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關於天擇本性的爭論

The Debate over the Nature of Selection

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摘要

近期關於天擇本性的爭論關注在兩個問題上：天擇是否是因果過程，以及天擇應如何刻畫。本論文檢視在此爭論中最具代表性的三個理論：Matthen 和 Ariew 的形式樣式觀，Bouchard 和 Rosenberg 依附於適存性概念的刻畫，以及 Millstein 不倚靠適存性概念的刻畫。Matthen 和 Ariew 主張一般所認為的作為因果過程的天擇並不存在，天擇其實是由特定一個數學定理所代表的形式樣式。Bouchard 和 Rosenberg 認為天擇是一類因果過程，其原因為適存度關係而結果是生殖成功度差異。Millstein 則認為天擇是一類因果過程，其原因是族群的性狀差異性而結果是生殖成功度差異。他並認為性狀差異性是一個族群層次的性質。這三個說法都有重大缺陷。其中，前兩個主張因為錯誤太過根本所以必須完全放棄。Millstein 的說法雖然概念上有混淆之處且形上學上也有瑕疵，但經過進一步釐清並重新詮釋之後可以引導出一個更令人滿意的對天擇的刻畫。此一刻畫是，之於不同可定性狀的天擇是不同類的涉及整個族群一整個世代的因果過程，每一個這樣的因果過程類是由複數個不同但相似的涉及生物一生的因果過程類所刻畫，而後面這些不同的因果過程類則是刻畫成同一可定性狀之下的不同類的確定性狀之對不同範圍的確定生殖成功度有因果貢獻。

本論文結構如下。第一章介紹這個爭論的背景，區分相關的問題與無關的議題，並指出一些基本假設以及本文的目標。第二章檢視天擇的形式樣式觀。本文認為作為形式樣式的天擇並非如 Matthen 和 Ariew 所稱是可多重實現的，而且此一意義下的天擇根本沒有解釋上的效用。進一步，本文反駁他們對於作為因果過程的天擇並不存在的論證，同時釐清並重建這個意義的天擇概念。第三章處理 Bouchard 和 Rosenberg 的看法。此處將指出，在適存性確實是生物的一個性質的假設下，適存性是一個二階功能性質，因此，說適存性對生殖成功度或其差異有因果貢獻會招致形上學上必然相依性問題以及原因互斥問題。對於幾個試圖解決前一個問題的方式以及對於原因互斥論證的反對此處亦將一一反駁。第四章處理 Millstein 的看法。本文澄清在他的說法裡，天擇其實並不是一類因果過程，而是一群不同類的因果過程。此處將詳細對比依附於適存性概念的刻畫以及獨立於適存性概念的刻畫，最終，適存性作為一個生物性質以及依附其上的對天擇的刻畫

都將被揚棄。然而，Millstein 所謂的族群層次性質本身也有原因互斥問題，本文亦將批評 Haug 對解決這個問題的嘗試。族群層次性質不具因果效力且其在存有論上是冗贅的將促使我們重新詮釋 Millstein 的看法。最終，我們可以得出一個更令人滿意的對天擇的刻畫，它不仰賴任何形上學上可疑或存有論上冗贅的設定，同時不會有任何語意上的扭曲或造成其他不必要的麻煩。第五章為本文結論。

關鍵詞：天擇、適存性、原因效力、多重可實現性、原因互斥論證

Abstract

Recent debate over the nature of selection centres upon the questions of whether selection is a type of causal process and how selection should be characterised. Three representative accounts of selection in this debate are critically examined in this thesis: Matthen and Ariew's formal-pattern account, Bouchard and Rosenberg's fitness-dependent characterisation of selection and Millstein's fitness-independent characterisation of selection. Matthen and Ariew contend that there is no selection as a causal process as normally conceived; instead, selection is a formal pattern characterised by a mathematical theorem. Bouchard and Rosenberg assert that selection is a type of causal process identified by the fitter-than relation being the cause of difference in reproductive success. Millstein characterises selection to be such a causal process that trait-variations as population-level properties are causally relevant to differences in reproductive success. All these accounts will be shown to be seriously defective. However, the former two are so fundamentally mistaken that they will be rejected outright. On the other hand, Millstein's account, while conceptually confused and metaphysically flawed as it stands, can be clarified and re-interpreted so that it can pave the way for a more satisfactory characterisation of selection. In this latter account, selections with respect to different trait-determinables are different types of population-wide, generation-long process each jointly identified by a plurality of distinct yet similar types of organismal-level generation-long process that are identified by different types of trait-determinates under a common trait-determinable being causally contributory to different ranges of determinate degrees of reproductive success.

The present thesis proceeds as follows. Chapter one introduces the background of the current debate, distinguishes what is at issue from what is not, and states some basic assumptions and the objective of the thesis. Chapter two examines the formal-pattern account. It is argued that selection as a formal pattern is, contrary to what Matthen and Ariew claim, not multiply realisable and has no explanatory utility at all. Meanwhile, their arguments against the ordinary talk of selection as a causal process will be refuted and the ordinary idea be clarified and re-established. Chapter three is devoted to Bouchard and Rosenberg's account. I'll show that, on the assumption that fitness is an organismal property, it is a second-order functional property, and therefore the assertion that fitness is causally responsible for reproductive success or the difference thereof suffers from the problem of metaphysically necessary dependency and the causal exclusion problem. Some specific attempts to solve the former problem and some general objections to the exclusion argument will be rejected. Chapter four explores Millstein’s fitness-independent characterisation. It will be revealed that selection in her account is actually a family of different types of process rather than a single type of process. This leads to a comparison between the fitness-dependent characterisation and the fitness-independent one and ultimately to the rejection of the posit of the property of fitness as well as the fitness-dependent characterisation. Yet, the posit of the so-called population-level properties also has the exclusion problem; Haug’s attempt to save their causal efficacy will be criticised and rejected. Their causal inefficacy and ontological redundancy prompts a re-interpretation of Millstein’s characterisation and eventually a more satisfactory alternative that does not rest upon any ontologically redundant or metaphysically suspicious posit and does not create any semantic twist or other unnecessary complications. Chapter five is the conclusion.

Keywords: natural selection, fitness, causal efficacy, multiple realisability, causal exclusion argument

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1. Introduction

It is widely acknowledged that natural selection is a cause of evolution. The most important cause for that matter. What is evolution? Population genetics formally defines evolution as the cross-generational change of allelic frequency of a population of organisms. This definition is straightforward and incurs no substantial controversy. By contrast, we don’t have an equally explicit and unproblematic definition of selection in evolutionary biology. So, what is selection? How should it be best characterised? This is where all sorts of disputes arise and is the topic of the present thesis.

1.1. Aspects of the Debate

The idea of natural selection was originally an analogy with that of artificial selection (Waters 2003). In artificial selection, the most prominent factor regarding the evolutionary change of domestic groups of organisms is human’s act of selection (selective breeding). In the wild, however, there is an absence of intentional act of selection. Accordingly, the idea of natural selection, in its nascent stage, is understood in a metaphoric way, as if it were Nature that did the selection. Natural selection is thus selection by Nature or natural environments. This is the idea of natural selection as it was explicitly presented in the early versions of the *Origin* (Sloan 2010).

Personified natural environments and the metaphorical sense of selection soon raised criticisms. Among them are the lack of clarity of the whole notion and the excessive non-naturalistic, intentional/agentive talks. In response, Darwin expressly adopted Huxley’s now well-known cliché that selection is “the survival of the fittest”. This way of understanding natural selection erases those charges. But it also definitely makes natural selection a sufficiently distinct notion from artificial selection. Natural selection is no longer metaphorically conceived as an act, performable only by intelligent beings. Rather, when using the notion of natural selection to explain evolution, the most prominent explanatory factor becomes some attributes on the part of organisms (here fitness, presumably). We have thus a completely naturalised notion of natural selection, as one may say, and therefore can do away with the initial, heuristic yet troublesome analogy.

Nonetheless, new problems arise. Since explanation (of evolution or, more narrowly, of reproductive success) by selection boils down to explanation primarily by some attributes on the part of organisms, it is undoubtedly a question of paramount importance as to what exactly these attributes are. Phrases such as “(being) fitter than” and “(being) fit (in an environment)”, which Darwin made use of when illustrating selection, have been taken to express, or to be based upon, the property that accounts for selection. This (supposed) property is known as “fitness”. But, then, what is fitness? Is it a causal property or not? Or perhaps terms like “fitness” do not designate any property at all? Provided that selection has to be characterised in terms of fitness, the nature of selection obviously largely hinges upon the nature of fitness, and no wonder why questions like these occupy a locus of the current debate. Moreover, any account of fitness cannot fail to address the notorious tautology problem: What does, say, “being fitter than” mean? If it means “having better survival/reproductive result/success”, then the particular (albeit loose) characterisation of selection that fitter organisms have better survival/reproductive success (than less fit ones) is a patent tautology. This creates a serious problem for those who think that the notion of selection has empirical content and the notion of fitness is essential for characterising selection. Accordingly, the debate over the nature of selection also concerns how the tautology problem is supposed to be solved and, more fundamentally, whether it is indeed avoidable.

Another part of the debate has to do with whether selection is a force. Selection, along with migration and so on, has often been said to be an "evolutionary force". This was a mere metaphor or a casual way of speaking in the first place. It was not until Sober (1984) set up the analogy with Newtonian force that the force talk took a clearer shape and got philosophically anchored. The force analogy attempts to establish the thesis that population genetics, the theory of evolution, is a theory of force, by showing that it shares the same structural elements with Newtonian mechanics. However, are the two theories substantially similar in formal terms, even if we acknowledge that they belong to different domains and most likely do not completely resemble each other? Further, are there any reasons beyond formal similarity, or lack thereof, to Newtonian theory of force, in view of which selection and so on cannot literally be forces? And if selection is not a force, then what is it? All these also fall within the scope of an account of selection.

Now, mainstream philosophical opinions, at least before the turn of this century, are that fitness is a causal property and that selection is a force. However, from a decade ago, these views have been vigorously challenged by several philosophers, notably Matthen and Ariew (2002; 2005; 2009) and Walsh, Lewens and Ariew (2002). These authors contend that fitness is reproductive growth rate and hence is a statistical construct rather than a causal property. And they argue that selection is not a force, partly by disputing the force analogy. On these bases they go further to claim that there is no selection as a cause of evolution. Instead, selection is something else: It is the "formal pattern" between time rates of changes in proportions of different types of components of an ensemble and those proportions over time, e.g. between fitnesses or reproductive growth rates of genotypes or trait-types in a population and their representations over generations. As a formal pattern, selection is "not even a biological phenomenon as such" (Matthen and Ariew 2009: 222) and is completely domain-unspecific.

This characterisation of selection strikes many philosophers and biologists as unacceptable, for selection is regularly understood exactly as a cause of evolution. Saying that selection is something else is tantamount to elimination of selection in this normal sense. As Matthen and Ariew and Walsh *et al.* found this view upon both a criticism of the force analogy and a non-causal account of fitness, to defend against it possible strategy to take is: (1) either insist that selection is indeed a force or, while conceding that selection is not a force, maintain that it remains nonetheless a cause of evolution, *and* (2) either hold that fitness is really a causal property or decouple the idea of fitness from the notion of selection. Regarding (1), most defenders of the traditional view nowadays no longer stick to the force analogy. They tend to speak of selection as a causal process, with the contents of both "selection" and a "causal process" varying with one's choice on (2). If an account of selection as a causal process is built upon a theory of fitness, the question of what fitness is again comes to the foreground. Alternatively, one may try to give a non-conventional characterisation of selection that does not require the notion of fitness. This line of thought brings up a whole new set of questions to consider: Is fitness essential for characterising selection? If not, how does a fitness-independent account of selection differ from a fitness-dependent one? How should selection be characterised without using the notion of fitness, then, and without engendering other problems? And does such a characterisation successfully ground the view that selection is a causal process? All of these fall under the rubric of the debate over the nature of selection.

1.2. The Focus, the Assumptions and the Objective

The present thesis deals only with the metaphysical question of what selection is. Other controversies concerning selection are beyond our scope, which include, notably, the plausibility of selectionism, the distinction between selection and drift, and the unit-of-selection problem. The first is fundamentally an empirical issue: Taken either as an empirical thesis or as a methodological proposal, whether selectionism is plausible is determined by whether selection is (mainly) responsible for many enough evolutionary changes, which is certainly an empirical matter. The second is chiefly about drift, which will not receive explicit treatment here since we focus only upon selection. However, implicit in our discussion of selection is a minimum notion of drift (the so-called “product view”), where drift is nothing but deviation to the most probable outcome (*viz.* sampling error) of selection (when understood in terms of sampling). This is largely for the sake of argument; if one embraces any alternative notion of drift, one just substitutes “non-typical result” or “less probable outcome” for “drift” in our sense and reserves “drift” for whatever is intended. That would not affect our discussion of selection in any important aspect.

The unit-of-selection debate, too, is orthogonal to the present topic, although this may not be immediately obvious. The former, given a fixed content of the term “unit of selection” (say, the type of “interactor” involved in selection) and the relevant set of empirical facts about selection and evolution, concerns questions such as: Are all cases of selection accountable in terms of gene/allele as the only unit of selection? Gene/allele aside, is there unit of selection in addition to organism? Specifically, are kin group and even species also units of selection? And are issues like these purely a matter of explanation/description, or they also have certain metaphysical or ontological basis? By contrast, the current debate addresses what selection is *irrespective of* what the unit(s) of selection is(/are). *If* gene/allele is a/the unit of selection, then we ask: Is selection a force upon genes/alleles, a process involving them or a formal pattern between the growth rates of different genes/alleles (types) in a gene pool and their representations over generations? In that case, presumably we can talk about the fitness of genes/alleles. The question naturally arises as to what such fitness is: a causal property of genes/alleles, their growth rates or perhaps something else? Also, is the notion of fitness of genes/alleles essential for characterising selection (when gene/allele is the unit of selection)? If organism is a/the unit of selection, then the organismal versions of all these questions mentioned at the end of the last section follow. And equivalent questions in case kin group and even species are also units of selection.

Nevertheless, our discussion will be set within the framework of organism being the unit of selection. Although any alternative can also do the job, this one is somewhat preferable because its adoption should introduce the least amount of unnecessary complications: For most, if not all, leading philosophers in this debate frame their arguments in just the same way. Besides, many relevant notions (especially fitness) are originated in such a framework and apply most naturally to organisms. True, these alone do not justify the thesis that organism is the (sole) unit of selection. At the very least, one may simply take it as an assumption for the sake of argument.

There are other noteworthy assumptions and unchallenged ideas to be mentioned at the very beginning. We'll accept the current definition of evolution, even though there is no reason to insist upon its being couched in terms of generational time interval (Sober 1984) or even in terms of allelic frequency distribution. For this reason we always assume discrete populations. Biological concepts are taken in their under-elaborated versions whenever that does not affect our discussion. For example, we don't distinguish between survival fitness and reproductive fitness; by "fitness" we mean exclusively the latter since only it has to do with cross-generational changes. Moreover, we leave unchallenged the evolutionist tenet of “population thinking”. The basic idea of population thinking is that evolution, and hence selection, is about a population of organisms taken as a whole, as opposed to organisms taken individually. It is manifested by expressions such as “a population underwent selection” and “selection acts upon a population”. Implicated in such an idea is that the notion of selection applies only to a population. This, however, has been challenged by Bouchard (2011) on empirical grounds: He cites, for instance, the case of a single genet among his counterexamples, where the notion of a population is not applicable yet it is reasonable to apply the notions of selection and evolution to them. We leave out such cases and agree with many philosophers and most biologists that selection involves a whole population. How this is to be taken care of in the metaphysical or ontological dimension is another matter, though.

Also left unchallenged is the distinctively biologist’s causal talk, as in the statement “selection causes evolution”. This is problematic from the view of standard metaphysics, for selection and evolution (instance) are not events in any obvious sense. One might simply reject that selection and evolution are causally related for that reason, but chances are that the seemingly metaphysically illegitimate talk is a loose and abbreviated way of expressing something in the neighbourhood of causation if not exactly causation proper. Clarification of such casual causal talks require, as may have been expected, a charitable interpretation of them and a somewhat more liberal notion of event/process. We’ll attempt this in order to preserve the insights of Darwinism, so long as no major philosophical principle is violated. An understanding of selection may (and actually will) thus result which not only can do justice to the scientific theory and practise but is ultimately accommodable in standard metaphysics. So our position is always: To make sense of distinctively biological as well as ordinary ways of talking as far as possible, *on condition that* they can sit well with all relevant metaphysical principles. In cases where the two desiderata are incompatible after all, metaphysical considerations nonetheless take precedence.

We'll soon accept that selection is a process (more refinements as we proceed). Although we will not precisify the operative, relaxed conception of a process, it at least allows a process to span a long time, to consist of or to involve multiple objects and properties (instances), to be composed of other events/processes each of which consists of a subset of the objects and properties constituting it (and may last shorter than it), and to have no well-defined intermediate stages or events. The distinction between an event, a process (a series of events) and an interaction (an event or a process) is thus intentionally blurred, and in our discussion the corresponding terms are usually interchangeable (for "sub-selection" occurrences alone; we won't call selection an event or an interaction). I believe that something like this is tacitly assumed by those philosophers who advocate that selection is a causal process. Now, what is it that makes a process a *causal* process? In our context, it hinges upon some of the properties that constitute selection being *causal properties*, specifically properties (the instantiations of) which causally contribute to certain reproductive results. This, too, is a framework implicated in the current debate. Again we'll follow it, as it is also plausible by itself. Then, we'll take accounts of selection which support that selection is a causal process to be differing primarily in respect of the sort of (supposedly) causal properties as well as objects that are constitutive of selection. Accordingly, our main concerns are twofold: Are those supposedly causal properties genuine causal properties (or really properties at all)? And more fundamentally, which sort of objects and properties best identify selection as a process, causal or otherwise?

Hence, our goal is towards a satisfactory characterisation of selection. We approach it by critically examining three major accounts of selection. In the next chapter we first evaluate the radical view, as propounded mainly by Matthen and Ariew (2002; 2005; 2009), that there is no selection as a cause of evolution but only selection as a generic formal pattern. While their arguments against the force interpretation are persuasive, their own conception of selection is suspicious and their exposition of that is metaphysically problematic. Besides, they don't quite rule out the possibility of making positive sense of the ordinary idea that selection is a process and is a cause of evolution. At this stage, as a first approximation, selection is considered a type of process involving organisms of a population within a generation, such that their differential reproductive results are in accordance with their fitness differences.

Chapter three is a criticism of the view that selection is a causal process (type) identified by the causal relation between fitness and reproductive results. This part is directed at Bouchard and Rosenberg (2004), Rosenberg and Bouchard (2005) and the propensity interpretation of fitness as well. I'll argue that, if "fitness" expresses a property at all, that property is a *second-order functional* property and therefore cannot be a causal property due to the *problem of metaphysically necessary dependency* and the *exclusion problem*. Thus no fitness-dependent characterisation of selection is compatible with the thought that selection is a causal process-type.

In chapter four we explore the fitness-independent characterisation of selection put forward by Millstein (2006). While this position is more plausible than the preceding two, the original characterisation as it stands is metaphysically confused and calls for clarification and further revisions. I'm going to elaborate upon it by first making the distinction between selection (*simpliciter*) as a second-order process-type and selections with respect to particular traits as first-order process-types. This contrast also helps understand the fundamental differences between fitness-dependent and fitness-independent characterisations of selection and why the latter is more appropriate. Then I shall point out that Millstein's taking physical differences between organisms, or variations in their traits, as the so-called "population-level properties" not only are causally inefficacious but may also be ontologically redundant. This suggests us to take a step back: to hold that the only properties on the part of organisms that are constitutive of selections with respect to particular traits are their *respective* traits. At the end I'll give a sketch of a more conservative yet metaphysically more tenable account of selection with respect to particular trait. This is followed by the conclusion in the final chapter.

2. Problems of the Formal-Pattern Account of Selection and the Possibility of a Process Characterisation

The current debate is initiated by Matthen and Ariew (2002) and Walsh, Lewens and Ariew (2002). They refute the force view reinforced by Sober and contend that there is no population-level cause of evolution. Selection is then re-identified to be a formal pattern. It is further said to be multiply realised and realised even in non-biological substrates. Is this correct? And does their criticism of the force analogy preclude the possibility of a process view, which preserves the biological insights as well as makes better sense of the ordinary process-talk? This chapter aims to show that the answers are no and no.

2.1. From the Force Analogy to the Formal-Pattern Account

Sober (1984) distinguishes four structural similarities between the theory of evolution and the Newtonian theory of force: that the former features some laws or generalisations formally comparable to Newton’s principle of inertia (a zero-force law), to Newton’s second law of motion (a consequence law), to (classical) force laws like the law of gravitation (a source law) and to a principle of force combination. For that very reason he asserts that the theory of evolution is a theory of force. Further, the terms in the theory of evolution that play the similar role to the force term in the Newtonian theory are terms representing the “strength” of migration, mutation and selection. So, by analogy, migration, mutation and selection are forces with respect to evolutionary change. This is the gist of the Soberian force analogy. It obviously rests upon two things: The two theories are indeed similar and their similarities are sufficient for migration, mutation and selection to be forces. Most philosophers dissatisfied with it put into doubt some claimed similarities or other between the two theories. For example, Brandon (2006) argues that drift is not a force by demonstrating, amongst other things, that the supposed population-genetic zero-force law (i.e. the Hardy-Weinberg principle) is not a law and there is no zero-force law in the theory of evolution. Attacks by the proponents of the formal-pattern account, on the other hand, are directed mostly towards the (alleged) principle of force combination and the (alleged) consequence laws in population genetics. Matthen and Ariew (2002) contend that there is no principle of force combination in the theory of evolution and those population-genetic counterparts of the Newtonian consequence law are not genuine laws. We begin our discussion by examining their counter-arguments.

Matthen and Ariew hold that there is nothing in population genetics like the principle of force combination. Their main reason is that component selective factors do not combine in a way component forces do. Forces always combine linearly (as well as vectorially). The joint effect of two or more forces upon an object (at a given time and position) is the (vectorial) sum of the individual effect of each of those forces (were they present individually). This is an example of what is known as the superposition principle in physics. By contrast, selective factors do not obey the superposition principle. In statistical terms, this is because causal factors usually have “interactions”. Since forces cannot interact in this sense, selective factors and their resultant, i.e. selection, are not forces.

One may think that the above conclusion follows from the requirement that forces be superposing independently. This is not correct. It stands still even if we just demand that individual forces be combined in a *universal* way, independently or otherwise. And it is plausible to think that there is no universal principle for combining selective factors, as there is no reason to believe that all selective factors interact with each other in the same way. Thus, the attempt by Stephens (2004) to save the force analogy by stipulating that *non*-Newtonian forces need not combine additively is in vain. This move is not only *ad hoc*, but misses the real point of the argument. One may further drop the requirement of a universal principle of force combination, but then how is the notion of a force supposed to be distinguished from that of a cause? Such a generic, non-physical notion of force is too thin to be significant. It appears that selective factors are called forces merely because they can be conceived as if they pushed, pulled and balanced out. No causal factors cannot be so conceived, however. And we just have to admit that the force talk is, at bottom, a matter of metaphoric use of words.

So, we can accept that this argument successfully establishes that evolutionary factors, or at least selection and selective factors, are not forces. At the very least, the notion of force is superfluous in the theory of evolution. Yet it leaves open the question whether selection is a cause of evolution. This question is addressed, partly and partially, by the argument against there being population-genetic consequence laws. Both Sober and Matthen and Ariew take as relevant in this regard those population-genetic equations between allelic frequencies at the next generation and those at the current generation weighted by coefficients representing the strengths of migration, mutation and selection. For Sober, these are the consequence laws in the theory of evolution. For Matthen and Ariew, however, they are *mathematical theorems* and *ipso facto* are not (empirical) laws. And selection is said to be characterised by Li’s growth-rate theorem, which is a population-genetic theorem of that ilk. It is, therefore, a *formal pattern*, and a formal pattern is not among the right sorts of things that can be causes (e.g. events and property-tokens). Hence, selection is not a cause of evolution. Now, how plausible is this second argument?

I agree that, like Li's growth-rate theorem, the population-genetic equations containing *only* frequency variables and coefficients representing the strengths of selection are theorems. They key, of course, lines in the semantic content of those coefficients. They are exactly what are routinely called "fitness coefficients/parameters" or simply "fitnesses" in population genetics. By both Matthen and Ariew's interpretation and the standard population-genetic definition (e.g. Futuyma 2009: 306), fitness is basically the cross-generational time rate of change (owing to reproduction) of the frequency or proportion of a (sub-)type of organisms in a population. Population-genetic equations and theorems of the aforementioned family follow solely from the meanings of "fitness", "(number) frequency" and so on, as well as from the syntax of relevant mathematical operations. They are *a priori* truths. Since they lack empirical contents, they are not empirical laws. So far, so good.

Yet they go on to claim that selection is characterised by such a theorem. This is where things start going weird. Li's theorem, which is a simplified version of Fisher's fundamental theorem of natural selection, states that "in a subdivided population, the rate of change in the overall growth rate is proportional to the variance in growth rates" (Matthen and Ariew 2002: 72). It surely states a formal pattern. It's no doubt a truth out of verbal necessity and hence not a law. It definitely cannot be a cause. But, does it represent the notion of selection?

It is the predicament associated with the current debate that there is in fact a plethora of notions of selection among philosophers. When different authors disagree upon the nature of selection, it is likely that they are actually talking about the nature of different things. So, a justification of the adequacy or advantage of a proposed conceptual content of "selection" is no less important than the correct metaphysical categorisation of what a given notion of selection is about. An account of selection as radical as the formal-pattern one, in contrast to the more conventional ones in which selection is a cause of evolution or a causal process, requires extra efforts to justify. Matthen and Ariew are certainly aware of this. They attempt to justify it along three lines. These are outlined below in the order of increasing importance.

The first has its roots in the view that population genetics is *the* theory of natural selection. Population genetics is concerned with mathematically representing all sorts of (biological) evolutionary phenomena and exploring possible as well as ruling out impossible evolutionary consequences with the aid of involved mathematical/statistical techniques. It is held to be different from, albeit complementary to, field/lab studies of evolution: It is said to give explanations of evolution in terms of fitness whereas the latter provide explanations of evolution in terms of traits and environmental conditions. In other words, it yields “explanation *by* fitness” but doesn’t offer “explanation *of* fitness” (Byerly and Michod 1991a); that is, it doesn’t address why a given type of organisms of a certain population in a given environment has the fitness (i.e. reproductive growth rate) it does. Since population genetics is the theory of selection, what it says most generally about selection (i.e. Li’s theorem or Fisher’s theorem, together with the equations containing only fitness parameters and frequency variables), and nothing else, are about selection proper.

The second centres upon the *universal applicability* of the notion of selection. The fact that the general statements about selection in population genetics are mathematical theorems, and the interpretation that “fitness” means, and can only mean, “reproductive growth rate” in population genetics, are two sides of the same coin. And both of these are further connected to the idea that the notion of selection applies to populations of all sorts of organisms. If “fitness” meant anything else, the notion of selection would not be applicable to all types of organisms across the board. Nevertheless, Matthen and Ariew do recognise an alternative notion of fitness which they call “vernacular fitness”. The latter is precisely the Darwinian notion of fitness which appears in the cliché “the survival of the fittest”. They accept that it expresses an organismal property that is tightly connected to traits, and Walsh, Lewens and Ariew (2002) even conceded that that property is a causal property in regard to an individual organism’s reproductive success. Yet, they all insist, the Darwinian notion of fitness does not enter into the population-genetic theory of evolution. Consequently, it is not part of the notion of selection.

Now, Matthen and Ariew carry further the generality of selection, for the growth-rate theorem applies not only to populations of organisms. It applies to *any* sub-typed ensemble regardless of what sort of things/events constitutes it or whether, and how tightly, its constituents are spatially or causally bound together. Also, the constituents of an ensemble need not literally reproduce; all that matters is that the number proportions of the different types of its constituents can change over time for whatever reason. Thus, what enters into the growth-rate theorem is neither the Darwinian fitness nor the population-genetic fitness; it is the more general notion of “time rate of change in number proportion”, a.k.a. “growth rate”. This generic notion as well as the growth-rate theorem itself is required to characterise selection proper, because “[s]election also occurs in nonbiological realms: in the economic domain, for example, as well as in “clonal selection” in the mammalian immune system, in classical conditioning, and, according to some, in the propagation of theories and other cultural artifacts” (Matthen and Ariew 2002: 71). For that matter, selection is said to have “multirealizability” and is “realized in many substrates”. It is itself “not even a biological phenomenon as such” (Matthen and Ariew 2009: 222). It is “wholly abstract, then, but its realizations are shaped by concrete relations—these concrete relations are what determine the value of the abstract parameters of natural selection" (*loc. cit.*). Matthen and Ariew consider this characterisation of selection better than its competitors for the reason that it has universal applicability and covers whatever is called selection, especially those outside the domain of biological evolution.

The third line of justification is their direct rebuttal of the traditional claim that selection is a cause of evolution. At the heart of this rebuttal is the "constitution thesis": "Ensemble-level selection events are constituted without remainder by individual-level selection events; consequently, the causes of ELSEs are the causes of the ILSEs that constitute them. Thus, ELSEs are wholly caused by [the causes of] ILSEs." (Matthen 2010: 2) In the domain of biological evolution, an ELSE is just an evolutionary change, whereas an ILSE is the birth or death of an individual organism. This thesis is plausible. Yet it entails that there is no population-level cause of evolution that is to be identified with selection. Colloquially, one may say that the causes of births and deaths of individual organisms, e.g. predations and matings, are causes of evolution. But this is not metaphysically adequate. The correct way is to say that they (severally) cause those births and deaths, and the latter (collectively) constitute an evolutionary change. That is, there is no cause of evolution: It is an *epiphenomenon*. There are certainly causal processes that are explanatorily relevant to evolution, but they are to be identified only with those processes involving individual organisms.

But what about statements such as "variation in wing darkness caused the evolution of the moth population"? While Matthen and Ariew (2009) admit that it is a statement about a causal relation in the sense of cause as probability raiser, they refuse to acknowledge that it means that selection caused the evolution of the moth population, for "variation in wing darkness" doesn't mean the same thing as "selection" does. According to them, saying that selection is a cause of evolution is reifying selection. It amounts to positing a *tertium quid* called “selection” over and above, say, variation in wing darkness and evolutionary change, such that it (arises from the former variation and) acts upon the moth population to produce the latter change. Evidently this *tertium quid* is explanatorily redundant. Therefore, they claim, from the ontological consideration there is no selection as a cause of evolution.

Thus are their justifications of the formal-pattern account of selection. Even though there are many insights in them (which will be picked up in due courses), they have serious problems that they fail to justify it after all. The first line, in itself, manifests an unduly preoccupation with mathematical representations and statistical reasonings to the exclusion of empirical studies. Moreover, there are population-genetic equations that are by no means mathematical theorems but nevertheless are thought to model selection. There are no convincing reasons why they are not about selection. So, the first line of justification is very flimsy by itself. It has to be backed up by the other two in order to maintain that those fitness-free equations are not about selection proper and that they cannot be taken to mean that selection causes evolution. This suggests that what really do the supposed justificatory work are the concern about generality and the argument against the existence of selection as a cause of evolution. These latter two, on the contrary, are more substantial and demand more elaborated treatments to show that they are mistaken. To these we now turn.

2.2. Objections to the Formal-Pattern Account of Selection

This section attempts to refute the formal-pattern account of selection by arguing against the second and positive line of its justification outlined above. We specifically focus upon the metaphysical character of selection as a formal-pattern and the explanatory utility of such a notion. Our starting question is: Is selection as a formal pattern multiply "realisable", as declared by Matthen and Ariew? As we shall see immediately, saying so betrays a confused metaphysics, which eventually can be traced back to what is wrong with the formal-pattern account.

To answer the question, a clarification of the notion of realisation is required. As a philosophical jargon, the term "realisation" makes its debut along with Putnam's introduction of machine functionalism into philosophy of mind, where it is used to convey the idea that "a physical computing machine "makes real" or "brings into concrete reality" an abstractly characterized Turing machine, a mathematical entity" (Kim 2010: 103). In its current use, however, it is more closely associated with role functionalism. The latter dictates that mental properties are functional/causal "roles", i.e. they are functionally/causally identified or defined. In the physicalist framework, a functionalist mental property, being a functional/causal role, is said to be played, occupied or "realised" by some physical property or other. And the realised (functional) property is also said to be a second-order (functional) property, i.e. the property of having some (first-order) property or other that satisfies a given functional role. Thus, realisation as it is now understood is a *relation between properties*, and the standard talk of realisation is tied to the *second-order view about functional properties* (*op. cit.*).

Given this standard notion of realisation, selection as a formal pattern is *not* multiply "realisable". For what is realisable is a property, not a pattern. Moreover, realisers have to be properties too. But Matthen and Ariew seem to take the category of objects or just things to be the realiser in this context, as they speak of selection as being realised in many "substrates". So, saying that a formal pattern is (multiply) realisable in or by (different types of) objects or things is making category mistakes and is misusing the notion of realisation, at least as it is currently understood.

Then, can we say that selection as a formal pattern is realisable in different things in the same sense as “an abstractly characterized Turing machine, a mathematical entity” is realisable by different “physical computing machine”? Not really. First of all, in the latter case realisers are concrete things, i.e. physical objects. Contrarily, the supposed realisers in the current case include “things” like one’s account balance, ideas, theories and cultural practises. These “things” are certainly not physical objects. In order for the original philosophical notion of realisation to be correctly employed, at least the possible realisers must be confined to the physical realm. Suppose we do so. Then there are further problems concerning the realised. In the case of machine functionalism, the realised is an entity. This can be understood in two ways: First, it is an abstract entity, i.e. a universal, and then realisation is nothing but instantiation in a universal ontology. Second, it is just a property, say, being a mental thing, for which that universal is posited, and therefore realisation is merely the ontologically noncommittal type-token relation. We can simply take up the second understanding because the choice of ontology is a topic independent of the talk of realisation. Now, does realisation taken as the type-token relation apply to our case, since the realised is a formal pattern of change rather than a property? Provided that some modifications are made, yes, it does. The talk of type and token/instance isn’t restricted to objects; it applies to changes, i.e. occurrences, as well. If a type is a pattern of change, its instances are particular changes exhibiting that pattern. Consequently, the instances or “realisers” in our case are not the particular physical objects involved in or partially constituting the particular changes described by the said mathematical theorem; instead, they should be the particular occurrences themselves. So, saying that a formal pattern of change is multiply realisable is shorthand for saying that it is a type of change of which many particular changes are instances and its different instances can involve objects of different physical types, *if realisation is just the type-token relation*.

But this isn't the whole picture. Even if realisation in the context of machine functionalism is taken basically as the type-token relation, it is a special one: It applies to all and only those cases where the type is a functional one, or otherwise the talk of "multiply physical" realisation loses all its significance. This is presumably a reason why realisation is now understood as a relation between a functional property and a physical property. The key question to ask in our case is thus: Is a formal pattern of change a functional type? One might think that it is, for its different instances do involve physical objects of different types, and, like an abstractly or mathematically characterised Turing machine, it is "wholly abstract" and is characterised by a particular mathematical theorem. The truth is that it is *not*, however. A functional specification certainly abstracts away from all sorts of physical details: It undoubtedly contains terms that do not designate physical properties or state-types, and functional patterns of changes/transitions between property- or state-instances surely can be couched in terms of syntactic or mathematical operations. But not all formal patterns of changes so characterised in are functional patterns. It depends upon whether the characterising operations are over terms that designate functional properties or state-types. If not, the formal patterns are not functional types; they have instances or instantiations (in the ontologically noncommittal sense) but not realisations. As shown below, the terms in the mathematical theorems that Matthen and Ariew think represent selection proper do not designate functional properties and thus the formal patterns characterised by those theorems are not functional patterns.

The terms of the growth-rate theorem are mathematical functions of number growth rates. Also, the terms used in the basic selection models in population genetics are number growth rates (of genotypes) and number proportions (of allele types). Number proportions are obviously quotients of number counts, and number growth rates are essentially differences of number counts (since the only meaningful time interval is one time-unit). Thus, underlying the mathematical theorems that characterise selection construed as a formal pattern of change is the sole concept of number count. It is not in any intuitive sense a physical concept. Yet it is not a functional concept either: It is not a concept defined as a role in a functional specification, and it is not such a concept that all the objects belonging to its extension have a certain common function or common set of causal powers. Objects' belonging to the extension of "(having) number count of one" are so not in virtue of their falling under a common functional description; they are so just in virtue of their being each individual objects under whatever principle of individuation. And a collection of 1,001 objects' belonging to the extension of "number count of 1,001" is so just in virtue of its being a collection of 1,001 individual objects. In fact, the concept of number count applies not only to every object and every collection of objects. It applies to occurrences as well (which Matthen and Ariew tacitly accept, as they agree that selection also occurs in conditioning, i.e. in sets of behaviour-tokens), since events and processes are themselves also countable. All the more, it applies to non-physical "things" such as ideas and theories. This strongly suggests that the concept of number count doesn't even pick out a type/kind; that is, it doesn't designate any property at all. If so, it should remain a mere concept/predicate. Accordingly, there is no multiple realisability of a functional property named "number count". If anything, there is only the multiple, or literally universal, applicability of the concept of number count.

In so far as the concept of number count does not designate a property, all concepts mathematically derived from it alone do not designate properties either. These include those of number (count) proportion and number (count) difference/change, i.e. growth rate, in our case. As a result, any mathematical equation wholly composed of such terms, which in fact states a conceptual relation between themselves, does not express a pattern of change between property-instances when it is used to describe particular occurrences, phenomena or histories. It has multiple or even universal applicability just as are all its constituent, number-count-defined concepts. But again, there is no multiple realisability of a pattern of change in the sense of a functionally specified occurrence-type having as its instances particular occurrences that are instances (also) of different physically characterised occurrence-types. For what are describable by such a theorem do not even collectively constitute a type of occurrence, let alone a functionally specified one. Therefore, selection as construed in the formal-pattern account is not multiply "realisable".

This antithesis is not going to be defeated in the following ways. First, one may try to limit the scope of application of such a theorem so that what are describable by it do collectively constitute an occurrence-type. The problem is: how? Trivial cases, i.e. single-member collections, collections of a plurality of exactly similar things and, generally, no changes/differences in number proportions over time, can be omitted for sure. Differences in number proportions of two unrelated collections may be excluded too. But any constraint beyond these is *ad hoc*. There is nothing inherent in the concepts of collection, subclass (within a collection), number proportion and change/difference in number proportion or in the growth-rate theorem itself and like equations that confines any such theorem to any specific type of things, collections, partitions or occurrences. Moreover, even if, by stipulation, it is restricted to populations of organisms and trait-partitions and the concept of growth rate restricted to reproductive growth rates of trait-subclasses, what are describable by it still do *not* collectively constitute an occurrence-type. For traits are not a single property, and by extension occurrences characterised partially in terms of number proportions and reproductive growth rates of trait-subclasses of a population of organisms do not form a single type. It follows that even limiting *ad hoc* the range of application of the growth-rate theorem to the domain of biological evolution cannot make selection as construed in the formal-pattern account something that can have realisations.

Alternatively, one may be willing to recognise “arithmetic types/kinds” and hence arithmetic or formal properties. In that case, number counts and number proportions are formal properties of object-collections (suppose we’re talking only about objects and also ignore trivial cases). Then, occurrences describable by an equation such as the growth-rate theorem may be said to constitute a formally characterised occurrence-type, since any of the terms composing such an equation either designates a formal property or is based upon some such properties. But do these permit one to say that selection construed as a formal pattern of change, now a formally characterised occurrence-type, is multiply realisable by different physically characterised occurrence-types? Absolutely not, because formal properties and occurrence-types, while being themselves types, are not functional types, and *ipso facto* are not realisable. And, given that the concepts of number count, number proportion and change in number proportion are universally applicable, any of the supposed formal properties and occurrence-types they pick out is instantiable concurrently with *any* single physical property or occurrence-type (and even with any set of physical properties or occurrence-types when the object-collections are arbitrary or inhomogeneous ones). This plainly renders pointless all the talk of realisation (and supervenience as well) in the case of such formal types.

Lastly, one may think of growth rate as a property of an object-collection that “causes” the change in its number count/proportion in a certain way (in unit time), or a disposition/propensity of it to change in number count/proportion in a certain way. Such a property, disposition or propensity can indeed be considered functionally specified and thus can be said to have physical realisers, which are certain physical properties of the object-collection or of the objects of the collection. Then selection understood as an occurrence-type partially characterised by such a property may be said to have physical realisations. This line of thought is very akin to Bouchard and Rosenberg's view about selection. However, if one thinks about growth rate in this way, one is not thinking about growth rate as such and is really not talking about Matthen and Ariew's account of selection. Growth rate is about the change of number count/proportion; it is not about any cause of, or causal property in relation to, that change. Selection and the properties characterising it may be said to have multiple realisability in a different account of selection. But that is totally another matter.

So, Matthen and Ariew confuse the universal applicability of a mathematical theorem with the multiple realisability of a functionally specified type. The correct understanding of the metaphysical character of selection as they construe it, however, strongly suggests that the sort of universal generality with which they identify selection has little to no explanatory utility. It's difficult to imagine an occasion in which one would find that, *given the information about the (constant) number growth rates of the various subclasses of an object-collection*, (the decrease in) the time rate of change in the overall number growth rate would require an explanation at all, or at least would be explained by (the decrease in) the variance among those subclass number growth rates. For any (discrete series of) change in the number count/proportion of any object-collection, the set of the number growth rates of its subclasses and the time rate of change in the number growth rate of the whole collection are two different arithmetically/statistically partial- or under-descriptions of the same change, and the variance among those subclass number growth rates is yet another, more informationally reduced statistical under-description that is mathematically entailed by those subclass number growth rates. To be sure, all these complex terms are derivationally related. But they don't seem to be able to stand in explanatory relation with each other, because they are just different *arithmetic/statistical under-descriptions* of *one and the same change*. At any rate, none of them could explain another in the empirical sense of explaining an occurrence by its cause or explaining the instantiation of a property by the concurrent instantiation of a realiser or a micro-base.

Similarly, at the type-level, it's highly doubtful that one would ever find the formal patterns of change at issue in need of explanation. Each of those patterns is at bottom a conceptual relation between two different ways of arithmetically/statistically under-describing a single change, which in turn boils down to a matter of mathematical definitions of the describing terms themselves. This is the only *reason* as to why those patterns hold (and hold necessarily). If this counts as an explanation, then so be it. But it is evidently not an empirical explanation. The simple relation between growth rate and number counts/proportions is purely definitional; it requires no explanation and itself explains nothing in the empirical sense of explanation. It is perhaps because it is so blatantly trivial that Matthen and Ariew do not directly identify it with selection. On the other hand, the conceptual relation expressed by the growth-rate theorem might not seem so trivial, yet describing changes in terms of it is of little epistemic significance. Since such a description is essentially the application of a mathematical theorem; it is non-empirically and necessarily true of the occurrences described. For this reason, it neither elicits nor furnishes explanations and so cannot further our understanding of the described changes. You can devise a descriptive pattern such that it is void of any empirical content and hence is necessarily true, universally applicable, completely domain-generic and what not. But then you also take away every bit of its explanatory utility and epistemic significance.

By saying that the formal pattern that holds between the time rate of change in overall number growth rate and the variance among subclass number growth rates neither explains nor is in need of explanation, that those two arithmetic/statistical under-descriptions don't stand in explanatory relation, and that they themselves don't call for explanation *given the set of subclass number growth rates*, however, I don't mean that there's nothing to be explained about the changes so described. Those number growth rates, i.e. the changes in number counts/proportions themselves, obviously require explanations. We'll want to ask why the subclasses' number proportions change in the way they do, as well as why the whole collection's number count changes in the way it does. We'll also want to know why the subclasses are divided in the way they are, that is, what grounds the way(s) of partitioning the collection based upon which the given under-descriptions are made. All these are empirical questions and have to be answered by supplying additional information beyond informationally reduced under-descriptions of the changes one after another. Yet, in Matthen and Ariew's account, selection is not a matter of changes in subclass number proportions of an object-collection; it is a relation between two certain arithmetic/statistical features of the same change in the number count/proportion of an object-collection (with another such feature of that change being a set of changes in subclass number proportions). Consequently, explaining the changes in subclass number proportions is not explaining selection, and using subclass number growth rates to predict long-term trend or, perhaps, to explain it in a wider context is not using selection to predict or explain. Selection as construed in the formal-pattern account is such a useless notion that we can totally dispense with it without affecting our understanding of the world. And precisely this is what is fundamentally wrong with the formal-pattern account of selection.

This conclusion has an interesting and important implication. It implies that it is pointless to extend the notion of selection as used in the studies of biological evolution so that it becomes applicable to all sorts of changes across the board that merely share a common necessarily true arithmetic/statistical (under-)description and hence becomes completely domain-generic. The extended notion is useless in so far as it has no explanatory work to do. And its supposed merit of being domain-generic is superficial and delusional. The commonality among the changes that fall under such a description is neither physical nor functional; it is non-empirical and merely linguistic. Both of these strongly prohibit the positing of any corresponding properties and occurrence-types. Accordingly, extending the original notion of selection from the domain of biological evolution to other domains does not go beyond the realm of analogy. And a purely verbal analogy resting solely upon a formal similarity for that matter.

Note that this view is not assumed in the first place. I didn’t argue against Matthen and Ariew’s account of selection by criticising that they are not really talking about selection or that they are merely talking about an analogy with selection. Before any substantial argument, it is largely a preconception to think that the notion of selection (proper) concerns biological evolution alone. Rather, I rejected their notion of selection on the grounds that it doesn’t pick out an occurrence-type and cannot play any explanatory role. These provide convincing reasons for accepting that it can only remain an analogy-based idea in relation to the original notion of selection. However, none of these implies that the latter notion is more tenable. Recall that the formal-pattern account of selection is partially founded upon a rebuttal of the ordinary talk of selection as a cause of evolution. If that is a successful rebuttal, then perhaps there is no defensible notion of selection and we may have to recognise that the whole talk of selection is misguided. If it is not, only Matthen and Ariew’s account collapses and we should go on to explore other accounts of selection that stick to the conventional idea.

2.3. Redemption of the Traditional Notion of Selection

Traditionally the notion of selection is specific to the domain of biological evolution: It is about a cause of evolution. In addition, most philosophers as well as biologists nowadays also speak of selection as a process (if not a force). But, every case of evolution (by selection), and hence every case of selection, is wholly constituted by organismal-level occurrences, i.e. processes/events each involving only some (not limited to one), but not all, of the organisms of a population. According to Matthen and Ariew, this constitution thesis speaks against the reification of the term "selection": It renders redundant the posit of something called "selection" which is over and above the organismal-level occurrences in a case of what is called "evolution by selection" and which caused the given evolutionary change. Further, although "variation in wing darkness caused the evolution of the moth population" is an acceptable causal claim, it doesn't have the same meaning as "selection caused the evolution of the moth population" and doesn't talk about certain process. Can the traditional idea of selection survive these challenges? Yes it can. Below we'll see that the conventional talk can be understood in a way that is consistent with the constitution thesis. In addition, a careful assessment of the above attacks will reveal that they themselves rest upon several mistakes so that they pose virtually no threat to the traditional notion.

We start by establishing the grounds for reifying "selection" in the ordinary sense of a process such that variations in certain features amongst a population of organisms lead to the variation in their reproductive success. This latter idea is nothing but the Huxley-Darwin cliché and Lewontin's well-known HVF formula (that the condition for selection is *h*eritable *v*ariation in *f*itness) precisified and neutralised (by leaving unspecified what exactly those features are). It is also what underlies the two accounts of selection to be examined in later chapters. At this stage, we can leave aside questions like whether the characterising features are causal properties or even properties at all, whether selection so characterised is a causal process, and whether the characterising sentence expresses a law or even a causal law. We just ask: In a particular case of evolution by selection, what is picked out (or can be picked out) by the term "selection" so understood? This is essentially a question regarding the reference of "selection" in any given case. The term can be reified only if there is a process to which it can plausibly refer in any given case. Does such a process exist, then?

Of course it does. It is basically just the collection of every event/process involving any of the organisms of a given population within a given generation, which includes any sort of activity, suffering, mutual interaction, interaction with organisms of other populations and reaction to the environment. Or, equivalently, it is the congeries of life histories of any and all of those organisms. This is only natural because "selection" doesn't apply to any particular organismal-level occurrence; nor does it pick out any specific type of organismal-level occurrence or any fixed set of specific types of such occurrences in any given case. It obviously applies to the whole case. And a case of selection (as well as evolution) is constituted not only by the subject, i.e. any and all organisms of a population within a generation, but also by each and every organismal-level occurrence in which any of those organisms participated in any time-interval within that generation-time (i.e. from their births until the reproductive events/processes or their deaths in a discrete population). Or better put, a case of selection is constituted by all and only those organismal-level occurrences of each of which some organisms of a given population within a given generation are a part. So long as the attribution of "being a case of selection" is based upon the connection between features of organisms of a population within a generation and their reproductive success, the reference to such a collection of indeterminate organismal-level occurrences is natural and inevitable, for whatever effects of features of organisms are parts of organismal-level occurrences and those features as well as their immediate effects are connected to reproductive success only through those occurrences. Thus, “selection” picks out any such collection of organismal-level occurrences that also exhibits a certain connection between variation(s) in certain feature(s) of organisms and the variation in their reproductive success. It is (presumably) a *type*-term, and accordingly any particular collection of organismal-level occurrences picked out by it is an *instance* of selection. In any given case of selection, “selection” just denotes such a whole collection of organismal-level occurrences.

The above shows that the term “selection” in the conventional sense is referential and therefore selection so understood exists. This fully establishes the grounds for reifying “selection” in that sense. And it also implies that the reification of “selection” is fully compatible with the constitution thesis and, hence, Matthen and Ariew are mistaken in asserting otherwise. They go wrong in two aspects. First, they think that reifying a term amounts to posting something extra. This is not generally true and is not true of our case. What is posited is not a separate force that acts upon a population of organisms, an additional something that can be metaphorically understood as a motor that drives them to exhibit a new allelic frequency distribution, or a *tertium quid* that mediates between variation(s) in certain feature(s) of organisms and a new allelic frequency distribution among them. Rather, it is a type of collection of certain pre-existing organismal-level occurrences as characterised above. Its posit is grounded in a new general (and presumably explanatorily useful) way of describing and classifying collections of those pre-existing occurrences. In fact, when Matthen and Ariew identify selection with a formal pattern of change, they are also attempting, though unsuccessfully, to posit a type of change or occurrence, any instance of which in the domain at issue is exactly one such collection of pre-existing organismal-level occurrences. So, they are reifying "selection" too. The difference between their account and the ordinary talk is only that the two (supposed) types being posited are different: While the one in the former is supposed to be defined by a mathematical theorem concerning number count, the one in the latter is based upon a regular connection between variations in features of organisms of a population.

Second, it is erroneously to think, from the very beginning, that the constitution thesis furnishes a reason for the non-existence of selection as traditionally understood. What the constitution thesis expresses is, no more and no less, that selection is token-identical to a collection of organismal-level occurrences. This alone already asserts that selection-instances exist, which then implies that selection as a type of certain collection of organismal-level occurrences exists. Matthen and Ariew here seem to make a mistake similar to the confusion between (ontological) reduction and elimination. The fact that something is constituted by some other things doesn't suggest the elimination of the former in the slightest. A ball doesn't cease to exist because it is wholly constituted by a collection of atoms. Likewise, an occurrence that is a ball's breaking a window doesn't cease to exist because it is wholly constituted by a collection of atomic-level occurrences. And the same goes for selection.

Similar considerations also indicate that selection-instances are themselves processes and, arguably, causal processes. Given that an instance of selection is a collection of organisma-level processes, I see no reason why it cannot itself be an occurrence or process. It is surely not an organismal-level occurrence; we may say that it is a population-wide and basically generation-long process. Or, relative to those constituent organismal-level occurrences, it is a macro-process. Moreover, in so far as membership of a biological population is partially defined in interactive terms, the organismal-level occurrences constitutive of an instance of selection are causally bound (and temporally confined if not also more or less spatially localised), and there should be no difficulty in accepting that a selection-instance is a causal process. Both of these reveal that, once we accept the constitution thesis, it is just a small step away from also recognising that a selection-instance, a said collection of organismal-level occurrences, is a process and a causal process for that matter. This suggests that the question of whether selection is a causal process is almost trivial. The debate over whether the various regularities regarded to characterise selection are causal or not is a totally different matter. Failure to distinguish between the two betrays the confusion between causation and causal efficacy of properties, a crucial distinction to be made clearer at the end of this chapter.

Now, in a given case of evolution by selection, did selection so understood cause evolution? In Matthen and Ariew's opinion, the correct way of talking about the matter is to say that various organismal-level events like predations and matings severally caused the deaths and births of organisms (which are themselves also organismal-level events), which then collectively "constituted" evolution. This way of looking at the matter is flawed in two respects. First, such a picture makes the identification of selection in the ordinary sense particularly non-straightforward if not precluding it *a priori*. It makes no sense to say that a collection of births and deaths is an instance of selection, and incorporating only births and deaths and their respective immediate causes into an instance of selection is too narrow-scoped. As seen above, the talk of selection applies to the whole of the lives of organisms of a population in a generation: It is a regularity exhibited by some such collections of life-histories that is based upon certain (supposed) properties of organisms and their reproductive successes. Those property-instances don't influence reproductive successes just in the immediate causes of births and deaths. Their influences in that respect are usually cumulative and indirect and the causal conditions complex and holistic, so that the talk of selection should apply to the whole collection of causal chains featuring organisms of a population within a generation during all their lives. This suggests that we fuse all the births and deaths and their immediate causes and everything those organisms participate before those events up to their own births and talk, instead, of a macro-process that lasts generation-long. And these considerations are missing in the picture given by Matthen and Ariew.

Second, saying that births and deaths constitute evolution doesn't quite capture the notion of (biological) evolution as cross-generational change in allelic frequency distribution for a population of organisms. Such a "change" is actually a difference between two feature-tokens (two allelic frequency distributions) of two different collections of organisms (those in a given generation and those in the next generation). So, in any case of evolution by selection, evolution is a matter of a collection of parent organisms' reproducing a collection of offspring organisms having any of certain collective genetic features that is different from that of their parental generation. Making this explicit unveils what causes evolution in such scenarios. For asking what caused evolution in a given case of evolution by selection is asking what caused the reproducing, by a certain collection of parent organisms, of a certain collection of offspring organisms having a certain collective genetic feature. And the answer is exactly the particular instance of selection in that case, i.e. the macro-process that is the whole of the organismal occurrences involving the parent-generation organisms, which extended from their births until at least the births of their offspring and which betokened a certain regularity that is based upon their certain properties and their reproductive successes (in virtue of the tokening of which it is an instance of selection). The traditional idea of selection being a cause of evolution is thus thoroughly intelligible and perfectly feasible.

There may be some worries about the talk of causation in this context. Standard metaphysics dictates that causal relata are events. Yet selection-instances are said to be processes. And a token of evolution, being basically a collection of certain reproductive events or results, may not be legitimately taken as a macro-event, for the reproductive events may not be spatiotemporally contiguous. Moreover, in so far as an instance of selection comprises everything the parent-generation organisms participate from their births until the last reproductive events at least, the collection of all the reproductive events itself is to be considered a part of the selection-instance. As a result, even if a process is allowed to cause, there is no separate (macro-)event to assume the role of the other causal relatum. Such worries, however, can be removed by appealing to the notions of process and product. A case of evolution by selection can then be regarded as a (macro-)process, a selection-instance as previously identified, producing a final product, a collection of offspring-generation organisms having certain collective genetic features different from those of the parental generation. This eliminates the need for segregating a collection of organisma-level occurrences into two non-overlapping aggregates or dividing the life-histories of the parent generation into two temporally distinct series. It also erases the demand that the proper talk of cause and effect apply only to event-pairs. And there is a perfectly good sense in which a collection of offspring-generation organisms having certain allelic frequency distributions dissimilar to the parental generation is a product of the parent-generation organisms' being involved in the organismal-level occurrences during their lives so that their certain properties and their reproductive successes exhibit a certain pattern that is an instance of the regularity characterising selection. More crucially, the notion of production is so akin to that of causation that no one would deny that it is a causal notion. Consequently, the ordinary talk can still be properly understood as a causal talk, albeit a non-standard one.

The correct understanding given here also indicates that it is a mistake for Matthen and Ariew to acknowledge that “variation in wing darkness caused the evolution of the moth population” is a legitimate causal claim, yet to insist that it doesn’t have the same meaning as “selection caused the evolution of the moth population” and doesn’t represent a process. No doubt variation in wing darkness is not identical to selection: The former is a collective feature and the latter is a macro-process. But in the context of explaining a given case of evolution, those two sentences should be interpreted as talking about one and the same thing: the complex of the macro-process which is the particular selection-instance and its product which is the particular token of evolution in that case. Alternatively, we may literally take “variation in wing darkness” to mean a certain collective feature of the parent-generation organisms. In that case, one is just citing the most important characteristic or constituent of the macro-process to explain the presence of the product. In fact, so long as the first sentence is accepted as a legitimate causal claim, variation in wing darkness has to be taken as a property and its instance can be said to cause only on condition that it is a constituent of some occurrence, which in our case is simply an instance of selection. Hence, the claim that variation in wing darkness caused the evolution of the moth population just implies the claim that selection caused the latter, and both are talking about a certain macro-process producing the given result or product.

Matthen and Ariew’s fundamental mistake in thinking that evolution cannot have a macro-cause called selection but can only be constituted by micro-occurrences that are births and deaths is, I believe, that they fail to (fully) appreciate the fact that *the same occurrence can be described in different ways*. This is also what underlies their peculiar idea, as exposed above, that reifying “selection” is positing, redundantly, some extra cause of evolution. Their objection to the posit of a separate force or the like is argued, essentially, by setting up a *causal exclusion problem* for any such extra cause of evolution and is well taken. There can be a causal exclusion problem only when the competing causes are *distinct*. In the case of positing selection as a cause of evolution that is over and above the organismal-level occurrences or the organismal properties, selection is distinct from the latter and there is indeed a causal exclusion problem. That exclusion problem can be readily avoided by eliminating altogether such a putative cause, i.e. by refraining from positing selection in this sense at the very beginning, because it is blatantly explanatorily superfluous. But the same problem cannot arise for the ordinary talk as explicated above since selection, as a type of macro-process, is token-identical to, and hence *indistinct* from, a collection of organismal-level occurrences. The macro-causal/productive relation between a selection-instance, a macro-process, and a collection of offspring organisms, a collective reproductive result or product, may be said to be constituted by a collection of organismal-level causal and productive relations. Surely this doesn't imply that the macro-causation doesn't exist, "any more than the fact that [a] baseball is composed of microparticles entails its nonexistence" (Kim 2003: 167). It does imply, however, that there is in reality only *one* set of causations. The key point is: This single set of causations can be described in different ways. It can be described collectively as a single macro-causation or separately as a lot of organismal-level causations (or a myriad of atomic-level causations and so on). Although there is a sense in which some descriptions are more fundamental than others, all of them are equally legitimate in the sense that none of them is truer or more authentic than another. All of them are about the same things and occurrences. The same set of occurrences, causes and causations won't mysteriously cease to exist just because they are described as a single macro-causation rather than as a lot of organismal-level causations.

But selection is essentially a *type* (of process), and so is evolution (a type of product). And they are not type-identical to any other types. More importantly, selection is *not* type-identical to the type that is the collection of all life-histories of all organisms of a population within a generation (if this is indeed a type), for it is further characterised by a regularity concerning variations in certain organismal properties and variation in reproductive success. That is, not all collections of all life-histories of all organisms of a population within a generation are selection-instances. Likewise, not all collections of all births of offspring organisms (and all deaths of the parent-generation organisms) are tokens of evolution. Then, the type of macro-causal relation the instance of which is between a selection-instance and a token of evolution is *not* identical to the type of macro-causal relation the instance of which is between the occurring of a collection of all life-histories of all organisms of a population within a generation and the occurring of a collection of all births of their offspring organisms. The latter is just reproduction taken population-wide and generation-long. The former, evidently, is much more than that since it is a special case (a subset) of the latter. Matthen and Ariew fail to see this or at least do not take it seriously, when they say that selection/evolution is just a collection of organismal-level occurrences. That is true at the token-level but not at the type-level. Not recognising this makes the regularity characterising selection in the ordinary talk completely absent in their depiction of selection and evolution, which makes the ordinary notion of selection what it is not, trivialises it and, worst, deprives it of its unique explanatory role with respect to evolution that is solely conferred by that regularity.

This difference is huge. The whole point of, and justification for, recognising the existence of selection as a type of macro-process characterised by a certain regularity and the existence of a type of macro-causation that is colloquially referred to as "selection causes evolution", is precisely that selection in the ordinary talk is genuinely explanatory with respect to evolution. One can describe a given case of evolution by selection by recounting, one by one, the life-history of each and every organism in the parental generation until the births of its last offspring or its death, without paying attention to any pattern between certain properties of the organisms and their reproductive results. A description so pointless like this hardly explains anything; it is not clear under which type is subsumed that particular collection of life-histories. It is just a raw description, not focusing upon any specific information and contributing no understanding of the case. However, if the case is described in terms of a pattern between the variation in certain property of the parent-generation organisms and the variation in their reproductive success, then it is subsumed under selection and is properly recognised as an instance of selection. Such a description, in conjunction with correct information and/or reasonable assumptions about the genetic mechanisms of the specific type of organisms, genuinely explains why their offspring generation has certain allelic frequency distributions different from theirs. If you don't use the notion of selection, i.e. if you don't cite such a pattern or if you don't describe the collection of life-histories in terms of such a pattern, you have no general way to explain cases of evolution by selection, instead of merely reporting a collection of raw data in each case. Such patterns, along with their generalisations, are what do the explanatory job and what are at the heart of the ordinary notion of selection. In a sense, saying that selection exists is saying that there are such patterns and their generalisations. And "selection" can be justifiably reified just because of that. These patterns and generalisations can be discerned only when we take organisms' lives population-wide and generation-long and think about them collectively rather than individually—the doctrine of population thinking in evolutionary biology.

2.4. Summary and Prospect

In sum, there is no problem with the conventional thought that selection is a cause of evolution and it is a (type of) process: no extra (type of) entity, no redundancy and no causal exclusion problem—and all these are consistent with the constitution thesis. In fact, they *follow from* the constitution thesis. All that is important is that selection is essentially a macro-process-type, the instances of which are collections of life-histories of all organisms of a population within a generation that share a common collective pattern that is described as a connection between variation(s) in certain organismal properties and the variation in reproductive success. And the posit of selection is licensed by the explanatory utility of such a common pattern in respect of evolution. I thereby conclude that the ordinary notion of selection is redeemed.

There are, no doubt, other disputes over it. We have yet to decide which organismal properties are such that the connection between variation(s) in them and the variation in reproductive success should characterise selection. Obviously this is a main locus of disagreement. Also, we've left the nature of that connection unaddressed. Most authors either claim that it is based upon certain causal connection or assert that it is itself a causal connection. Whether or not the said causal connections are genuinely causal is a big question. There may be, for example, causal exclusion problems with them. Such an exclusion problem is, however, different from the one discussed previously: Given a pair of properties that is said to be causally connected, it concerns the causal efficacy of instances of one of them with respect to the instantiations of the other. Such a talk makes sense only when the two properties are instantiated, respectively, in instances of two types of occurrences (events/processes/products) the instances of which are causally related. That is, there must already be a type of causal relation, and instances of the two properties are (partially) constitutive, respectively, of the cause and the effect (of each instance of that type of causal relation), so that we can sensibly ask: Whether the property the causal efficacy of which is in question is such that its instantiation in the cause, as opposed to the instantiations of some other properties the instances of which are also constitutive of the cause, brought about, or was causally responsible for, the instantiation of the other property in the effect. The type of causation, or rather causal process, that grounds the talk of the causal efficacy of the properties in the said causal connections that directly or indirectly characterise selection is nothing but selection. Since it has been shown above that selection-instances are causal processes, we can proceed to tackle the questions concerning the causal efficacy of those properties. These questions, and the ones regarding which properties should characterise selection and so on, are all dependent upon what we've established here: Selection as a type of macro-process the instances of which are macro-causal processes, exists.

3. The Account of Selection as a Type of Causal Process Characterised by Fitness and the Causal Inefficacy of Fitness

This chapter deals with the account of selection that characterises it in terms of the connection between variation in fitness and variation in reproductive success. It is a direct result of conceptual refinement of and philosophical investment in the Huxley-Darwin cliché, and is represented primarily by Bouchard and Rosenberg (2004) and Rosenberg and Bouchard (2005), along with the propensity interpretation of fitness. It regards the connection, or something akin to it, as a causal connection. That is, it takes, or at least has to take, fitness to be causally efficacious with respect to reproductive success. But is this correct? This certainly depends upon what fitness is or can be, a question that has spawned a lot of disputes revolving around the notorious tautology problem. In the following, I'll argue that fitness can only be a second-order functional property. For that very reason, the claim that it is causally efficacious with respect to reproductive success suffers from the problem of metaphysically necessary dependency and the causal exclusion problem. These problems persist despite some attempts to solve or dissolve them.

3.1. Fitness and the Principle of Natural Selection (PNS)

Bouchard and Rosenberg's view of selection is neatly summarised by a variant of the so-called "principle of natural selection" which they endorse. It reads: "(x)(y)(E) [[If] x and y are competing organisms in generation n, and x is fitter than y in [environment] E, then probably (there is some generation n', at which x has more descendents than y)]" (Bouchard and Rosenberg 2004: 695). It is intended as a law, and the only law in biology for that matter (Rosenberg and McShea 2007). Obviously it is considered a probabilistic law. And, above all, the fitness that figures in it is said to be a causal property and the law itself is deemed a causal law. For the sake of argument, in this chapter we'll *assume* that the fitness-talk in the domain of selection and evolution does pick out a property called "fitness" and that (this variant of) the PNS is indeed a law. My goal then is to show that fitness cannot be a causal property and, by extension, the PNS cannot be a causal law. If this is the case, and if we think that selection can and should be characterised in causal terms, then it cannot be characterised by fitness and any associated laws or regularities.

Some modifications and clarifications have to be made about the PNS in the first place. The indefinitely long time-frame in which the effect is specified is pointless. For the PNS to be at least a *prima facie* causal law, the effect has to be the fitter organism's reproducing more offspring than the less fit one. That is, the time-frame has to be limited to a single generation. A property-instance cannot be causally responsible for whatever happened long after it had ceased to exist. If an organism's fitness can be causally responsible for some reproductive results at all, it can be so only for that organism's own reproductive success, i.e. the amount of that organism's (immediate) offspring, and similarly for an organism's being fitter than another. It may be said to be linked to the amount of that organism's distant descendants at a remote generation via a very long causal chain. But that doesn't imply that it is itself causally linked to the latter, for causal relation is not generally transitive. In our case, the environment and other causal conditions may and often vary from generation to generation, and partly because of this different generations of descendants of the same organisms may not and often do not have the same fitnesses or fitness-variations. For these reasons, fitness-instances cannot be causally responsible for any aspect of non-immediate reproductive results and, therefore, an organism's being fitter than another cannot have as its causal consequence the former's having more descendents than the latter if those descendants are not reproduced by them.

Besides, if one merely wants to accommodate the idea that fitter organisms do not always reproduce more offspring, because some background causal conditions do not always obtain and/or because fitness-instances are genuinely indeterministic in relation to reproductive successes, the probability operator contained in the consequent-clause of the PNS has already done the job. If, however, one just intends to talk about series of differences between the amounts of two organisms' respective descendants over generations, then the PNS is actually not characterising selection but is describing trends of selection, i.e. series of selection-instances. Since descendants of the fitter are not always fitter than descendants of the less fit by more or less the same degree, especially over generations, there is little reason to concatenate what are ordinarily thought of as successive yet distinct instances of selection into a single process. And, even in cases where (most) descendants of the fitter ancestor are actually fitter than (most of the) descendants of the less fit ancestor (by more or less the same degree) over several generations and, moreover, in some of those generations the descendants of that fitter ancestor do outnumber those of the less fit one, what can be causally responsible for such outnumbering in each of those generations is the larger groups' parents' being (generally) fitter than the smaller groups' parents in the previous generation, rather than the former's distant ancestor's being fitter than the latter's in the starting generation, or the former's being descendants of that fitter ancestor while the latter are descendants of the less fit one. Different generations of organisms are simply involved in different "rounds" of selection. The indefinitely long time-frame in the consequent-clause makes no sense and has to be replaced by the single-generation time-frame, or otherwise the

PNS cannot be said to characterise selection and cannot be a (*prima facie*) causal law.

The case for the antecedent-clause is much trickier. It picks out pairs of (life-histories of) competing organisms living in a common environment and having the relation of fitter-than. But Bouchard and Rosenberg didn't clarify under what circumstances are two organisms competing ones or how environments are supposed to be individuated in their talk of selection. Depending upon whether the two organisms are allowed to be members of different populations, these may pose special difficulties for their account of selection, since the ordinary notion of selection neither counts upon the idea of competition nor demands a special principle of individuating environments. The ordinary notion applies to the whole of the organisms of a population: For all practical purposes, a biological population is nothing but a group of conspecific organisms that is relatively isolated from other groups of organisms in respect of gene flow and/or environment. While this does not in itself implies that organisms of the same population typically interact to a considerable degree or interact more frequently/closely with one another than with non-members, it does however suggest so, especially for animals (e.g. mating behaviours). But it by no means suggests that they compete in any straightforward sense, locally or globally, between individuals or between groups within the population, unless one resorts to a notion of competition in which organisms sharing the same environment compete for the (limited) resources in it, and hence for their respective survival and reproductive success. Such a notion is obviously redundant in the common talk of selection since members of the same population must live in the same environment and so must compete in that trivial sense. Meanwhile, the environment for a population is understandably coarse-grained, so that organisms of the same population can be said to share the same environment yet there can also be inhomogeneities within the environment where they live. Such a coarse-grained way of individuating environments is inherent in the practical notion of a biological population. The ordinary talk of selection simply builds upon the latter along with the associated way of individuating environments.

Now, if the antecedent of the PNS applies to all and only organism-pairs within the same population that enter into the relation of fitter-than, saying that such organism-pairs are competing organisms is as redundant as in the case of the ordinary talk, and the environment for each of those organism-pairs is exactly the same coarse-grained environment for the population to which it belongs. In that case, as we shall see immediately, Bouchard and Rosenberg's account isn't fundamentally different from the ordinary population-based talk. If, however, two organisms belonging to the same population can be non-competing organisms or be said to live in different environments so that the antecedent of the PNS does not apply to them, or two organisms belonging to different populations that live in partially overlapping (coarse-grained) environments (hence members of the two populations are of different species) can be competing organisms and one of them can be said to be fitter than the other in a certain finer-grained environment shared by them so that the antecedent of the PNS does apply to them, then Bouchard and Rosenberg's account is distinct from the population-based notion of selection. We certainly don't want to say that two organisms of very different species are competing organisms even if they share a very fine-grained environment or there are frequent/close interactions between them. Also, it probably makes little sense to say that one of them is fitter than the other in such a scenario or that they (and they alone) are involved in a selection-token. Without a general criterion for discriminating competing organism-pairs from non-competing ones and another for sorting out when two organisms share the same environment and when not, the scope of the PNS is simply left open. Then, unfavourable cases like those mentioned above cannot be ruled out and thus are potential threats for Bouchard and Rosenberg's account of selection if they intend it to be different from the population-based notion (which I believe is the case). Note that I didn't mean that there cannot be such criteria. But responsibility lies with the advocates of the PNS to spell them out in order for us to further evaluate it.

For the sake of argument, however, we'll assume that the antecedent of the PNS applies to all and only organism-pairs within the same population that enter into the relation of fitter-than. For the thesis to be argued later, that fitness is causally inefficacious with respect to reproductive success, is independent of the precise scope of the PNS because, for any proposed scope of it, the thesis holds true for each and every instance of it. So we'll drop the notion of competition and take environments to be coarse-grained so that all members of the same population (at the same generation) are considered to live in the same environment. With some verbal rearrangements, the revised PNS now reads: For any two organisms of the same population (at the same generation), if one is fitter than the other in the environment they live, then the former is likely to reproduce more offspring (i.e. to have greater reproductive success) than the latter. Quantification over environments is abandoned because it is problematic to quantify over causal conditions and because it is readily understandable from the antecedent-clause that the PNS is not restricted to any specific set of environments. And, since the PNS is advertised as a causal law, the revised PNS can be thus abbreviated: An organism's being fitter than another within the same population is usually responsible for its having greater reproductive success than the latter. Or, equivalently: Difference in fitness between two organisms of the same population is usually causally responsible for difference in reproductive success between them.

While further modifications will be made in later discussions, for now we can be content with the current re-formulation. It is formally very similar to the regularity characterising the ordinary, population-based and fitness-dependent notion of selection, which, if also regarded as a causal regularity, can be expressed as: Variation in fitness amongst organisms of the same population is causally responsible for the variation in reproductive success among them. This gives us an opportunity to see that Bouchard and Rosenberg's view about selection is not really distinct from the ordinary, population-based talk. In the ordinary notion, a selection-instance is a population-wide (as well as generation-long) process, a macro-process that involves all and only organisms of a population at a generation. For Bouchard and Rosenberg, however, a selection-instance is a pair-wise process, involving only a pair of organisms of a population at a generation. The population-wide process is then regarded as an aggregation of such pair-wise processes. But, while they claim that the pair-wise processes are causal processes, they are reluctant to accept that the aggregated process is a causal process. They take the population-wide process to be characterised by variations/differences in population-genetic fitnesses, which they think are mathematical/statistical constructs each derived from the reproductive results of one or more groups of organisms of the same genotype or trait-type, are attributed to organism-groups and are not causal properties. Only the "ecological-comparative" fitness they advocate, i.e. the fitter-than relation, is explicitly said to be a causal property. Presumably, the talk of the fitter-than relation presupposes and is based upon the existence of a "non-comparative" fitness that is attributed to individual organisms. *If* fitter-than is a binary causal property, then there is no reason why the fitness that is attributed to individual organisms, an ontologically more fundamental unary property, cannot be a causal property. Suppose the population-genetic notion of fitness can be interpreted as the group version of such a notion of organismal fitness. Then Bouchard and Rosenberg owe us an explanation as to why the former cannot pick out a causal property or even a property at all. If group fitness is itself a causal property, then, given that fitter-than is a causal property, there is a high chance that variation/difference in group fitness also represents a property, say, of the whole population. If so, the regularity between variation in group fitness and variation in group reproductive success may be deemed a causal regularity and the sort of population-wide process characterised by it a type of causal process. Thus, Bouchard and Rosenberg have actually opened this possibility, if their claim that fitter-than is a causal property is true.

But the ordinary, population-based talk of selection we're talking about does *not* rest upon the notion of population-genetic fitness or that of group fitness. The notion of fitness it builds upon can only be applied to individual organisms and, naturally, can only be the same as the one which Bouchard and Rosenberg advocate or, more precisely, presuppose. So we don't have to worry about whether group fitness itself is a causal property or whether it is a property at all. And Bouchard and Rosenberg are completely silent about whether variation in fitness amongst *all organisms* of a population is a causal property or not. Again, *if* variation/difference in fitness between two organisms is a causal property, I don't see why variation in fitness among many organisms cannot also be a causal property. Such a property of course cannot be a relation between organisms, because populations are far from being constituted by the same number of organisms and it is a metaphysical requirement that all instances of a relation should have the same number of terms. As a workaround, we follow Millstein (2006) in saying that variation in a property amongst all organisms of a population is a unary property of the population (which she calls a population-level property).

Here, one may resort to something like a micro-macro causal exclusion argument and conclude that variation in fitness amongst many organisms is not a causal property given that fitter-than is. The trouble is that if that argument goes, then fitter-than is not a causal property either on condition that fitness is. In the same vein, one may say that it is unnecessary to posit the property of variation-in-fitness-among since, if posited, it is clearly ontologically reducible to fitter-than and nothing seems to be explainable by citing it without also being explainable by citing the more basic fitter-than relation. But, likewise, if that argument is correct, then we don't have to posit fitter-than either, since it is evidently ontologically reducible to fitness and there is nothing such that it is explainable by citing fitter-than but not by citing fitness. This is the line of thought that we'll pursue in the next chapter in rejecting that property-variations themselves are causal properties or that they are properties at all. But the point here is that Bouchard and Rosenberg's account of selection and the ordinary, population-based talk are in the same boat regarding the causal efficacy and the redundancy of the supposed causal property that characterises selection in respective views. If in our case there is a dividing line between causal properties, laws/regularities and process-types, on the one hand, and non-causal ones on the other hand, it should lie between the organismal ones and the pair-wise and population-wide ones. Hence, for our purposes, Bouchard and Rosenberg's account and the ordinary talk do not represent two distinct views.

So, whether fitter-than is a causal property, whether the PNS is a causal law, and whether the sort of pair-wise process characterised by the PNS is a type of causal process, all depend upon whether fitness is causally efficacious with respect reproductive success, assuming that there is indeed such an organismal property. But what sort of property is fitness, after all? Obviously an adequate account of the nature of fitness is in demand. And the next section is devoted to it.

3.2. Fitness as a Second-Order Functional Property

Assume that "fitness" does designate a property. Then fitness is a property of organisms. It is not a physical property but is a supervenient property (Rosenberg 1982; Sober 1984; Bouchard and Rosenberg 2004). It is a posit that grounds the population-genetic notion of fitness, which Matthen and Ariew aptly point out is simply the (actual or expected) growth rate of a (within-population group of organisms of the same) genotype or trait-type. And it is supposed by some philosophers including Bouchard and Rosenberg to be causally efficacious with respect to reproductive success. These are the common grounds for an account of the nature of fitness. Actually, they already strongly suggest that this supposed property is a second-order functional property, although no philosophers have ever recognised that. To expound and consolidate the thesis, we first consider what fitness is *not*.

First, fitness is not what it grounds. The population-genetic notion of fitness is either a descriptor of change (actual growth rate) or an estimator of change (expected growth rate). Either it picks out the whole change/process or it is a mere concept. In either case, it does not designate a property. So the fitness we're talking about cannot be identified with it. Note that the reason lies in that it cannot be said to designate a property, rather than that it applies to organism-groups as opposed to individual organisms. A group version of the fitness we're talking about is readily conceivable, which would be a property of organism-groups and would directly ground the notion of growth rate, actual or expected. Of course, there are additional metaphysical or methodological difficulties for accepting such a property. The point here is just that the sort of thing that can be possibly identified with fitness has to be a property, not a change (however it is described) or a mere concept devised to estimate a change.

Second, fitness is not actual or expected reproductive success of an organism. It cannot be identified with expected reproductive success because the latter is also an estimator and hence a mere concept.[1] And it cannot be identified with reproductive success for a very obvious reason: The latter is its supposed effect. Furthermore, even if we don't suppose that fitness is causally efficacious with respect to reproductive success, identifying fitness with reproductive success amounts to saying that fitter organisms are those having greater reproductive success, which simply renders the (modified) PNS tautological (the probability part aside, which will be addressed in the next section). Thus, if we want the PNS to be a law, and/or if we want fitness to be a causal property, then fitness definitely has to be kept distinct from reproductive success.

Third, fitness is not the trait-complex that is causally responsible for an organism's reproductive success. The reason is simple: Organisms of different types have different trait-complexes that are causally responsible for their respective reproductive success. This holds not only for organisms of different species but also for organisms of different trait-types (or genotypes) of the same species or within the same population. And this is precisely why fitness has to be regarded as a supervenient property that supervenes upon a set of trait-complexes. Identifying fitness with each of the different trait-complexes in the subvenient set either violates the principle of transitivity of identity (so long as fitness is still recognised as *a* property) or simply eliminates fitness altogether. This is a key reason why it is generally acknowledged that supervenient properties are ontologically irreducible to their subvenient properties. Besides, for those who insist that selection is essentially characterised by the PNS, elimination of fitness is out of the question, or otherwise their account of selection cannot even get off the ground.

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[1] Beatty (1984) claims that population-genetic fitness, which he equates with expected reproductive success, is a property (specifically a propensity). This claim does not take population-genetic fitness literally; that is, it is already a theory of fitness. The literal interpretation of population-genetic fitness, underscored by Matthen and Ariew and also accepted by Bouchard and Rosenberg, is that it is the estimator of (actual) growth rate (or reproductive success). An estimator is not itself a property. So Beatty's claim should be understood as a *posit* of a property named "fitness" that proprietarily grounds fitness-the-estimator-of-growth-rate. A better semantics of the fitness-talk in population genetics, which is compatible with the plain idea of fitness as expected reproductive success yet does not simply take it to be an estimator, however, has to wait until the next chapter, after we argue against any such posit there.

Despite these, however, it is quite surprising that some philosophers who assert that fitness is a supervenient property should say something that strongly suggests that they identify fitness with trait-complexes. For example, fitness has been said to be the property of “overall adaptedness” of an organism in its environment (Brandon 1978), to have “components” which are an organism’s traits (Sober 1984), and to be the “overall solution to the design-problems set by the environment” (Bouchard and Rosenberg 2004). The major problem with all these arises from the fact that fitness is taken to be some sort of sum total. A property certainly cannot be a mereological sum. So the only way to make sense of the talk of fitness as an overall something or a composite is to understand it to be saying that fitness is a conjunctive property the conjuncts of which are also properties. For Sober such a conjunct-property is straightforwardly a trait. For Bouchard and Rosenberg it is, on the face of it, a solution to the design-problem set by certain aspect of an environment. I take it that such a solution is nothing but one or more aspects, i.e. traits, of an organism. For Brandon, it is also a trait since, according to Byerly and Michod (1991a), Brandon shifts the sense of “adaptedness” between expected reproductive success and “adaptive capacities”, which are simply traits that causally contribute to the reproductive success of an organism of a certain type. The trouble here is not that conjunctive properties are not acceptable. They are, and we’d better have them even in our case. The real problem is that the conjunction of traits (or of partial adaptedness or of partial/particular design-problem solution should these are not just traits but some exotic properties) that is identified with fitness is type-specific. Thinking fitness in terms of a conjunctive trait is exactly identifying fitness with each of its subvenient properties. The result is that fitness cannot be a property, and thus there is no fitness in the sense we’re talking about. The talk of fitness as a total/overall something is contradictory to the idea that fitness is a supervenient property. If we want to keep fitness alive, the former has to be abandoned.

Now, fitness is not only a supervenient property but also a *functional* property. The functional nature of fitness has never been laid bare. It comes to the fore when we ask: What exactly is the subvenient property-set for fitness? That set cannot be determined by enumerating its elements because the subvenient property-set (or realiser-set) for a supervenient property (or functional property) is characteristically an open set (Fodor 1997). It has to be determined by the similarity among the trait-complexes, or conjunctive traits, that are members of it. What the similarity consists in is quite obvious: The trait-complexes that are subvenient properties for fitness are all causally responsible for reproductive success. This similarity effectively works as a specification of a causal/functional role, and any trait-complex that satisfies this role-specification is a subvenient property or, better, a realiser of that causal/functional role. And, above all, fitness itself is essentially that causal/functional role. That is to say, fitness is a functional property. Also, since it has multiple realisers, it is a multiply realised functional property.[2]

I'll stick to the talk of realisation instead of supervenience. And I'll adopt the second-order view about realised functional properties and understand fitness to be the second-order functional property of *having some trait-complex or other that is causally responsible for reproductive success*. This is consistent with the idea that fitness

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[2] Standard functionalism, i.e. causal-role functionalism (which is also called analytical functionalism), is adopted here. The functionalism about fitness is intended to encompass a wide variety of proposed understandings of fitness, including the propensity/disposition interpretation, the (categorical) property view (to which Bouchard and Rosenberg's proposal belongs) and the ability/capacity talk (e.g. Haug 2007). For in all these understandings, fitness is defined (or should be taken as defined) essentially by reference to its supposed effect, i.e. reproductive success, which is uncontroversially also an effect of indefinitely many different trait-complexes. This is where supervenience and functionalism enter the scene. Also, it follows from functionalism about fitness that we take the functionalist view about disposition/propensity if fitness can be appropriately considered a disposition/propensity (which will not be questioned in the present thesis, but see Byerly and Michod (1991a, 1991b) for an objection). Functionalism about disposition/propensity has *prima facie* plausibility in our case since the propsensity interpretation of fitness, too, accepts the supservenience of fitness upon traits, which are themselves also properties and the instantiation of which is simlultaneous with the instantiation of fitness. These invite accounting for the posit of fitness in the propensity/disposition interpretation metaphysically as a causal role of trait-complexes.

supervenes upon trait-complexes. Moreover, according to some philosophers, the notion of a realised or second-order property helps understand why supervenience, which is at bottom a pattern of concurrent property-instantiations, ever holds at all. For the supervenient property, here fitness, is instantiated *in virtue of* the instantiation of one of its subvenient properties, in our case a trait-complex that is causally responsible for reproductive success. This is the main reason why the talk of realisation is more preferable. Plus, an idea very akin to that of property-ordering will prove to be a key to a satisfactory characterisation of selection in the next chapter. Nonetheless, the following arguments against the causal efficacy of fitness depend only upon its being a supervenient functional property. So we can take advantage of the notion of a second-order functional property without loss of generality of our arguments.

A note on property-ordering in our case before we proceed. Orders of properties are always assigned relatively. We take fitness to be a second-order property and all traits and trait-complexes first-order ones. This only signifies that fitness is one order above the latter. It surely doesn't imply that there are no properties one or more orders below traits. Nor does it imply that there is no ordering among traits. I believe that there is a plurality of intersecting ordering hierarchies below fitness. Yet none of these is our concern. The only ordering contrast relevant to the current debate is between fitness and traits. Also, first-order properties in our case are not all physical properties. Some traits, like shape and colour, are physical properties or readily reducible to physical properties. Other traits, like visual acuity and cold tolerance, are capacities or dispositions and are themselves functional properties and thus irreducible to other properties. Given the causal exclusion argument presented later, these latter traits cannot be causal properties either. But their physical bases (one or more orders below), which are still traits, are nonetheless causal properties. Thus, when saying that realisers of fitness are causally responsible for reproductive success, it is to be understood that the traits constitutive of a realiser are either physical traits in the beginning or physical traits that are physical bases of some functionally identified traits. So, although for functionally identified traits we'll directly mention them rather than their physical bases, there is no problem asserting that the first-order, realiser properties in our discussion are causally efficacious with respect to reproductive success in the final analysis. For this reason, we'll continue to take all traits as belonging to one and the same order for convenience.

3.3. The Problem of Metaphysically Necessary Dependency

Being a functional property, fitness as a causal property is affected by the problem of "metaphysically necessary dependency". This problem afflicts all functionally, i.e. causally, identified properties, including functionalist mental properties (Rupert 2006; there it is called the problem of "metaphysically necessary effects") and all genuine dispositional properties (Segal 2009). It takes the following basic form in our case: Given that the talk of fitter-than presupposes the unary property fitness, there is presumably a "causal law" corresponding to the (abbreviated modified) PNS that reads: An organism's fitness is causally responsible for its reproductive success. But, metaphysically, fitness *is* the causal-role property of having some trait-complex or other that is causally responsible for reproductive success. Accordingly, what is claimed by the "fitness law" is essentially this: An organism's having the property of having some trait-complex or other that is causally responsible for reproductive success, is causally responsible for its reproductive success. This claim is true, of course. However, it is a *conceptual truth*, which reveals the fact that the dependency of reproductive success upon fitness is a *metaphysically necessary dependency*. Causal dependency, the regularity expressed by a genuine causal law, is commonly taken to be metaphysically contingent. The same holds for tokens of a causal dependency as well. Since the dependency between fitness and reproductive success is metaphysically necessary, it is stronger than is allowed for a causal dependency in terms of modality and *ipso facto* is *not* a causal dependency. An organism's fitness *metaphysically necessitates*, but does not cause, its reproductive success. Hence, the "fitness law" is not a really a causal law, and fitness is not causally efficacious with respect to reproductive success.

Now, what has been explicitly said so far is about fitness taken as a determinable. Yet, as is evident in the talk of "being fitter than" and "difference/variation in fitness", fitness comes in degrees and is typically considered at the determinate level. And the same goes for traits and reproductive success. The current thesis, however, holds true for fitness determinates and the associated "fitness-determinate laws" too. This is only natural: Since fitness-determinates are nothing but determinates for a determinable causal role, they are themselves causal roles at the determinate level. A "fitness-determinate law" would say: An organism's having a certain fitness-determinable, say fitness-level *n*, is causally responsible for its having a certain degree of reproductive success, say reproductive-success-level *n* (which, if one likes, can be simply taken to mean the reproduction of a total of *n* offspring; the probability element is again omitted until we specifically deal with it later). Then again, fitness-level *n* is the causal-role property of having some trait-determinate-complex or other that is causally responsible for having reproductive-success-level *n*. So once again the "fitness-determinate law" states a conceptual truth: An organism's having the property of having some trait-determinate-complex or other that is causally responsible for having reproductive-success-level *n*, is causally responsible for its having reproductive-success-level *n*. This indicates that it is not a genuine causal law and that the dependency between fitness level and degree of reproductive success is metaphysically necessary and non-causal in nature. Consequently, having fitness-level *n* is *not* causally efficacious with respect to having reproductive-success-level *n*.

Given these, it is very easy to see that the PNS itself is not a causal law either. For the PNS is essentially saying this: Two organisms' being such that (they belong to the same population at the same generation, and) one of them has the property of having some trait-determinate-complex or other that is causally responsible for having reproductive-success-level *n*, and the other has the property of having some trait-determinate-complex or other that is causally responsible for having reproductive-success-level *m*, where reproductive-success-level *n* is distinct from reproductive-success-level *m*, is causally responsible for their being such that the former has reproductive-success-level *n* and the latter reproductive-success-level *m*. Or, better, it is, or is an entailment of, the conjunction of (any) two distinct "fitness-determinate laws" (given that the two organisms belong to the same population at the same generation). Either way, the PNS is conceptually true and therefore expresses a metaphysically necessary dependency as opposed to a causal dependency. Then again, the fitter-than relation or fitness-difference between two organisms (of the same population at the same generation), together with its determinates/degrees, is causally *inefficacious* with respect to having-greater-reproductive-success-than or difference in reproductive success between two organisms, along with its various determinates/degrees. By extension, multinary versions of the PNS are not causal laws either. Nor are fitness-variations among three, four up to all organisms of the same population at the same generation causally efficacious with respect to variations in reproductive success among three, four up to all organisms of the same population at the same generation, respectively. And all these are rooted just in the functional nature of fitness.

There are three things to note about the problem of metaphysically necessary dependency. First, it affects fitness and the fitness—reproductive success dependency (and the variational versions of these), not its realisers and the associated dependencies. Indeed, it is wrong to say that the dependencies between trait-complexes (determinates) and degrees of reproductive success are metaphysical necessary, for they are exactly causal dependencies. There is nothing contentious about the thought that a given dependency between a certain trait-complex and a certain degree of reproductive success does not obtain in some (metaphysically) possible worlds. But the dependency between fitness-level *n* and reproductive-success-level *n* obtains in all possible worlds (in which they are ever instantiated). One might conceive a case in which organisms having fitness-level *n* in virtue of having a certain trait-complex in a certain environment (regularly) have reproductive-success-level *m* in some other possible worlds that is distinct from the one they actually (regularly) have (i.e. reproductive-success-level *n*), and see it as a counterexample to the previous assertion. This would be a mistake, however. For in that case, those organisms have fitness-level *m* in those possible worlds, and reproductive-success-level *m* still depends upon fitness-level *m* there (*n* and *m* are dummies). So it is by no means a counterexample. Nonetheless, it shows that the trait-complex that is an actual realiser of fitness-level *n* realises fitness-level *m* in those possible worlds. This implies that the dependencies between trait-complexes and fitness-levels do not obtain in all possible worlds. Like the causal dependencies between trait-complexes and reproductive-success-levels, they are contingent dependencies even though they are non-causal ones. While this is perfectly acceptable, neither of these two sets of dependencies is to be confused with the metaphysically necessary ones between fitness-levels and reproductive-success-levels.

Second, this problem cannot be solved by appealing to any particular theory of causation. Semantically, the statement "an organism's having the property of having some trait-complex or other that is causally responsible for reproductive success, is causally responsible for its reproductive success" would remain a conceptual truth no matter which notion of causation is invoked. Metaphysically, the source of the problem is the functional nature of fitness, not the application of any particular theory of causation. Furthermore, theories of causation typically do not distinguish between the modal force associated with causal dependency and metaphysical necessity. As is observed by Kim (1973) in the case of the counterfactual approach, causal relation is but one sort of dependency, yet all sorts of dependencies, including necessary ones, can be expressed by counterfactuals. Accordingly, a dependency cannot be concluded to be causal just because it satisfies the criteria laid down by a theory of causation that are intended to distinguish causal relations from mere correlations. The dependency between fitness and reproductive success is non-causal not because it has no modal force. On the contrary, that's because its modal force is stronger than is allowed for a causal dependency.

Third, this problem is the metaphysical version of the tautology problem. Or rather, the tautology problem is essentially a metaphysical matter. The tautology problem is commonly apprehended as a (mere) semantic question that concerns what "fitness" or "being fitter than" means. Attempts to solve it have led to the proliferation of notions or interpretations of fitness. Yet the real question is *what fitness is*, given that there is such a property. And *that* is a metaphysical question. It cannot be answered by interpreting "fitness" one way or another. One doesn't locate the nature of fitness simply by supplying alternative names to the property (by, for example, renaming the property "overall solution to the design-problems" or "overall adaptedness"). It is the functional nature of fitness that gives rise to the problem of metaphysically necessary dependency, the semantic side of which is precisely the tautology problem, or more aptly, the tautology problem refined. It is not the original tautology problem since fitness is not flagrantly identified with actual or expected reproductive success. It is nonetheless a tautology problem because the claim that fitness is causally responsible for reproductive success results in a conceptual truth. Given that the tautology problem also has its roots in the nature of fitness, it cannot be solved by re-labelling fitness either. Any supposed interpretation of "fitness" has to be consistent with property's being the sort of second-order functional property as is proposed. If it is not, it is not about fitness. If it is, it just cannot solve the tautology problem.

However, the propensity interpretation of fitness is proposed precisely with an eye to solving the (original) tautology problem. A propensity is an indeterministic disposition. Whilst I don't oppose treating fitness as a disposition, I've avoided doing so. On the one hand, I don't want to invoke the standard semantic analysis of disposition (which doesn't allow a disposition to be said to cause its manifestation) and hence excludes the possibility of fitness's being causally responsible for reproductive success from the very beginning. On the other hand, fitness does not have a clearly defined or easily specifiable manifestation condition. Since the notion of a second-order functional property is broader than that of a based disposition, we simply stick to the former, noting that thinking of fitness as a disposition is basically correct and is not really distinct from our account. On the contrary, saying that fitness is an indeterministic disposition is a substantial commitment. We may accept that having a certain fitness level is probabilistic with respect to having a corresponding degree of reproductive success. But being indeterministic is over and above being probabilistic. In fact, as we shall see, the proponent of the propensity interpretation may just mean that fitness is a probabilistic disposition instead of an indeterministic one. Also, in terms of solving the tautology problem, what is really active in the propensity interpretation is merely that it probabilifies, and hence (supposedly) "weakens", the connections between fitness levels and degrees of reproductive success; the indeterministic component is nominal. In any case, we'll mainly take those connections, i.e. the "fitness laws", to be probabilistic but not necessarily indeterministic, considering that this is compatible with both determinism and indeterminisim. Then the question is: Does probabilifying fitness and the associated laws solve the problem of metaphysically necessary dependency, hence the tautology problem?

Not really. The first and foremost thing to note is that functionalist laws, i.e. laws concerning (realised) functional properties, are distinctively *ceteris paribus* laws. Most, if not all, laws require some (not systematically/completely specifiable) background conditions to obtain in order for them to hold. They may or may not be called *ceteris paribus* laws just because of that. However, functionalist laws are *ceteris paribus* laws for yet another, more significant reason. Each and every instance of a functionalist law is also an instance of some realiser law (i.e. a law concerning a realiser of a functional property). There are multiple realiser laws, and so instances of a functionalist law are subsumed under different realiser laws. These realiser laws are already each associated with a specific set of background conditions. For the functionalist law to hold, its own background conditions must include all these different sets of background conditions, which as a whole are too indefinite in number and too heterogeneous in type to permit of explicit specification (at any order). The *ceteris paribus* clause associated with a functionalist law is meant to subsume such assorted background conditions. It is an inherent part of a functionalist law, even though it is usually left out and only tacitly understood.

The fitness-level *n*—reproductive-success-level *n* law, as well as all variational versions of it including the PNS, is a functionalist law and hence a characteristically *ceteris paribus* law. Its *ceteris paribus* clause subsumes any and all background conditions associated with each and every affiliated realiser law, i.e. the trait-complex—reproductive-success-level *n* law, where the trait-complex (determinate) is a realiser of fitness-level *n*. Among those background conditions are recurrent environmental conditions and normal physiological and behavioural patterns for organisms having a particular trait-complex that realises fitness-level *n*, and, certainly, for all trait-complexes that are each a realiser of it. Unusual environmental conditions, aberrant physiological or behavioural patterns or deviant tokens, and accidental episodes that affect organisms' reproductive successes, then, are not subsumed under the *ceteris paribus* clause. They are among the reasons why organisms having fitness-level *n* do not all have reproductive-success-level *n*, i.e. why having fitness-level *n* is probabilistic with respect to having reproductive-success-level *n*. But that is so because the background conditions have been neglected. Suppose all non-typical cases are owing to irregular background conditions. Then the supposed probabilistic law turns out to be a *ceteris paribus* law in disguise. It is inappropriate to regard a distinctively hedged functionalist law simply as a conditionless probabilistic law. When the *ceteris paribus* clause reinstated, the probability terms should be discharged accordingly.

In consequence of the fact that the "fitness law" is a hedged law, the background conditions subsumed under its *ceteris paribus* clause become an integral part of the functional specification of fitness-level *n*. This leads to the latter's being identified by reference to its typical (supposed) effect-type, i.e. reproductive-success-level *n*. Put explicitly, fitness-level *n* is the causal-role property of having some trait-determinate-complex or other that is causally responsible for having reproductive-success-level *n*, *ceteris paribus*. Substitution of this into the *ceteris paribus* law yields: An organism's having the property of having some trait-determinate-complex or other that is causally responsible for having reproductive-success-level *n*, *ceteris paribus*, is causally responsible for its having reproductive-success-level *n*, *ceteris paribus*. Since both *ceteris paribus* clauses subsume exactly the same extent of background conditions, this is again a conceptual truth, which implies that the dependency between fitness-level *n* and reproductive-success-level *n* remains metaphysically necessary and non-causal despite the fact that it is conditioned. Therefore, in so far as the probabilistic aspect of the connection between having a certain fitness level and having a corresponding degree of reproductive success is just a matter of a characteristically hedged law taken without the hedge, it cannot “weaken” the conditioned dependency expressed by the hedged law and, as a result, cannot solve the tautology problem.

Is determinism tacitly assumed in the above? Not quite. However, to make things clearer, we have to make sure how indeterminism is supposed to bear upon our issue. Mutation may be indeterministic. Brandon and Carson (1996) have shown that quantum indeterminacy can “percolate up” to the occurrence of point mutations. This alone may suggest that evolution is indeterministic since mutation (that alters allelic type) is an evolutionary factor. But it doesn’t suggest that selection is indeterministic, for selection and mutation are independent evolutionary factors. To positively argue for the indeterminism about selection, one must show, by the same quantum percolation strategy or otherwise, that organisms’ physiological mechanisms and/or behaviours can be indeterministic, *and* such indeterminacy can give rise to the indeterminacy in organisms’ reproductive successes, *and* the latter indeterminacy can be massive enough so that many enough organisms having exactly the same trait-complexes can have significantly different reproductive successes under exactly the same environmental circumstances. Most of the principal figures in the current debate don’t think that this is the case (Matthen and Ariew 2002, 2009; Millstein 2003; Bouchard and Rosenberg

2004; Matthen 2009). The only notable exception is Brandon (Brandon 2005; Brandon and Ramsey 2007), who explicitly claims that selection, as well as evolution, is indeterministic in the sense of what he calls “autonomously indeterministic”. The basic idea is that if the results of a type of process cannot be non-probabilistically predicted solely by means of the variables available *within* our best theory about that type of process, then that type of process is (autonomously) indeterministic. No one would deny that the former holds true for selection. But that is a mere epistemic point. It is very akin to Matthen’s view that the deliberate suppression of probabilistically or explanatorily relevant factors that lie outside the domain of selection and evolution is the reason why explanations and predictions about the outcomes of selection are probabilistic (Matthen 2009). However, for Matthen this implies nothing whatsoever about the indeterminism about selection. Obviously, autonomous indeterminism is *not* indeterminism. Saying that selection is autonomously indeterministic is just saying that there are no hidden-variables within the domain of selection and evolution such that their incorporation into the known set of intra-domain variables can help explain or predict the outcomes of selection with certainty. It doesn’t say that there are no such variables, hidden or known, outside the domain of selection and evolution. By contrast, saying that selection is indeterministic is saying that there are no such variables *anywhere*. So, the autonomous indeterminism about selection does not imply that selection is indeterministic. On the contrary, it is compatible with the *determinism* about selection. Thus, Brandon does not truly claim that selection is indeterministic, and fitness is *not* a genuine propensity, i.e. indeterministic disposition, but is actually just a probabilistic disposition for him.

Now, the fact that no one in this debate truly asserts that selection is indeterministic surely does not mean that it is in fact not indeterministic. Even though it is not considerably indeterministic, it may be indeterministic all the same. Suppose it actually is. Would this make the dependency between fitness levels and degrees of reproductive success contingent and hence causal? I don’t think so. As said above, the indeterminacy presumably manifests itself in organisms’ physiological mechanisms and/or behaviours, including those due to the indeterministic occurrences of rare physical environmental conditions. If such indeterminacy does have impact upon reproductive success, it makes more sense to think that it is part of the connections between *realisers* of fitness (i.e. trait-complexes) and reproductive successes, than to say that it is part of the connection between fitness itself and reproductive success. It even seems invariably understandable as a source of abnormal physiological or behaviour patterns/tokens or environmental conditions. Either way, it can be accommodated by subsuming unlikely occurrences, deterministic or indeterministic, under the *ceteris paribus* clause that accompanies the “fitness law” and is at the same time also an inherent part of the functional specification of fitness. Hence, even if selection is indeterministic, there is no need to regard fitness itself as indeterministic, and the metaphysically necessary dependency between fitness levels and degrees of reproductive success stays intact.

What about fitness itself is just a real propensity and the connections between fitness levels and degrees of reproductive success are irreducibly probabilistic (even when they have been suitably conditioned and the background conditions have already included whatever realiser-order indeterminacies)? While this is difficult to imagine, in that case the probability patterns between fitness-determinates and degrees of reproductive success should be incorporated into their functional specifications, as in the case of probabilistic machine/mental-state transitions (Rupert 2006). Then, distinct causal-roles can differ with respect to their causal-*cum*-probability specifications as opposed to their causal profiles alone (i.e. with non-trivial probability specifications omitted). Anyway, since fitness-determinates now become both causally *and* probabilistically identified, the dependencies between fitness-determinates and degrees of reproductive success must remain metaphysically necessary. To wit: An organism's having the property of having some trait-determinate-complex or other that is likely, with likelihood p, to be causally responsible for having reproductive-success-level n, is likely, with likelihood p, to be causally responsible for its having reproductive-success-level n. The probability specification can be made more complicated but the point is the same. In so far as fitness is functionally identified, be it a disposition, a propensity or otherwise, at the determinable or the determinate level, it cannot be causally efficacious with respect to reproductive success. Nor are fitness-variations causally efficacious with respect to variations in reproductive success.

Seeing that the propensity interpretation cannot escape the tautology problem, Rosenberg (1982, 1991) deliberately keeps the notion of fitness as a primitive term within theory of evolution and thereby attempts to diffuse the tautology problem. The same idea can be traced back to Williams's axiomatisation of the theory of evolution (Williams 1970, cited in Byerly and Michod 1991a), where she makes "fitness" a primitive of the theory and declares that this dissolves the problem about defining it. Is this a plausible strategy to avoid the tautology problem as well as the problem of metaphysically necessary dependency, since they are really two sides of one coin?

Of course not. First, so long as one denies that the notion of fitness is mere expected reproductive success or growth rate and recognises "fitness" as designating a property, "fitness" is just a name of a property. Defining "fitness", if that makes any sense, is renaming the property. As we have seen, that is pointless. What is required, evidently, is an account of the nature of the property of which "fitness" is a name. Second, the problems to be solved are philosophical problems and any account of fitness that is supposed to be able to solve them has to be a philosophical account. "Fitness" might be a primitive of the theory of evolution, but that is irrelevant to the possibility of giving a substantial philosophical account of fitness. The philosophical problems won't cease to be real or relevant just because what fitness is cannot be accounted for by resorting to the conceptual inventory of the scientific theory of evolution alone. And, above all, one cannot posit a property without saying what sort of property it is. This is especially so when a claim about it is directly challenged. Those who insist upon the fitness-dependent characterisation of selection are obliged to come to grips with the problems concerning the causal efficacy of fitness. If not, they are merely dodging the problems.

We may take another perspective on this line of strategy. To all intents and purposes, it attempts to keep away from characterising fitness by reference to reproductive success. Yet fitness is nonetheless accepted as a supervenient property. So, we may further ask: Can fitness be a mere supervenient property without also being a functional property? The answer is no. For the question naturally arises as to what sorts of organismal properties are its subvenient properties. One cannot leave it unanswered, because an organism can possess other supervenient properties that are distinct from fitness (notably mental properties). Nor can one say that all traits of an organism are, since biologically there are indeed traits that are typically irrelevant to reproductive success and for that very reason we don't want to say that they conjoin with other traits to constitute some of the subvenient properties of fitness. Moreover, the contrast between selection and drift requires, in part, the distinction between those traits that regularly contribute to reproductive success and those that don't. And the distinction between traits that *causally* (as well as regularly) contribute to reproductive success and those that are *merely correlated* with the latter is of prime importance to those who emphasise the difference between selection-for and selection-of. Given that it is theoretically important to distinguish selection (i.e. selection-for) from drift and also from selection-of, the traits the conjunction of which is a subvenient property of fitness must be limited to those, and only those, that are jointly causally responsible for reproductive success. Once we have specified the range of trait-complexes upon which fitness supervene, the mere supervenient property ends up a second-order functional property.

Therefore, we have no choice but to recognise that fitness is a (realised) causal-role property. The fitness-as-a-primitive strategy clearly doesn't work. It simply fails to address the issue. So it solves or dissolves nothing, and the real problems stand still.

3.4. The Causal Exclusion Problem

Being a second-order or a supervenient property, fitness is also a target of the standard version of the causal exclusion argument. By "standard version" I mean the one that is systematised by Kim (1997, 2003) in showing that non-reductivist physicalist mental properties, and more generally all second-order or supervenient properties, are causally inefficacious. It has four general premises. As will be shown immediately, any of them either holds true for our case or is a widely agreed metaphysical principle. Hence, the consequence that fitness is causally inefficacious with respect to reproductive success is inevitable.

The first premise is the *irreducibility* of the properties the causal efficacy of which is in question to any of the properties the causal efficacy of which is accepted and is to exclude that of the former properties. Fitness is irreducible to any trait-complex. And it is beyond doubt that trait-complexes are causally efficacious with respect to reproductive success. So, apparently, the relation between fitness and trait-complexes satisfies the irreducibility thesis.

The second is the *supervenience* or *realisation* thesis: The causal-efficacy-to-be-excluded properties are supervenient upon or realised by the properties the causal efficacy of which is to exclude the former's. Fitness supervenes upon trait-complexes and is multiply realised by the latter. Thus this thesis also holds true in our case.

The third premise is what Kim (2003) refers to as "weak physical (causal) *closure*". In the discussion of mental causation, it is formulated in this way: If a physical event has a cause at an earlier time, then it has a physical cause at that time. Note that it alone doesn't exclude the possibility that the physical event has a mental cause at that time, because it doesn't say that the physical cause is the only cause then (and so it is intended to be distinguished from "strong closure", by which a physical event can only have physical causes). Its parallel in our case is: If there are some properties possessed by an organism that are causally responsible for its reproductive success (given all relevant environmental and background conditions), then some of its traits are causally responsible for that. This is very much a truism. And it is also a case of physical closure because traits, environmental and background conditions and reproductive success can all be regarded as belonging to the physical. As mentioned in the end of §3.2, when talking about causal efficacy, traits are either physical traits in the beginning or physical traits that are physical bases of some functionally identified traits. Environmental and background conditions here are all physical conditions. Reproductive success is basically a matter of total amount of offspring reproduced. It is usually thought of as a property of an organism but it is better considered an aspect of an organism's reproductive results. Either way, since it has to do with amount of objects, it may be regarded as physical or reducible to the physical. For the sake of argument, we just treat it to be physical. The only important thing is that fitness is neither a trait nor a physical property; it is distinctively functional. Nonetheless, as in the case of mental causation, the truth of weak physical closure doesn't by itself preclude the possibility that the same organism's fitness is causally responsible for its reproductive success.

The last premise is the causal *exclusion* principle: Barring genuine cases of causal overdetermination, an event or property-instance (or an aspect of a result) cannot have more than one sufficient cause at any earlier time. This is a metaphysical principle. It is intuitively acceptable and is widely acknowledged. Clearly, in conjunction with the closure thesis, it is what forces us to rule out the causal efficacy of all second-order or supervenient properties.

A step-by-step derivation of the exclusion argument against the causal efficacy of fitness analogous to the one given by Kim in the case of mental causation is due here. Suppose, given all relevant environmental and background conditions, an organism's fitness is causally responsible for its reproductive success. By the supervenience or realisation thesis, that organism also has some trait-complex, say T_c, upon which its fitness depends. By the irreducibility thesis fitness is distinct from T_c. Suppose its T_c is also causally responsible for its reproductive success. By the exclusion principle, since this is not a genuine case of causal overdetermination, either its fitness or its T_c, but not both, is causally responsible for its reproductive success. But which? Suppose its fitness is. Then, by the closure thesis, it must have some traits, either conjointly T_c or otherwise, that are causally responsible for its reproductive success. Then the exclusion principle applies again. The closure thesis and the exclusion principle would alternately apply unless and until some trait-complex is taken to be causally responsible and fitness not. And we should stop the regress as early as possible, by taking to be causally responsible the trait-complex that is first available as an option in the argument, i.e. T_c. Therefore, it is the organism's T_c, but not its fitness, that is causally responsible for its reproductive success. Thus completes the exclusion argument against the causal efficacy of fitness.

In so far as fitness is a second-order or a supervenient property, there is nothing specifically about it which one can resort to in order to avoid the causal exclusion problem. And none of the four premises seems abandonable. It’s not an option for those who recognise fitness as a property/disposition/propensity to give up the irreducibility and/or the supervenience/realisation theses, for otherwise fitness would not be the sort of thing they advocate and for them that would amount to the elimination of fitness. The closure thesis is self-evident. The exclusion principle cannot be discarded either since its purpose is to prohibit positing superfluous causes. So, if one still insists upon the causal efficacy of fitness, one has to demonstrate that the exclusion argument, in its general from, is somehow mistaken. To my knowledge, general objections that have been raised against the exclusion argument fall into three categories. Although this is not the place to give a full examination of them, before closing this chapter I want to briefly discuss them in order to point out that they are not enough to refute the exclusion argument.

First, many authors require that overdetermining causes should occur independently (Schlosser 2006; Shapiro and Sober 2007; Raatikainen 2010; Shapiro 2010; Carey 2011). The reason is that in genuine cases of physical causal overdetermination, the overdetermining causes are independent causes. This idea of independent occurrence, or something near, is regarded by those authors as constitutive of the very notion of causal overdetermination. Since the instantiation of a supervenient property depends upon the instantiation of one of its subvenient properties, an instance of a subvenient property of a supervenient one and the concurrent, dependent instance of that supervenient property are thus not deemed overdetermining causes. There is, then, no question of causal overdetermination in such occasions, and the causal efficacy of supervenient properties become compatible with that of subvenient properties. The exclusion principle is thus rendered irrelevant/inapplicable to second-order or supervenient properties, or it is viewed as problematic as it stands.

This line of objection is obviously *ad hoc*. It doesn't give any reason as to why dependent but distinct causes cannot be overdetermining causes. Besides, it intimates that the notion of causal overdetermination can always be tailored to one's purpose and thus can hardly be the real issue. The rationale underlying the exclusion principle is essentially a version of Occam's razor. When it is applied to properties, its content has been perspicuously articulated by Rives: "[W]e [should] not posit any more causally efficacious properties than we need in order to account for the causal powers of particulars" (Rives 2005:25-26). It doesn't hinge upon the notion of causal overdetermination, and it is no less intuitively acceptable and no less legitimate an ontological principle than the exclusion principle. We can directly appeal to it instead of the exclusion principle in the exclusion argument without being bothered by the notion of causal overdetermination, and the conclusion is the same. Unless one explicitly shows what is wrong with it, there is no reason not to accept it and its implications.

Second, some philosophers find the standard talk of realised properties as second-order properties unsatisfactory and put forward alternative conceptions of realisation or other notions to understand the relation between realised and realiser properties, for the purpose of solving the exclusion problem. Chief among them is Shoemaker (2001, 2007), whose subset theory of realisation says, roughly, that a property's being realised by a physical property is such that its causal profile is a proper subset of the causal profile of its physical realiser. Thus realised properties are first-order properties, and the causal powers which a realised property bestows upon an object are the same as some of the causal powers which its realiser property bestows upon the same object. Since realised properties confer causal powers upon objects, they are *ipso facto* causally efficacious. Similar strategies include taking the relation between mental and physical properties as that between determinables and determinates (Yablo 1992), along with its various descendents which propose such metaphysical notions as “inclusion” (Schlosser 2006) and “co-instance” (MacDonald 2007) to explicate the relation between instances of realised properties and instances of realiser properties. In all of these, instances of realised properties and the concurrent instances of the corresponding realiser properties are tightly related in such a way that the causal efficacy of the former just coincides with (part of) the causal efficacy of the latter. Because of this, realiser properties are reckoned as causally efficacious properties.

Without going into detail about each proposal and its specific problems, I agree with Kim (2010) that this line of strategy assumes the conclusion from the very start: The theories or notions that are supposed to be able to explicate the relation between realised and realiser properties or between their instances have presupposed that realised properties do confer causal powers. If the exclusion problem is real, it won’t help to redefine “realisation” or to devise new concepts so that the exclusion argument as it stands doesn’t seem to go under alternative accounts of the relation between realised and realiser properties or between their tokens. According to Kim, the original problem of whether realised properties have causal efficacy is just verbally transformed, in the subset model of realisation, to the problem of whether they are “realised” at all, that is, whether they do have causal profiles or confer causal powers at all. In addition, theories modelled upon the determinable-determinate relation are poor ones because determinables are subject to ontological elimination (Heil 1999; Gillett and Rives 2005). And, if any alternative to the second-order view about realised properties is aimed at motivating the idea that, since the instantiation of a realised property depends upon the instantiation of any of its realisers, the causal efficacy of an instance of the realised property just depends upon that of the depended instance of the corresponding realiser but instances of realised properties nonetheless have causal efficacy, then that is plainly in vain. That is something like the notion of "supervenient causation" or the principle of "causal inheritance" which Kim himself advocated in the past but has long renounced. As he observes, supervenient causation and inherited causal efficacy are still subject to the exclusion problem, unless one simply stipulates that they are not so (Kim 2003).

The third objection is the "generalisation problem" raised by Block (2003), which concerns the overwhelming consequence of the exclusion argument. It initially arises against the backdrop of the idea of a layered ontology, in which the psychological constitutes an autonomous level and mental properties are regarded as higher-level properties. If mental properties' causal efficacy is excluded by physical properties' causal efficacy as dictated by the exclusion argument (which is originally directed at mental properties), then, by the same token, all other higher-level properties' causal efficacy should also be excluded by lower-level properties' causal efficacy. The result is that only the properties of fundamental particles can be causally efficacious, given that the level of fundamental particles is the bottom level for the conventional layered view of reality. This is intended as a *reductio*: Since the exclusion argument leads to such an implausible consequence, it should be abandoned and one need not worry about the causal efficacy of mental and all other higher-level properties.

It is always possible to rebut such an objection by insisting that we cannot but bite the bullet in so far as the argument is sound. But we have to ask the more substantial question: Does the exclusion argument generalise to higher-level properties? It is crucial to make the distinction between second-order properties and higher-level properties. There are properties that are essentially functionally identified and those that are not. When the causal-role that is identified with a functional property is satisfied by each and every property from a set of properties, the first property is said to be a property over, or an order above, the latter set of properties. Thus the order-distinction is purposively a distinction between functional properties and their realisers. On the other hand, the level-distinction is used much less systematically. In the usual level-talk, including the talk of a layered ontology, the level-distinctions are really a mixture of order-distinctions and macro-micro distinctions (Kim 2002). The latter sort of distinction is based upon the mereological relation: Macro-objects are mereologically composed of (not always homogeneous) micro-objects, and macro- and micro-properties are respectively properties of macro- and micro-objects. Such a distinction is evidently very different from the order-distinction, and it is useful to distinguish them apart by reserving the notion of level for it.

Now, as observed by Kim (1997), the order-distinction does not track the level-distinction. It is part of such notions as supervenience, realisation and hence property-ordering that the two sets of properties bearing these relations are properties of the same objects, i.e. properties of the same level. And properties of different levels can not be possessed by the same objects at a fixed level. The implication is obvious: Properties of a given level cannot be supervenient upon or realised by properties of a different level. As a result, the exclusion argument does not apply to higher-level properties, because the supervenience/realisation thesis does not hold for properties that are of different levels. This should come as no surprise: An object’s causal powers, which are endowed by its own properties, are distinct from any of its micro-constituents’ or its proper part’s causal powers, which are endowed by the latter’s own properties. Hence the causal efficacy of higher-level properties is not excluded by the causal efficacy of lower-level properties. So, none of these three lines of objections is well founded.

3.5. Summary and Prospect

Now a brief summary of what we’ve discussed about fitness and the PNS in this chapter. If there is an organismal property called "fitness" that grounds the talk of expected reproductive success or expected growth rate, then it is a second-order (or supervenient) functional property. Because it is a functional property, it, as well as the dependency between it and reproductive success, suffers from the problem of metaphysically necessary dependency. The same goes for its determinates, difference/variation in its determinates and all associated dependencies. Therefore, fitness-levels and fitness-difference/variation are not causally efficacious with respect to degrees of reproductive success and difference/variation in reproductive success, respectively. Nor is the PNS a causal law. All these remain true even when the connections between fitness-levels and degrees in reproductive success are taken as probabilistic or even indeterministic. Moreover, because fitness is a second-order property, it cannot escape the causal exclusion problem. Thus, again, fitness, *etc*., are not causally efficacious properties and the PNS is not a causal law.

If one thinks that selection is characterised by the PNS or any version of the "fitness law", then selection cannot be a causal process-type, even though each instance of selection is no doubt a causal process. Conversely, since selection is considered a causal process-type, it cannot be characterised by a type of connection that has fitness or its relative as its causal term. Fortunately, a fitness-free characterisation of selection not only is possible but also has been actually proposed. That proposal is not problem-free as it stands but it is good start. At least it is not troubled by the problems raised in this chapter. Lastly, I believe that the property of fitness has neither theoretical function nor explanatory utility and hence is redundant. If a fitness-free characterisation of selection is plausible, that would itself be a cogent reason to dispense with fitness. As we shall see in the next chapter, this is indeed the case.

4. Selection with Respect to a Trait as a Type of Process and Its Characterisation

Both Matthen and Ariew's and Bouchard and Rosenberg's characterisations of selection contain the term "fitness", although it has diametrically opposite meanings in them. Both of them are necessarily true (albeit for different reasons), although for Matthen and Ariew that is exactly part of the nature of selection whereas for Bouchard and Rosenberg (and all advocates of the propensity interpretation of fitness) it is a problem to be avoided. And selection is not a causal process-type in either account, although Matthen and Ariew recognise and embrace that (even though selection is not a type at all in their account) while Bouchard and Rosenberg (as well as the proponents of the propensity interpretation) refuse to acknowledge it. Apparently, however "fitness" is interpreted, no fitness-dependent characterisation of selection can make selection a causal process-type and genuinely explanatory, which are presumably two major desiderata for an adequate account of selection. This suggests that we should seek a fitness-independent characterisation of selection if possible. Millstein (2006) gives such a characterisation. We'll explore it in this chapter. Specifically, I'll show that it has initial plausibility but has metaphysical/ontological problems as well. In the course of tackling those problems a more satisfactory characterisation of selection is hinted. It will be made explicit and a case made for it at the final part of the chapter.

4.1. The Fitness-Free Characterisation of Selection and the Redundancy of Fitness

Millstein (2006) brings forth a characterisation of selection that makes no mention of fitness. It is quite straightforward. It reads: Selection is “a discriminate sampling process whereby physical differences between organisms are causally relevant to differences in reproductive success” (Millstein 2006: 640). It is intended to contrast with drift as “an *in*discriminate sampling process whereby physical differences between organisms are causally *ir*relevant to differences in reproductive success [italics added]” (Millstein, *loc. cit.*). Adhering to the conceptual distinction between selection and drift propounded by Beatty (1984), this pair of characterisations also looks upon both of them as sampling and distinguishes between them in terms of sampling discriminacy. That distinction will be put aside, since I agree, with (almost) all other main participants in the current debate, that drift is nothing but (cases with) non-typical reproductive results, and since we are concerned with selection alone. In addition, we won’t speak of selection as sampling: It is not literally sampling but is apprehended via the analogy with sampling. That analogy, however, tells us nothing about the nature of selection more than the well accepted fact that selection is about population of organisms and it is (somewhat) random/probabilistic (or, better, the laws/regularities characterising it are *ceteris paribus* laws/regularities, which are said to be probabilistic ones when taken absent the hedge). Hence, I’ll regard Millstein’s characterisation of selection as simply saying that selection is such a process in which physical variations, or variations in physical properties, amongst organisms (of a population within a generation) are causally responsible for variations in their reproductive success. This makes it plain that Millstein’s characterisation of selection differs from the variational version of the “fitness law” discussed in the last chapter only by a word: “Fitness” is replaced by “physical (properties)”. As we shall see, that makes a world of difference.

A few semantic points concerning the talk of physical properties and variations in physical properties in the present context should be noted before we embark upon the main task. Millstein's talk of the "physical" actually regards traits. As has been pointed out in the previous chapter, traits are not all physical properties. But, as was also said there, in cases where causal efficacy of properties is an issue, a trait is always tacitly understood to be either a physical trait as it stands or a physical trait that is one of the physical bases of some functionally identified trait. So, traits in our context should include only physical traits, and for that reason I'll use "trait" and "physical (property)" interchangeably like many other authors do. Further, the talk of (there being) a variation in a physical property, or trait, amongst organisms of a population within a generation is intended to convey this: The organisms therein do not have the same determinate of a certain trait-determinable. For this to be true, tokens of at least two different determinates of that determinable have to be present, although the exact number of tokens of each of those determinates, the exact number of those different determinates (so long as it is greater than one), and how different are those determinates (as well as how different counts as different, which is largely a matter of biology) are all irrelevant. What is important here is that a trait-variation is said of a whole population: It is not said of organism-pairs, organism-triplets or the like of which no two organisms have the same trait-determinate of a certain trait-determinable, and thus any trait-variation is *not* a (binary, ternary or whatever) relation between organisms. A variation in a trait is intended by Millstein to be a *property of the whole population*. This will be tentatively *assumed* until later we come back to it. With these in mind, we now go on to see the metaphysical and epistemic implications of fitness-free characterisation of selection as compared to the customary, fitness-dependent ones.

Metaphysically, the fundamental difference between fitness-dependent and fitness-free characterisations of selection is this: Selection as identified by the fitness-dependent characterisation is *a* process-*type* (albeit not a causal one), whereas selection as depicted in the fitness-free characterisation is *not* a process-type. The source of this difference lies in the fact that fitness is a property but traits, or physical properties of organisms, are a multitude of properties (all considered at the determinable level). If the talk of variation in a property itself also picks out a property, then, by extension, variation-in-fitness is a property but variations-in-traits, or trait/physical-variations, are a myriad of properties. Presumably, dependencies/regularities and causal types are type-individuated by both the dependent- or cause-types and the depending- or effect-types. Therefore, the variation-in-fitness—variation-in-reproductive-success dependency is *a* dependency and identifying selection as a dependency is characterising it as *a* type (of process). By contrast, “variations in traits are causally responsible for variation in reproductive success” does not express *a* dependency or *a* causal type, since “variations in traits” is not a name of a property/type but an indefinite plural noun phrase that does not pick out any fixed set of properties/types. There are indefinitely many traits the variation in each of which is causally responsible for variation in reproductive success. For any two different such traits T_1 and T_2, the variation-in-T_1—variation-in-reproductive-success (supposedly causal) dependency is distinct from the variation-in-T_2—variation-in-reproductive-success dependency. Consequently, in the fitness-free characterisation of selection there is actually an *open set of distinct process-types*, each of which is characterised by a particular dependency between variation in a *particular* trait and variation in reproductive success.

So, in the fitness-dependent characterisation selection is *a* process-*type* characterised by a dependency the dependent property of which is variation in a *second-order functional property* (i.e. variation-in-fitness), whereas in the fitness-free characterisation there is an open *set* of process-*types* each of which is characterised by a dependency the dependent property of which is variation in a *first-order physical* property (e.g.

variation-in-T_1). Let's call the process-types in that set "selection with respect to T_1", "selection with respect to T_2" and so on. These are the determinable correspondents to selection for some determinate of T_1 (or against some other determinate, depending upon the determinate under the focus of description in a particular case), selection for some determinate of T_2, *etc*. Then, in the fitness-free characterisation, saying that a population-wide generation-long process is a case of selection is saying that it is at least an instance of selection with respect to a particular trait (determinable), which in turn is saying that it is at least an instance of selection for some determinate of that trait. There is thus nothing in it corresponding to selection *simpliciter* in the fitness-dependent characterisation. This suggests that the unqualified term "selection" in this account serves only as a case-specific abbreviation of "selection with respect to T_1", *etc*. There is strictly no selection as a process-type in it; instead, there are only a lot of process-types that are selection with respect to T_1, selection with respect to T_2, so on and so forth.

Alternatively, one may extend the idea of property-ordering to process-types and regard selection as a second-order process-type over the open set of process-types that are each selection with respect to a particular trait, since a population-wide generation-long process is a case of selection in virtue of its being at least an instance of selection with respect to a particular trait. More substantially, instances of selection with respect to different traits, that is, of distinct dependencies between variations in different traits and variations in reproductive success, are similar in respect of the depending property-instances: Their depending property-instances are all variations in reproductive success. Certainly they cannot be subsumed under a single dependency or causal type just because of that. But they may nonetheless be considered to constitute a second-order process-type for that very reason. At the very least they are all called "selection", and it is fine to have a metaphysically admissible way to accommodate this practise.

Nevertheless, whether or not selection as a second-order process-type is recognised over and above the indefinitely many first-order process-types, it remains true that there is no dependency or causal type to be identified as selection *tout court* in the fitness-free characterisation. In the eyes of the adherent of the fitness-dependent characterisation that is exactly the consequence of eliminating fitness and dispensing with the like of the PNS and is bound to be intolerable. More importantly, it may be objected that the fitness-free characterisation essentially splits a law/dependency and a process-type into indefinitely many laws/dependencies and process-types, and hence fails to capture the explanatory generality of the notion of selection as well as the law/dependency that characterises it. For there is neither a single property nor a single law/dependency which can be cited to explain every case of variation in reproductive success amongst organisms of a population within a generation; nor is there selection *simpliciter* which can explain every case of evolution due to variation in reproductive success. This is precisely the epistemic implication of the fitness-free characterisation of selection. It may seem to pose a grave problem to the fitness-free characterisation since generality is a real concern. Then, we may have a compelling reason to reject the fitness-free characterisation in favour of the fitness-dependent one, even though selection is not a causal process-type in the latter. However, as I shall argue below, explanatory considerations do not motivate the thought that there is a single process-type that is selection *simpliciter*. And, above all, fitness is explanatorily redundant and there is thus no reason to accept the fitness-dependent characterisation of selection.

First of all, unless (for certain species) a token of a particular T_1-determinate is regularly co-present with a token of a particular T_2-determinate, a token of the other particular T_1-determinate is regularly co-present with a token of the other particular T_2-determinate and so on (due to extensive overlapping genetic pathways of T_1 and T_2), selection with respect to T_1 does *not* explain evolution *in respect of* T_2, i.e. cross-generational differences in one or more frequency distributions of organisms (of a given population) by allele-type for one or more *genes primarily related to* T_2. The general statement that selection with respect to T_1 causes evolution in respect of T_2 is plainly wrong. And it is wrong not metaphysically but empirically. These clearly imply that there is no single type of change/difference (or single feature/aspect of collective reproductive results) that is evolution *simpliciter*. This is even implicated in the population-genetic definition of evolution, in which “evolution” is partially defined by an indefinite noun phrase “allelic frequency distribution” that does not pick out a particular type of allelic frequency distribution, as frequency distributions by allele for different genes (especially for different genes related to different traits) are biologically different features of a population of organisms or different aspects of collective reproductive results. Accordingly, changes/differences in different types of allelic frequency distributions are different types of changes/differences. Thus, instead of a single type of change/difference, there is actually an open set of distinct types of change/difference that are each evolution in respect of a particular trait.

Like in the case of selection, one may recognise a second-order type of difference over those indefinitely many types of difference. But the crucial point here is that if tokens of those different first-order types of difference are due to variation in reproductive success, then they call for different types of explanations that cite variations in different traits and laws/dependencies between variations in different traits and variations in reproductive success, amongst other things. That is, they are explained by citing instances of different process-types that are each selection with respect to a particular trait. So, explanatory considerations actually suggest that we distinguish between selection with respect to T_1, selection with respect to T_2 and so forth, as well as between the different dependencies that characterise them each, rather than demanding that we recognise a single law/dependency which characterises selection *simpliciter*.

What about the general statement “selection causes evolution”, then? We don’t have to understand it as literally expressing a causal type (or a process-product type). Instead, it can be regarded as an abbreviation of “selection with respect to a particular trait causes the evolution in respect of that trait”. I believe that we always tacitly understand “selection causes evolution” in that way. And undeniably concrete explanations do and can only work that way. There is simply no such explanation as selection *simpliciter* explains evolution *simpliciter*; nor is there such causal type as selection *simpliciter* causes evolution *simpliciter*. The sentence “selection causes evolution” isn’t a genuine generalisation that is more general than “selection with respect to T_1 causes evolution in respect of T_1”. It is an incomplete expression or, so to speak, a generalisation schema, which requires a particular trait to be specified in order to function as a concrete generalisation and to express a definite causal type. Potentially, there are as many such definite general causal statements as there are traits; while they are similar to one another they remain nonetheless distinct from one another. This well motivates and squares with the thought that there are as many process-types that are each selection with respect to this or that trait as there are traits, and they are all different albeit all similar.

In so far as selection *simpliciter* plays no explanatory role, its posit is simply redundant. This implies that the fitness-dependent characterisation of selection is inadequate, for it takes selection to be precisely such a single (first-order) process-type. The latter in turn implies that the posit of fitness has no theoretical import, since the whole point of positing such a single property is just to make possible the single law, i.e. the PNS or the variational version of it, that characterises the single process-type selection *simpliciter*. The theoretical redundancy of the posit of fitness, plus the causal inefficacy of the posited property, strongly suggests that we should abandon that posit. If these are not enough, a third reason can be added: Fitness itself, and fitness-difference/variation as well, does no explanatory job. This is an inevitable consequence of the lack of explanatory utility of selection *simpliciter* in conjunction with the fact that the sole theoretical function of the posit of fitness is to characterise selection *simpliciter*. The same point, however, can be argued in more substantial ways, some of which are given as follows.

First, variation in reproductive success *with respect to* T_1—which is such that within a population (and generation) of organisms, many enough of those having a particular T_1-determinate have around a particular degree of reproductive success, many enough of those having the other particular T_1-determinate have around the other particular degree of reproductive success, so on and so forth—and variation in reproductive success *with respect to* T_2 are obviously *different* collective features of a population of organisms (or *different* aspects of their collective reproductive results). Again, unless (for certain species) a token of a particular T_1-determinate is regularly co-present with a token of a particular T_2-determinate, *etc*., variation in reproductive success with respect to T_1 is *not* explained by variation in T_2. A token of variation in reproductive success with respect to T_1 is explained by citing variation in T_1 and some environmental conditions for the particular population, in conjunction with the generalisation/law saying that variation in T_1 is causally responsible for variation in reproductive success with respect to T_1 in the environmental conditions similar to those for that population (*ceteris paribus*). So, tokens of different types of variation in reproductive success that are each variation in reproductive success with respect to a particular trait are explained by citing, amongst other things, tokens of different types of trait-variation that are each variation in a particular trait, along with different laws/generalisations each of which expresses a (conditioned) dependency between variation in a particular trait and variation in reproductive success with respect to *that* trait. Apparently this applies to all tokens of all types of variation in reproductive success. In consequence, no single explanatory work is left for fitness-difference/variation and the like of the PNS.

A further, related argument is this. Suppose a population of organisms have both the collective features of variation in reproductive success with respect to T_1 and variation in reproductive success with respect to T_2 (which implies that they also have variation in T_1 and variation in T_2). Also suppose they exhibit no statistical dependency between T_1-determinates and T_2-determinates and all environmental and background conditions are normal. What collective features of that population of organisms explain the variations in their reproductive successes? The answer: Variation in T_1 amongst them explains the variation in their reproductive success with respect to T_1, *and* variation in T_2 amongst them explains the variation in their reproductive success with respect to T_2. What is left for variation in fitness amongst them to explain here? Absolutely nothing. Does the explanations given above separate explanations? Yes. Does that imply that variation in T_1 and variation in T_2 are separately causally responsible for variation in reproductive success with respect to T_1 and variation in reproductive success with respect to T_2? Not at all. It only indicates that the particular population-wide generation-long (causal) process is subsumed under two different causal types and hence there are two distinct causal factors. Yet that alone suggests nothing whatsoever about whether and how the two different causal factors interact. There is thus no worry of returning to the old Soberian force view. And there is no need to posit a distinct, "composite" collective feature variation in fitness that takes care of the joint causal contribution of all trait-variations that jointly causally contribute to all facets of variations in reproductive successes. That is utterly superfluous.

Similar thing can be said of fitness itself and the fitter-than relation. Suppose, in a certain type of environment and for organisms of a certain species, having a particular T_1-determinate and having a particular T_2-determinate, along with other traits relegated to the background, (jointly) causally contribute to having reproductive success level n (or a level around that), and having the other particular T_1-determinate and having the other particular T_2-determinate, along with other traits, causally contribute to having reproductive success level m (which is lower than level n). Suppose, within a particular population (and generation) of organisms belonging to that species and living in that type of environment, an organism having the first T_1-determinate and the first T_2-determinate had reproductive success level n, and another organism having the second T_1-determinate and the second T_2-determinate had reproductive success level m, with all environmental and background conditions being normal (including both having the traits relegated to the background). What properties of the two organisms explain their respective reproductive successes? Evidently, the former organism's T_1-determinate and T_2-determinate together explain its having reproductive success level n and the latter's its having reproductive success level m. There is definitely no need to invoke an explanatorily intervening property-token that is the former organism's fitness level n, such that it alone explains that organism's having reproductive success level n and it is itself explained by citing, amongst other things, that organism's T_1-determinate and T_2-determinate (or these plus all those traits of it that are left to the background). Likewise, the differences in T_1 and in T_2 between the two organisms, along with certain features of the environment for the population, together explain their difference in reproductive success (by that degree). It is completely superfluous to say that those two organisms had those particular differences in T_1 and in T_2, *so that the former was fitter than the* *latter (by the corresponding degree)*, so that the former had better reproductive success than the latter (by the corresponding degree). The intervening part serves no explanatory purpose whatsoever. The joint causal contribution of all traits of an organism that jointly contribute to its reproductive success or of all trait-differences of a pair of organisms to their difference in reproductive success cannot justify the posit of a distinct property or relation proprietary to reproductive success or the difference thereof. Such a posit is doomed to be explanatorily inert.

The theoretical, causal and explanatory redundancy of fitness and all its relatives command that we reject their posits. Fitness as a property of an organism simply *does not exist*. Nor do fitter-than as a relation between two organisms and variation-in-fitness as a property of a population of organisms. Their non-existence implies that "fitness" does *not* pick out a property, which should be fine if we stick to the population-genetic notion of fitness as expected/actual reproductive growth rate or expected reproductive success. It also implies that the talk of expected growth rate or reproductive success is *not* grounded in a *single* property: As stressed by Byerly and Michod (1991a), expected growth rate is expected based upon *a lot* of properties that are organisms' *traits* as well as environmental conditions. It is not expected based upon some proprietary property called "fitness". And, quite clearly, expected reproductive success is literally the estimator of reproductive success; it is not the estimator of some attribute called "fitness". There is no such a notion as "expected fitness" in population genetics. There is no such a law as the PNS in evolutionary biology either. The property of fitness and the like of the PNS are both extra posits. They are futile philosophical investments in the unfortunate Huxley-Darwin cliché. There are always ways to make sense of the ordinary biological talk of fitness and fitter-than without positing fitness or the PNS. There are always traits and trait-differences/variations to do the entire explanatory work and to assume the whole causal responsibility in respect of reproductive success and the difference/variation thereof. Plus, it is trait-variations, as opposed to fitness-variation, that are relevant to the adequate characterisation of selection. Hence, we are justified to the maximum extent to reject both posits of fitness and the PNS, together with the fitness-dependent characterisation of selection.

The fitness-free characterisation of selection bypasses fitness and the like of the PNS and so avoids all the troubles rooted in those posits. It essentially takes selection as a family of physically distinct process-types, which is adequate as the notion of selection *simpliciter* is useless and there is no genuine and more general law or generalisation that can correspond to such a single (first-order) process-type. However, the fitness-free account of selection as advanced by Millstein asserts, in addition, that selection is a "population-level causal process", in the sense that it is such a process that trait-variations, as themselves "population-level properties", i.e. properties of a population, are causally responsible for variations in reproductive successes (with respect to the corresponding traits). This is purported to contrast with Matthen and Ariew's view that selection is a mere aggregation of "individual-level" or organismal-level occurrences and not itself a causal process. Two questions immediate arise: Are such properties really causally efficacious? And are trait-variations properly considered properties? To be sure, these two questions do not exclusively concern Millstein's account: The first also concerns all variants of the fitness-dependent characterisation claiming that fitness-difference/variation is causally responsible for difference/variation in reproductive success, and the second has to do with the variant which regards fitness-variation itself as a property as well. But, since the fitness-dependent characterisation has been discarded, we'll take trait-variations as the sole example in the next discussions.

4.2. The Causal Inefficacy of Population-Level Properties

In justifying the ordinary talk of selection in §2.3, I argued that every case of selection is a macro-causal process and affirmed that selection is a type of process. Now, in light of the above arguments, the latter affirmation has to be corrected: Selection with respect to a particular trait, as opposed to selection *tout court*, is a type of process, and selections with respect to different traits are different types of process. But, selection with respect to T_1, for example, is nonetheless a process-type, and any token of it (and/or of selection with respect to any other trait as well) is still a macro-causal process. So far, however, I have asserted the latter only in the minimal sense of a population-wide causal process as a collection of all and only those organismal causal processes each of which involves some organisms of a given population within a given generation. Also, asserting this alone is far from specifying selection with respect to T_1 as a (unique) type of macro-process, let alone a type of macro-causal process. By contrast, Millstein's account of selection, that selection with respect to T_1 is such process that variation-in-T_1 is causally responsible for variation in reproductive success with respect to T_1, is obviously over and above that. It is essentially about the type. It effectively specifies selection with respect to T_1 as a type of macro-causal process. It asserts that a token of selection with respect to T_1 is a macro-process in the sense of involving a population of organisms that instantiates the population-level property variation-in-T_1 and having the collective reproductive results that exhibit the collective feature variation in reproductive success with respect to T_1. And it is tantamount to claiming that a token of selection with respect to T_1 is a causal process in virtue of its constitutive property-token variation-in-T_1's being causally responsible for the tokening of variation in reproductive success with respect to T_1 in its collective reproductive results.

Now, if variation in T_1 is not properly considered a property, then selection with respect to T_1 cannot be a type of macro-process in Millstein's sense of a population-level process. If it is indeed a property, but its instantiation is not causally efficacious with respect to the occurrence/tokening of variation in reproductive success with respect to T_1, then selection with respect to T_1 cannot be the type of causal process intended by her. In this section, we *assume* that variation in T_1 is a (population-level) property and proceed to ask whether it is a causally efficacious property. We'll see that such a supposed property has no causal efficacy at all, in consideration of the exclusion-style argument again, or something very akin to it. So, whilst it may be fine to characterise selection with respect to T_1 in terms of the "variation in T_1—variation in reproductive success with respect to T_1" generalisation/law, it is incorrect to regard the latter as a causal generalisation/law or, which is the same, to say that tokens of selection with respect to T_1 are (population-level) causal processes in virtue of the respective instances of variation-in-T_1 involved being causally responsible for the respective tokening of variation in reproductive success with respect to T_1.

We begin by first considering Haug's (2007) defence of Millstein's view against Matthen and Ariew's formal-pattern account. Continuing their talk of selection as being "realised", Haug proposes a notion of "causal realisation" of process and claims that under that notion, tokens of selection (with respect to a particular trait, say T_1) are causal processes in virtue of their being tokens of selection in Millstein's sense of a type of population-level causal process. The notion of causal realisation is based upon Gillett's (2003) "dimensioned" view of realisation, by which a higher-level property is realised by a set or combination of lower-level properties and relation(s) just in case all objects instantiating the former property instantiate it in virtue of their (proper) parts or (micro-)constituents instantiating the latter properties and having the latter relation(s). This is expanded by Haug into his notion of causal realisation: A (macro-)causal process that is a (macro-)causal connection between a higher-level property-instance and another, is said to be causally realised by a plurality of (micro-)causal processes that are each a (micro-)causal connection between a lower-level property-instance and another, just in case the two higher-level property-instances that are the cause-term and the effect-term, respectively, of the first (macro-)causal connection are realised, respectively, by the two combinations of lower-level property-instances that are the cause-terms and the effect-terms, respectively, of the latter set of (micro-)causal connections. He explicitly asserts that a population's variation-in-T_1 is realised by the combination of its members' T_1-determinates, and presumably holds that a population's variation-in-reproductive-success(-with-respect-to-T_1) is realised by the combination of its members' respective degrees of reproductive success. These, together with his idea of causal realisation of process, allow him to say that a token of selection with respect to T_1, a population-level causal process that is a causal connection between an instance of variation-in-T_1 and an instance of variation-in-reproductive-success-with-respect-to-T_1, is causally realised by a plurality of organismal-level causal processes, each of which is presumably a causal connection between the T_1-determinate of an organism (that is a member of the given population) and its degree of reproductive success. This, then, is taken by him as having established that, in spite of the fact that tokens of selection with respect to T_1 are realised by sets of organismal-level causal processes, they are causally realised and therefore are themselves population-level causal processes.

Against Haug's argumentation I raise three criticisms. First, he totally misses Matthen and Ariew's idea of selection, yet takes for granted their misguided talk of realisation. He apparently thinks that Matthen and Ariew maintain that selection as normally understood is not a causal type because it is realised. This is certainly wrong. As we see in chapter two, when Matthen and Ariew say that selection is realised in physically different substrates, what they mean by “selection” is a formal pattern, not a (supposedly) causal type. They never claim that selection as ordinarily understood is multiply realised. I have shown that there is nothing functional in their idea of selection, and thus their talk of realisation is actually an abuse of that notion. And the reason that selection as understood by them is not a causal type lies in the fact that the content of their notion of selection is an arithmetic theorem, a reason that they recognise and has nothing whatsoever to do with realisation. As regards the conventional talk of selection as a causal process, however, it is their fault to think that it is inconsistent with the constitution thesis, that a case of selection is wholly constituted by (certain) organismal-level occurrences. But they nonetheless don’t affirm that a case of selection is not a causal process because it is realised, as opposed to being constituted, by a collection of organismal-level occurrences, or causal processes. So, their (mistaken) reason against the causal-process view of selection as normally understood does not hinge upon their (misguided) talk of realisation, either. Taken as it is, then, Haug’s argument is not quite a defence against Matthen and Ariew, contrary to what he himself conceives.

Second, Haug’s own talk of realisation, while different from Matthen and Ariew’s, is misguided too. In the standard talk, whatever is realisable is multiply realisable and is a functional property, whatever realises a functional property is itself a single (physical) property and different realisers of the same functional property are mutually distinct properties. It is fine to speak of “process realisation” in Haug’s sense since it is nothing but a definition that builds upon the common talk of property realisation. We may also grant the dimensioned view of realisation upon which his whole talk of selection as realised process depends (since, as Kim himself says, one can freely define “realisation” so long as the resulting notion is useful in elucidating philosophical problems). Yet, even if trait-variations and variation in reproductive success are deemed properties, they are by no means functional properties. Then in exactly what sense can a property like variation-in-T_1 be said to be multiply realisable? That is, what distinguishes between two supposedly different realisers of it?

Haug just leaves this fundamental question unanswered because, when talking about variation-in-T_1's realiser, he is talking about token-combination of property-tokens instead of *type of combination* of property-tokens. Any realisation relation is essentially a type-type relation. It's alright to take realisation as a token-token relation at bottom, but then what is important is realisation type, and to identify a realisation type is to identify the pair of types each token-pair of which is a realised-realiser pair. So, token-realisers of (tokens of) the same supposedly multiply realisable type must themselves be subsumed under different types in order for the latter to be multiply realisable. Given that every token-realiser of variation-in-T_1 is a combination of tokens of two or more distinct T_1-determinates (that are possessed by organisms of the population within the same generation), the question translates into: Under what different types are subsumed such different token combinations of property-tokens? Do two such combinations that do not comprise tokens of exactly the same T_1-determinates belong to different types? Do two such combinations that comprise tokens of exactly the same T_1-determinates but not exactly the same numbers of tokens for all those T_1-determinates belong to different types? There just doesn't seem to be any set of different laws/regularities or different types of explanation that requires us to recognise the allegedly different types so differentiated. On top of that, there is an obvious sense in which all such combinations of property-tokens belong to the same type: They are all combinations of tokens of two or more distinct T_1-determinates. Therefore, unless there is any cogent reason to the contrary, variational properties like variation-in-T_1 are *not* multiply realisable and hence are *not* realised properties at all. Then, by Haug's definition, variational process-types like selection with respect to T_1 are *not* realised process-types, either. And soon we'll see that the relation between variation-in-T_1 and any definite combination of tokens of multiple T_1-determinates is actually something other than realisation.

Third, whether or not population-level properties like variation-in-T_1 are realised properties, Haug really fails to establish that they are causally efficacious properties in the face of a version of the exclusion problem which he himself recognises. He admits that organismal-level occurrences are causally sufficient for the tokening of variation in reproductive success with respect to T_1, and hence that some organismal property-tokens, among them the most important ones being organisms' different T_1-determinates (tokens), together with some environmental and background conditions, are causally sufficient for the tokening of the latter. Also, he affirms that variation-in-T_1 is not identical to any specific combination of particular numbers of tokens of particular T_1-determinates, and that a token of variation-in-T_1 is distinct from the co-present token-combination of tokens of plural T_1-determinates that he says realises the former. And, whilst it is wrong to apply the notion of (dimensioned) realisation to variation-in-T_1 and a combination of two or more different T_1-determinates, no doubt the instantiation of variation-in-T_1 by a population of organisms depends upon the instantiations of two or more, but no matter which, of the different T_1-determinates by its members (regardless of the number of instances of each T_1-determinate instantiated). Haug recognises that the three assertions above, plus the exclusion principle or some equivalent, make up a version of the exclusion argument against the causal efficacy of variation-in-T_1. He quickly dismisses it, however. He believes that properties like variation-in-T_1 "do not compete with their realizers for causal sufficiency because the properties they involve are abstract 'logical parts' of the properties that realize them (in the same way that *being red* is part of *being scarlet* […])" (Haug 2007: 439). How is this to be taken and is it plausible at all?

In so far as he applies “being a logical part of” both to variation-in-T_1 and a definite combination of tokens of multiple T_1-determinates and to being-red and being-scarlet, his idea of a property being a “logical part” of another or a combination of some others is essentially Yablo’s (1992) and Schlosser’s (2006) idea that a mental property or a determinable is “metaphysically included” in, or is a “metaphysical part” of, a certain subvenient/realiser physical property of that mental property or of a certain determinate of that determinable. The latter idea is originally intended to apply to any pair of distinct properties the instantiation of one of which depends upon the concurrent instantiation of the other. It is held that a property that is a “metaphysical part” of another cannot compete against the latter for causal efficacy, just as a determinable cannot compete against any of its determinates for causal efficacy. Grant that it is legitimate to broaden the whole talk of metaphysical inclusion in a way parallel to the dimensioned view of realisation, so that it is also applicable to a higher-level property and a distinct but depended combination of lower-level properties/relation instantiated by the constituents of the possessors of that higher-level property. This way, Haug can have recourse to the talk of metaphysical inclusion, maintaining that variation-in-T_1 is a metaphysical/logical part of any definite combination of tokens of multiple T_1-determinates and so cannot compete against the latter for causal efficacy.

But the truth is that the talk of metaphysical inclusion cannot dissolve any exclusion-style argument, for it offers virtually no reason as to why a metaphysically included property and any including counterpart property or property/relation-combination cannot compete for causal efficacy. In fact, it just *stipulates* that the latter is the case. It is also begging the question since metaphysically included properties are nothing other than the targets of the exclusion-style argument. And it is *ad hoc* because it amounts to saying that the exclusion principle is not to cover any property that is distinct from but dependent upon another property or property/relation-combination. Further, so long as determinables are reckoned as *prima facie* causal properties, it is thoroughly wrong to presume that a determinable and any of its determinates do not compete for causal efficacy. Consequently, the analogies or comparisons with such a “model case”, be they appropriate or not, cannot serve the purpose of sustaining or motivating the idea that physicalist mental properties, realised functional properties or properties like variation-in-T_1 do not suffer from the exclusion problem. So, contrary to what Haug believes, invoking the talk of metaphysical inclusion and comparing variation-in-T_1 to being-red are far from being able to secure the causal efficacy of population-level properties from the threat of the exclusion problem.

Now, granted that variation-in-T_1 is a property of a population, it is undoubtedly a higher-level property relative to the T_1-determinates of organisms. Since it is not a functional property and, as I have specifically argued, not a realised property, it should be a higher-level physical property. In §3.4, we observed that higher-level physical properties are not affected by the exclusion-style argument. Yet, on the other hand, we’ve also seen that variation-in-T_1 is not causally efficacious with respect to variation in reproductive success with respect to T_1, since definite combinations of tokens of multiple T_1-determinates are and they are each distinct from but depended upon by variation-in-T_1. This is certainly an exclusion problem, which Haug attempts but fails to dismiss. But isn’t variation-in-T_1 a higher-level physical property and, consequently, immune to the exclusion problem?

No, it indeed has the exclusion problem. However, that isn’t because it is a realised property (which it is not) or a higher-level property (which is irrelevant). The true reason is that it is treated as essentially a *determinable*. What Millstein calls population-level properties, *viz*. variations in particular traits and variations in reproductive success with respect to particular traits if the latter variations are also deemed properties, are explicitly taken by Haug as determinables. This seems a very natural interpretation. For the moment suppose it is the only plausible one. Then, each such property is mere variation in a certain organismal determinable amongst organisms of a population, as opposed to a determinate variation that is essentially an exact number distribution of organisms of a population by the different determinates of that organismal determinable. On the assumption that determinables are properties, no determinables are causally efficacious properties. This is the case because determinates alone are causally sufficient for any effects, or accepting them alone as causally efficacious properties are sufficient for accounting for all causal powers, and any determinable is distinct from any of its determinates yet its instantiation is nonetheless dependent upon the instantiation of any of its determinates. To avoid overdetermination of effects or of causal powers, then, we should not grant causal efficacy to determinables. This, clearly, is an exclusion-style argument against the causal efficacy of determinables. It affects any determinable, whether it is a functional or physical one and no matter of which level it is, for being a determinable is a matter over and above these. The so-called population-level properties such as variation-in-T_1 are determinables apart from being higher-level physical properties, and are causally inefficacious because, and only because, they are determinables.

With the understanding that variation-in-T_1 is a determinable, we can now see clearly the correct relation between it and a definite combination of tokens of multiple T_1-determinates: They are related as a higher-level physical determinable and what may be called a lower-level determinate of it or, better, a combination of lower-level physical properties that is identical to a determinate of it that is of the same level as it is. Haug apparently mistakes this determinable-determinate plus trans-level relation for the trans-level realisation relation, thus misapplying the dimensioned view of realisation to the likes of variation-in-T_1. There are indeed several similarities between a higher-level determinable and a higher-level functional property. Like a higher-level determinable, a functional property is also distinct from yet dependent upon any of the properties, or combinations of properties/relations, the instances of which are causally sufficient for all the effects its instances are presumed to cause, or are sufficient for accounting for all the causal powers its instances are supposed to account for. Accordingly, both of them are targets of the exclusion-style argument and are hence causally inefficacious. And they are both so independent of their being higher-level properties and regardless of whether their causal rivals (realisers and determinates, respectively) are of the same level as they are or of a lower level. We can always identify the realisers of a functional property or the determinates of a determinable at the same level as the functional property or the determinable is, and alternative versions of the exclusion argument are bound to conclude the same. In the current case, we can certainly have population-level determinates of variation-in-T_1, which are nothing but the exact number distributions by (different determinates of) T_1. Still, variation-in-T_1 is distinct from but dependent upon any of these distributions and, in view of the exclusion argument, remains causally inefficacious all the same. Thus, the supposed trans-level relation that involves it is thoroughly irrelevant to its causal inefficacy. Haug's fused talk of the trans-level relation and the determinable-determinate relation (which he mistakes for the realisation relation) strongly but just wrongly suggests that the trans-level relation is part of the reason for the causal inefficacy of what are called population-level properties but are essentially determinables.

On the other hand, even though determinables and functional properties are both causally inefficacious and for the same reason, they are crucially different and should not be lumped together. Unlike functional properties, determinables, *qua* determinables, are definitely not causally identified. Moreover, it makes no sense to speak of a determinable as multiply realisable by its different determinates. Different realisers of a functional property are mutually different types: They differ from one another in type. However, different determinates of a determinable differ from one another in degree, and it would be too far-fetched to say that all different determinates of a determinable constitute mutually distinct types. In our case, no biological or any other scientific studies, I believe, would recognise populations of organisms with minimally different number distributions/compositions by a particular trait (determinable) as constituting different types in virtue of that. And the same holds for collective reproductive results of populations of organisms. These are important differences between determinables and functional properties. If one, like Haug, mixes them up and call both of them realised properties, one not only renders the otherwise potentially illuminative notion of realisation insignificant, ambiguous and even confusing, but also misses the opportunity to see correctly the ontological status of determinables. The correct ontological treatment of determinables will prove pivotal to a better characterisation of selection with respect to a particular trait in the final section. But the point here is simply that population-level properties as understood by Haug have no causal efficacy because they are determinables, not because they are realised properties.

So, since variation-in-T_1, as a determinable property, is not causally efficacious with respect to variation in reproductive success with respect to T_1, selection with respect to T_1 is *not* a process-type the token of which is such that an instance of the population-level property variation-in-T_1 is causally responsible for the tokening of variation in reproductive success with respect to T_1. That is, selection with respect to a trait as characterised by Millstein *and* understood by Haug is not a causal process-type. Just like, and inasmuch as, population-level properties are not causally efficacious properties only because they are determinables (and not because they are higher-level properties or realised properties), selections with respect to particular traits in Haug's interpretation of Millstein's characterisation are not causal types only because they are types of connections or dependencies between determinables taken as properties (and not because they are dependencies between higher-level properties or between realised properties). There are simply no population-level causal processes in the sense of a variational determinable property-token causing another.

But, aren't tokens of selection population-wide (and generation-long) causal processes? Can't we say that tokens of selection with respect to a particular trait constitute a population-level causal process-type in some sense albeit not in the above sense? Yes; however, we need a plausible alternative characterisation of selections with particular traits in order to make sense of that. As I'll show below, such an alternative can be given, which not only keeps the part of the ordinary conception of selection preserved in Millstein's own original characterisation, but also reflects the part that is neglected by her. And, more importantly, it is a characterisation that is metaphysically/ontologically more feasible.

4.3. Towards a Satisfactory Characterisation of Selection

The preceding section furnishes a strong metaphysical reason against taking the Millstein-style characterisation of selection with respect to a particular trait at the determinable level: The talk of causal connection between an indefinite variation in (unspecific) determinates of a particular determinable amongst objects of an object-collection (organisms of a population of organisms in our case) and another such variation is metaphysically mistaken because, on the assumption that such variations are properties (of the whole collection), they are distinctively determinables and *ipso facto* are not causally efficacious properties. In this section, we'll see that further reasons to keep away from such a talk eventually suggest an alternative characterisation that is in many ways more satisfactory than the one proposed by Millstein.

One of these extra reasons is ontological: Determinables are ontologically redundant and should be eliminated (Heil 1999; Gillett and Rives 2005). That is, in so far as properties are considered ontologically real, all properties are determinate properties and there are no such properties as determinable properties. This, however, does not mean that "determinable terms" are empty terms or don't express types. Even though they don't literally designate determinable properties and hence don't pick out tokens of determinable properties, they nonetheless each pick out tokens of different determinate properties corresponding to a determinable. So, they remain type terms all the same. Yet, given that determinables are not properties at all, there is simply no question of causal connections between determinable property-tokens such as the determinable variations that are taken by Haug as properties.

The non-existence of determinable properties prompts the following alternative interpretation of the Millstein-style characterisation of selection with respect to T_1: The term "variation in T_1" may be regarded as picking out tokens of different determinate variations in T_1 instead of tokens of the determinable property variation-in-T_1. On top of that, the general statement "variation in T_1 is causally responsible for variation in reproductive success with respect to T_1" may be construed as expressing a causal type the instances of which include causal connections between pairs of tokens of different pairs of determinate variational properties corresponding to the two determinable variations, rather than as literally expressing a causal type the instances of which are all causal connections between pairs of tokens of the same two determinable variational properties. Then, since the supposed causal connections are now between tokens of determinates, which are really properties and are causally efficacious ones, we may accept that they are really causal connections and, accordingly, that selection with respect T_1 is a population-level causal type in the sense of a type of causal connection between two population-level, or variational, (determinate) property-tokens that are subsumed under two types expressed by two variational determinable terms. This interpretation is metaphysically tenable and indeed can save Millstein's idea of selection with respect T_1 as a type of population-level causal process. But there are two important things to be noted.

First, Millstein herself doesn't expressly take her own characterisation this way. And Haug evidently understands terms like "variation in T_1" as literally designating determinable variational properties and the types of causal connections in terms of which Millstein characterises selection as between determinable variational property-tokens. This is manifested in his talk of variation-in-T_1 (token) as being realised by, and irreducible to, a combination of different T_1-determinate-tokens. If he did adopt the alternative interpretation, he would not invoke the talk of (trans-level) realisation and would not even find the exclusion argument a potential threat. For a determinate variation in T_1 is simply an exact number distribution by T_1 that has non-zero counts over plural yet definite T_1-determinates, which is just identical to a combination of T_1-determinate-tokens so distributed. Since irreducibility doesn't hold, the exclusion argument won't go through. Thus, although Millstein's characterisation of selections with respect to particular traits can be understood in a way that is sustained by a better ontology and, as a result, also helps avoid the exclusion problem, such an understanding is nonetheless neither what is explicitly intended by her nor what is espoused by Haug.

Second, even though under the alternative interpretation the proposed population-level, or variational, causal types can indeed be causal, they are perfectly reducible. This is a consequence of the identity between a determinate variation (amongst objects of an object-collection) in a determinable (of which the objects of that collection each possess a token of some determinate) and a unique determinate collection of tokens of determinates of that determinable. In so far as tokens of a range of different determinate variations in the same determinable constitute a determinable variational type, tokens of a range of different determinate collections of tokens of determinates of the same determinable also constitute a determinable collection type. So, at the determinable/type level, too, a determinable variational type is identical to a determinable collection type.

Now, as Haug makes explicit in his talk of causal realisation of process, a causal connection between two variations is nothing but a collection of causal connections the cause-terms of which collectively comprise the variation that is the cause-term of the first causal connection and the effect-terms of which likewise its effect-term. Thus, selection with respect T_1, i.e. the causal type the instance of which is such that its cause-term is a token of a determinate variation in T_1 and its effect-term is a token of a determinate variation in reproductive success with respect to T_1, is identical to the type of collection of causal connections that are such that: (1) each of them is between an organism's own T_1-determinate and its own reproductive success; (2) the cause-terms of them comprise a determinate collection of tokens of multiple determinates of the same trait-determinable T_1, a collection which is a token of the determinable collection type that is identical to the determinable variational type variation in T_1, and (3) the effect-terms of them similarly a token of the determinable collection type that is identical to the determinable variational type variation in reproductive success with respect to T_1. In addition, though not explicit in Haug's talk, it is conspicuous that the causal connections between organisms' different T_1-determinates and their different, respective degrees of reproductive success are clustered into different causal types: In a given type of environment, having a T_1-determinate of a certain type causally contributes to having a determinate degree of reproductive success within a certain range, having a T_1-determinate of a different type causally contributes to having a determinate degree of reproductive success within a different range, *etc*. Then, the population-level, or variational, causal type identified by the two determinable variational types variation in T_1 and variation in reproductive success with respect to T_1 is identical to, or reducible to, the type of collection of within-population instances of multiple, discrete yet similar organismal-level causal types identified by different trait-types under the same trait-determinable which is T_1 and by different ranges of determinate degrees of reproductive success under the same determinable which is reproductive success.

Such reducibility can be understood in another way. Suppose a collection of life-histories of a population of giraffes is such that long-necked giraffes each eventually reproduced a lifetime total of 4 or 5 offspring, and short-necked giraffes each eventually reproduced a lifetime total of 1 or 2 offspring. Further, for each of those long-necked giraffes, its having a (long) neck of the determinate length it did (e.g. having a 2.34-m neck or having a 2.718-m neck) did causally contribute to its having the determinate degree of reproductive success it did (i.e. its reproducing a lifetime total of 4 offspring or its reproducing 5 offspring) in certain characteristic ways: for example, via the former's being causally efficacious with respect to its eating leaves located at various determinate heights below a certain limit (say, 6.1 m) on different occasions, the accumulation of the (immediate) effects of those feeding events leading to its having a certain (good) level of nutritious status, culminating in its having the determinate degree of reproductive success it did. Similarly, for each of those short-necked giraffes, its having a (short) neck of the determinate length it did (e.g. having a 1.23-m neck or having a 1.414-m neck) did causally contribute to its having the determinate degree of reproductive success it did (i.e. its reproducing a lifetime total of 1 offspring or its reproducing 2 offspring) in other characteristic ways. This is no doubt an instance of selection with respect to neck length. It would be described by Millstein and by Haug as variation in neck length among giraffes being causally responsible for their variation in reproductive success with respect to neck length. But, obviously, it can also be described this way: In the given population, a giraffe' having a neck of a determinate length of the long-necked (determinable) type causally contributed to its having a determinate degree of reproductive success falling within the range of reproducing a lifetime total of 4 to 5 offspring, and a giraffe' having a neck of a determinate length of the short-necked type causally contributed to its having a determinate degree of reproductive success falling within the range of reproducing a lifetime total of 1 to 2 offspring. Apparently, the former description is just an abbreviation of the latter in collective terms. It should generalise to cover the same range of cases as does the latter. There's no reason an abbreviated general description can express a (causal) type that is distinct from its unabbreviated counterpart. Therefore, the variational causal type that is intended by the abbreviated description is identical to and reducible to the collection type of causal connections distributively subsumed under multiple yet similar causal types that is expressed by the unabbreviated description.

Clearly, the reducibility in our case is quite special and straightforward. No empirical knowledge is required to see that a determinate variation in T_1 is identical to a determinate collection of definite numbers of tokens of specific T_1-determinates, or that the determinable type variation in T_1 is identical to the determinable type of collection of tokens of multiple T_1-determinates. These identities hold true out of conceptual necessity alone. And the same holds true for the identity between a variational causal type and the corresponding type of collection of causal connections that are distributed into multiple yet similar causal types as illustrated above. This suggests that within-population variation with respect to individual organisms' T_1-determinates is not among what are commonly considered higher-level properties or types, i.e. properties or types of macro-objects. It's fine to say that populations of organisms exhibiting variation in T_1 constitute a type of population; however, such a type is fully identified by reference to organismal properties or types, albeit a plurality of them, in a totally transparent sense. Even though a population of organisms is not an arbitrary aggregation of conspecific organisms, saying of a population of organisms as having variation in T_1 is only referring to a plurality of organisms collectively or as a collection, as opposed to referring to them as a single macro-object; it is talking about their T_1-determinates collectively, rather than about some single distinctively higher-level property-token. One may call a determinate variation in T_1 a variational property, a collection property, a system-level property or simply a population-level property, but that won't change the fact that a population of organisms is picked out by "variation in T_1" according to the different T_1-determinates possessed by the organisms alone. Describing a collection of organisms in respect of their T_1-determinates as having variation in that respect and describing them as being of different types in exactly the same respect (i.e. some being of a certain T_1-determinate-type, others being of a certain different such type and so on), won't even result in different type-terms, let alone different types. This is a purely conceptual fact. And, in a sense, there is nothing to be reduced from the very beginning. Within-population variation in organismal properties/types consists in organisms within the bounds of a population being distributed into different types. There is nothing over and above that.

Similarly, variational causal types are not distinctively higher-level causal types. Causal connection between within-population variations in organismal properties/types is not a matter of change of some distinctively higher-level properties or macro-states attributable to a population of organisms as a macro-unity in any straightforward sense. Instead, it consists in there being (many enough) instances of multiple, discrete yet similar organismal-level causal types, identified by different types of determinate of a certain trait-determinable and by different ranges of determinate degree of reproductive success, within the bounds of a population (as well as a generation). So, it’s really not a variation in organismal properties/types causing another. Rather, the talk of population-level or variational causation is actually an abbreviation. Again, it’s all right to say that population-wide and generational-long processes, i.e. collections of life-histories of all organisms of a population within a generation, containing (many enough) instances of multiple organismal-level causal types each of which is of the form “organism’s having a certain type of T_1-determinate (in such-and-such environment) causally contribute to its having a determinate degree of reproductive success within a certain range (*ceteris paribus*)”, constitute a type of population-wide and generation-long process (which we call selection with respect to T_1), or a population-level causal type given that each collection of life-histories of all organisms of a population within a generation can be considered a causal process. Yet, such a causal type is fully identified by reference to organismal-level causal types, albeit with a plurality of them and with indefinite number of instances. That is, although the individual population-wide and generation-long causal processes do indeed each involve a whole population of organisms, they are subsumed under the type which we call selection with respect to T_1 and is also variously categorised to be a type of population-level causation and a type of causation between within-population variations, because (many enough of) the lives of the organisms of each of those populations—the lives of the organisms that constitute each of those population-wide and generation-long causal processes and that can themselves also be considered causal processes—are themselves distributively subsumed under multiple organismal-level causal types of the aforementioned form. Therefore, selection with respect to T_1 is a causal type that is in fact characterised by a plurality of organismal-level causal types. This just implies that there are really no distinctively higher-level causal types as compared to organismal-level ones in our case.

However, one may nevertheless insist that selection with respect to T_1 is such a causal type that is characteristically identified by determinate (but not determinable) variations in T_1 as causal properties. Fine. Suppose it is metaphysically legitimate to attribute properties to collections of objects that are not themselves objects and to regard determinate variations in T_1 as among such properties. But each determinate variation in T_1 is blatantly identical to a determinate collection of definite numbers of tokens of specific T_1-determinates as said above. This is properly called reduction. And determinate variations in T_1 are causally efficacious properties at all only because they are so reducible. Then, what is the significance of insisting the talk of within-population variations in organismal properties/types as causal properties? I'm afraid that there is virtually none.

Besides, there are additional reasons not to accept the Millstein-style characterisation of selections with respect to particular traits. First, it is essentially an abbreviated characterisation. It adds nothing new to the unabbreviated version and is not more illuminative than the latter. Second, it conceals, or at least doesn't verbally make explicit, the fact that there are sets of organismal-level generation-long causal types, each set of which is identified by different types of determinate of a certain trait-determinable and by different ranges of determinate degree of reproductive success.

They are fundamental to understanding what selection with respect to a trait is and are what underlie both the abbreviated and the unabbreviated characterisations.

Third, it is actually an inappropriate abbreviation. The faithful description of a case of selection with respect to a trait-determinable is that tokens of different types of determinate of that trait-determinable differently (i.e. distributively) causally contributed to tokens of different types of determinate degree of reproductive success. It's wholly a matter of property-tokens different in type causing property-tokens different in type, not a property-difference-token causing a property-difference-token as literally suggested by the abbreviated characterisation. The latter is a distorted way of describing the matter.

Fourth, it creates unnecessary complications. A case of selection with respect to T_1 typically contains outliers: Not every organism's life-history within the given collection of life-histories (of all organisms of the given population within the given generation) is subsumed under one of the multiple organismal-level causal types identifying the given collection of life-histories as a case of selection with respect to T_1. Whilst a considerable portion of organisms within a population having a certain type of T_1-determinate would be such that their having T_1-determinate of that type causally contributes to their having determinate degrees of reproductive success within a certain range, by virtue of their frequently behaving in certain distinctive ways due causally to having T_1-determinate of that type, the remaining portion would not. This happens because, for each organism of the latter group and its life-history, some background conditions for such a causal type fail to obtain. Now, for such a population of organisms, what exactly is the determinate variation in T_1 that is picked out by "variation in T_1"? That is, which collection of T_1-determinate-tokens does it pick out? It cannot pick out the collection of T_1-determinates of all organisms within the population, since what it picks out is supposed to be causally efficacious but some tokens of T_1-determinates of the given type just aren't causally connected to having determinate degrees of reproductive success within the given range (or aren't causally connected to the latter in the right way). On the other hand, it would be weird to say that it picks out the collection of T_1-determinates of all organisms but the outliers, for then it would pick out only a part of the population, which would spoil the idea of determinate variations in T_1 as properties of the whole population. This complication will certainly cease to disturb once we stop talking organisms' T_1-determinates collectively and simply return to the unabbreviated characterisation of selection with respect to T_1.

In view of all these, then, why bother sticking to the Millstein-style characterisation given that there is a way more satisfactory alternative readily available?

I suggest that we frankly characterise selection with respect to a particular trait, say T_1, in the following way: Selection with respect to T_1 is such a process that it is a collection of life-histories of all (and only) the organisms of a population within a generation that exhibits the following features: The organisms have different T_1-determinates and different determinate degrees of reproductive success. A considerable portion of the organisms having a certain type of T_1-determinate are such that they have determinate degrees of reproductive success clustered within a certain range, *and* for each of them, its having the T_1-determinate it has causally contributes to its having the determinate degree of reproductive success it has via its T_1-determinate's being causally efficacious with respect to its frequently behaving (and/or its physiological economy typically operating) in certain distinctive ways in certain types of organismal-level occurrences during its life. And a considerable portion of the organisms having a different type of T_1-determinate are such that they have determinate degrees of reproductive success clustered within a different range, *and* for each of them, its having the T_1-determinate it has causally contributes to its having the determinate degree of reproductive success it has via its T_1-determinate's being causally efficacious with respect to its frequently behaving in other distinctive ways in certain types of organismal-level occurrences during its life. And so on and so forth. This lengthier characterisation is better than Millstein's abbreviated talk in the following three aspects.

First, it is not beset by any of the troubles discussed so far. The only properties it mentions are organisms' (physical) traits (determinates), which no one would deny are genuine properties, and are distinctively causal properties for that matter. It doesn't rest upon the posit of causally inefficacious and ontologically redundant properties (e.g. determinable variation in T_1 as a property) or of the ontologically reducible properties of determinate property-variations. It is free of the worries of the legitimacies of the latter properties and of attributing properties to object-collections that are not themselves objects, and of the complication of whether a part or the whole of a population of organisms is picked out by terms of organismal property-variations. It is semantically straightforward and undistorted. It is not an abbreviated general description; it needs only be literally understood. In a nutshell, it avoids all metaphysically burdensome talks and is semantically transparent.

Second, it brings the oft-overlooked typological element inherent in the talk of selection into view. The types that are fundamental to the processes of selection are types of organisms, and are types according to the different traits (determinates) they bear, not types in the taxonomical sense. Different trait-determinate-types and different ranges of determinate reproductive success are precisely what identify the multiple, different organismal-level causal types that characterise the type of process that is selection with respect to a certain trait-determinable. Although an organism's certain trait must contribute to its reproductive success via a typical range of events which happen to it during its life and to the results of which the trait is causally efficacious, and the results influence its reproductive success often in a cumulative manner, I'm willing to accept the distinctively selective talk of causal contribution of a trait of an organism to its reproductive success, or simply causal connection between the two. Also, I don't mind calling generalisations of such causal connections causal laws, although they are definitely *ceteris paribus* laws or, if one likes, probabilistic laws. But such causal connections concern organisms or, better, their lives; such causal laws govern organisms' life-processes. The causal types are basically types of organisms' life-histories. All these are about organisms, and are about them as belonging to this or that type. True, they are about organisms in the context of a population. But they are neither about populations of organisms each taken as a unitary something nor about them as belonging to this or that type. Nor are the causal types or laws about within-population groups constituted by organisms of the same trait-type: If one thinks so one would be confusing types with aggregates. They each subsume or pick out organisms' life-histories individually, not collectively, even though for a case of selection with respect to a trait, they each subsume or pick out a plurality of life-histories of individual organisms within a population.

Thus, although selection is about a whole population of organisms, the multiple causal types or laws characterising selection with respect to a trait-determinable are about individual organisms. There's nothing strange about this; the two are perfectly compatible. It is the preoccupation with the so-called population-thinking that motivates Millstein's talk of population-level causation in the sense of causal connection between variations in organismal types. Yet in truth it is the organismal causal types or laws that are different or have variations; that is, it is actually the variation in organismal causal types or laws, as opposed to there being types of causal connections between variations in organismal types. It’s fine to say that a plurality of causal types or laws having the said variation collectively concerns the population, or at least many of the organisms within the population. However, that still doesn’t amount to a single variational causal type or law as conceived by Millstein and by Haug.

Third, my recommended characterisation keeps the populational element in the right place. Trivially, the populational element lies in the fact that each case picked out by the characterisation remains a collection of life-histories of all organisms of a population. This, of course, is simply a consequence of our following the common biological practise of applying “selection” to a whole population of organisms or the whole of their life-histories. But there is something more substantial: The different, multiple causal types that jointly characterise a case as an instance of selection with respect to some trait-determinable and hence have each a plurality of instances within the given population, are associated with two important common sets of environmental and background conditions. One of them is the common coarse-grained environmental conditions for the whole population of organisms. The other is the presence of other peer organisms, with some coarse-grained joint frequency distribution for certain trait-determinables, within the same population. The latter means that the rest of the organisms of a population form part of the environment for each individual organism within that population, which in turn suggests that the life-process and especially the reproductive success of an organism are typically not independent of the life-processes of other organisms within the same population. Thus, when it comes to selection, it is sensible to treat a population of organisms as a whole and the collection of their life-processes as a single process.

However, it doesn’t follow from these that the whole population must be regarded as an object or that the single, collective process must be characterised in terms of a single causal connection between a property-token and another (in order for it to be a case of selection). No. Even though the life-processes of organisms of a population are embedded in a complex causal web, in so far as what matters most to the notion of selection is the sort of causal types that have to do with traits and reproductive success, such causal types are naturally the assorted types of causal connections between individual organisms' (different) traits and their respective (different degrees of) reproductive success. To repeat: While the multiple causal types jointly characterising a case as an instance of selection with respect to a trait-determinable may be said to be collectively about the population as a system, they are each nonetheless an organismal causal type and are each about organisms in the system. The population serves as part of the environment/background for those causal types and as part of the causal context in which individual organisms' traits causally contribute to their respective reproductive success in so far as they actually do. All these are consistent with various biological practices in the domain of selection and evolution (Glymour 2006) and with the tenet of population-thinking.

Finally, in addition to the above reasons to accept the present characterisation of selection with respect to a trait-determinable instead of Millstein's talk of variational causal connection, it gives a clue to a more viable semantics of the fitness-talk. Strictly speaking, since, as I have argued at length in §4.1, the posit of fitness as a (causal) property (or relation) is triply redundant and therefore there is no such a property/type, a feasible semantics of the fitness-talk basically is not an essential component of a satisfactory account of selection. Even so, it would be good to have one because of the widespread talk of fitness and fitter-than both within and outside the domain of biological selection. Fortunately, what we have hitherto examined does implicate one, which, shown below, will complete my own account of selection here.

Within the domain of biological selection, a complete fitter-than statement goes like this: Organisms having a certain type of determinate of a certain (simple or complex) trait-determinable are fitter than organisms having another type of determinate of the same trait-determinable in a given type of environment. What does this mean given that “fitness” does not designate a property and “fitter than” not a relation? It means that organisms of the former trait-type typically have better reproductive success than those of the latter trait-type in the given type of environment. Does this imply that “fitter than” just mean having better reproductive success than? No. The correct interpretation is that each fitter-than statement is actually an abbreviated expression of two *ceteris paribus* (or probabilistic, if one prefers) generalisations/laws: Organisms of the former trait-type (eventually) have a determinate degree of reproductive success within a certain range in the given type of environment, *ceteris paribus*, whereas those of the latter trait-type (eventually) have a determinate degree of reproductive success within another range in the same type of environment, *ceteris paribus*, and the characteristic determinate for the former range of reproductive success is higher in degree than that for the latter range.

Such generalisations/laws differ from those that jointly characterise selection with respect to a trait-determinable only in one regard: The traits (determinates) mentioned in them need not be causally efficacious with respect to reproductive success. In cases where they do, the generalisations/laws are causal ones and express precisely the same causal types as those that jointly characterise selection with respect to a trait-determinable. In cases where they don’t, the generalisations/laws are non-causal ones and may be said to characterise what Sober calls (mere) selection of a (type of) trait (determinate amongst others of the same trait-determinable). Thus, it is not essential to the talk of fitter-than that it expresses (a pair of) causal types even though it can indeed express causal types. The important thing is that in the domain of biological selection, every talk of fitter-than is actually a talk of a pair of *ceteris paribus* generalisations/laws that mention organisms of different trait-types (along with the same set of important, coarse-grained environmental conditions) in the antecedents and their different characteristic degrees of reproductive success in the consequents. It is, in a certain sense, a comparison of a pair of such generalisations/laws or of the two different types of changes or life-processes expressed by them. And it makes very good sense to compare them because the different trait-types of organisms are not arbitrarily different: The different traits they bear are of the same trait-determinable, and they are members of the same population and, accordingly, live in the same type of coarse-grained environment and are of the same species and hence and are much more similar to one another than to organisms of other species.

This understanding of the talk of fitter-than agrees well with the standard population-genetic notion of fitness as expected reproductive success. Whilst organisms' degrees of reproductive success may be said to be their properties, expected degrees of reproductive success are not properties at all. Saying of an organism of a certain trait-type as "having" a certain expected reproductive success in a certain type of environment is not assigning an attribute to it. Rather, it is saying that organisms of that trait-type typically have that characteristic degree of reproductive success in that type of environment; that is, it is in fact saying a *ceteris paribus* generalisation/law or talking about a type of change or life-process expressed by such a generalisation/law, not merely talking about a type of object. So, "fitness" does not designate whatever (causally efficacious or inefficacious, functional or physical) property mentioned in the antecedent of such a generalisation/law; nor does it designate the determinable reproductive success mentioned in the consequent. The fitness-talk is the talk of the whole generalisation/law. Organisms of a certain trait-type typically have a certain degree of reproductive success in a certain type of environment *not because* they have a certain fitness-level in that type of environment; "organisms of a certain trait-type have a certain fitness-level in a certain type of environment" *just means* that they typically have a certain degree of reproductive success in that type of environment. In the same vein, organisms of a certain trait-type typically have better reproductive success than do those of another trait-type in a certain type of environment *not because* they have a higher fitness-level than the latter do in that type of environment; "organisms of a certain trait-type have a higher fitness-level than do those of another trait-type in a certain type of environment" *just means* that they typically have better reproductive success than the latter do in that type of environment.

The present semantics provides a link between the fitness-talk and the notion of selection in the domain of biological selection, on the one hand, and comparable talks and notions outside it, on the other hand. Recall Matthen and Ariew's generic notion of fitness as expected number count growth rate (but not mere number count growth rate), or expected number count change in a given time unit, of "things" of a certain type in certain conditions within a certain collection or certain bounds within which there are also things of other comparable types. It suggests that every talk in other domains that is comparable to the fitness-talk in the domain of biological selection is a talk of a generalisation/law, too, that has the following form: Things of a certain type typically have/exhibit a certain level of per capita number count change in a given time unit in certain conditions. Depending upon the category of the things and/or the content of the conditions mentioned, some of the generalisations/laws having such a form, for example those mentioning ideas/theories as held by individual intellectual beings and Matthen and Ariew's own peculiar example of account balance change due to fixed interest rate, may be more properly considered rules or their consequences as opposed to generalisations/laws since they involve distinctively epistemic/normative elements. But we may ignore this for our present purpose. For what is crucial here is that all talks comparable to the fitness-talk in the domain of biological selection are talks of *different* generalisations/laws that have the *same form*, not talks of the same generalisation/law. They are different because the things are of different types and it is different as to how (and why) the number count of each type of thing changes over time in the mentioned conditions. For this reason, they express different types of change or process, or causal types in cases where the number count changes are somehow causally connected to the properties/attributes of the things according to which they are subsumed under the mentioned types, as in the cases of the causal types characterising selections with respect to particular trait-determinables. These different types of changes/processes might be regarded as remotely similar in the sense that they all have to do with number count change over time within a larger context. But this is mere formal similarity at best. It is not a physical or causal or any natural or scientifically/explanatorily significant similarity. Therefore, the different fitness-talks or related talks in different domains are only formally and analogously related.

The same applies to the generic talk of fitter-than and hence to the generalised notion of selection. All fitter-than statements are essentially joint comparisons of a pair of generalisations/laws that share the following form: Within a certain collection or certain bounds, things of a certain type typically have/exhibit a certain level of per capita number count change in a given time unit in certain conditions, and things of another, comparable type typically have/exhibit a different level of per capita number count change in the same time unit in the same conditions. Or, in brief, things of a certain type typically have/exhibit a higher (or lower) level of per capita number count change, or are “fitter” (or “less fit”) when that sounds smooth, than things of another type, in the same time unit and within the same collection or the same bounds, in certain conditions. Again, this is a mere form of statement/comparison, not a specific statement/comparison. In cases where specific statements sharing such a form, or rather the specific pairs of generalisations/laws being abbreviated or compared in them, do plausibly characterise specific types of macro- or collection-level- or system-level change/process, those types of change/process are mutually distinct types. Such different types of changes/processes have nothing substantial in common except some formal similarities: The things within the different types of collection or bounds are of two (or more) types, and the per unit time per capita number count changes for the different types of things are different and are typically so under specified conditions. Even though, as I have pointed out in §4.1, selections with respect to different trait-determinables are physically/causally different types of process, they do share many substantial similarities besides the formal ones so that we can reasonably say that they constitute a single second-order type of process that is commonly called selection. But I see no point of saying that there is a single, even higher-order type of process that is constituted by all types of processes sharing the above formal similarities alone. At the very least, unlike selection, just no scientific domain recognises such an alleged type of process.

So, if one intends “selection” to pick out all such merely formally similar processes, then one is using a word that is homonymic to the “selection” as used in the domain of biological selection. To formally distinguish the two we may say that the former, semantically more general one is “selection^{2}” whereas the latter, conventional one is “selection^{1}”. One certainly can freely stipulate that “selection^{2}” should apply to all types of processes that share the aforesaid formal features in terms of their characterisations. That however doesn’t make “selection^{2}” a name of a single natural or scientifically established type of process. Only "selection^{1}" denotes such a type. From the perspective of explanation, we may think that "selection^{2}" stands for a class of explanations that are formally similar in that each of them cites a pair/set of *ceteris paribus* (or probabilistic) generalisations/laws having the previously mentioned form, plus the fact that things within a given collection/system or given bounds belong to different yet comparable types, along with certain important conditions, in order to explain the fact that there is difference/variation in per unit-time per capita number count change among those different types of things within that collection/system or those bounds. The fact that explanations in diverse domains can share such a common explanatory scheme is, I believe, a prime reason why selection^{2}-talks are so popular and why such explanations are called selective explanations after all. Like the explanations of differential reproduction by trait-variations in the domain of biological selection, all selective-style explanations are explanations of a variation by another (or others). But they are just explanations. There are no such causations as causal connections between variations. Nor is there such a process-type as selection^{2}.

5. Conclusion

There's no doubt that selection involves causations. But what sort of causation? The account proposed in the end of the last chapter recognises that the sort of causation involved is causal connections between organisms' traits and their respective reproductive success. Such causal connections are surely causations in a looser sense: Traits can be causally connected or contributory to reproductive success only via the occurrences of organismal-level events/processes, and their causal contribution to reproductive success is usually cumulative and/or indirect. But there are no special metaphysical concerns against them. They are generation-long causal connections. They go well beyond causal relations between organismal-level events such as predations causing deaths. And they are subsumed under types. Such generation-long causal types are what selection consists in. Or rather, selection with respect to a trait-determinable consists in a multiple of those causal types that differ from one another in this way: The cause-types are different types of determinate under that trait-determinable, and the effect-types are different ranges of determinate degree of reproductive success. So, like all other process views of selection, the present account accepts that a proper characterisation of selection requires the recognition of the sort of causal type the cause-type of which has to do with organisms' having certain properties and the effect-type of which concerns their having certain reproductive success.

However, the sort of causal type that selection consists in does not go beyond that the instance of which is the causal connection between an individual organism's having a certain determinate property (or having certain determinate properties) and its having a certain determinate degree of reproductive success. Specifically, selection does not consist in the sort of causal type the instance of which is the causal connection between variation (or difference) in a certain property (determinable) amongst a population of organisms (or between a pair of organisms) and variation (or difference) in their reproductive success (with respect to that property (determinable)). This is where I part company with all other proponents of the process views of selection in the current debate. My reasons against this sort of causal connection are largely metaphysical: If the talk of variations/differences is taken at the determinable level, then either determinable variations/differences are really properties but are causally inefficacious ones due to the exclusion problem, or there are no such properties/relations such as determinable variations/differences due to the ontological consideration that it is unnecessary to accept determinables as ontologically real. Either way, there are no causal connections of the aforementioned sort. If, however, the talk of variations/differences is taken at the determinate level, then causal connections of that sort may be acceptable. Yet it is a matter of conceptual necessity that any type of those causal connections is identical to, or reducible to, a certain type of collection of multiple causal connections each between an individual organism's having a certain determinate property (under the determinable the variation/difference in which is the variation/difference in a given case) and its having a certain determinate degree of reproductive success. The talk of such causal types is thus nothing but an abbreviated talk of the latter types of collection of multiple organismal-level generation-long causal connections. Besides, it is semantically strained and characterising selection in terms of it creates unnecessary complications. These plainly suggest that we'd be better off without it, at least in characterising selection.

By contrast, failing to recognise the sort of generation-long causal connection between an individual organism's having certain properties and its having a certain degree of reproductive success leads Matthen and Ariew to say that selection in the ordinary sense is a mere aggregation of organismal-level events. Every case of selection is surely a collection of organismal-level events. But selection is more than that. And its truth implies that there is indeed selection in the ordinary sense rather than the contrary. Matthen and Ariew apparently have an overly restricted conception of causation, event and process, so that they don't acknowledge that a suitable collection of events can itself be considered an event or process, or that an object's having a property can be causally efficacious with respect to its being so and so long afterwards in a cumulative manner, via a series of causally related short-term events in which it is involved and with respect to some of which its having that property is causally efficacious. Admittedly, talks of causation and process in special sciences are usually loose talks from the view of standard metaphysics. But many of them are readily compatible with it. Some efforts in this regard have been made when we try to metaphysically accommodate the distinctively evolutionary biological talks of selection being a cause of evolution and its being itself a (causal) process. Since we can make sense of these talks in the standard metaphysical framework, and since evolutionary biologists do *not* apply the notion of selection to a case because it exhibits the formal pattern identified by the growth-rate theorem, there is no reason to change the subject when giving an account of the nature of selection. Even though every case of selection necessarily exhibits that formal pattern, it is just not what selection in the ordinary sense consists in.

The case of fitness is more complicated. The current use of the term "fitness" as an evolutionary biological jargon comes principally from population genetics, where it means (standardised) expected (per capita) reproductive growth rate or simply expected reproductive success. Given this notion of fitness, saying that a trait-type is fitter than another—where the two trait-types are two different determinate-types under the same determinable(s)—in a certain type of environment is just saying that organisms of the former trait-type typically have a degree of reproductive success within a certain range in that type of environment and those of the latter trait-type typically have a degree of reproductive success within another range, where the two ranges are significantly different. Hence, the talk of fitter-than is essentially a talk of two generalisations. Such generalisations are not in themselves causal ones. They are causal generalisations only when the traits mentioned in them are indeed causally efficacious with respect to reproductive success. A plurality of such causal generalisations which mention different trait-types under the same trait-determinable, different levels of reproductive success and the same environmental conditions then express exactly the same multiple organismal-level generation-long causal types which selection with respect to a trait consists in. So long as the notion of fitness and the talk of fitter-than are so understood, there is no problem with the commonsensical idea that selection is a matter of some organisms' being fitter than others.

But that idea is fine *only* in the above understanding of the fitness-talk. Selection does not consist in the connections between different trait-types' fitnesses taken as reproductive growth rates and their number proportions. Nor does it consist in the mathematical relations between two different mathematical functions of growth rates. Such connections are mathematical in nature: All collections of life-histories of a population of organisms within a generation (with different trait-types having different reproductive growth rates) necessarily exhibit any of those connections, and their exhibiting each of them can all be known *a priori*. Identifying selection with any of them deprives all the epistemic significance of a presumably explanatory notion. And again, none of them captures the idea of what evolutionary biologists call "selection".

Nor does selection consist in the supposed causal type the instance of which is the supposed causal connection between fitness difference and difference in reproductive success, where fitness is taken to be the property that specially grounds the population-genetic talk of fitness, i.e. reproductive growth rate. If we have to posit such a property at all, then it has to be a second-order functional property, whether it is said to be a unary property or a binary one (i.e. a binary relation) and regardless of what it is otherwise called. Its being functionally or causally identified by reference to reproductive success as its supposed effect-type precludes it from being causally efficacious in the latter respect (the problem of metaphysically necessary dependency). And since it is a second-order property, due to the exclusion problem it is again causally inefficacious in respect of reproductive success. Either way, there is no aforementioned causal type. Furthermore, fitness in this sense is explanatorily superfluous. For whatever is supposed to be explainable by it in conjunction with various environmental and background conditions can be fully and directly explained by traits in conjunction with the same conditions. Plus, nothing in the current evolutionary biological use of the term “fitness” requires us to recognise that it designates a property. Therefore, the posit of the property of fitness is redundant, and we should refrain from characterising selection in terms of it.

The fact that selection consists in causal types that differ from one another in their cause-types, i.e. the different traits, not only at the determinate level but also at the determinable level indicates that selection is really an open set of distinct types of process as opposed to a single type of process. This point is also suggested by the idea that selection must be selection with respect to a particular trait (determinable) and there is simply no selection *simpliciter*. While selections with respect to different traits may be said to be similar to one another primarily in virtue of their all consisting in causal types the effect-types of which concern reproductive success, such a similarity is non-causal and is largely formal or linguistic. And extending the talk of selection to cover cases both within and outside the domain of biological selection, so that they are all called "selection" because they can all be described as a process that is such that the number proportions of the different types of things within a given boundary change within a given time (and why this happens has something to do with why those things are of different types), is nothing but devising a summarily sentence scheme that mentions no types at all. There is nothing inherently wrong with this. But there is nothing significant either.

A broader implication of selection's being an open set of distinct types of process is that evolutionary biology is not distinguished by a handful of all-inclusive laws. I don't find this a problem. Yet it suggests that we view evolutionary biology in a more liberal way. The PNS, or something like that, is often taken as a law, or the law, that lies at the core of the theory of evolution. Now that the posit of the property of fitness has been rejected, what, then, lies at the core of the theory of evolution? More fundamentally, what does the theory of evolution consist in? One thing is for sure: It doesn't consist in a set of mathematically necessarily true equations or theorems in population genetics. Perhaps, we should follow many evolutionary biologists to regard evolutionary biology as an interdisciplinary or integrated study that does not have its own core of theory distinguished by a few laws but, instead, relies upon all relevant bodies of knowledge—distinctively biological or otherwise—in order to offer explanations of differences in reproductive success and of evolution and to furnish more global hypotheses about the evolutionary history of life on the earth. This doesn't make evolutionary biology a less well-established scientific practise than any area of physics. I see this a better way of viewing evolutionary biology than presuming that it has a few proprietary, all-encompassing laws lying at its theoretical core. At any rate, any candidate of such laws should not be sought in population genetics, in *a priori* and/or conceptually true general statements, or in something associated with the idea of fitness.

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