-Kantian distinction between the Naturwissenschaften and the Geisteswissenschaften that Wilhelm Windelband posited in the late nineteenth century. The underlying presupposition of this view is that there are two different kinds of subject matter for the scientist to explore. On the one hand we have the natural world, on the other we have the human world. Because humans differ from nature, their study requires different methods.

Danto thinks finds it misguided to belief that the distinction between nature and man gives the natural sciences and the historical sciences their own character. With this move he takes an important step towards the narrative philosophy of history. Instead of presupposing a difference between the human world and the non-human world, Danto distinguishes between scientific language and historical language. “The difference [between the natural sciences and history] has to do with the kind of organizing schemes employed by each. History tells stories.” Danto shows how the logic of the historian’s linguistic organizing schemes, the logic of historical narratives, determine how we think and write about the past.

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114 White, Foundations of historical knowledge, 27-29.
116 Danto, Analytical philosophy of history, 111.
The analysis of historical language makes clear how there is a difference between truthful sentences about the present and the future, and (certain) truthful sentences about the past. The argument that Danto makes starts with the idea of an Ideal Chronicle (I.C.). This I.C. is an imaginary complete description of all events that happened in the past. Being complete means that every event is exhaustively described with a set of sentences. Every time an event takes place, some all seeing chronicler adds a new set of sentences to this I.C. According to Danto, the common sense idea is that “[w]e may now think of the various parts of the I.C. as accounts to which practising historians endeavour to approximate their own accounts.”

When historians write their histories, they are, in this view, trying to approach the I.C. as close as possible: historians are trying to make accurate copies of the world.

But, as he shows, this view must be mistaken. It misses an important point about certain descriptive sentences that are essential to historical narratives. These narrative sentences, as Danto calls them, are not the only sentences in a historical narrative. But they are a necessary part of historical language. These narrative sentences are true statements about past events. In these sentences the first event is described with a reference to a later event. The first event E-1, taking place at t-1, is the event described. But the second event E-2, taking place at a later time t-2, is referred to in the description of E-1. Danto gives the example of the sentence “The Thirty Years War began in 1618.” A sentence that could not have been uttered until 1648. The start of the war, in 1618, is described by referring to the end of this war in 1648. In 1618, however, knowing that the war took thirty years, would be impossible to know. One can, of course, utter this sentence as a prediction in or before 1618, but this would not (yet) be a historical true or false statement. Predictions are not (yet) discriminately true or false.

The structure of the narrative sentences lead Danto to conclude that the I.C., which pretends to be a complete description of all events, cannot exist. The I.C. is a definite description which cannot be altered after it is written down. But the nature of the narrative sentence demands the possibility of just such a revision. The complete description of a certain event, as is presupposed to be the case in the I.C., is not on par with these narrative sentences. The narrative sentences give more complete descriptions of events than the I.C. can give. Therefore, Danto concludes, the idea of an I.C. is mistaken. Since this idea is mistaken, the consequential claim that historians are in the business of trying to copy the I.C. is mistaken as well.

117 Ibidem, 149.
118 Ibidem, 151ff.
119 Ibidem, 152.
This brings us back to our starting point. Danto claims that differences in subject-matter are not the essence of the difference between the historical and the natural sciences. The narrative sentences show that a difference in linguistic organization is the essence of the incompatibility between the two domains of science. And this difference in linguistic organization comes down to the narrative sentence which, and this is important, can only be constructed with hindsight. It is the key element in Danto’s analysis that, because of these specific sentences, it is a logical property of language about the past to be more endowed with meaning then any potentially or actually uttered language in the past. “[N]ot being witness to the event is not so bad a thing if our interests are historical.”

For only if we are looking back at the past can an appropriate designation of the significance of events be given. Only after the effects of an event are known can we judge what role the specific events played.

This is quite different within the natural sciences. There, whether a specific event is significant or not depends on the importance of the event (as a kind) within the hypotheses, theories, or models that the scientist uses. Within the natural sciences, the significance an event has in respect to the scientist and her work will never change because of the occurrences of certain later events, as it happens in the case of historical language. This does not mean that events do not have any influence on the natural sciences. Far from it. A scientific experiment is an event. These events are, however, not influencing the significance of other events that occurred at an earlier time, but rather the significance of the hypotheses, theories, or models the scientist uses. Danto has thus shown how historical language is different from language about the present and the future.

One problem with Danto’s narrative sentences is that while he calls these sentences narrative sentences, he fails to analyse the narrative these sentences are part of. For him, analyzing narrative sentences is enough in the analysis of the historian’s language. The next step towards a mature philosophy of historical narratives is the acknowledgment of larger textual structures. William Walsh stands out in this respect.

Walsh does not exclusively consider the individual sentence as his object of study. He looks at a more extended linguistic part of the historical narrative – extended meaning that it comprises of more than just sentences. This specific part of historical language is the colligated concept.

The underlying assumption [of this concept] is that different historical events can be regarded as going together to constitute a single process, a whole of which they are all

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120 Ibidem, 152-153.
parts and in which they belong together in a specially intimate way. And the first aim of the historian, when he is asked to explain some event or other, is to see it as part of such a process, to locate it in its context by mentioning other events with which it is bound up. ¹²¹

The notion of colligation is used for the first time by the nineteenth century philosopher William Whewell – the inventor of many new words used in science and philosophy. For Whewell, colligation is part and parcel for inductive reasoning. Induction, in his sense, means that a series of different phenomena are brought together with one conception or idea. Whewell called this process colligation. Only with this idea can the scientist elevate the series of phenomena to real scientific knowledge. Whewell believes that the Empirists do not understand knowledge when they claim that induction is a mere conjunction of sense data. For Walsh, the historian does something similar to Whewell’s scientist. The historian also uses colligation to bring a series of phenomena under one concept. For a good understanding of the meaning of colligation it must be noted that Walsh’ position shifted over the years. The first exposition of his ideas on colligation in the 1951 first-edition of An Introduction to Philosophy of History is still written under the guise of Collingwood. Colligation is only possible because it is about humans.

Now it is important to realize that the historian's ability to use this form of explanation depends on the special nature of his subject-matter. It is only because of his concern, rightly stressed by the idealists [like Collingwood and his like-minded], with actions, that he can think in this way at all. It is the fact that every action has a thought-side which makes the whole thing possible. ¹²²

The thought-side of action he is referring to is the fact that humans set goals that are extended over time and space: “[A]ctions are, broadly speaking, the realization of purposes, and because a single purpose or policy can find expression in a whole series of actions, whether carried out by one person or by several, we can say in an intelligible sense that some historical events are intrinsically related.” ¹²³ The explanation of the process of the attainment of these goals is the purpose of the historian. So the building of the Notre Dame de Paris is a goal that took almost over two centuries of building time. This building process cannot be historically explained, at least according to Walsh, in a Collingwood-like intentionalistic way, for it

¹²¹ Walsh, An Introduction to Philosophy of History, 24-25.
¹²² Ibidem, 59.
¹²³ Ibidem, 59-60.
comprises many people, thoughts, and actions. While there is an overall intention, the individual intentions, which are the target of the intentionalistic explanation, cannot be used to explain this overall intention. But it cannot be explained by law-like generalization either, for there is an intention to build this cathedral. And intentions are rather difficult to reduce to laws. In Walsh’ earlier view, the historical explanation of this building is the colligation of all the events that lead to this ultimate intention under the concept of “building the Notre Dame de Paris”. This, according to Walsh, is one of the important ways in which historians explain. In summary, Walsh believed that historians try to give intentional explanations. But because historical reality is too complicated – humans have all kinds of long-term goals that extend the singular action – the historian has to resort to a “degraded” intentionalistic explanation using colligation.

Walsh changed his position in the article “Colligatory Concepts in History”. The origin of this change of mind was already present in his Introduction of 1951. In an earlier part of that book Walsh criticizes Collingwood’s intentional explanations because these explanations do not consider the unintended consequences of intentional action. But this argument against Collingwood can be used against Walsh’ earlier conception of colligation as well. Even the “degraded” intentional explanation does not leave enough room for unintended consequences. Therefore he replaced the focus from the intentions of the historical actors to the historical process as such.

The original idea on colligation cannot live up to its promise to show how historical explanations work. Even if people intended to complete the Notre Dame de Paris, the building process might have failed. And because of this uncertainty, the written history of all the intentions and actions to construct this church is different from the set of actual intentions and actions of that construction process.

We can see this in the way Danto defines and analyses the concepts of project verbs and temporal wholes. Project verbs are word groups (they consist of more than just verbs) which describe an action by referring to the intended result of that action. Danto’s example is “planting roses” in the sentence “A man is planting a rose.” The man is actually not planting a

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124 It should be noted that this is Walsh’ interpretation of Collingwood. Collingwood would say that the building of the Notre Dame de Paris could be explained by stating all the separate intentions and the following situations.

125 I call Walsh’ notion of colligation in An Introduacion degraded because he characterizes explanation by colligation as a semi-teleological explanation. But with this move, he makes it clear that teleological explanations, i.e. intentional explanations, are the ideal. Since explanation by colligation cannot attain this ideal, it must be degraded. Walsh, An Introduction to Philosophy of History, 60-61.

rose all the time, for there is no single specific action that can be designated as planting a rose. He is rather digging a hole, putting seeds in the ground, watering the soil, etc. Moreover, he can do all kinds of other things between these planting activities that have no bearing on planting a rose: he could go out, sleep, or write a philosophy thesis. Yet, when someone looks at a raked pitch with puzzlement, we will say that the owner is planting roses. Sentences that use project verbs have the same structure as Danto’s narrative sentences. They describe some event E-1 at time t-1 with reference to some expected end-state, event E-2 at time t-2. There is, however, an important difference. Only when the result of all the actions that we associate with planting a rose are achieved, can we say that the rose was actually planted. But unfortunately, as everyone knows who has tried to grow plants, such an activity might fail. Project verbs are, therefore, no guarantee for success. This does not mean that we cannot say that a man was not planting a rose as soon as we find out he failed, for he surely was planting a rose. It means that the reference to the future event E-2 is not part of the truth condition of a statement containing a project verb. Whether he succeeds in bringing about full grown roses or not does not matter when we say that he planted roses. Danto now calls the set of events that form the whole execution of some project verb a temporal whole.127

These temporal wholes are what Walsh – in his earlier views – believes to be colligated under one concept. Temporal wholes consist of the events that are consecutively preformed when people execute a project verb like “building the Notre Dame de Paris”. Bringing events under one colligatory concept means, thus Walsh, describing such a temporal whole. This leads to the idea that the better the description of the temporal whole coincides with the real temporal whole, the better this description is. And as such it is about making a copy of a part of the world. There is, however, a problem with Walsh’ approach. For the historian knows how, and if the desired goal at the end of the temporal whole has been attained. Yet, as we saw with Danto’s analysis, during the period a temporal whole happens this is not at all certain. A project verb’s future event, the goal, need not to be true for the project verb to take place. In a colligated concept on the other hand, all the descriptions of the events do need to be true. The colligated concept about the events of the invasion of England in 1588 cannot include the description of an event in which the Spanish troops land on the English shores, yet Philips II was certainly invading England – i.e. the project verb “invading England” was appropriate for the events of the armada. This also applies to the example of the Notre Dame de Paris, for the intentions of the initiators were different from those who finished it. Multiple

architectural changes were applied, and many things changed. Colligation can thus not be about creating an as accurate as is possible copy of the world.

Therefore, just like the narrative sentences show that the idea that historians try to copy an Ideal Chronicle is mistaken, so does the idea of the colligation of concepts under a Collingwoodian guise fails to give what it purports, namely a copy of a goal directed long-term process.

All this leads to a new perspective on colligating concepts in “Colligatory Concepts in History”. Walsh now acknowledges the special position of the historians in that they know the outcome of the events they study. "The object of the whole colligatory exercise is to increase understanding, and it is obvious that the understanding in question is that of the modern student of history who looks back on a given set of events from the standpoint of the present.”

As we just saw, explaining intentional actions, whether long-term or short-term, cannot be the main characteristic of the writing of history.

The main difference with his earlier position is that Walsh views colligation as the cognitive task of interpretation. At first, Walsh followed William Dray’s critique on the covering law model in stating that history is not only about explanations of why questions, but also about explanations of how questions. But Walsh came to see that there is more to history than multiple explanatory strategies. He came to see that “the historian and his reader initially confront what looks like a largely unconnected mass of material, and the historian then goes on to show that sense can be made of it revealing certain pervasive themes or developments.”

When the historian shows what was going on at the time, he both describes the different events of the past, and “tells us how to take them.” This process is not an explanation in the sense of bringing phenomena under a law, nor is it an interpretation in the sense of the heuristic method of Dilthey or Collingwood. It is the process of selection of events and the forming of those events into a coherent whole. So Walsh views the component of selecting and describing events, and the component of seeing those events together under one coherent concept, as interpretation.

Interpretation is not about why, how, or what something is, but about how we should look upon something. Walsh did not believe that historians first come up with a general concept and then go about selecting evidence to support this concept. It is rather a process by which

129 Walsh, “Colligatory Concepts in History”, citing 75.
the two elements – the description of the events and the colligating concept – are mutually responsible for the outcome. It could not even be otherwise, because the colligating concept can only exist with the description of the events.

Examples of colligating concepts are the Renaissance, the Second World War, or the position of women in the twentieth century. These are all concepts that historians use, and which colligate a whole range of events under that single concept. In these example the concepts have a name, but it should be noted that historians use nameless concepts as well. Bringing events together in one coherent view means colligating them. And this is what historians do.

To conclude this part on Walsh we can say that he made a turn away from Collingwood and the intentionalistic philosophy of history. Walsh first viewed colligation as the idea that historians try to explain history when they make copies of the temporal processes of long-term goal oriented action, by subsuming all the events of that process under one concept. In his new position on colligation he excludes all intentional notions. Furthermore he believed this process to be one of interpretation rather than explanation. With this idea of colligation, we arrive at a position in which the individual sentence is not the target of philosophical analysis anymore. Since the process of colligation consists of both describing events, and viewing these events under one concept, this process is not exclusively analysable on the level of events. The colligating concept is the next step on the ladder towards a narrative philosophy of history.

We saw that Danto made an argument that historical language is unique in that the historian always uses language with hindsight, attributing significance to historical events. He shows that the difference between the natural sciences and the historical sciences is a difference of language use. The problem with Danto’s analysis is, however, that he remains on the level of the individual sentence. Walsh analyses the larger textual unit. His colligatory concepts are recognized as whole textual units. But Walsh is not convinced of the idea that language use is the determining difference between the historical and natural sciences. He believes that interpretation, although an important part of historical language, is only one of the many linguistic devices available to the historian. For him, explanations with laws are part of historiography as well. In the thoughts of Louis Mink we see the convergence of Danto’s thesis about the linguistic separateness of the sciences and Walsh’ thesis about colligation.

Mink follows Danto when he places the difference between science and history in language, or, to be more precise, he places the difference in the multiple ways in which humans can know the world. Mink identified three separate modes of comprehension. The
three modes are the categorical comprehension, the theoretical comprehension, and the configurational comprehension. The first is the comprehension of objects as examples of one category. Mink recognizes this as the preferred mode in philosophy. The second mode, the theoretical comprehension, is the comprehension of objects as instances of the same general law. This mode he believes to be dominant in the natural science. The last mode is the configurational mode. In this mode elements of knowledge are comprehended as part of a single complex of concrete relations.\footnote{Louis O. Mink, "Modes of Comprehension and the Unity of Knowledge" in: Brian Fay, Eugene O. Golob and Richard T. Vann ed., \textit{Historical understanding} (Ithaca etc., Cornell University Press, 1987 (1960)) 35-41, citing 37-40.}

Mink believes these three modes are the only modes by which we can know things because of the process of knowing: This is a process through time. “The fact to which any theory of knowledge must return is the simple fact that experiences come to us seriatim in time and yet must be capable of being held together in an image of the manifold of events. The steps of a proof, the actions of a narrative, the notes of a melody, and even the words of a sentence are experienced one after the other, but must be considered in a single mental act before they even constitute data for significant discourse.”\footnote{Ibidem, citing 36.} Because of this insight, Mink understood that he had to improve upon Danto’s narrative sentences. That is to say, the idea of the narrative sentence does not do enough justice to fact that the act of reading a historical book takes time. To overcome this, Mink extends the narrative sentences with a system of five different types of linguistic constructions.

1) Contemporary descriptions of events as they are recorded by actual witnesses;
2) Possible contemporary descriptions of events, which are actually not recorded for whatever reason, but could have been;
3) Descriptions of events only possible a certain period after the occurrence of the event because of later knowledge;
4) Descriptions of events only possible after the occurrence of the event because they require certain techniques of acquiring knowledge;
5) Descriptions of events only possible after the event because they depend on later \textit{conceptual} modes of comprehension.
The first two are those which would form the Ideal Chronicle of Danto. The third is the latter’s narrative sentence. But it is the last one that really caught Mink’s attention. How can a historian apply the concept of social class on the past, while the idea of social class did not exist during, for example, classical Greece? Using the concept of social class helps the historian and her reader to think all kind of facts together. The concept organizes all kinds of facts about people and their occupations, social mobility factors, different art expressions, etc. This is, of course, very similar to Walsh’ notion of colligation. And just as Walsh came to see that colligation has to do with interpretation instead of explanation, Mink came to see that configurational comprehension is a kind of understanding instead of a kind of knowledge.

But for Walsh colligation is one kind among other historical devices. The configurational comprehension of Mink is, however, the mode linked to the narrative as a whole. A historical narrative is a colligated concept writ large. It is one of Mink’s most important insights that he understood that “[historians generally do not adopt one another’s significant conclusions unless convinced by their own thorough inspection of the argument; unlike scientists in general, they must read one another’s books instead of merely noting their results.” And a philosophy of history must, according to Mink, do justice to this fact that conclusions are not detachable. Doing justice to this fact means treating the complete historical narrative as a configurational comprehension. Historical narratives are interpretations of the past in which the knowledge elements, descriptive statements of historical facts, are ordered in such a way that we understand a specific part of that past.

With this view of narrative histories as configurational comprehensions we will, however, come to conclusions which might seem untenable. The first is that the act of comprehension is not restricted by the original temporal order of the events. Since historians work with hindsight, the original temporal sequence of the narrated events disappears. The historian can make connections between events that would be impossible to make in that past itself. Mink used the metaphor of a river to explain this: “To comprehend temporal succession means to think of it in both directions at once, and time is no longer the river which bears us along but

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133 For a concise overview of the development of Mink’s thoughts about the role of narrative within history see Richard T. Vann, "Louis Mink’s Linguistic Turn", History and Theory 26 (1987, Issue 1) 1-14. For a broader view on Mink’s intellectual development see the introduction of the editors in Mink, Historical understanding.
135 Ibidem, citing 77-81.
the river in aerial view, upstream and downstream seen in a single survey." 136 This might be considered an unacceptable loss, but the loss is only unacceptable insofar it goes against the believe that historical language should try to make a copy of the world. Danto's analysis of the Ideal Chronicle and the narrative sentences already did away with this believe. Furthermore, such an appeal is much too prescriptive. It presupposes that historians should copy the temporal order of the original events, while Mink merely describes how historians use language to speak and think about the past. Doing history means using the configurational comprehension, and within such a comprehension, whether the philosopher likes it or not, the original succession of events is not the determining factor of the narrative's order.

A second conclusion is connected with this first one. This is the conclusion that historical narratives do not refer to the past, at least the parts of narratives that are more than the conjunction of sentences. This lack of reference seems quite problematic. Mink, for one, was rather concerned about it. 137 He understood that if historical narratives are textual constructions in the configurational mode of comprehension, the reference to reality, qua order, will be lost. Since a configurational comprehension is not knowledge, but a form of understanding, the knowledge elements are the only things that have a reference to reality.

To come to terms with these problems he analysed the way in which people thought about the relation between their written histories and the past. Up until the nineteenth century the believe was cherished that there was one universal history of man which historians strive to copy, but this Enlightenment idea lost ground during the nineteenth century. Although this idea of a universal history is declared dead, Mink contends that it is still a background presupposition in our common way of thinking about (our) past. We still treat the past as a single historical story, waiting to be copied by the arduous working historians. 138 He shows that this presupposition is still present by pointing out the ways in which this presupposition goes beyond the limits that narrative language imposes on our speaking about that past. Commonly, people think that two histories should be aggregative. Historians should be able to add one narrative to the end of another. So a narrative about the eleventh century can, so goes the common argument, be added to one of the twelfth century. But since being a narrative means having a beginning, a middle, and an end, two narratives cannot be added together. The point of writing a history is giving significance to certain structures and events, and giving a

137 As Vann makes clear in Vann, "Louis Mink's Linguistic Turn".
138 Mink, "Narrative Form", citing 192-195.
closure to those structures at the end. But combining two histories, adding the end of the one to the start of the other, will destroy the closure of the first. How then can we combine them except by creating a new narrative? This new narrative will be a different interpretation with a different closure. It is clear that if we require “histories” to aggregate, while “narratives” cannot, “historical narratives” are caught up in the middle. The same can be said about the truth of historical narratives. A history (being more than the mere conjunction of statements) claims to represent some part of the past, but “as narrative it is a product of imaginative construction, which cannot defend its claim to truth by any accepted procedure.”

This analysis leads to the following dilemma: we do not accept the idea of one universal history anymore, but we implicitly still adhere to such an idea. Mink’s answer to this dilemma is that we should adjust one of the sides in this dilemma. But since it seems quite untenable to reacquire the believe in the universal history – it would result in the return of the speculative philosophies of history – he concludes that the best thing to do is to drop our presupposition about the universal history. This is the important insight that is expressed with Mink’s adage that “stories are not lived, but told.”

The abandonment of this presupposition leads, however, to the dissolution of the traditional dividing line between historical narratives and fictional narratives. If historical narratives are not connected to some kind of reality, the criteria for dividing between history and fiction do not lay within the domain of the truth. This means that these criteria must lay within the domains of aesthetics or ethics. Hayden White, for one, accepts this relativistic conclusion wholeheartedly. Mink is more concerned, and calls for the development of an argument to save historiography from “myth”. His untimely death probably prevented him from doing so.

With the ideas of the German philosopher Hans Michael Baumgartner we reach a better understanding of the relationship between reality and the historical narrative. In his book *Kontinuität und Geschichte* Baumgartner asks how continuity and history relate to each other. He is interested in the way historians deal with continuity and change, and as such,}

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139 And deform and weaken the closure of the second. Notice that pointing out that this problem is caused by the “subjective idiosyncrasies” of the historian is not a counter-argument, but rather a positive argument for the believe in an universal history. The one who makes this argument believes in an object-subject distinction and thereby presupposes the actual universal history. Ibidem, citing 195-197.
140 Ibidem.
141 Ibidem, citing 197-199.
143 Mink, ”Narrative Form”, citing 201-203.
he has an open eye for the ontological questions regarding continuity and change. What is the thing that undergoes the change in a historical narrative, Baumgartner asks. When we take a look at Danto, we see that he is much more concerned with epistemological questions about historical knowledge. When discussing the specific subjects of history Danto states that “[h]owever this issue is to be decided, from a formal point of view a narrative requires a continuant subject.”¹⁴⁵ Danto thereby shifts the problem of “the subject of change” from epistemology to ontology, trusting that all the metaphysical problems concerning the ontological status of those objects can be solved at a later time. Baumgartner disagrees with this strategy. The believe that the ontological explication of historical objects is not necessary for a formal exposition of narrative language means letting the problem of historical objects out of the front door, while letting it sneak back in through the back. Danto’s analysis makes this visible. Because he believes that an ontology of historical objects is possible, he already presupposes that there is a historical reality. But as Baumgartner points out “Dies würde aber einschließen, daß auch „temporal structures“ offenbar vorweg schon existieren und die narrativen Zusammenhänge, die sie entwerfen, auf objektive Zusammenhänge gegründet sind.”¹⁴⁶ This means that Danto implicitly accepts a historical reality, i.e. Danto presupposes a universal history (or Ideal Chronicle).¹⁴⁷ And this does not go well with his narrative sentences argument. All this makes clear that any analysis of historical language needs to answer the question what the thing that does the changing in history is. This is the important question of continuity and unity of historical objects the Historists already wrestled with.

To Answer this question Baumgartner does not propose some (set of) class(es) of ontologically real objects. Instead he gives an transcendental argument. To elaborate this argument we can contrast the status of sortal concepts with those of historical subjects. We can, for example, speak about Napoleon. This was a real person, just like the computer screen I look at is real. These are individual persons or things which can be pointed at. It is by definition that sortal concepts have continuity and unity through time. But if we take a look at a concept like the “Renaissance”, we see a concept that cannot be pointed at in such a way. The “Renaissance” does not posses the continuity and unity of a sortal concept. It is, rather, the other way around: these concepts create continuity and unity. Only because of the specific character of historical language is continuity and unity of historical objects possible. As Baumgartner puts it: the identity of a historical concept „[…]ist selbst nur noch narrativ zu

¹⁴⁵ Danto, Analytical philosophy of history, 249.
¹⁴⁶ Baumgartner, Kontinuität und Geschichte, 291.
leisten. Das aber heißt: die narrative Konstruktion selbst tritt hier an die Stelle der abstrakten Identität in der Zeit.¹⁴⁸ Narrative makes the identity through time possible. And as such, narrative is the transcendental condition for historical thinking.

That this is not only true for concepts like the “Renaissance” will be clear. Just as Mink envisaged that narrative wholes should be viewed as Walsh’ colligating concepts, so the transcendental argument applies to the narrative whole as well. A historical narrative is a linguistic device that creates continuity and unity. Within the narrative, the past is ordered to the effect that it tracks the changes of a unified subject-matter, while this unity and change is created within that order.

This insight is also the solution to the problems Mink identified with his proposal to discard the universal history. The biggest problem with this proposal was the conclusion that the traditional demarcation between fiction and history, based on the putative difference of truthfulness, became difficult to sustain. But this problem dissolves in light of the transcendental argument, for it is now clear that historians try to see unity and continuity in the past. This is different for fictional narratives. While the latter is often also continuous and unified, fictional narratives do not have it as their goal to create this continuity and unity. Since historical narratives are the condition for creating continuity and unity, this creation cannot but be a fundamental function of historical language. Any criticism directed at the narrativist’s believe that the order in historical narratives is not an order of the past itself, misses this important point.

A second conclusion can be drawn from this transcendental analysis. It is about the fact that historians interpret the past. An interpretation is a way to look at the past. But interpretation is a cognitive process that is often associated with subjectivity. History as interpretation instead of being about explanation, so would the sceptic claim, is not scientific. But if the narrative is the condition for historical knowledge, an actual interpretation of a historical period will also be an explanation of that period. To know the past means to use a historical narrative. Contrary to what some critics claim, this does not mean that every historical narrative is a good explanation.¹⁴⁹ Some are so bad, that we cannot even conceive such a narrative. It does mean, however, that any interpretation is, by fact of being a historical narrative, an explanation.

¹⁴⁸ Ibidem, 298.
3.4 From Historical Idea to linguistic narrative

If we look back at the philosophy of the Historists, we see that these ideas have many commonalities with the narrative philosophy of history. Both distinguish between historical facts and narratives – and the related phases of historical research and historical writing. These historical facts are all connected to, and brought under one general concept. This general concept is what the Historists call the Historical Idea, while the narrativists call it the narrative. And both view the historian’s interpretative powers as the cognitive process that brings all those facts under this general concept. Laws do not play a role in this process. So we see many similarities between both philosophies.

But there are also some differences. The first difference is the focus on the specific content of historical writing. The Historists prescribed that history should be political history that focuses on the nation-state. The narrativists are, more or less, indifferent regarding the content of history. A second difference is that the Historists see history predominantly as being written in the narrative form. Histories, according to them, should be true novels about the past. The narrativists do not make such claims. History can be written in the narrative form, and often it is, but the philosophical claims of narrativism are not limited to the narrative forms of historiography. But the most fundamental difference is that according to the Historists, the historian finds the historical concepts within the past, while the narrativists claim that the logical structure of the historian’s language is the birthplace of the historical concepts. The Historical Idea is transformed, as the analysis’s of Danto, Walsh, Mink, and Baumgartner have shown, into a linguistic concept that is the transcendental condition of historical knowledge.

Now we can come to the following conclusions about how historical thinking is analysed within the narrative philosophy of history. Historical narrativism analyses the constraints of the historian’s language. It holds that the historian works with a two-sided process of fact collection and writing, and that this division has to be mirrored in the philosophical analysis. During the collection process the historian validates, verifies, and falsifies the historical facts. In the writing process these historical facts are combined and brought under a single linguistic structure called a narrative. This narrative cannot be reduced to a mere conjunction of its consecutive statements, since this would remove the order the historian has imposed upon the facts. This means that the narrative order is something more than just that conjunction of statements. This addendum of order is the result of the process of historical interpretation. While the individual statements refer to the past, the narrative whole cannot do so. Since the
narrative whole does not refer to the past, the historians job cannot be the creation of narratives that copy the past. While some philosophers draw relativistic conclusions regarding historical knowledge, this position is not so bleak as it might seem. For it is only through the historical narrative that historical thinking is possible. Historical narratives are the condition for historical knowledge.
4 Biological knowledge as narrative knowledge

While some implemented ideas of the philosophy of history into the philosophy of biology, no attempts have been made to see if contemporary narrative philosophy of history can help us understand the way biological knowledge works. There have been certain philosophers of biology who have stressed the historical character of biology. Some, like David Hull, even draw on a form of narrative philosophy. But they fail to see the full philosophical implications of the narrative character of the historical sciences. With the rise of post-positivism in contemporary philosophy of biology, the question of narrative explanations lost all power. In the following sections it will be shown that the steps taken by Danto, Walsh, Mink, and Baumgartner also apply to the field of biology, and that the abandonment of the narrative in the philosophy of biology was premature.

4.1 Narrative sentences in biology

The first step of our inquiry is to see whether the kind of sentences that Danto calls narrative sentences are present in evolutionary biology. Of course there is no single field of evolutionary biology. Nevertheless we can say that there is one central idea in evolutionary biology, namely the principle of natural selection that made the biological diversity and apparent adaptedness of all species possible. Up until Darwin’s theory of evolution, there was no viable causal-mechanistic alternative for the argument from design. With Darwin’s natural selection this all changed. Showing that narrative sentences are a necessary part of this idea is a first step towards the application of Danto’s conclusions about history to the whole field of evolutionary biology.

In this thesis the position is defended that biology uses narrative knowledge. But narrative knowledge should not necessarily be identified with narrative texts. The latter texts are stories written in a mostly informal language that tries to induce a feeling of reality – and in case of histories this is the real order of the past. In textual appearance they are, so to say, more elaborate versions of bedtime stories. Narrative knowledge is something different. This is the

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kind of knowledge that is constituted with the mental act of comprehending a broad set of separate phenomena into one coherent story. Nonetheless, the majority of historiography is written in a narrated manner, although this is certainly not a necessity.\textsuperscript{151} So in history most narratives are also narrativized. Not so in biology. A lot of biological research is presented in the form of a report. These reports have a part with some observation results – relying heavily on empirical data – and a part containing a discussion – trying to link the former part with one or more theories. It seems that this kind of knowledge is far from similar to historical knowledge, and finding narrative sentences in them is a daunting task to say the least.

Nonetheless the narrative sentence can be found in the writings of evolutionary biologists, especially when important changes in the natural history are discussed. An example, due to O’Hara, is the discussion about the appearance of jaws. If we consider the following sentence we see a narrative sentence plain and simple:

\[
\begin{align*}
\text{One of the great events or revolutions in the history of vertebrates was the appearance of jaws. The importance of this evolutionary development can hardly be overestimated, for it opened to the vertebrates new lines of adaptation and new possibilities for evolutionary advancement that expanded immeasurably the potentialities of these animals.}\textsuperscript{152}
\end{align*}
\]

The importance of the jaws could never be known at the time jaws first appeared. And, in line with Danto’s analysis, this is a narrative sentence. Another example is the Cretaceous–Tertiary extinction. The meteor impact that occurred sixty million years ago made the development of the mammalian taxonomical group into the contemporary predominant position possible. The impact eventually led to man! Setting aside some final-causal overtones, it is clear that if a sentient being was so unlucky to have witnessed this impact – or more lucky if it was an alien traveller in a space-ship – it could never have uttered a true sentences like the previous ones. These kind of statements, often found in biological textbooks introductions, can be explicitly identified as narrative sentences. But there is a more fundamental way in which narrative sentences are part of evolutionary biology.

One of the debates in biology is about how new species arise, and more fundamentally, what species are. The former debate is about the question whether new species arise because groups of species are geographically separated, which is called \textit{allopatric} speciation, or

\begin{itemize}
\item White, "The Value of Narrative", citing 2-3.
\item For a defence of why non-narrativizing historiography like Huizinga’s \textit{Waning of the Middle Ages} nonetheless constitutes narrative knowledge see Mink, "Narrative Form".
\end{itemize}
whether a new species can arise at the same geographic location as its parent species, which is called *sympatric* speciation. Furthermore there are some in between positions called *peripatric* and *parapatric* speciation.\(^\text{153}\) The latter debate is about what makes a species a species. Another question is how to define a species. Even more positions exist in this debate. Luckily there is one idea that has been discarded by all biologists and philosophers, namely that species can be identified with essential properties. Since Darwin’s *Origin* it is clear that a species has no essence or set of necessary and sufficient properties that define it. For the process of evolution prevents any trait to be permanent in a species. So there are no biological properties that occur in all and only the members of a single species. Any biological property that is thought to be essential to a specific species can be left out. Furthermore, some species can have a biological property that is also predominantly present in another species. A species is, so is the general idea, not a term defining a class.\(^\text{154}\)

This anti-essential nature of species is also visible in the speciation process. Speciation, in whatever of the above mentioned forms, is a relatively slow process. It is unlikely that a sibling and a parent organism differ in such a degree that we would call the sibling a new species. The process is gradual and takes many generations. It is, in fact, one of the tenets of the neo-Darwinian theory of evolution that it dismisses Saltationism, the instantaneous forming of new species after a substantive mutation, as the source of new species.\(^\text{155}\) So groups that start to differ in traits from the original group, be it because of allopatric speciation, or because something else, do not become a different species in just one generation. When differences in traits between two sub-groups of one original population start to occur, we cannot tell whether this leads to speciation or not. Only after a definite difference in traits, and this can only happen after many generations, can we say there is a new species. When looking at an organism in the present, there is no internal or intrinsic property that shows that the organism will become a new species.\(^\text{156}\) A small divergence of traits in a sub-group of a population might mean that a new species is emerging, but these traits might also disperse throughout the original population, and thus fail to bring about a genuine new species.

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\(^{154}\) For a recent defence of viewing species as classes see, among others, J. Dupré, "In Defense of Classification", *Studies in the History and Philosophy of Biology and the Biomedical Sciences* 32 (2001) 203-219.


\(^{156}\) Dennett, *Darwin’s dangerous idea : evolution and the meanings of life*, 96-100.
This inability to specify a new species is clearly visible in some recent speciation research. The field biologists Peter and Rosemary Grant study a group of reproductively isolated Darwin finches on one of the Galapagos islands – a single male got lost, and ended up on the other island. Compared to the other finches of their “new home”, these finches have a different morphology and sing a different song. And because of this, they do not reproduce with the other finches on this island. What is of interest is that Grant and Grant refrain from speaking about a new species: “[I]t is too early to tell whether reproductive isolation is transitory or likely to be enduring.”\(^\text{157}\) Biologists are clearly aware of the fact that they cannot speak about a species until, at some future point, depending on the chosen definition, the species really does become one.

But if biologists can only speak about species after they have established that such a species actually exists, with what other kind of utterance can they speak about species than with Danto’s narrative sentences? Recall that in a narrative sentence an event is described with reference to an event that happened later than the event to be described. Every mentioning of a species implies a speciation event – the forming of new species is one of the core ideas of evolutionary theory. This speciation event is the event that marks the moment that a certain organism or group of organisms become a different species. But speaking about such a speciation event is only possible after the speciation happened. At the moment of speciation there can be no talk about speciation, because a speciation event always refers to a later event, namely the event that confirms that it is actually a species we are talking about. The latter event happens when the descendants of the original group of organisms become separated from the original group. Therefore, mentioning a species necessarily implies mentioning its speciation event, an event which refers to a temporally later event, i.e. the event when a species starts to exist according to the chosen definition. The exact criteria for identifying such a speciation marking event is a difficult task. But it shows that such an event is implied when biologists speak about a species, and this implies a narrative sentence.

But there is a catch to all this, because it hinges on the definition of the species concept. If species can be identified in an earlier state, the argument is in trouble. The species concept is, however, more elusive than the holy grail. Some biologists define species as a group of organisms of which all members have the propensity to successfully breed with each other. This is known as the biological species concept. Others, seeing that this definition excludes asexual organisms and leads to problems with hybrids, see species as a group that occupies a

specific niche in an environment. Proponents of this ecological species definition argue that a species is a group of organisms of which the members all live in a certain ecology space, and deal with the problems this ecology presents them in a similar manner. Then there are biologists who define species as those members of a group that can recognise each other as potential mating partners, or those who define species according to a phylogenetic relation. The list can go on for a while.\textsuperscript{158} Whatever the case may be, the above argument is valid for all definitions that treat species in light of the principle of natural selection, because natural selection treats species as historical entities that have a beginning and, mostly, an end.\textsuperscript{159} Therefore, they all bear in mind that at the start of speciation it is uncertain whether a specific species will form or not. All the definitions that rely on this view of species as historical entities imply narrative sentences.

There are, however, certain species definitions that try to evade the inclusion of natural selection. Inspired by Popper’s critique on natural selection, some biologists argue that the theory of evolution should be subjected to rigorous empirical scrutiny, using the hypothetical-deductive method. Since the species concepts that somehow incorporate the process of speciation involve the idea of the principle of natural selection, these biologists claim that any hypothesis that refers to a species automatically includes a reference to natural selection. And, accordingly, this would, in the eyes of these “strict Popperians” lead to a circular argumentation: the hypotheses that should prove natural selection to operate on a given species is already presumed in that hypotheses. Their solution was to give a number of non-historical, non-evolutionary definitions of the concept of species. The best known example is the phenetic species definition – utilizing the Wittgensteinian idea of family resemblance. According to this definition species should be ordered by overall similarity. The problem with this definition, and for that matter, all other non-historical definitions, is that there is no non-arbitrary way to choose the aspects of measurement. Which parts of an organisms ought to be used in a definition? And how are we to decide which organisms are or are not of a single species? With different points of departure different classifications can, and have been made.\textsuperscript{160} What use do arbitrarily chosen taxonomies have for the biologist that wants to

\textsuperscript{158} An overview of the different species definitions is given in Hey, ”The mind of the species problem”.

\textsuperscript{159} In population biology, drift, or genetic drift, is the process in which non-selectively differential genetic variations can lead to changes in gene frequency in a population.

\textsuperscript{160} Maureen Kearney, ”Philosophy and Phylogenetics: Historical and Current Connections” in: David L. Hull and Michael Ruse ed., \textit{The Cambridge Companion to the Philosophy of Biology} (Cambridge, etc., Cambridge university press, 2007) 211-232, citing 222ff. Another problem with the phenetic definition is that there are species with multiple morphological sub-species. An obvious example is the differences between the sexes in a single population. Males and females often have a different morphology while belonging to the same species.
explain the biological world? It seems biologists have no use for such taxonomies. They want a species concept that is connected to evolution, for it is one of the key ideas of evolution that species are the product of natural selection. Defining species without this process leads nowhere. Whether philosophers like it or not, biologists that endorse evolution by natural selection have to treat species as historical entities, and use narrative sentences when speaking about such species.

Even though mentioning a species implies narrative sentences, biologists do not explicitly utter many narrative sentences. They often refer to species without mentioning the moment species started to speciate, nor the moment it finished. But the mere implication of these kind of sentences is enough to imply the narrative nature of biological explanations that mention species. All speciation concepts' exact meanings lead to narrative sentences. The fact that narrative sentences are implied is enough to see that Danto’s conclusion about the historical sciences is also valid for the biological sciences. This conclusion is that these sciences do not differ from the natural sciences because of their subject-matter, but because of the way their knowledge is organized.

Historical and biological knowledge is a different kind of knowledge because it is knowledge in a narrative form. And since the argument about the imaginary Ideal Chronicle shows that historical knowledge cannot be about making an exact copy of the world, biological knowledge can also not be about making an exact copy of the world. In biology there is the same gap between the past and speaking about that past.

This can easily be understood as a sceptical claim about biological and historical knowledge, but such conclusions must be resisted. Danto makes it clear that narrative sentences about an event are more endowed with meaning about a particular event than actual or potential sentences uttered at the time of that event. Simply put: Historical knowledge is not bad knowledge, it is a different, and more enriched kind of knowledge.\footnote{See page 45ff. for the details of Danto’s argument.}

Since it is about the kind of knowledge of biological explanations, all this also indicates that an argument about the difference between physics and biology that depends on the difference in subject-matter will not easily succeed. Some earlier attempts to use narratives to account for biological knowledge often failed because they focussed on this difference in subject-matter, rather than on the difference in language.

Dealing with these kind of problems often leads back to notions of mating, which, consequently, makes a return to natural selection, and thus narrative sentences, inevitable.
In one of the first philosophical works on evolution after the synthesis of the first half of the twentieth century the philosopher Thomas Goudge argues that biology needs narratives because biologists “[…]are unable to ‘read off’ from the fossil record a detailed account of critical phases of this history.” According to Goudge, the complexity of certain biological events prevent us from explaining these events with covering laws. He thinks that the complexity of the biological world makes these events unique. And unique events cannot be subsumed under a law. In the specific passage Goudge is somewhat ambiguous about whether this uniqueness is based on the epistemic problem that the fossil record is inaccessible, or whether the events are intrinsically complex. In the former case the complexity of a specific event is due to a lack of evidence. If, in that case, we would have witnessed the event, we could have explained the event using laws. But then the problem is a problem of scientific method instead of a problem of philosophical justification. However, in other passages Goudge endorses the claim that certain events in the biological world are intrinsically unique because “organisms are literally historical creatures.” Because of this historicity, some events become too complex, and thus unique, and consequentially, cannot be explained using laws.

Yet uniqueness is not a distinguishing feature of historical facts. All facts are just what they are: facts. Only under a description will a fact become “of a kind”, or “unique”. The Mount Everest is a uniquely identified entity, namely a specific mountain in the Himalaya mountain range. But it is also of the kind mountain, the kind mountains higher than 8000 meters, and the kind non-humans. It all depends on the description whether we shall view a fact as a unique fact or as a fact that is “of a kind”. Uniqueness cannot be the character that sets biology apart from physics.

Danto’s analysis also throws light on the arguments that use nomic inhibitors to show that there are no laws in biology. We have seen that, among others, John Beatty and Alexander Rosenberg believe that the character of natural selection prevents the forming of exceptionless regularities about biological systems. Natural selection creates biological systems that have nomic inhibitors. Consequently, it is the character of natural selection that makes evolutionary biology different from physics. But the question is what this means.

163 Ibidem, 62.
164 Ereshefsky, ”The Historical Nature of Evolutionary Theory”.
165 See the paragraph Laws, regularities, and models in biology at page 6ff.
There are two possibilities. First it could mean that there really are certain things or processes in the world that cause infinite uniqueness. When this path is taken, it must be shown that natural selection always leads to uniqueness, and that this is a phenomenon that occurs in the biological world. This requires an ontological claim about the living world. This will lead to problems, for it was exactly a similar ontological claim that brought the Historists into problems. The Historists believe that acquiring real knowledge about the past can only be done with Historical Ideas. As we have seen, the problem is that Historical Ideas cannot be proven to exist; it is downright metaphysical speculation to presume otherwise. The reason why the Historists nonetheless believe in this Historical Idea is because of their reliance on the claim that human nature is infinitively creative. This human creativity is, according to the Historists, the fundamental difference between history and the physical sciences. But this claim about human creativity is a scientific claim, not a philosophical one. A falsification of this claim would also falsify the philosophical argument.

In a similar vein the argument that the process of natural selection leads to infinitely new biological forms, regularities, and systems relies on the scientific claim that natural selection can really do all these things, i.e. it means that natural selection is an ontologically real creative process. But, as is shown above, it is difficult to define the process of natural selection in an unambiguous manner. It is a process that defies all attempts to describe it as a set of exceptionless regularities. Such a description is, however, needed if it is to function in the justification of biological knowledge.

But even if this process could be spelled out in an unambiguous way, it would make the philosophical arguments like those of Beatty and Rosenberg susceptible to being falsified by biological empirical findings: Biologists could make an observation that falsifies the claim that the process of natural selection takes place as they always thought it did. Natural selection, as is spoken about by Beatty cs., could be wrong. The consequence of this (hypothetical) falsification would be that the nomic inhibiting characteristics of natural selection could also be falsified. Yet biology as a science would still remain, and would still need a philosophical justification. The point is that this shows that relying on the claims of a scientific theory in philosophical arguments to justify those same claims places the philosopher at the mercy of its subject-matter – in this case the scientific views on natural selection. Reading Beatty’s and Rosenberg’s arguments about the lack of laws as a claim that natural selection ontologically leads to nomic inhibitors is, therefore, not very fruitful in a philosophical sense.
This leads us to a second interpretation of the arguments about the impossibility of laws in biology. In this view, these arguments do not rely on the scientific truth of the process of natural selection. They are rather indications of how the whole idea of a process of natural selection implies a different kind of knowledge. A knowledge that is in the form of historical language. So it is not that natural selection as a process prevents laws, it is that the language that evolutionary biologists use to describe the world is historical language. This interpretation is in line with the conclusion that the concept of biological species implies Danto’s narrative sentences. On the linguistic level, and more specifically in the way that the knowledge is ordered, it becomes clear that evolutionary language is intrinsically historical. And this historicity means that this language is, just as with history, always temporarily separated from the world it is about.

Biological knowledge is historical knowledge, and historical knowledge comes in the form of narratives. Since Danto’s analysis is restricted to the size of the sentence, we need to step beyond it, and show how the analysis of textual wholes by Walsh and Mink fit with the philosophy of biology.

4.2 Configurational comprehension in biology

The way the species concept implies narrative sentences hints at the intrinsic historical nature of evolutionary biological language. Since Danto’s analysis remains silent on the subject of the narrative as a whole, a further analysis is required. Just as the concept of species played an important role in the previous section, it will be our starting point in this one.

Thinking about species as permanent entities has a long tradition, going back as far as Aristotle. Within contemporary biology this “permanence view” on species has, however, been dropped. Darwin made it acceptable to think about species as changing. But what, then, are they if they are not permanent? As argued earlier, if species are not permanent, they lack a universal and timeless essence. Specific atoms have a fixed number of protons and neutrons that enables the identification of instances, a species cannot be defined in such a manner, the latter’s makeup changes constantly. Therefore species cannot be used as classes that have organisms as their members. Nevertheless they need to be identified. This is quite a pressing matter since species play an important, if not central role in biological explanations.

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166 A history of these ideas, and a nuance to seeing Aristotle as a radical species essentialist see Grene and Depew, *The philosophy of biology : an episodic history*.
The biologist Michael Ghiselin and the philosopher of biology David Hull solved the problem of species by showing that biologist treat species as individuals. According to them, species are defined ostensively. They think biologists treat a specific species just like we treat a single member of, say, a mammalian species. Hull writes that “[r]egardless of whether one thinks that "Moses" is a proper name, a cluster concept or a rigid designator, "Homo sapiens" must be treated in the same way.” And just like the mammalian Moses needs to be spatiotemporally and causally connected – if this was not the case, he would surely die – a species must be spatiotemporally and causally connected as well. Being a member of a species is, consequently, not due to essential properties, but, so argue Ghiselin and Hull, due to a causal relationship with the other organisms that are part of the species. Organisms are related to a species as a part to a whole instead of as a member of a class. And this whole is a single evolving lineage. When taxonomists describe a new species by means of a type-specimen, when, for example, the morphological and behaviour characteristics of a new species are identified, they do not give the essential properties of the new species. This specific type-specimen is rather part of the species because it is part of the “ancestor-descendant nexus” of this species. The process of definition is reversed: a species is not defined by its type-specimen, but a type-specimen is recognized as part of a species.

Ghiselin and Hull base their individuality thesis on the way natural selection is believed to work. They analyse the concept of species as a theoretical term within the theory of evolution, i.e. they look at how biologists use species concepts.

Hull argues as follows. He states that when a certain trait, genetic or otherwise, evolves by means of natural selection, this trait needs to be hereditary. Without hereditary transmission between generations, no increase of the trait in a population, i.e. selection of a favourable trait, would be possible. But since hereditary transmissions must occur for selection to work, there must be a spatiotemporal connection between the different generations of organisms that undergo this change. Natural selection would be meaningless otherwise. And this leads to the view that there is a single individual entity, namely the spatiotemporal lineage of organisms that forms a species. The conclusion is that “species may not seem to be spatiotemporal particulars, but according to evolutionary theory, that is precisely how they must be treated –

169 Hull, "A Matter of Individuality".
deeply entrenched commonsense intuitions notwithstanding.\textsuperscript{171} For without the spatiotemporal connection and the subsequent individuals, natural selection would not work. Species must, therefore, be individual things.

Ghiselin’s and Hull’s proposal does pose a problem for explanation. As shown above, in the received view, scientific explanation consists of subsuming observations with initial conditions under a law. And the general tendency of science is to make these laws more exceptionless and universal. But if species are individuals, they cannot figure in these kind of exceptionless universal laws. So according to Hull and Ghiselin, the species as individuals thesis makes laws about specific species impossible. This conclusion is not surprising, and looks a lot like the earlier arguments that show that laws are not possible in biology because of certain nomic inhibitors. The unavoidable follow-up question is again: how do biologist explain if they cannot use laws?

Hull’s answer is that explanations of historical individuals, like species, can only be done with historical narratives. And in this he is quite right. Explanation is not only the process of subsuming phenomena under a law or pattern. Hull sees two kinds of explanatory activities in biology. On the one hand there are the attempts to see past patterns and events in light of processes that are happening in the present. Finding out the most generally applicable patterns is the goal of scientists that use these explanations. Population biology is the subfield of biology that Hull associates with this kind. Population biologists seek out how populations change under various circumstances, and try to make general models of these processes. In their view understanding evolution is a steady improvement of a set of models that relate to the world like a set of maps relate to reality. The better the maps become, the more is presumed to be known.\textsuperscript{172} They strive for models that are as broadly applicable as is possible, and therefore think that the influence of historical contingency is something that, like background noise, needs to be minimized.\textsuperscript{173} In a similar vain this kind of explanation can be compared with the socio-scientific and economic explanations that are used in history.

On the other hand there is what Hull calls the reconstruction of the historical records. In this reconstruction it is exactly the population biologists’ “noise” that is of interest: it is all about

\textsuperscript{171} Hull, “Model of Scientific Explanation”, citing 75.
\textsuperscript{172} This is a “presumed” improvement because it is not at all sure what “better” means. While the invisible details of a geographical map do not matter with respect to the function of the map – we do not see every single tree on a city map, but we can easily navigate with it – the invisible details of the biological models, I think, do matter for the explanations of evolutionary phenomenon. The specific details of the biological world make that genetic changes do or do not correlate to a change in fitness. In the first case, population biologists call the change an adaptation, in the second case it is drift. But no general model can be constructed that deals with these kind of details. See the following sections for more details.
\textsuperscript{173} Population biologists are, in that respect, not quite unlike economists.
the particulars. The paradigm subfield of biology that explains in this manner is palaeontology. Palaeontologists try to reconstruct how life on earth developed, basing their conclusions on the available historical records. They make descriptions of how historical entities develop through time. Hence, thus Hull, their goal is to know more about the particulars of the past. They reason that a one-sided focus on the patterns, the thing population biologists do, obscures the fact that these particulars are what evolution is really about.\textsuperscript{174}

The latter kind of explanation, the reconstruction of the historical records, is, according to Hull, similar to what he labels as the reconstructions that historians make of the past. For historians are also interested in the particular.

For the current thesis this latter comparison is especially interesting, because Hull believes these reconstructions are the result of a configurational operation of seeing a diversity of events in a single whole. He follows Mink closely when he writes that a historical event “is explained by integrating it into an organized whole.”\textsuperscript{175} The understanding of all the diverse events into one configurational comprehension is the actual explanation of those events. Biology, especially the historical part, is a science, thus Hull, that operates and explains by means of narratives. And this is all due to the fact that species are individuals. This, in turn, is due to the way biologists view natural selection, namely as an operation that presupposes lineages of organisms.

While Hull pays lip services to Mink, he does not mention Walsh or the concept of colligation. This is not surprising since Walsh’ earlier ideas about colligation are based on an intentionally philosophy of history. As we have seen, Walsh’ first argument starts with the presumption that historians want to explain human intentions. Since intentions have no part in a mechanistic evolutionary perspective on life, this first concept of colligation is of no use to the philosophy of biology. Walsh’ second version of colligation also fails to impress Hull. Or in any case, he never mentions it. This is probably because Walsh believes that colligation is a form of interpretation instead of explanation.\textsuperscript{176} And in Hull’s eyes interpretation is not a very scientific thing to do with respect to the biological world.\textsuperscript{177} According to him, biologists do not interpret the past, they reconstruct it.

However, Hull’s neglect of Walsh is informative in that it shows how Hull circumvents the major issues about historical knowledge that Walsh introduced, and of which Mink draws the

\textsuperscript{174} Hull, “Model of Scientific Explanation”.
\textsuperscript{175} Ibidem, citing 77.
\textsuperscript{176} Walsh especially stresses the interpretative character of colligation. See Levich, ”Review: [untitled]”, Walsh, ”Colligatory Concepts in History”.
\textsuperscript{177} Hull believes it has no place in history either. Hull, ”Central Subjects and Historical Narratives”.
consequences. Looking at the way Hull analyses historical narratives makes clear how he fails to account for the explanation of narratives, because he fails to acknowledge its interpretational aspect.

According to Hull, historical narratives consist of two elements that are both necessary if the narrative is to explain the biological world. In the first place there are the descriptions of the historical entities, substances, or main actors the narrative is about. These are descriptions of an ontological real object that ensure the continuity and unity of the main actor(s) of the narrative. In the case of biology we can think about organisms, species, or whole higher-order taxa (when, for example, discussing the extinction of the dinosaurs, or the evolution of sexuality.) Hull believes that all historical sciences use these historical entities in their narratives. He gives the example of a biography in which “the physical continuity of the person through time forms the primary continuity for any historical narrative concerning him.”\textsuperscript{178} So the first element of a historical narrative is the description of the continuity and unity of some historical entity. This entity is the central subject of the narrative.

The other element consists of the description of all the connections between the central subject and the events it is part of, i.e. the causal chains these historical entities are a part of. In biology these are the descriptions of the events and causal chains in which the organisms, species, or higher taxa are the participants. This element consists of descriptions of what influenced the development of the historical subject(s). Again, Hull believes that this also applies outside of biology. In the biography example these are the events the person undergoes: which influences he underwent, the oppositions he encountered, what he achieved, etc. In biology, these descriptions are the events that shaped the course of the history of the entities the narrative is about. So, to cite an extreme case, the meteor impact associated with the Cretaceous–Tertiary extinction is one of the last events that can be related in the narrative about the taxa of the dinosaurs.

According to Hull, both elements are present in biological language about the genealogical actors and the ecological play of these actors in the narratives. On the one hand there are the evolutionary units that are the main actors on the evolutionary scene, and on the other there are the events that have happened to these units.\textsuperscript{179}

\textsuperscript{178} Ibidem, "Model of Scientific Explanation", citing 76. Compare this with Baumgartner’s remarks that often people confuse a biography, which is a story, with the actual life of the subject-matter. Baumgartner, Kontinuität und Geschichte, 301ff.

\textsuperscript{179} Hull, "Model of Scientific Explanation".
We can now see that, while Hull believes narratives are comprehended as a whole, he does not follow Walsh or Mink in their formal analyses of this whole. Hull believes that a narrative is the result of the ordering of a unordered set of historical facts into a coherent whole. And he does, following Mink, believe this ordering into a whole is explanatory in itself. But for Hull, the unordered state of all the historical phenomenon is not overcome by the act of seeing them under one colligating concept. It is rather overcome by placing these phenomena into a narrative that is structured as a set of statements that ensure the continuity of the ontologically real central subject, and that causally connect the events associated with this central subject. Regarding the formal properties of the narrative, Hull is much more a follower of Morton White and his idea that a narrative is a causally connected chain of events. But at the same time, Hull thinks narratives explain because, following Louis Mink’s analysis, they are configurational comprehensions.

This is a duality in argumentation that cannot be upheld. On the one hand we see that Hull believes that species are individuals, and that explanations that include species are narratives. And since a narrative does not explain by means of a deductive argument, some other explanatory scheme is needed. Mink’s idea that configurational comprehensions are inherently explanatory seems a ready-made solution. On the other hand we see that Hull does not take the step of seeing historical concepts or whole narratives as the result of an interpretation of historical phenomena into a single coherent whole. Yet the analyses that narratives explain relies on this insight. It is in the act of seeing together all the phenomena of the past, an act of interpretation that is, that an explanation of those phenomena is given. If the explanatory nature is linked to the idea that narratives fundamentally rely on the causal connections that apply to a central subject, a deeper level of explanatory analysis is introduced: Narratives, in that view, do not explain because they impose an order upon the phenomena of the past, but because they purport to show the causal order of the different events that relate to the central subject. Narratives are about reconstructing the “causal nexus” of the past. And this brings Hull back to using causal terms which, notwithstanding many attempts to the contrary, defies any coherent analysis except those in terms of Humean regular observations. And that, in turn, brings us back to the covering law model, or one of its more recent and elaborate derivatives. Narratives, in this position, eventually explain because they

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180 Ibidem, "Central Subjects and Historical Narratives", citing 260ff. Ernst Mayr also analyses biological narratives in the sense of Morton White. He explicitly refers to White in Ernst Mayr, This is biology : the science of the living world (Cambridge, MA. etc., 1997), 65. White’s analysis of narratives as a set of causally connected statements can be found in White, Foundations of historical knowledge. 223-225 and chapter 4.
subsume phenomena under a law. Trying to hold on to causality while claiming that the coherence of the comprehension explains, cannot but lead to an inconsistent position. Mink draws the conclusion that notions about the causal order of the past cannot play a role as a configurational comprehension. The order in a history is created, not found. And that act of creating order applies to the order of causality as well. It was in this manner that Mink expanded on Danto’s lesson that historical language is separated from the past it is about. But this lesson is unacceptable for Hull, and most other philosophers who think biology uses narratives.\textsuperscript{181} Yet the acknowledgement that biologists treat species as individuals leads to the inevitable conclusion that biological explanations cannot be law-like. We tell histories about individuals. As Mink shows, historical explanations are configurational comprehensions in which knowledge \textit{elements} are ordered into a linguistic \textit{whole}. Copying the causality of the past makes no sense in such a comprehension.

The reason why Hull, nonetheless, insists on the basic causal nature of biological narratives is because he presupposes that Darwinism relies on a mechanistic worldview. It is precisely the success of the Darwinian project to have replaced explanations based on teleological causes for mechanistic explanations that are based on efficient causes. Biology textbooks often scornfully denounce Lamarckian ideas of progressive development and other forms of goal-oriented thinking. In light of this, the philosophical analysis of biological language should, or so seems to be Hull’s thought, account for this presupposition as well. In such an intellectual environment it is considered an act of heresy to claim that biological explanations are not about finding causes, but about creating order. Yet the opposition between teleological explanations and efficient explanations is not the point of departure for a correct analysis of biological language. The correct point of departure is the linguistic opposition that Danto recognized. It is the opposition between historical and non-historical language.

We saw that Ghiselin’s and Hull’s arguments about the individuality of species rely on the way in which biologists use evolution. Species are individuals because the principle of natural selection presupposes evolving lineages. This principle reconciles, in Hull’s view, the diverse and contingent facts of the biological world – the part of reality that narratives disclose to us – with a strictly mechanistic world view. And consequently, it is this general mechanism that stands at the core of Hull’s view. Natural selection explains how the biological world has come about. It secures, in his view, that the biologists’ narratives are about the correct kind of

\textsuperscript{181} See Ernst Mayr, \textit{What makes biology unique? Considerations on the autonomy of a scientific discipline} (Cambridge etc., 2004), O’Hara, “Homage to Clio, or, Toward an Historical Philosophy for Evolutionary Biology”, Steen, \textit{Evolution as natural history}. 

76
individuals, i.e. species, genes, or some other biological kind. The principle of natural selection provides them with their kind of central subject.\textsuperscript{182} Yet even this principle of natural selection cannot be understood without historical narratives.

To see why, we have to take a look at the tautology problem once again. In a previous section it was shown that defining the principle of natural selection fell upon several difficulties. The most problematic part was to get the meaning of fitness clear without turning natural selection into a tautological statement. But things might not be such a problem after all. The philosopher Wim van der Steen believes that the definition of fitness does not lead to a tautology.\textsuperscript{183} There is, however, a price to be paid, or rather won, for he concludes that evolutionary biology leads to natural history.

If we take a simple textbook population genetics explanation we can see why.\textsuperscript{184} Van der Steen’s argument begins as follows:

Consider a population with two asexually reproducing types of organisms, $A$ and $B$, in which $A$ consistently has more descendants than $B$. […] If limits to the growth of the population exist, it will ultimately [only] consist of $A$ organisms. Population geneticists describing this would say that $A$ has a greater fitness than $B$ (meaning that $A$ is better at reproductive survival, that it has more descendants, than $B$), and that this explains the survival of $A$ in the population.\textsuperscript{185}

A criticism towards this kind explanation is that it is tautological because the greater fitness of $A$ compared to $B$ is measured as the better survival of $A$ compared to $B$. This would mean that the survival of $A$ is explained with by pointing out the survival of $A$, and that is clearly a tautology. Yet, as Van der Steen contents, this explanation is not tautological because the word survival has two different meanings for the biologists that explains in this manner. On the one hand there is the survival of organisms or organism types. This is the description of the “greater fitness” of $A$ compared to $B$, i.e. the consistent better reproduction of more descendants of $A$ compared to those of $B$. It is a statement that the differences in structure, behaviour, and efficiency of the $A$’s and $B$’s influence their reproductive success. The other survival is the persistence of types in the population, “the survival of $A$ in the population”. In

\textsuperscript{182} Hull does allow for a certain intuition that decides what the central subject of histories are, but as soon as better theories are devised, these intuited central subjects must be subjected to theoretical scrutiny and, if found lacking, be discarded. Hull, “Model of Scientific Explanation”, citing 77-78.

\textsuperscript{183} Steen, \textit{Evolution as natural history}.

\textsuperscript{184} That ecological biology is closely related to natural history seems obvious enough, but to show that the heavily mathematical population genetics implies natural history as well is more daring and rewarding.

\textsuperscript{185} Steen, \textit{Evolution as natural history}, 11. Note that the types of organisms can be types that are identified by their genes alone, or could even be genes in a population.
the above explanation this is the eventual survival of the A’s, and the extinction of B’s after a
certain period, within the population. This statement is a ceteris paribus regularity that states
that given the greater fitness of A compared to B, and given other factors like mutations,
migrations, and environmental circumstances do not interfere, A will survive in the population
while B will perish. The first kind of survival, i.e. fitness, is thus a part in the explanation of
the second kind of survival. A’s do better than B’s qua reproductive survival, therefore A’s
will eventually prevail, given that the other factors do not interfere too much. As such, the
explanation is an empirical statement that says:

Because A is fitter than B – meaning that A’s leaves more offspring than B’s – in the
period t₀ until tₙ, the number of B’s in population P will decrease to zero, while the A’s
will persist in P.

This is a straightforward empirical statement that is based on a principle of natural selection.
But it should be noted that this principle of natural selection is not an exceptionless regularity.
Leaving more offspring, meaning having a better fitness, is no guarantee for an organism’s
persistence in a certain population. The statement is only true in so far all the different ceteris
paribus conditions are satisfied. And these conditions are things like that there can be
influence from mutations, migrations, frequency-dependent selection, the environment, or
other factors. But since these influences, these ceteris paribus clauses, cannot be known a
priori, no guarantee can be given that the principle will apply in all concrete situations. No
finite list of exceptions to this principle of natural selection can be given, thus the principle is
not a general regularity.¹⁸⁶

So we have a first “principle of natural selection that says that heritable fitness differences
cause population change provided that other factors do not interfere.”¹⁸⁷ And according to
Van der Steen, this principle of natural selection is not a tautological principle since it
involves two different things: fitness of organisms and types, and survival in a population. But
this first principle is not an exceptionless law-like principle. At most it is a heuristic statement
that does nothing more than to give an indication that in a specific population, individual
reproductive success leads to differentiation of types in the long run. This principle cannot be
treated as an empirically verifiable or falsifiable claim, and cannot lay at the bases of a
justifiable science.

¹⁸⁶ Thus far, the argument is quite similar to those arguments that show that the principle of natural selection
cannot be a law. See page 25ff.
¹⁸⁷ Steen, Evolution as natural history, 15.
The second step of the argument on the historical character of natural selection expands on this, for this first principle of natural selection is in need of refinement. If the first principle of natural selection is a statement that has many conditions, seeing how and when these conditions apply is a pressing matter. In other words, there has to be an account that shows why the different ceteris paribus clauses do or do not apply in some specific situation. The question in the above statement about the A’s and B’s is why the differences in reproductive success happened. For even though A is fitter than B, it all depends on the certain ceteris paribus clauses whether A really does increase in number compared to B. But to explain the differences between A and B in this manner means knowing all the features of the organisms involved, and all the features of the environment they were in. These (reproduction facilitating) features are often associated with good design and with the concept of “engineering fitness”. The question is thus relocated to what makes something a good design. A bird has a good design if it is able to do its job well. A crocodile has a high engineering fitness if it is fit for the job it is engineered for. In short, design and engineering are done for a certain purpose. They are functional terms. But what, so asks Van der Steen, could this function be? The most convenient purpose would be to say that design is about getting a high reproductive success, i.e. getting many offspring. But, as we have seen in the ecological interpretation of fitness above, this leads to a tautology again. Defining a higher engineering fitness as a higher reproductive success does, self-evidently, not explain that reproductive success. It leads to the statement that A’s survived instead of B is caused because A is better engineered than B, which means that A has survived instead of B. And the circle is again completed.

To come to terms with engineering fitness, the concept of design can better be interpreted to mean that an organism or biological system has a certain form, or make-up that is suited to particular activities in particular circumstances. “A general definition of the concept [of engineering fitness] cannot refer to particular or generic features of organisms, because organisms are designed in different ways.”\(^{188}\) In one situation a design might be favourable in comparison to another design, but in a different situation things could be reversed. Since engineering fitness depends on this design, engineering fitness is an abstraction that is used to explain many different features in many different contexts. But if this concept explains many different things, so concludes Van der Steen, than using a concept like engineering fitness in evolutionary biology means that the principle of natural selection must be seen as natural

\(^{188}\) Ibidem, 13.
history. For the only way to give an adequate explanation of these diverse situations and features is by giving historical explanations. Consequently, “much seemingly general theory needs to be transformed into natural history.” And this is the result of the fact that the most central principle, the principle of natural selection, relies on a concept of fitness that explains only by means of explicit, or implicit historical narratives.

To sum up, Van der Steen sees two principles of natural selection. The first one, used in population biology, is the reasonably straightforward claim that differences in reproductive success between two types of organisms lead, when certain conditions apply, to long-term differences in the population make-up. But because of the many ceteris paribus conditions, this straight-forwardness is inversely proportional to its explanatory value. We need to specify why the specific conditions apply, and how the reproductive successes comes about. This specification leads to a second, more informative principle of natural selection. But this more informative claim comes down to a principle of natural selection that, when unpacked, actually is natural history. And this natural history lacks any universality. Since the principle implies natural history, it implies narrative knowledge as well, i.e. it implies that the kind of knowledge used, is that of ordering knowledge elements into a coherent whole.

Now a few words on Van der Steen’s analysis of narrative. While he identifies the historical nature of biological explanations, there is, however, a problem with his conclusions. He believes that the biological historical narratives are essentially structured around low-level generalities that indicate which factors are causally relevant. But, as is already shown above, causality is not the determining factor in the construction of a narrative. Therefore I will not delve any further into Van der Steen’s analysis about how narratives explain.

If we now return to Hull, we see that his reliance on biological theory in order to provide biologists with the correct kinds of central subjects is unfounded. The division he makes in narratives between, first, a description of the central subject, and second, a description of the causal chains of events this subject participates in, is an attempt to cash in on the law-like nature of biological theory. This theory must secure the objective establishment of the kinds of central subjects biologists use. For while historians and biologists do, according to Hull, have certain intuitions that guide them in their choice for the kind of central subject, he thinks

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189 Van der Steen does, however, think that population genetic explanations using the first principle of natural selection are more general than natural historical statements. Yet even though he shows that they are not more general in the sense that the used theoretical terms cannot be reduced (bridged) to kinds of phenomena, as is thought to be the case with theoretical terms in physics, he fails to show what “more general” does mean. In what sense is it more informative to use? Ibidem, 18, 22ff.
190 Ibidem, 13.
191 Ibidem, passim.
it would be better to use the more reliable theoretical terms as soon as they become available. So, to give an example, the historian that uses the concept “superpower” as a kind of central subject in his narrative about the Cold War, has chosen this central subject because he has certain intuitions that this is the correct one for getting insight into the given period. But as soon as better theoretical knowledge comes around – a knowledge, I presume Hull to mean, that originates from sociology – this historian should, if the theory does not support his intuition, stop using “superpower” as a kind of central subject. As Hull stresses, “[t]he theories in which laws function determine which entities are theoretically significant, and theoretically significant theoretical entities take precedence to commonsense entities.”

Hull needs the theoretical terms because he wants biological narratives to be about species as they really existed in the past. But since the principle of natural selection cannot explain except as a historical narrative, it cannot be the foundation of a biological theory that provides such kinds as ontological real entities. Evolutionary theory cannot give biologists a theoretical species concept that is based on an exceptionless and universally valid principle. Biologists, just like historians, cannot expect theories to provide the subject-matter for their narratives. They cannot rely on these theories in virtue of their empirically verified and/or falsified status because, on closer inspection, these theories resort to natural history.

As such, it does not matter whether the theory of evolution is presented in terms that make it look like a formal scientific theory. For when the meaning of the central component of this theory, i.e. natural selection, is analysed, natural history emerges. Any philosophical analysis that fails to account for this natural history fails to grasp the thorough historical nature of evolutionary biology.

This leads to a view of biological narratives in which there is no division between a central subject and the causal chains this subject participates in. There is only a narrative in which a text gives order to an otherwise unordered past. No coherence in the past exists that is copied in the biologists’ narratives. The biologists apply coherence to the past by means of their narratives. So Hull’s analysis not only fails on behalf of the putative causal nature of

192 Hull, “Model of Scientific Explanation”.
194 Hull, “Model of Scientific Explanation”, citing 77. Hull believes that the distinction between the central subjects and the causal chains these central subjects participate in is less visible in history because “our current scientific theories about human actions and the development of societies are relatively weak and poorly formulated when compared to those we have concerning the evolution of biological species”. Ibidem, “Central Subjects and Historical Narratives”.
narratives, it also fails on behalf of the idea that a division between central subjects and the events is necessary, and that evolutionary theory can provide the kinds of central subjects.

We should, however, not impose too harsh a judgement upon Hull, for his position is not unlike that of the Historists in the nineteenth century. Just like the Historists thought there are Historical Ideas that exist in reality, so Hull believes species (or other biological concepts) really exist. These Historical Ideas and theoretically grounded species are the respective subject-matters of the narratives. With these entities, both Hull and the Historists try to overcome the problem of change: How can change occur if the essence of a thing is its history? For change presupposes that some substance stays the same, yet historical entities appear and disappear. Both Hull and the Historists, however, fail to see that these entities are linguistic rather than material entities. It is apparent that Hull is wrestling with this problem when he writes in a footnote that “the notion of genidentity [,the view that species are historical individuals,] presupposes throughout this paper is not without its difficulties; e.g., how many parts of an individual can be replaced and how rapidly before it ceases to be the same individual and in what sense of “same”? […] I happen to think that such questions cannot be answered in the context of ordinary language. However, they can be in the context of well-formulated scientific theories.”

Hull correctly sees that the way in which biologists speak about, and think about natural selection implies that species cannot be viewed as classes. He is also right about what this implies with respect to biological explanations, for these explanations are narrative in nature. And finally, he is also right in invoking Mink’s idea that the act of the configurational comprehension, the act of understanding such a narrative, leads to an explanation. He is wrong, however, in positing that evolutionary theory can provide the kinds that form the essence of the biologists’ narratives. Evolutionary theory cannot live up to this, for the central mechanism, the principle of natural selection, leads to historical narratives instead of to a robust theoretical framework. Therefore, we can conclude that evolutionary biology uses narrative knowledge.

This kind of knowledge, as Mink has shown, leads to two important conclusions. The first is that the original temporal order of the events is of no concern to the temporal order within

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195 Hull, ”Central Subjects and Historical Narratives”, citing note 18, 261.
the narrative. It is the biologist who makes the connection between the different historical events, and this is done with the advantage of hindsight. The other is about the truth of the narrative. Narratives as wholes are not true or false, because as wholes they are linguistic orderings of knowledge elements. And an ordering of knowledge elements cannot be knowledge itself, only the knowledge elements contain actual knowledge. These conclusions might seem far fetched to those involved in the philosophy of biology. They surely seem farfetched to some philosophers of history. Especially the post-positivistic developments in the philosophy of science have led to criticisms on the narrativist’s arguments. But we will see that these reactions are misplaced.

The main critique against narrativism is that it is based on a “drastic restriction of the significance of form”, and an unfounded “denial that [the ordering of knowledge elements] enhances our knowledge.” The claim is that the view of the narrativists that such an order is not knowledge is the result of a “‘hangover’ from scientism”. It is due to a contrast of the field of history with the triumphal view on the natural sciences of the logical-positivists during the first six decades of the twentieth century. Narrativist philosophy is framed to utilize a “reversal of the traditional positivistic view, [and this philosophy] shares its fundamental conceptual structure [with the positivist view].” But ever since this earlier positivism has been replaced with a more advanced post-positivism, such a contrast becomes, so the opponents of narrativism claim, the source of confusion. Philosophers like John Zammito or Chris Lorenz, but there are many more, claim that the post-positivist philosophy of science has much more in common with the philosophy of history than the proponents of narrativism accept.

These post-positivists specifically point out that narrative philosophy of history falsely emphasizes a distinction between individual facts and the narrative whole. Post-positivism, building upon Quine’s ontological relativism, accepts that our language schemes are always the result of an arbitrary history. Our history determines the way we connect our words with the world. Consequently, we can choose any language scheme. But we cannot conceive an object except through one language scheme or the other. We just cannot do without language. This is, indeed, a useful analysis. There are no “givens” in reality, i.e. objects do not present themselves to us without our historical mediated language. How, then, can we come to

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197 Lorenz, “Can Histories Be True?”.  
agree that Paris refers to the capital of France? The reason that we agree that certain words refer to certain things in reality is because we have reached an inter-subjective agreement to do so. In our daily life it would be very un-pragmatic to ignore the inter-subjective reference of the kind “bus” when someone warns you with the utterance: “Look out for that bus!” Yet the kind “bus” could, in another world, refer to something else, say to those things in reality we now call “birds”. The point of those who criticize narrativism is that this is as true for buses as it is for the larger textual units in histories. Saying that, contrary to a bus, the totality of an ordering of knowledge elements does not refer to something in the world is, according to them, based on the misconception that on the level of the narrative, language can only refer if there is one objective way of connecting that language to the world. But, as the conclusions of Quine’s ontological relativism makes clear, the connection between language and the world is always arbitrary. Narratives should, therefore, not be treated in the special way narrativists do.

These criticasters will readily admit that coming to an inter-subjective agreement about the referent of “the Renaissance” is more difficult than for the type “bus”, or the proper name “John”. Specifying the former referent is clearly a more complicated matter than the latter two. But they deny that “the conceptualization of colligatory concepts in history has a more radically constructed character than theoretical terms in natural-scientific theory”. 199 In effect this implies that the idea of a Universal History is not to be discarded. This Universal History is surely not the naïve and optimistic one that was often believed to exist up and until the nineteenth century. But, so their argument implies, some form of a Universal History exists nonetheless.

These post-positivists think that Mink’s and Walsh’ ideas about narrativism are not essential to an understanding of history and biology, because the conclusions of the philosophical analysis of linguistic units like sentences are, for the most part, also applicable to these larger units. Modern philosophy of language already covers the ground that narrativism claims for itself, for the former has a good understanding of how our language and the world are connected. The claim that narratives as a whole are not connected to the world is, in this view, plainly false. In the end, post-positivism cannot but lead to the abandonment of narrativism as a useful theory for both the philosophy of history, and the

199 Zammito, “Ankersmit and Historical Representation”.

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philosophy of biology. Both history and biology do not need a philosophical account that is based on the narrative.\footnote{Some writers apply some nuances. Zammito, for example, concedes that “[historical] representations cannot be \textit{reduced} to reference.” (His emphasis.) But he believes that reference has a part in the analysis of narratives. Ibidem.}

Despite these arguments, narratives differ qualitatively with statements about facts. And this difference necessitates a different philosophical approach towards narratives. This difference can be highlighted by considering how things get names, i.e. how we assign a proper name to a set of features in the world. There are two major approaches towards this issue in modern philosophy of language.\footnote{They are not the only two approaches. For the way in which these, and other approaches connect with the philosophy of history see “Special issue on the philosophy of language and the philosophy of history,” in: \textit{Journal of the Philosophy of History} (2010) 241-449. The insights of this issue has not been included in the current thesis.} The first approach, named the description theory of reference, states that a proper name refers via the descriptive content the speaker associates with a name. This content uniquely determines the names referent. It turned out, however, that for a name to designate an object, it is neither necessary nor sufficient that a speaker associates believes that denote the object with the uttered name.\footnote{Michael Devitt and Kim Sterelny, \textit{Language and reality : an introduction to the philosophy of language}, 2nd editie. (Cambridge, MA, 1999), 54-57. While Saul Kripke’s possible world argument relies, according to many, on his rather idiosyncratic intuition about possible worlds, the so called problems of ignorance and error are decisive. The problem of ignorance can be made clear by imagining that someone believes that Einstein was a physicist. This is a true description, but it will fail to pick Einstein out of all available physicists. The problem of error occurs when someone believes that Einstein was the inventor of the atomic bomb and then utters: “Einstein was a genius.” This person would, according to the descriptive theory of reference, pick out Oppenheimer as a genius.} The other major approach, the causal or historical theory of reference, states that a term refers to whatever it is causally linked to it in a certain way. A speaker does not, in this view, need any beliefs about the referent. When using the name “Einstein”, most people know who the man is, while they never met the man or grasped his scientific work. To explain how people still correctly refer to Einstein, the causal theory has divided the process of name usage into two parts. The first step is the reference fixing, or grounding of the name. This part explains how people first got to use a certain name for a certain thing. The idea is that the thing to be named is perceived by the name giver, i.e. the name giver is causally affected (mostly by perception) by the thing, after which the name is given. This name giving is called a dubbing, and can be a formal or informal event. “In short, those present at the dubbing acquire a semantic ability that is causally grounded in the object.” An important point is that a description of the thing to be named plays no part in this. The second step in the usage of names is called reference borrowing. This step is an explanation of how the usage of names transfers between people
within a society. People not present at the dubbing will causally acquire the (correct) name via those who were present at this dubbing. In the end, everybody can use a name, if, that is, they causally acquired the usage of it. 203 Even though this theory solves many problems of the description theory, it is not without its pitfalls. Some descriptive components are always needed for a successful reference. Therefore, a combination of a descriptive theory and a causal theory, called a hybrid theory, is for most philosopher of language the direction to go to. 204

For our current inquiry, one specific problem, the *qua*-problem, is especially interesting. 205 Within a causal theory of reference it is stated that persons can use names without having any beliefs about the object this name refers to. Yet Michael Devitt and Kim Sterelny show that this is a too stringent characterization for the step of reference fixing. Consider the dubbing of the name “Nana”, to use Devitt and Sterelny’s example. The name “Nana” was grounded when she was in perceptual contact with the one(s) who gave her this name. But on the moment of this fixing, the whole of the person Nana was not visible. Only a part of Nana can be visible in both a spatial and temporal way. Nana was partly spatial visible because during the naming we only see one side of her, and it could also be the case that only her head was visible. She was partly temporal visible because the fixing occurs at a specific time. Yet Nana will probably keep on existing after the naming event, and chances are she has been existing before it as well. This shows that there is something about the mental state of that person who fixes the name that makes sure the thing that gets named is experienced “*qua whole object*”. Since it could not be mere intention that decided to take the whole of Nana – on what is this specific intention based? – “[i]t seems that the grounder must, at some level, “think of” the cause of his experience[, i.e. seeing the person Nana,] under some general categorial name like ‘animal’ or ‘material object’.” 206 Only because the perceptual information can be subsumed under a category term is it possible to fix an object, and give it a name. Therefore, a descriptive component, the categorial term, is always needed when fixing a certain name. Thus, a successful fixing can only happen when the person who does this fixing is not categorial mistaken, and indeed intents (consciously or unconsciously) to refer to the thing as being of this category.

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206 Ibidem, 80. This can be done consciously or unconsciously.
How this relates to the post-positivist critique on narrativism can be shown in the following way. The philosopher John Zammito criticizes the claim of the narrativist Frank Ankersmit that historical narratives do not refer to reality. Zammito seems to agree with Ankersmit that qua reference these narratives are to be treated like proper names. But Zammito believes that since the analysis of the causal theory of reference indicates how such proper names can refer to reality, narratives can also refer to reality. Or they at least do so for a substantial part. He writes that it is his “impression […] that much philosophical work on the “causal theory of reference” stresses precisely what Ankersmit calls “compulsions of experience.””207 This “compulsion of experience” is about how our experience dictates and forms our way of speaking about the world. Zammito thus stresses that the analysis of the causal theory of reference indicates how experience (partly) dictates the manner in which historians construct their histories. The stories of historians are somehow related to, or dictated by the world, or so he claims.

Now the *qua*-problem comes along. As Devitt and Sterelny point out, a pure causal reference theory will fail to account for the way we use names. Fixing a name is only successful when a descriptive component is present that describes the correctly applied and intended category the thing is an instantiation of. Therefore, any claim about how the causal theory of reference can clarify the way in which narratives refer, must provide an account of the categories that are used in the grounding procedure of such a narrative. There has to be a “kind” of historical object before such an object can be said to refer in the manner appropriate for the causal theory of reference.

But, if we do accept the causal theory of reference, what are these categories in the case of historical narratives? And how do we describe them? To get things clear we will take a look at a historical narrative N that describes historical thing H that would be a member of the category E. So H is predicated as an E, and N is a narrative about H. Say that a thing is of kind E if it has the properties \(a\), \(b\) and \(c\). So if E would be something like the kind “enlightenments”, \(a\) and \(b\) could be things like “people writing about natural right philosophy”, “a general believe in the rationality of the world and the mind”, and “an advancement of scientific method and understanding”.

The question is now if there can be another E thing. And the answer is no. To see why, we have to imagine that the properties \(a\), \(b\) and \(c\) of an E thing are observed again. Will this mean that an E thing has happened again? The historian who wrote N will surely protest against this

207 Zammito, “Ankersmit and Historical Representation”.
conclusion. She will state that there can be all kinds of reasons why an E thing is not happening this time, for the circumstances can be wholly different with respect to the earlier E she described. There can be a whole set of conditions that makes seeing this event as an E impossible. If we would ask her why she did, nevertheless, presumed that an E thing did occur in the past she described in N, she would probably answer with: “Go read my narrative”, i.e. she would say that N shows her reasons for thinking why an E thing happened in H. Without N, there would, so she will say, not be enough reason to describe certain phenomenon as N. In other words, historians do not presuppose a kind E in the manner that is required for the causal theory of reference to work. If we stick to the terms of this theory, we could say that historical narratives and the putative historical kinds collapse into each other. Historians do not use kinds of historical things. Historians rather create unique entities.

This leads to another complication (and feature!) of historical narratives. Sometimes historians give a name to certain historical phenomena. They say, for example, that H is the “American revolution”. But since the whole narrative defines this specific name, competition is to be expected. Every other narrative that is about something close to H will be different, and thus, will see a different “enlightenment”, or perhaps no “enlightenment” at all. And the debates are, accordingly, not about whether a kind of historical thing has occurred in the past – remember, narratives do not refer – but they are rather about how to best see the past. Every historian gives another view, a different rearrangement of the past. And as such, every narrative is a new attempt to give a part of the past a certain specific form.

To sum up, we see that the causal theory of reference cannot be used to determine the reference of a narrative. Since the fixing of a name requires, as Devitt and Sterelny show, a kind of thing – you fix a thing qua kind x – there have to be such kinds for narratives as well. But in the case of narratives, these kinds cannot but collapse into the whole narrative, making the causal theory of reference useless. The positive claim about the existence of a Universal History cannot be sustained on the ground that narratives refer like proper names in the sense of the causal theory of reference. Of course, there are lower linguistic levels on which certain groups of words can, and certainly do refer to things in reality. Narrativists will not deny this. The different knowledge elements of a narrative all do refer to things in historical reality. But these elements are only to be found at the lower linguistic levels. At the level of the narrative, reference to the historical reality is not to be found. Since this kind of reference is lacking, a Universal History cannot, contra to the post-positivists’ convictions, be the thing historians are after. A Universal History requires the possibility of reference to reality, and this should be true for the whole narrative. Yet historical narratives as narrative lack this ability.
This leads to the insight that there is a sliding slope between linguistic things that (inter-subjectively) refer, and those that do not; yet the extremes of this slope are always both present in historical narratives. And any serious philosophical analysis should account for both these extremes.\(^\text{208}\)

If we now look at the situation in biology we see a striking similarity. For just like it is not possible to define the kinds of historical things that historical narratives are about, it is also not possible to give these historical kinds in case of biological explanations. The obvious candidates for the kinds of biological things that explanations are about are the (causal) processes that population geneticists formalize in their models. These models ought to reflect the way in which actual evolutionary processes occur. The idea is that giving an explanation about a certain biological development in the past, say explanation \(E\), is an explanation of a process that can be described as being an instance of the biological model \(M\). The issue of the \textit{qua}-problem that Devitt and Sterelny identified in respect to the causal theory of reference, can thus be resolved. \(M\) models the kind of processes that can be described so that \(E\) refers in the manner of a proper name.

Yet as we have seen with Van der Steen’s analysis, specifying the set of ceteris paribus clauses that must be met to say that an \(M\) actually happened is a necessary step for giving a sufficient explanation. And the only way to specify this is by giving a historical account. Giving a historical account thus means that, just like with the historian, the biologist has to specify all the reasons why she thinks this specific model applies in this specific situation. The biologists has to show why all things do, or do not remain equal in the case she tries to explain. Therefore, the description of the kind of thing the explanation is about collapses, just as within historical explanations, into the whole explanation. This renders the causal theory of reference as useless to evolutionary biology as it is to history. The consequence is that a Universal History of biology cannot exist either. Since narratives do not refer, speaking about the truth or falsity of a historical explanation as a whole is as meaningless in biology, as it is in history.

A biologist might reply that the discipline \textit{is} about truth and falsehood. The difference with the above, so will be said, is that with biological explanations a common agreement about what certain words mean, is, and will be reached. At a certain moment, so will be claimed, a narrative about a certain period will be accepted by almost the whole scientific community.

\(^{208}\) Therefore, reflecting on the debate between Ankersmit and Zammito, I think that the correct departure for finding the \textit{just milieu} between post-modern philosophy and historical practice must be the narrative philosophy of history.
and no new narratives, as *is* the case in history, will be considered. Alas, this is a too distorted view of the science of biology. Biologists do not come to agree on evolutionary narratives that easy.

This is, again, due to how biologists treat natural selection. There a few factors that are thought to act in a process of evolution. Fitness, the first factor, is already discussed above. Two other important factors are constraint and drift. Disentangling these latter factors from that of fitness has proven to be a difficult task to say the least.

First Constraint. Constraint is about the way in which older morphological and chemical structures constrain the further evolutionary development. An example is the way in which vertebrates consist of one torso and four limbs. This construction is due to the way a foetus grows, and cannot be easily altered afterwards. Natural selection cannot just change the number of limbs, for it is a basic structural feature. It has to work with the structure that is at hand. In other words: changing certain traits would require too much genetic change in too little time.

As has been argued, without some constraint, no evolution would be possible. Species would change into perfectly adapted organisms, each organism fitted exactly to its environment. It is clear that species do not change indiscriminately: the tree of life could only be constructed if there are homologous traits. A fork in a phylogenetic tree can only be recognized if there are both things that change, and things that remain the same.

Problems now arise when an observed fact can be explained both by adaptation and by constraint. Zebras, for example, have small ancestors. Answering the question why the zebra is larger than its ancestor can, as a thought experiment, be given in two manners. The first is a selection argument: being larger gave some ancestors of the current zebras a fitness advantage. They were able to eat more, or something like that. The second answer invokes constraint and selection: having longer legs gave some ancestors of the current zebras a fitness advantage. Because, for example, they had to cross fields with high grass. But since the leg length and the body size could be correlated, the increased body size could be the result of the selection of larger legs. In this second explanation, the constraint that the leg length and the body size are correlated made the zebras grow larger. So in this case, the

209 The basic structure of the body plan is considered to be coded by the so called *Hox* genes. These genes are critical components in the early development of fetuses, and control the basic layout of most multicellular creatures. For a (technical) overview of Hox genes see T.R Bürglin, "Homeodomain Proteins" in: R.A. Meyers ed., *Encyclopedia of Molecular Cell Biology and Molecular Medicine* (Weinheim, Wiley-VCH Verlag GmbH & Co., 2005) 179-222.


211 A homologous trait is a trait shared by two species due to a common ancestor.
question is partly answered with the selection for longer legs, and partly by the constraint of the body size. No we have two hypothetical answers to the question why zebras became larger. Which to choose?

The obvious answer is to go and look into the details, for it is an empirical matter: Whether the first explanation or the second explanation is true depends on what was the case during the development of zebras. Were there high grass fields; did longer legged zebras have an advantage over the shorter variants; are leg length and body size correlated? But also: is being larger an advantage? These questions are not always answerable, especially for those organisms that have been extinct for quite some time. But next to this problem of getting enough evidence there is another, more theoretical problem in that biologists interpret the concept of constraint in different ways.\(^{212}\)

There are two general interpretations. First there are the adaptationist minded biologists. They believe that constraints are due to the fact that non-adaptive forms are eliminated by natural selection. In a world were no selection would be present, the organisms would surely cross the borders laid down by what we now call constrains. In such a world, mutations would appear that created differences that would not be possible if selection was still active. In this view selection causes constrains to occur. On the other hand are those biologists who endorse the developmental program. These state that constrains operate on and during the development of biological systems. They believe that when selection would not be present, the organisms would still be enclosed within the boundaries of the constraints of development. According to them, constraint is a characteristic of biological systems, not something induced by natural selection. In this selectionless world, organisms would still chance little, for the way they are build prevents certain chances to occur. These more developmentally guided biologists certainly do not state that no selection is at work in – for how else did the constrains got into place? – but the constrains that are now in place cannot change easily. The developmental body-plan simply does not allow it.

I do not have the credentials nor the intention to solve this problem – for it is a biological issue. But if we look at the hypothetical case of the zebra, we see that the difference in meaning of constraint marks a difference in how the development of zebras should be viewed in the second scenario – the scenario that the zebra became bigger because having larger legs was an adaptation, and leg size is constrained by body size. Constraint is presented as a factor

\(^{212}\) I follow the analysis of Amundson in the following discussion on how to interpret constrain. Ron Amundson, "Two Concepts of Constraint: Adaptationism and the Challenge from Developmental Biology", *Philosophy of science* 61 (1994, Issue 4) 556-578.
that caused the growth of body size. Adaptationists would state that this means that zebras with long legs and small bodies had a low fitness value, and would all die before they could procreate. Only those zebras that also developed larger bodies would, in their view, survive. The developmental biologists would disagree with this view. They would say that the zebras with long legs were selected for, and developmental constraints about the relation between body size and the length of legs made their bodies big. Both views would conform with the available evidence, and both views can be endorsed as correct explanations. Even though this case about the zebra is (partly) fictional, the point is that the actual problem of what evolution is, and how it works, cannot be extracted from the facts alone, nor do the facts force biologists to agree upon a single explanation. Two different explanations of an evolutionary change in morphology apply to the same data.

This shows how things are not as harmonious in biology as would appear at first sight. Yet there are more issues. For the third component of evolutionary explanations, drift, also leads to its problems.

Drift represents the process of the fixation of random genetic change in a population (that results in phenotypic expression.) Especially in small populations, harmful, yet not fatal mutations can spread by means of shear chance. For example, while the first sibling is more fit than his (genetically induced) less fit brother or sister, a lightening strike might prematurely end the oldest life. This possibility is thought to be a specific characteristic of biological explanations, i.e. evolution is, according to most biologists, an inherently stochastic process. The concept of probability expresses this stochastic nature in the principle of natural selection, and drift is the numerical representation of this probabilistic element in the genetic population models.

The philosopher John Beatty has, however, shown how difficult it can be to disentangle drift from selection in evolutionary explanations.213 Beatty takes one of the most famous evolutionary explanations, the selection of the peppered moth to make his point. The peppered moth has two variants: the light coloured moth, and a dark variant. Kettlewell’s famous study shows that the dark variant become more common than the white one when soot that was caused by the burning of coal during the industrial revolution, made the trees these moths

reside on more dark. Because the black variants were better camouflaged, they were less predated, and fitter in the given environment.\textsuperscript{214} Suppose now, writes Beatty, that there is a forest of which 40 percent of the trees is light-coloured, and 60 percent dark coloured. The trees are evenly distributed, so that the dark moths are more fit. But in certain seasons it could be possible that by chance more dark coloured moths land on light trees. When this happens, the dark population will, notwithstanding their higher fitness, decrease in comparison to the white population. While this is unlikely, evolution does not rule it out. It is the result of drift. Up until now things are still clear. But, as Beatty asks, what if an exact equal number of white moths landed on dark trees? These are easy targets, and their predation is plain selection.

What now, Beatty asks, is the difference between the effects of black moths landing on white trees, and the effects of white moths landing on black trees? Both are demographically equal, yet we call the former drift, and the latter selection.

In case of the moths the answer is in the details of the design problem the moth faced: there are birds that prey on them. Since the black moths had better camouflage, they had a better survival rate. But in most ordinary biological research the details of the design problems are not known. Biologists often reason from demographic changes in populations to fitness differences. And this, as Beatty argues, cannot work. Arguing the other way around, looking at the demographic differences to fill in the effects of drift and selection, leads to an inability to specify which part is drift, and which selection.

This problematic nature of drift relates to the question about whether biologists come to agree on biological narratives or not in two ways. First, it shows that taking only the demographical data, as most biologists do, can lead to very different narratives about how and why certain evolutionary development took place. But second, even if the design problem is known, as is the case with the peppered moth, we actually bump into a whole range of design problems and selection processes that must be accounted for. The reason for this is that the peppered moth is not only prey of certain birds, it has to cope with all kinds of diseases and viruses; it has to compete with the other members of its respective sex for a mate; it must avoid getting caught behind a window; it must constantly search for food, etc. Each of these processes and challenges play part in the development of a species. And given the fact that the environment changes constantly, the specifics of these challenges are infinite. Yet in all these cases it must be determined whether it is selection or drift.

\textsuperscript{214} The famous study is Bernard Kettlewell, \textit{The evolution of melanism : the study of a recurring necessity; with special reference to industrial melanism in the Lepidoptera} (Oxford, 1973).
Natural selection is not the simple process as is often thought. Things like the above mentioned design problems of the peppered moth play a part in the dynamics of evolution. They determine the complex relation between constrain, selection, and drift that govern contemporary evolutionary explanations. This means that giving an account of how and why a development took place, say the decline (and rise after the reduction of coal usage) of the white peppered moth always involves making selections and abstractions of the available facts. And this selection process is always done in light of the result of this development, i.e. it is done with the hindsight of the historian.
5 Conclusion

This thesis defended the idea that biological explanations are narrative in nature, and that these narratives are not about getting at the truth. This is a controversial conclusion, for contrary to historians, biologists never explicitly denied the existence of a Universal History. Historians would agree with Mink that the ideas of Hegel, Marx, and Toynbee are behind us, but biologists will not be so willing in accepting a similar conclusion for the claims of Darwin, or the claims of the great synthesis of twentieth century biology. The problems about the inability to aggregate two narratives and to speak about the “truth of narratives” will, in their eyes, seem absurd conclusions that originate out of a radical scepticism. But, as we have seen with Baumgartner’s transcendental argument, this conclusion is not as bad as it may seem. For it is only through historical narratives that historical knowledge is possible at all. Similarly, it is only through biological narratives that we can know the biological past at all. Only by means of the biological narratives can we explain the diverse facts of the biological past.

All this leads to a shift in attention from questions about the truth of biological explanations to questions about the appropriateness of biological explanations. Philosophy of biology should not be about whether this or that biological system is the correct unit when explaining the past, as is done in the debates about the levels and units of selection. These kind of questions will be answered in and through the biological explanations of the working biologist. The philosophy of biology could be much more effective if it would investigate what “being appropriate” means in the biological domain, where the border between biological facts and biological narratives reside, and what the further consequences of the intrinsic historicity of the field of biology are. But all these challenges can best be faced with the tradition of the narrative philosophy of history in mind. For this tradition has many valuable insights to share that will help accept the thorough historical nature of evolutionary biology.
Bibliography

Auinger, Robert, Darwinizing culture: the status of memetics as a science (Oxford etc., 2000).
Baumgartner, Hans Michael, Kontinuität und Geschichte : zur Kritik und Metakritik der historischen Vernunft (Frankfurt am Main, 1972).


Ibidem, *This is biology : the science of the living world* (Cambridge, MA. etc., 1997).


Rosenberg, Alexander, Darwinian reductionism, or, How to stop worrying and love molecular biology (Chicago, Ill., etc., 2006).


Russell, Bertrand, The problems of philosophy (New York, 2004 (1912)).


Ibidem, "Two Outbreaks of Lawlessness in Recent Philosophy of Biology", *Philosophy of science* 64 (1997) S458-S467.


