

# Selection in Modern Evolutionary Biology, Learning and Culture

*Sketches for a Philosophy of  
Interdisciplinary Science of Behavior*

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# 1. Introduction

## 1.1 Thesis

### 1.1.1 Main thesis

My main thesis is an argument for a foundation or sketches for a philosophy of interdisciplinary science of behavior. By interdisciplinary science I specifically mean an understanding of how biological evolution and culture co-determine individual human behavior—thus seamlessly incorporating a biological, anthropological and psychological perspective, respectively. More generally I mean an understanding of how biology, as base science, should relate to psychology, and how it subsequently should relate to cultural anthropology, as a model for other social sciences. The viewpoint of how these sciences relate provides an *implied*, broader framework, within which the more specific main thesis is argued and discussed, and which the subject matter of the essay is limited to.

**Specifically, the main thesis is an argument for a renewal or refinement of radical behaviorism, a distinct philosophy of psychology (Skinner, 1976), to accommodate modern evolutionary biology, thus providing a slightly improved, more seamless understanding of the relationship between biological evolution, learning and culture. I call this position Neo-Darwinian radical behaviorism.**

**A distinct framework of interconnected scientific disciplines, which I propose, also illustrates the main thesis. This framework, or table, is organized according to phylogenetic, ontogenetic and cultural levels along the X-axis, while organized according to proximate and ultimate explanatory modes along the Y-axis.**

I refer to a "foundation" or "sketches", as there will still be a lot of important details, elaborations, revisions, etc. to work out for such an ambitious project.

### **1.1.2 Sub-theses: Three syntheses**

The essay consists of three sub-theses, all of which form integral parts of the main thesis. Each sub-thesis has been assigned to a chapter of its own. Each sub-thesis also takes the dialectical form of a synthesis.

**The first sub-thesis, covered in chapter 3, “Synthesis between modern evolutionary biology and radical behaviorism”, is an attempt to describe the relationship between biological evolution (phylogenetic adaptation) and learning (ontogenetic adaptation (Cleaveland, 2002; Skinner, 1966)) consistent with some principles that mark modern evolutionary biology. Among them are individual genetic differences, and their role as variation in intra-population differential reproductive success, as part of the more modern understanding of natural selection, as opposed to older group selection theories.**

**The second sub-thesis, covered in chapter 4 “A modern synthesis for ontogeny”, is a suggestion for a synthesis between radical behaviorism and neurophysiology, encompassing mainstream, mentalist, cognitivist perspectives in psychology, modeled after the modern evolutionary synthesis between Mendelian genetics and Darwinian evolutionary biology. This sub-thesis builds naturally upon the argument for a “rich phenotype”, or large set of phylogenetic adaptations relevant to human psychology in chapter 3, further suggesting that operant conditioning is one of many phylogenetic adaptations, but shaping, governing and interconnecting these under what I call an operant regime.**

**The third sub-thesis, of chapter 5, “Seamless causal relationship between biological evolution and culture”, draws together the threads from the two former chapters, in an attempt to provide some insight into how an understanding of operant conditioning, based in modern evolutionary biology, can prove beneficial for culture theory. I argue, among others, that a theory of culture and cultural evolution can be formed, which has the autonomy that does justice to the phenomenon of culture, while still relating to natural selection and phylogenetic evolution—through reinforcement, as discussed in chapter 3.**

### 1.1.3 Recurring themes

Psychology, as a modern scientific discipline, suffers somewhat from the limited logical flexibility afforded to understanding how behavior is caused. Within a logical framework of "either-or", there is a debate about whether behavior is controlled by mental mechanisms *or* "reward and punishment" (Sterelny & Griffiths, 1999).

In practical application, on the other hand, an equally weak logic of "both" is often applied. For example, Hilgard's Introduction to Psychology defines psychology as "*the scientific study of behavior and mental processes*" (Atkinson, Atkinson, Smith, Bem, & Nolen-Hoeksema, 2000, p. 3), as if behavior and mental processes belong to the same explanatory categories, and as if mental processes are somehow non-behavioral. As I will argue, this is logically equivalent to defining biology as something like "the scientific study of genes, phenotypes and evolution." It doesn't sound that bad, but in biology, one of the major theoretical and conceptual innovations of the 20<sup>th</sup> century was the understanding of how genetics and biological evolution belong to different, complementary explanatory categories. Genes are *proximate* causes of phenotypes and behavior, while the phylogenetic evolutionary process is an ultimate cause, selecting the genes, which in turn provide a proximate causal contribution to the phenotype. In a related manner, genes are viewed as *replicators*, while organisms and organismic phenotypic traits are viewed as *interactors* in the evolutionary process—a core concept in the influential view known as *gene-selectionism*<sup>1</sup> (Dawkins, 1999, Sterelny & Griffiths, 1999).

Could it be that psychology may benefit from a similar distinction between behavior-environment functional *interaction*, most notably learning processes, which take form as the *ultimate* causal, explanatory categorical contribution of the lifetime history of the organism (ontogeny, culture), and the organismic structure, particularly brain and central nervous system, which encapsulate mental states, *replicating* and proximately

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<sup>1</sup> To be precise, Dawkins himself uses the expression "vehicles", to denote to organisms as vehicles, which genes use to replicate themselves, instead of interactors. See, for example (Sterelny & Griffiths, 1999, p. 55).

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causing behavior? Yes, I will argue, in fact the current paradigm of cognitivist evolutionary psychology, holding that “behaviors are caused by a small set of cognitive mechanisms” (Sterelny & Griffiths, 1999, p. 321) (parallel to saying that “genes, not evolution” cause phenotypic, adaptive traits in biological evolution), prevents a seamless understanding of how evolutionary biology and culture, through social learning, co-determine human behavior, by disregarding behavior-environment relations as ontogenetic, ultimate causes of behavior.

*The received view of the relationship between biology and psychology*

This weak and misguided logic stems from what I call the received view of the relationship between biology and psychology. The received view uses structural delimitation to draw a line between biology and psychology. Biology concerns physiology, neurophysiology, genetics and biological evolution, while psychology is the realm of mind, consciousness and sub-consciousness, mental states, language, mental symbolic manipulation, and even culture.

This essentially structuralist viewpoint views environmental (ontogenetic) variables as outside the organism, providing input in terms of stimuli or information. The organismic structure, most essentially its “psychological” structures—mental states—processes and represents the information, and returns an output in terms of a behavioral response. The “biological” structures, neurobiology, are considered to underlie these mental capacities and are thus identified with the organism, as a set of mediating variables.

This viewpoint may serve the psychologist fine as a theoretical model of how a single stimulus is mediated by the organismic structure, which in turn outputs, or returns, a single response—the S-O-R model, stimulus-organism-response.

The severe problems arise, however, when one attempts to describe how behavior is caused, or rather, *co-determined*, by environmental (ontogenetic) variables, versus genetic (by proxy, phylogenetic) variables. The structuralist delimitation between biology and psychology already identifies biology with the mediating *organism* in the

stimulus-*organism*-response model. The environment however, is identified with the *stimulus* and the *response*.

However, ontogeny is certainly not the only ultimate causal factor determining human behavior. The nature-nurture-dichotomy, for all its faults, at least correctly identifies genes and environment as the principal co-determinants of human behavior (though, as I will argue, in terms of incompatible causal categories).

Phenotype and behavior (or heritable dispositions as such) is firstly caused through phylogenetic evolution, most notably by natural and sexual selection<sup>2</sup>. Then the living organismic phenotype and its behavior are further shaped through the course of its lifetime, by what may broadly be called ontogenetic contingencies, particularly learning, including social, cultural learning. In this thesis I draw upon Skinner's radical behaviorist perspective, and its scientific core component *operant psychology*, which proposes operant conditioning (reinforcement) as a defining causal mode in ontogeny, similar to that of natural selection in phylogeny (Skinner, 1981).

### **A better way to understand the relationship between biology and psychology**

The problem with the structuralist model is that it distinguishes between psychology and biology along false lines. Mental states are just as much a product of biological evolution as their publicly observable neurological embodiment. In fact, mental states may not even fruitfully be considered as structurally distinct from their "underlying" biological structures, reminiscent of Cartesian substance dualism.

Furthermore, the environment (or ontogeny, including culture in the case of organisms capable of social learning) cannot possibly be represented in the form of a single stimulus, or any amount of S-O-R relations. The lifetime history of the organism involves an endless amount of stimuli and events influencing, changing its

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<sup>2</sup> I actually prefer to include sexual selection under a wider umbrella definition of natural selection, as prospective sexual partners may be seen as part of the (social) environment relative to a single organism, but I am no authority on the matter. I suppose this boils down to whether the social environment is considered distinct from the *natural* environment, or like I see it, a sub-component of it.

behavior, not just isolated from each other, but as incremental chains over long time spans, and by countless associations to each other.

### **The role of consequences**

S-O-R (stimulus-organism-response) also ignores the significance of a consequent stimulus, in one organism-environment relation (behaviorism stresses behavior-environment relation to be precise (Skinner, 1976)), as an antecedent stimulus in a future behavior-environment relation. To put this in a more comprehensible way, and more to the point, S-O-R ignores the role of consequences as shaping, and causal antecedents to, behavior.

This role of consequences is widely accepted as a principal causal mode (or functional-historical process) in the evolutionary history of species and (other) biological populations (phylogeny), most commonly called *natural selection*, whereby fitness (amount of offspring surviving to reproductive age, their subsequent amount of offspring, and so on) is the *antecedent* to phenotypic adaptive traits and behavior.

For many reasons, the role of past consequences in determining, shaping behavior during ontogeny has been widely ignored within psychology, with the exception of its championing perspective, behaviorism. The structuralist distinction between biology and psychology is held firm by folk psychological notions that that our actions are caused by our conscious will—our mental capacities, or mental processing (cognitivist lingo), or mental modules (cognitivist evolutionary psychology). History and controlling, shaping stimuli is necessarily associated with determinism in the sense of causal control, while the mediating *organism* in S-O-R may more easily be associated with choice, or internal locus control, as causally mediating factor.

Having said this, there are infinitely more sophisticated defenses of mental processing as principal cause of behavior, for example by Dennett. I will attend to some of his perspectives later in the essay.

## 1.2 Summary

In this chapter I briefly go through the coming chapters in the thesis.

Genes and environment are incompatible causal categories. Genes are proximate causes, while the environment remains a matter of ultimate causation, similar to its attributed role in evolutionary biology, only then shaping the organism and its behavior through the course of its lifetime (ontogeny), rather than the heritable traits (gene-pool) of its ancestral species/population through the course of its evolutionary history (phylogeny).

A better way to understand behavior is to compare compatible causal categories—the shaping of behavior through the lifetime history of the organism, compared to the shaping of behavior through the evolutionary history of species and populations; in other words, to exchange the nature-nurture perspective with a perspective of phylogeny-ontogeny.

Skinner (1966) puts forward such a perspective in *The Phylogeny and Ontogeny of Behavior (Phylogeny-Ontogeny)*. Meanwhile he presents a view of how behavior is selected and shaped on three levels, through natural selection in phylogeny, operant conditioning in ontogeny, and special modes of operant conditioning on a cultural level, in *Selection by Consequences (Consequences)* (Skinner, 1981).

Skinner's outline provides an interesting and valuable outlook on how behavior is shaped through phylogeny and ontogeny, and within cultures.

Problems and challenges with Skinner's outlook also arise, however.

Evolutionary biologist Maynard-Smith criticizes Skinner for having an outdated view of evolution (Smith, 1988), which I find to be the case.

Eysenck, the well-known personality psychologist, criticizes Skinner for ignoring individual differences—reminding Skinner that what may be positively reinforcing to an introvert can be negatively reinforcing to an extrovert.

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I argue for a solution to these problems by building a framework where behaviorism seamlessly extends modern evolutionary biology: A neo-Darwinian behaviorism.

In modern evolutionary biology the very process of natural selection is considered work on an intra-population level. In old Darwinism, the view that Skinner based his approach on (1990), whole species or groups were considered to be the proper units of selection, arising and competing among each other for survival—differential reproductive success between whole competing groups and species—called *group* selection. Among the insights of modern evolutionary biology, however, is that the central component of differential reproductive success is *within* the population.

Another important breakthrough in modern evolutionary biology was the modern evolutionary synthesis, which brought together Mendelian genetics and Darwinian evolutionary biology. This synthesis would combine two explanatory approaches, Mendelian genetics on the one hand and Darwinian evolutionary theory, that had hitherto been competing theories of human evolution, into a seamlessly complementary theoretical framework.

Meanwhile, in my criticism of nature-nurture I argue that that the environment in ontogeny is an ultimate causal factor or explanatory mode, appropriate to ontogeny, or psychology. If the environment is an ultimate causal factor in ontogeny, what then should we consider to be the proximate causal factor? Could it be that ontogenetic contingencies, especially learning, are ultimate causal factors appropriate to psychology, while the organismic structure, more specifically its neurophysiological structure, is a proximate cause of behavior. In doing so, I argue for a modern synthesis in psychology, for ontogeny; based on, and parallel to, the classical modern synthesis between Darwinism and Mendelian genetics, for phylogeny.

Rather than going into detail on the role of the brain and its neurophysiological properties (which could easily fit a dissertation of its own), I simply identify and propose the brain and central nervous system as the central locus of behavior replication in ontogeny, parallel to role of genomes (gene-pools) in phylogenetic evolution. Accordingly we should look the brain and central nervous system for

proximate explanations of the behavior of organisms throughout their lifetime, and for ultimate explanations of behavior of organisms in the combined product of ontogenetic, cultural contingencies and the phylogenetic contingencies they *extend*.

This certainly solves some problems related to the question of locus of control in psychology. It is not anymore a question of *whether* behavior is caused by mental mechanisms, *or* reward and punishment. A synthesis implies that behavior is proximately caused by neurophysiological states, which incorporate private, mental states, like the cognitivist (at least if he was versed in philosophy of mind) would hold, while ultimately caused by ontogenetic contingencies, particularly learning.

This outlook offers a standard by which to view phylogenetic, naturally selected adaptations. They may be considered to be shaped by natural selection through phylogeny, then be realized, tuned and further shaped through learning, principally operant conditioning, during the lifetime, under what I call an *operant regime*.

Finally, I try to draw a full circle, towards my original goal, providing an analysis of how we may view evolutionary biology in relation to culture.

I criticize one of the prominent theories of cultural evolution proposed by cognitivist evolutionary psychology, *mimetics*, for lacking a notion of selecting consequence, and lacking any distinction between replicator and interactor. All of which my proposed Neo-Darwinian radical behaviorism have, I argue, to great benefit for understanding the seamless causal relationship between evolutionary biology, the learning organism and culture.

Finally, I provide a summary and a conclusion. Among my concluding thoughts are that I think that the relevance of operant conditioning to human behavior and cultural evolution, is still one of the most interesting questions to be asked within evolutionary biology and psychology. Operant conditioning truly is *the* alternative theory to mainstream psychology. The question is if there is a selecting consequence in ontogeny—an omnipresent functional patterns that defines it, or, like Dennett

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suggests, natural selection would have never favored operant conditioning, over thousands of other adaptations relevant to human psychology (1995).

On the more general note of the implications of viewing the psychology of man in terms of causal categories, proximate and ultimate, I feel confident that this will one day be mainstream. Either there will have to be built a psychology of learning within cognitive psychology, which deals with how the behavior of man is incrementally changed by the environment over time, or it will have to truly accommodate the behaviorist approach to learning. Not based on naive S-O-R terms, where cognitivism describes the contribution of the organism ("O") and behaviorism describes the contribution of the environment ("S-R"). Rather, as I argue as a central sub-thesis, radical behaviorism (SD-R-SR) will deal with ultimate causes and explanations of behavior appropriate to psychology (ontogeny *extending* phylogeny), while neurophysiology, encapsulating the mainstream, cognitivist approach to psychology, will find its appropriate role in providing proximate explanations of human behavior. Without getting into complex questions of philosophy of mind, psychology as natural science identifies neurophysiology first, then mental states as private properties of that neurophysiology.

Let empirical evidence decide, I say, but let good philosophy of science provide a foundation for theories about the world to be built upon.

### 1.3 A note on causation

I have not dealt exclusively with the subject of causation in this essay. The Darwinian evolutionary process, of variation, selection and retention, is probably often seen as a distinct kind or type of causation, or "causal mode" (Skinner, 1981). I find it helpful to view biological evolution as simply a process (consisting of more basic causal parts), rather than necessarily a special mode or type of causation. Sometimes I refer to the Darwinian evolutionary process, and also operant conditioning, as historical-functional processes, denoting to how variation has been selected *historically* based

on a measure their *functioning* (for example, by reproductive success in the case of the Darwinian evolutionary process).

I use the word causation throughout the essay in a common sense, though scientific, sense to denote to temporal antecedents reasonably thought to be of causal significance. Addressing causation per se could of course be of great value to the subject matter, especially operant conditioning, as it would deal with the extent to which antecedent selecting consequences can be considered to cause behavior, rather than simply forming a functional or correlational (Skinner, 1969, p. 89) relation between independent (reinforcing stimulus, "reward") and dependent variable (behavioral response).

## 2. Background

### 2.1.1 A philosophical issue?

*Behaviorism is not the science of human behavior; it is the philosophy of that science. (Skinner, 1976, p. 3)*

To me philosophy of science, especially one addressing a specific science, like biology or psychology, is about laying the groundwork for scientists to do what they do best: To form hypotheses, theories and test them against empirical data. It can be argued that science can be done without much attention to philosophy of science besides a basic scientific method, but I imagine that much time will be wasted. Psychology is a good example. The one discipline that needs attention from philosophy the most, due to the difficult nature of its subject matter, seems to me to have left the nest of philosophy prematurely. While biology is sharply divided into specific fields, evolution, genetics, physiology, of which mutual and relative causal relevance are hardly ever mistaken, psychology remains a muddled field, pretending to study mental states, cognition, emotion, neurobiology, behavior and learning, and so on, without very serious attention to mutual and relative causal, explanatory categorization.

Questions that reach unbearable complexity and levels of speculation in the philosophy of mind, may, I think, at least be pragmatically and tentatively solved within philosophy of science. For example, the nature and function of consciousness may be out of reach of both current scientific research and settlement of analytic questions within philosophy of mind, but the causal status of mental states versus neurophysiology for the purpose of a less messy psychological science may be within reach. I envisage a psychology that is divided into studies belonging to distinct causal categories, much like genetics relative to evolutionary biology. Building on the *philosophy* of psychology known as behaviorism I seek to outline a seamless understanding of the causal relationship between evolutionary biology and culture, with psychology of learning as the linking discipline. This very process includes what I brand a modern synthesis for psychology, modeled after the modern synthesis of

evolutionary biology, between Darwinian evolutionary biology and Mendelian genetics, which is the essential premise of today's Neo-Darwinism. This includes the key distinction between proximate and ultimate causation, which greatly clarifies the subject matters of biology, or phylogeny, but which plays no role in psychology, or ontogeny. Psychology is still filled with questions of *whether* behavior is caused by mental states (cognitivism, or cognitive psychology) or by environmental contingencies like reward and punishment (straw man accounts of behaviorism, or behavior analysis) (Sterelny & Griffiths, 1999).

The interdisciplinary approach I take has a good view from philosophy of science. Philosophers are not disproportionately invested in one particular scientific field, from which one would then typically form a prism with which to view all other sciences. Besides their arm chair of course. I have not fought it very vigorously in this thesis. My focus is generally very philosophical and theoretical, leaving empirical support or dismissal to science itself. For example, I do not take a standpoint as to the relevance of learning versus evolutionary biology regarding some human behavioral repertoire. Instead I hope to outline what is perhaps a better way to answer such questions. I think that a philosophy of science that prematurely or statically answers empirical questions is simply bad philosophy of science. Good philosophy of science sets the stage for empirical data to work its magic. Then again good philosophy of science may be, or perhaps even should be, sensitive to empirical findings. If it turns out God is real for example and has endowed mankind with the gift of eternal souls and first cause, genuine free will, then perhaps standard philosophy of science based on physicalism and approximate determinism may not be a good idea anymore.

### **2.1.2 What is radical behaviorism?**

#### *Function and Consequence*

The folk psychological notion of behaviorism, that it is concerned with the reward and punishment of behavior, is not so much wrong, but a very simplified understanding of one its basic philosophical premises. The most significant

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philosophical influence on behaviorism is pragmatism (Saugstad, 2000). The idea, borrowed from pragmatism, is that the function, or one could even say meaning, of a behavior is determined by its consequences.

***Operant conditioning is a notion of the controlling stimuli of so-called voluntary behavior***

Voluntary behavior, associated with purpose and intention, is seemingly not subject to any controlling stimuli, which is part of the reason why the behavior appears so free of restraint, or even causation. There is not necessarily any stimuli eliciting a response, which is close in time.

***Operant conditioning is a creative mechanism which replaces a creative mind***

The cognitive scientist may want to ask himself; which proposed "mental modules" supposedly facilitate human creativity, advancement—the incremental production of novel, functional behavioral repertoires, besides a creative mind which itself cannot be explained. Can it really take place without a mechanism of variation, selection and retention in the course of the lifetime of the individual and/or in the culture over time? In behaviorism this challenge is confidently answered by proposing operant conditioning as a process of variation, selection and retention in individual learning, as well as the surrounding culture (Skinner, 1981). Some type of supply of behavioral variation, for example random responses by the organism, recombination of responses, or variations triggered by releasing stimuli in the environment constitutes the variation. Selection is firstly facilitated by consequences associated with stable historical reproductive value in the organism's ancestral populations/species (primary reinforcement), and other consequences, which have gained some logical association with primary reinforcers during the course of the organism's lifetime (secondary reinforcement). These consequences select behaviors, which then increase in likely frequency of reoccurrence (thus the name reinforcement). The next time the response occurs under similar settings (discriminating stimuli), it may be further shaped and elaborated upon, by further selecting consequences, thus creating novelty in terms of behavioral repertoires. This constitutes a "creative"—constructive—mechanism that does not presuppose a creator, or higher intelligence, just like natural selection

replaces the need for a creative intelligence or God to explain the existence of life, especially complex life.

### ***Behavior analysis—the science***

We have already covered the aspects of radical behaviorism. Radical behaviorism itself is the broad philosophical approach to psychology, operant psychology is the central psychological perspective itself. Finally, behavior analysis is the empirical science on which the philosophy and psychological perspective has been built, inductively. Behavior analysis, according to the radical behaviorist philosophy of science, is largely free from a hypothetic-deductive theoretical base. Rather it is an inductive empirical analysis of behavior, where theory is formed and shaped from what is observed experimentally. The goal of behavior analysis is prediction and control of behavior. The operant analysis of behavior, as such, began as an analysis of voluntary, purposeful behavior, which is of greater interest, especially in the service of understanding human behavior. Voluntary behavior, which seemingly is devoid of any triggering, releasing—*controlling*—stimuli. However, building on Thorndike's Law of Effect (Saugstad, 2000), Skinner found that the controlling stimuli were found in the antecedent consequences that shape behavior. Thus prediction and control of voluntary, purposeful behavior could be within reach after all, and an operant psychology saw the light of day.

### ***The notion of control***

In radical behaviorism (RB) there is a notion that contingencies control the behavior of organisms, including persons. This perspective may be counter-intuitive to many, as it may give an association to something similar to puppets with strings attached. Such analogy probably isn't totally way off either, although behaviorism does not view man as, or liken man to, a puppet. In RB control refers to the environment as the initiating agent, much like a puppet master initiates the behavior of a puppet. There are distinct differences though, first of all temporality. Controlling contingencies may lie far away in the past. Also the contingencies are thought to shape behavior as a matter of cumulative selection (Skinner, 1976). Although it is tempting to speak in terms of causation, and as such causal control, the reference to causation is often

wisely left out in favor of experimental control, in terms of relationships between independent and dependent variables (Skinner, 1969). Where the ice is at its thickest one could talk about correlation instead. A bolder, but more accurate and fruitful estimation, in my view, is that the contingencies are functional-historical processes. Functional because the selecting consequence assigns functional (pragmatic) value to the response. Historical because the selecting, shaping consequences form a history which the behaviors are a function of.

Let us go back to temporality. For phylogenetic contingencies, through the insight brought by the modern synthesis between Mendelian genetics and Darwinian evolutionary biology, it can be said that natural selection co-controls behavior by proxy, through the carriers of heritable traits, most centrally genes. This also helps explain the notion of how contingencies lying millions of years in the past can be said to co-control and co-determine contemporary organismic bodies and their behavior (phenotypes).

I invite the reader not to think in terms of unnecessary complexity, or something hidden and overly abstract when thinking about contingencies (although thinking in terms of contingencies requires some level of abstraction). A Darwinian contingency constitutes variation in terms of heritable traits, selection in terms of differential reproductive transfer of those traits, and retention by the transfer of those selected traits from one generation to another. The Darwinian contingency is fruitfully elaborated upon by distinguishing between replicator and interactor. The interactor is the organismic phenotype interacting—surviving and reproducing—with environments that pose different problems and challenges. The replicators are the genes contributing or corresponding to those traits, which then by definition are heritable (Sterelny & Griffiths, 1999).

### 3. Synthesis between modern evolutionary biology and radical behaviorism

#### 3.1 Nature-Nurture

Nature-nurture leads evolutionary, psychological thinking astray, as there is strong predisposition for asking about which causal factor is most significant and to what extent. This is almost always a fruitless, useless way to problematize the relation between phylogeny and ontogeny, because the ultimate causal contribution of each occurs at (1) different temporal stages, (2) concerns single organisms and populations (species) respectively, and (3) thus typically (but not always) provide different and complementary causal contributions to the determination of human behavior.

#### *Dynamic causal categories*

Since nature-nurture conveys a static image of the contribution of genes versus environment, the crucial aspect of change, intervention—changes in environmental variables, whether by design (read: explicitly) or occurring "naturally" (read: non-explicitly)—the dynamic reality of how phylogeny and ontogeny co-determines behavior is lost.

Take for example the notion of genetic versus environmental causes in various medical diseases. Based on the heritability factor, the most common form of quantifying nature-nurture, such and such amount of *variation* in a *population* may be considered due to genetic factors, while the remaining part is attributed to environmental factors. The most common criticism against accounts of heritability is that it only accounts for variation in populations. In a society where everyone smokes exactly 20 cigarettes a day, the relevance of genetic factors to lung cancer will skyrocket, since the environmental exposure to cigarettes will be very similar for every individual, leaving a lot to be determined by individual genetic differences. Meanwhile the environmental contribution to lung cancer per individual will be very significant, but masked by a system of calculation which is often useless for

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understanding genetic versus environmental causes in individual organisms and their behavior.

But there is even more damning criticism towards the nature-nurture approach, regardless of mode of quantification (in the above case, heritability): Nature-nurture inherently fails to recognize that the relative contribution of genes versus environment is a function of dynamic causal factors, such as changes in the nonsocial or social environment, whether non-explicit ("natural") or explicit (arranged, intervention). The problem with heritability is not just that it deals with variation in populations, but its inherent focus on fixed causal contribution accounts, instead of recognizing that one of the main interests when estimating the significance of ontogeny, is the potential for *intervention* — the extent to which the systematic manipulation of ontogenetic, cultural variables can influence x trait or behavior, whether it is a psychiatric disorder, a medical condition, or functioning in school or at work. Behavioral geneticists will thus often make the mistake of estimating how much the environment matters for x trait or behavior, based on accounts of twin studies, where the genetic dispositions are identical (identical twins), but the different environments in which these have been raised have no systematic account of differences in relevant ontogenetic variables (the identical twins have simply been raised in different families of varying cultural difference, for example as part of adoption). A truly interesting study, by comparison, would test the effect of *systematic* changes in environmental variables—intervention—between individuals (including twins and identical twins—typically in terms of control/non-intervention versus intervention) or between groups.

## 3.2 Selection by Consequences

In this chapter I will present the article that lays most of the foundation for my thesis and analysis: Skinner's article *Selection by Consequences* (Skinner, 1981).

Skinner argues that three causal modes govern human behavior: *Natural selection*, *operant conditioning* and a special implementation of operant conditioning where the

variation in the behavioral repertoire of organisms is acquired from *social learning*, amounting to cultural evolution (1981).

Skinner presents selection by consequences as a *causal mode* “found only in living things, or in machines made by living things” (1981, p. 501). As humans are cultural living things, Skinner suggests that three kinds of selection by consequences concern and govern human behavior. *Natural selection* is the first and principal mode of selection by consequences. Skinner attributes its beginning steps to “that extraordinary moment when a molecule came into existence which had the power to reproduce itself.” (p. 501). Skinner also identifies reproduction as the consequence in natural selection, although he prefers to describe the contingencies associated with natural selection as “contingencies of survival”, which I interpret to refer to his group or species selection perspective and selection in terms of entire species surviving, while others die out. Consistent with modern evolutionary biology, and an intra-population selection perspective, I will use the term *contingencies of fitness*.

Skinner identifies *operant conditioning* as a second kind of selection by consequences, one that has evolved as a product of natural selection. He describes its appearance as follows:

*It must have evolved in parallel with two other products of the same contingencies of natural selection—a susceptibility to reinforcement by certain kinds of consequences and a supply of behavior less specifically committed to eliciting or releasing stimuli. (p. 501)*

In other words operant conditioning is composed of variation in behavior that is not exclusively controlled by eliciting or releasing stimuli *and* is susceptible to reinforcement by certain consequences. The quote is somewhat confusing: It is behavior that is reinforced, so susceptibility to reinforcement must relate to the mentioned behaviors. However, the relation is flexible—a behavior can be selected—based on its consequences. Furthermore, the behaviors are per definition not

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specifically (exclusively) committed to eliciting or releasing stimuli<sup>3</sup>, when they are in fact sensitive to reinforcement, but the combination of initial elicitation and subsequent reinforcement of a behavior is presumably not uncommon.

Furthermore, it is explained that food is presumably not reinforcing to a species that eats food only *because* of its survival value. This is in need of explanation, and lacks the clarity usually typical of Skinner. Survival value is here presumably pointing to “contingencies of survival”, Skinner’s description of the central contingencies in phylogenetic evolution, typically associated with natural selection (see earlier definition and discussion of natural selection). The main point here is that an event (eating food, in the given species) that is only the product of natural selection is typically not reinforcing. The food will instead typically become reinforcing when the species has developed “special susceptibility” (p. 501) to it in the course of phylogenetic evolution. This special susceptibility facilitates ability to learn “new ways of gathering, processing, and ultimately cultivating foods”. The point then, is that when eating food is just a simple response in a species, food as such will not be a reinforcer and is presumably not reinforcing. In a sense it can per definition not be reinforcing, as reinforcement is commonly defined functionally in relation to a behavior—Skinner is probably implicitly pointing to how in the course of phylogenetic evolution, the mutations responsible for one functional trait can very well appear with significant temporal space between them. It would have been more clarifying if Skinner had simply written that certain events, like eating food, tend to become, or simply become, reinforcing to a species, when there has been phylogenetic selection for learning new ways to find food. Phylogenetic selection for learning new ways to find food, in the case of operant conditioning, constitutes that there must be a means of producing behaviors that are not fixed responses (variation) *and* that the future frequency of repetition of those behaviors must be sensitive to the (relevant) consequences that they have produced in the past.

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<sup>3</sup> Stimuli that evoke a response or releases a complex fixed response in the organism, in the temporal relation stimulus-response.

Of course, the ability to learn new ways to find food typically has obvious survival value, so Skinner's initial appeal to "survival value" as a non-platform for an event being reinforcing, is indeed confusing, even misleading. What Skinner hints at, however, and rightly so, is that reinforcement is not directly attached to survival value. Operant conditioning frequently selects dysfunctional and maladaptive behaviors. Skinner writes:

*New ways of gathering, processing, and ultimately cultivating foods and new ways of behaving sexually or of behaving which lead only eventually to sexual reinforcement can be shaped and maintained. The behavior so conditioned is not necessarily adaptive; foods are eaten which are not healthful, and sexual behavior strengthened which is not related to procreation. (Skinner, 1981, p. 501)*

Eating food that is unhealthy refers primarily to a reinforcing effect common to different stimuli and not to operant behavior specifically, and is as such not the best example of reinforcement of maladaptive behavior. The example of sexual behavior is better, I think, as it describes clearly how the reinforcing consequences (sexual contact) not only reinforce behaviors that have survival value (reproductive value). It is also an excellent, yet very simple account of sexual perversion. Sexual perversion can plausibly be thought to persist especially when there is no alternative reinforcement of sexual contact that has reproductive value—because then there is no *differential reinforcement* that functions to remove the maladaptive sexual behavior.

### **3.2.1 Maynard-Smith**

J. Maynard-Smith's critical response to *Consequences* is centered around the claim that Skinner has an inadequate understanding of evolutionary biology (Smith, 1988). I have already noted the distinction between "contingencies of survival" and "contingencies of fitness", which I hold to refer to "*survival* of the species" and, my correction, reproductive success, frequency of offspring, as the principal selecting consequence in phylogeny. When I first considered this distinction, and also initially upon reading J. Maynard-Smith's critique, I did not give it very much attention. I thought that it was a minor flaw, which would merely require a comment and a

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correction, without any great bearing on the evaluation of Skinner's central theoretical framework, operant psychology.

It was not until I read Skinner's last published article, *Can Psychology be a Science of the Mind? (Science-Mind)*, that it finally occurred to me how heavily Skinner's operant psychology is influenced by old, outdated Darwinian theory (1990). In *Science-Mind* Skinner writes:

*[...] the natural selection of the behavior of species (ethology), the operant conditioning of the behavior of the individual (behavior analysis), and the evolution of the social environments that prime operant behavior and greatly expand its range (a part of anthropology). (Skinner B. F., 1990, p. 1208)*

And also:

*All types of variation and selection have certain faults, and one of them is especially critical for natural selection: It prepares a species only for a future that resembles the selecting past. Species behavior is effective only in a world that fairly closely resembles the world in which the species evolved. (Skinner B. F., 1990, p. 1206)*

Skinner sees natural selection as selecting "the behavior of the species", while operant conditioning selects the "behavior of the individual". While a convenient distinction, this is a serious over-simplification. Here is what I think Skinner has thought: Since there are, in his view, phylogenetic contingencies selecting essentially whole species, there can be neatly isolable "species behavior" attributable to those contingencies.

Although there is definitely species selection in the sense that whole species can die out, whereas others continue to reproduce, due to their respective histories of natural selection, this is simply not the basic, fundamental way natural selection works. It is more of an emergent/extraordinary way, so to say—at best an auxiliary, peripheral part of the evolutionary process. The basic process of natural selection works on the intra-population level: Frequency of genes transmitted to subsequent instances of the gene pool of the reproductive population (species). Genes are carried by single organisms that transmit them differentially to the next generation, compared to other

organisms carrying slightly different genetic variation, thus the term *differential fitness*. New species (speciation) do not simply arise from variation, but from a population's *history of reproductive isolation* (Sterelny & Griffiths, 1999).

The group selection perspective becomes problematic in explaining why and how natural selection can cause adaptive traits of incremental sophistication. There is no variation until whole species have evolved (presumably without shaping by natural selection, but as a matter of pure chance), and there is no selection until one succeeds and the other does not (or to a larger degree). The species- or group-selectionist sees a whole species (or otherwise distinct biological population) emerging from random variation, only then to be subject to differential selection compared to other species. Skinner probably viewed the human species in this fashion—it has been favored over other species in terms of group survival, and has a species-specific behavioral repertoire to show for it. This is not principally how the human species has evolved, but it does not deviate enough from newer Darwinism to create a great problem for Skinner's theorization, yet.

It is when Skinner treats the level of the individual organism that the implications of old Darwinian theory have truly negative consequences: Having attributed "species behavior" to "contingencies of survival" of, presumably, entire species, in phylogeny, Skinner attributes "individual behavior" to "contingencies of reinforcement", in ontogeny. Not only does this neglect genetic individual differences, which are undoubtedly a matter of "individual behavior" of phylogenetic, naturally selected origin, but also more importantly, individual behavior is, apart from a foundation or endowment of "species behavior", attributed almost entirely to operant conditioning, which is held to be responsible for the selection of individual repertoires. The result is what I call "flat phenotype" organisms, which share some unfortunate similarities with another well-known notion, namely the blank slate, or *tabula rasa*, something Skinner has been criticized heavily for, but mostly from people who did not understand his overall theory, confusing it with S-R psychology and/or mistakenly requesting attention to the contribution of the organism, along the lines of the structuralist organism-environment dichotomy (S-O-R), most notably Chomsky

(1967). The flat phenotype organism is only endowed with a relatively simple set of innate behavioral repertoires, some biological constraints on learning, and a set of primary reinforcers. This is already far from a blank slate view, since primary reinforcers themselves have major implications on behavior, something which Skinner often pointed out, while also pointing out their phylogenetic origin (1981, p. 501). But as Eysenck points out (1988, p. 424), Skinner does not take into account (genetic) individual differences in reinforcement as a matter of (unlearned, innate) personality traits, something that would require a notion of individual behavioral disposition of phylogenetic (proximately, genetic) origin.

According to Skinner, the entire *individual* behavioral repertoire is largely, or even principally, built by operant conditioning during the lifetime. The main problem here is not so much the individual, because operant conditioning (and learning generally) probably accounts for a lot of the details in an individual-specific repertoire, but the notion that operant conditioning *alone* is mostly (if not solely) responsible for the behavior. The most fallacious result is that Skinner over-estimates the exclusive role of operant conditioning in shaping the behavior acquired during the lifespan of the organism.

*Table 3-1: Group selection versus intra-population (gene) selection and consequences for radical behaviorism*

	<b>Group or species selection</b>	<b>Intra-population selection (typically viewed in terms of gene selection)</b>
<b>Historical association</b>	Old Darwinism	Modern evolutionary biology; Neo-Darwinism
<b>Association to behaviorism</b>	<i>Radical behaviorism</i> (Skinner, 1976)	Proposed in this thesis as an integral part of synthesis between modern evolutionary biology and radical behaviorism. I have thus called

<p><b>Provenance of behavior</b></p>	<p>Phylogenetic contingencies govern species behavior, while ontogenetic contingencies govern individual behavior (Skinner, 1981; 1990)</p>	<p>the proposed position <i>Neo-Darwinian radical behaviorism</i></p> <p>Phylogenetic contingencies shape populations and species based on differential selection of individual genetic differences <i>within</i> the population. Individual genetic differences, shaped by phylogenetic contingencies, are thus an integral part of individual behavior.</p>
<p><b>Role of ontogeny versus phylogeny in shaping behavior</b></p>	<p>Phylogeny and ontogeny shape different behavioral repertoires (innate versus learned) (Skinner, 1990)</p>	<p>Phylogeny shapes a host of adaptations relevant to behavior, of which the learning processes are central, but not omnipotent, in ontogeny (division of labor between phylogeny and ontogeny—“innate versus learned” only occasionally fruitful distinction)</p>
<p><b>Implications</b></p>	<p>“Flat phenotype”—limited acknowledgement of naturally selected contribution to behavior; primary reinforcement, species-specific constraints on learning, innate, simple respondent behaviors (Skinner, 1990). Typically confused with <i>tabula rasa</i>, lacking understanding of the nature of primary reinforcement.</p>	<p>“Rich phenotype”—operant conditioning is one of many phylogenetic adaptations. Its special relevance to ontogeny and culture rests on the fact that it is a creative causal process (causal mode) (Skinner, 1981), and that it constitutes an ontogenetic measurement of function thus relating specifically to the natural functionalism and pragmatism</p>

One of my premiere goals in this thesis, if not *the* goal, is to establish a neo-Darwinian operant psychology, and in contrast with Skinner's attribution of levels of selection, I emphasize that ontogenetic contingencies are *extensional*. Instead of a flat phenotypic view of organisms, especially humans, one must acknowledge a *rich phenotype*, with a potential host of phylogenetic, naturally selected adaptations of relevance to behavior, such as memory, hypothesis testing (Popperian, advanced Skinnerian capability), even potential support for advanced verbal behavior. However, I insist that my efforts are mostly a matter of refining and updating the theoretical coherence between operant psychology and Darwinism—the changes are, I suggest, incremental and evolutionary in their own right, rather than so much a denouncement of traditional operant psychology, or a revolutionary change. At the heart of my project lies the translation of “contribution of the organism” (Chomsky, 1967) (which is a mistaken but understandable criticism against operant psychology) to contingencies, phylogenetic and ontogenetic. My contention is that the structural properties of the organism, which many seem to want to somehow accompany the operant analysis (an impossibility due to causal categorical incompatibility), can instead be taken into account theoretically by formulating them as naturally selected adaptations alongside operant conditioning (itself a phylogenetic adaptation). My Neo-Darwinian approach to operant psychology thus ambitiously suggests that a host of neurological/psychologically relevant phylogenetic adaptations work alongside operant conditioning during the lifetime of the organism—I call this arrangement an operant regime. The functional analysis of verbal behavior offered by Skinner is still of great importance, as culture—verbal communities—carries a lot of (if not most of) the behavioral variation and repertoires responsible for advanced verbal behavior. Cultural operant selection and evolution has no doubt greatly advanced the complexity of language. Language could probably never have evolved as a process of natural selection alone—it is a signature product of co-evolutionary process (see chapter 3.4 below for a discussion of the Baldwin Effect, which has some similarities

and properties in common with co-evolution). I do however suggest that there is naturally selected support for advanced verbal behavior in humans, something which Baldwinian selection alone, suggests.

The Neo-Darwinian view, as I formulate and see it, requires at least one important thing: Acknowledgement of the significance of other naturally selected adaptations than operant conditioning on the behavior acquired during the lifetime of the organism. A flat phenotype view sees the genetic endowment only as functional for contributing with a minimal set of responses, with which the organism has acquired from the “problem solving” that the species has encountered, whereas the rest of the “problem solving” is done by operant conditioning within the lifetime. A rich phenotype view acknowledges the *working* of phylogenetic adaptations throughout the lifetime of the organism. Operant conditioning is not the *only* kind of problem solving feature in ontogeny, though it is the most important one (since it features variation, selection, retention, where selecting consequences are found in the *real* environment).

Skinner’s species selection view also poses a problem for culture. Skinner borrows the emphasis on groups (species) in phylogeny, in species selection, and applies it to his treatment of culture. Thus he identifies whole cultures, their total combined set of practices, as the principal unit of selection in cultural evolution (Skinner, 1981) whereas he should, consistent with a neo-Darwinian analogy, simply identify single, particular *cultural practices* as the proper unit of selection. Given that cultural evolution on the basis of cultural contingencies of reinforcement actually occurs, the same principles of intra-population selection as apply to biological evolution, are bound to apply. Competition between whole sets of cultural practices encapsulated in different cultures lacks the abundance of variation, the subjection to persistent, same and similar selecting consequences, and consistent, relatively frequent self-reproduction, that marks a Darwinian-like evolutionary process. It is plausible that inter-culture selection is a peripheral, auxiliary evolutionary process, just like group selection in phylogeny might be.

### 3.2.2 Barlow—ultimate and proximate causation mistakenly identified with biological evolution and culture, respectively

Barlow (1988) presents an interesting criticism of *Consequences*. He argues that Skinner makes a mistake by not distinguishing between proximate and ultimate causal mechanisms, regarding kin-selection in phylogenetic evolution and selection of behavior benefiting the cultural group, at maladaptive cost of the individual, on the ontogenetic, cultural level. He claims that kin-selection favoring heroic behavior (example of behavior which is maladaptive to the individual, but beneficial to the group) is the ultimate causation, while teaching of heroic behavior in contemporary cultural communities is the proximate causation (Barlow, 1988, p. 22).

*What we have here is a failure to distinguish between proximate and ultimate mechanisms (E. O. Wilson 1975) [Barlow's reference]. The hero is taught to behave that way, which is the proximate mechanism. In small societies in which heroism must have evolved, the hero's kin enjoyed improved reproductive fitness to that of individuals who were not so easy to train. This is kin selection, the ultimate causation. A common generative principle is reasonable. (Barlow, 1988, p. 22)*

While I agree that a distinction between proximate and ultimate causation is essential to understanding levels of selection, Barlow is somewhat mistaken about attribution of those causal categories. He essentially bundles cultural *as* proximate causation and phylogenetic evolution *as* ultimate causation into one science (something like sociobiology perhaps?). Thus he blocks understanding of culture as an evolutionary—and causal—process distinct from biological evolution. Culture is of course *not* the proximate causation of biological evolution, which is what Barlow suggests (that would imply that culture is solely the product of natural selection, and can only evolve subject to contingencies of fitness).

The correct account, I suggest, is something like this: Phylogenetic contingencies, including natural selection, are ultimate causes appropriate to evolutionary biology. The genotype, and largely phenotypic genetic expression, consists of proximate structural causes appropriate to genetics and functional biology/physiology, respectively. Ontogenetic contingencies, including operant conditioning, are ultimate causes appropriate to psychology of learning (for example radical behaviorism,

operant psychology). The structure of the organism at any given time during ontogeny is a proximate, structural cause of its behavior, while the ultimate causes of its structure, as such, is a product of ontogenetic contingencies extending (combined with) phylogenetic contingencies. Cultural contingencies, when viewed as part of psychology of learning, are themselves ultimate causes of behavior.

Barlow's account is not really wrong though, but limited. His quoted reasoning is correct and even quite sophisticated, as he refers to kin-selected *learning ability*, thus actually correctly identifying the *susceptibility to learn* heroic behavior as a proximate explanation, relative to the ultimate, kin-selective contingencies. However, it is unclear which role the cultural community has here. Are causal properties assigned to it, distinct from biological evolution? Clearly not, besides embodying the proximate realization of kin-selective natural selection as ultimate cause. What becomes very important here is Skinner's assertion that operant conditioning is a *causal mode* (1981, p. 501), in a similar vein as natural selection. What this means is that operant conditioning produces (or, as I think, often refines, further develops) adaptations of incremental sophistication<sup>4</sup>, in the same way that natural selection does, both being proper variation-selection-retention contingencies. Given that no such "creative" process takes place in learning or cultural evolution, Barlow's account may be sufficient. After all, of what major significance is learning and culture at all, if not as a distinct process of creativity-without-creator that variation-selection-retention contingencies afford? It was because of this concern that Skinner rejected the "the creative mind" (1990), which he attributed to cognitive psychology, because it effectively functions as a smoke screen, covering the true contingencies at work, which are the three kinds of causal modes described in *Consequences* (1981).

When departing from a psychological to an anthropological (cultural) perspective, cultural contingencies are seen as part of a cultural whole, a cultural evolution, or at least a cultural history. Causes that are ultimate in psychology, focusing on the

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<sup>4</sup> I choose the word 'sophistication' over 'complexity', both for natural selection and operant conditioning, to avoid appearing to commit the fallacy of asserting that natural selection (and variation-selection-retention contingencies in general) always lead to complexity, which they do not. There may simply be selection against complexity or for simplicity.

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individual, are not ultimate causes in anthropology. Ultimate causes in anthropology are a matter of selection of practices extending (and ultimately co-evolving with) phylogenetic evolution, principally by cultural, operant selection, covered in chapter 5 below (wherein the properties of primary reinforcers is the most obvious contribution of natural selection, though far from the only one). The structure of whole social, cultural communities is the combined outcome of phylogenetic and cultural contingencies (including sub-cultural contingencies, for example child-rearing). In a densely populated social, cultural environment there are few ontogenetic contingencies that are neatly isolable from cultural contingencies (unless an eremitic, isolated lifestyle is the case), so there can be no helpful distinction between “psychological” and “cultural” ontogenetic contingencies in cultural, socially learning species.

Seeing how psychology offers a complicated intermediate understanding of ultimate causes appropriate to its field, which are no longer ultimate when considering the surrounding cultural whole, may make it tempting to skip psychology altogether (which is what gene-culture co-evolution theory and, to a certain extent, *sociobiology* does (Skinner, 1990)). Like Skinner, I think that may be a mistake:

*Sociobiology, for example, leaps from socio- to bio-, passing over the linking individual. (Skinner, 1990, p. 1208)*

The individual person is the basic systemic, or structural, unit of cultures, and her behavior, how she acquires and transmits new behavioral repertoires during her lifetime is a perfect starting point for understanding culture and cultural evolution. The exchange of socially learned behavior between individuals—practices, is the interacting, functional-historical unit of cultural evolution, as I suggest. Furthermore, understanding how the ontogenetic evolution of behavior extends phylogenetic evolution of primary reinforcers and other adaptations provides a perfect starting point for understanding how natural selection affects culture (including how it initially spawned culture by selection of social learning abilities). I propose that psychology is no less than the key science for creating a bridge between biological

evolution and culture, and I think Skinner also implicitly proposes this in *Consequences* (1981).

I actually favor a sociobiological perspective when it is restricted to the first level<sup>5</sup> of selection, natural selection—the science tells us a lot about naturally selected behavioral *disposition*, including social *disposition*. Level 2 requires a science of behavior studying ontogenetic adaptation (learning), and considering level 3 requires some sort of science of culture (anthropology), though the latter science can be slightly indistinct from its base sciences. Barlow implicitly suggests that level 1 analysis of culture is preferable and complete—something like an erroneous estimation of the scope of sociobiology or evolutionary biology. But his claim does prompt an interesting, important question: Why cannot anthropology be viewed simply in light of natural selection and phylogenetic evolution? Because culture has independent causal bearing on behavior, through naturally selected capacities for imitation, observation and verbal instruction, which enable inter-individual transfer of behavior—practices—which in turn are subject to operant selection. Why cannot psychology consist simply of analysis of contingencies of natural selection? Because organisms can learn. If there were not for learning and, by extension, social learning, Barlow's perspective would be correct. *There would be no need for analysis of behavior beyond evolutionary biology*. It would effectively devour psychology and anthropology (but then again there would be no culture, and no psychology with any substantial subject matter either).

My evaluation of Barlow's perspective is that it is an over-simplification that is much more serious than Skinner's account: Cultural practices are not proximate causes of phylogenetic contingencies, kin-selective or otherwise. Cultural practices are only partly determined by a history of phylogenetic selection. A history of operant selection of cultural practices, based on naturally selected primary reinforcers and capacity for imitation and observation, constitutes a more plausible case.

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<sup>5</sup> As I explain earlier, Skinner defines three levels, or kinds, of selection. The first level is natural selection in phylogeny, second level is operant selection in ontogeny and the third level is cultural operant selection (1981, p. 501-502).

But I agree with Barlow that there is a possible common “generative principle” (1988, p. 22) between the cultural practice of promoting heroism, and natural kin-selection of heroic behavior. Kin-selection may have favored reinforcers that constitute sensitivity to consequences affecting kin, for example enabled by secondary reinforcement, or more likely, by imprinting. Kin-selective contingencies thus extend to ontogeny as a matter of operant selection, but with reinforcement within the lifetime as the selecting consequence, rather than inclusive fitness, which forms a basis as selecting consequence on the phylogenetic level. Finally cultural, operant selection may have favored practices that promote heroism, and promotion of the practice will be most effective among relatively close kin. This is far from Barlow’s over-simplifying culture = proximate causation, biological evolution = ultimate causation. However, I also depart from Skinner on this point, who seems not to consider that kin-selection may affect the shaping of primary reinforcers, which fits his somewhat isolationist approach to the respective contribution of phylogenetic and ontogenetic contingencies, to behavior—in short, he does not see a common generative principle (1981).

*Similarly, altruistic behavior (i) may evolve through, say, kin selection; (ii) may be shaped and maintained by contingencies of reinforcement arranged by those for whom the behavior works an advantage; or (iii) may be generated by cultures which, for example, induce individuals to suffer or die as heroes or martyrs. The contingencies of selection at the three levels are quite different, and the structural similarity does not attest to a common generative principle. (Skinner, 1981, s. 503)*

Lastly, on a positive note, Barlow (1988, p. 21) informatively notes the lack of attention to the debate between group and individual selection<sup>6</sup> in Skinner’s *Consequences*, similarly to Maynard-Smith’s criticism (3.2.1 above).

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<sup>6</sup> I use and prefer the term intra-population selection, to refer to natural selection in the form of differential reproductive success *between* individual organisms, *within* a single biological population or species (illustratively, and typically, but not always), but “individual selection” also makes similar sense.

### 3.2.3 Plotkin and Odling-Smee on interconnectedness between levels of selection

Plotkin and Odling-Smee level two criticisms towards *Consequences* that I want to comment. Firstly, they stress “interconnectedness” between levels of selection, particularly between ontogenetic and phylogenetic:

*The interconnectedness of living systems may be complex, but interconnected they are. [...] learning as a more subordinate process in the hierarchy can never be decoupled from the less subordinate processes in the hierarchy [biological evolution]. Learning of any and every sort must be primed by more fundamental processes, be they genetic or developmental (Plotkin & Odling-Smee, 1988).*

I agree that the interconnectedness of learning and biological evolution should be stressed. I attend to this interest by conveying that ontogenetic evolution (learning) extends phylogenetic evolution, most fundamentally through selection and shaping of primary reinforcers in phylogeny, but also in terms of specific dispositions towards imprinting, altered, prolonged, enhanced effects of contingencies by indistinct modes of learning, particularly memory, constraints on learning, and finally, co-functioning with other phylogenetic adaptations of significance to behavior under an operant regime. This is attended to particularly in sub-chapter 4.2, “Popperian organisms”, where I offer a synthesis between Skinner and Dennett, regarding what Dennett calls “Popperian creatures” (Dennett, 1995).

Development (maturation) is not directly handled in operant psychology, but may indirectly grasped by realization of primary reinforcers and behavioral variation during the lifetime. The best example is puberty, adult sexuality, in which primary reinforcement by sexual contact is increased/realized, through maturation. On a side note, (Neo-Darwinian) radical behaviorism may give a much better account of “psycho-sexual stages” of personality development (Atkinson, Atkinson, Smith, Bem, & Nolem-Hoeksema, 2000, p. 456-457) than Freud’s own speculative account. Rather than so much broad expressions of sexuality, the three first stages, oral, anal and phallic, may simply be naturally selected growth and maturation processes, where primary reinforcement of learning to eat, disposing of feces and urine,

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respectively, sequentially increase in strength, enabling correspondingly intense, sequential learning periods for these skills.

Besides these considerations I find the criticism by Plotkin and Odling-Smee somewhat pessimistic. Operant conditioning, like natural selection, is a historical pattern of functional relations. These patterns potentially enable us to distill great explanatory power from otherwise chaotic fields of study, like biology and psychology. Plotkin and Odling-Smee seem to stress the complexity of things. Indeed, the world appears immensely complex, but only more so when defining causal, functional and historical patterns are ignored.

One example of “complex interconnectedness”, and the other aspect of Plotkin & Odling-Smee’s criticism I want to cover, is actually an implicit reference to the Baldwin Effect:

*[...] circular causal sequences immediately become apparent in the form of “upward” and “downward” causation [...] An example of the two-way causation in such a hierarchy occurs when genes are important and inevitable determiners of how and what is learned, but what is learned is often an important determiner of what genes are fed back (downward) into the gene pool (Plotkin & Odling-Smee, 1988, p. 55).*

First of all, this criticism does not strike me as disqualifying Skinner’s levels of selection. It is merely a specification, but as presented by Plotkin and Odling-Smee, a hastened complication. Genes are “important and inevitable determiners of how and what is learned” by dictating some species-specific constraints on learning, and more, but most significantly in (proximately) determining a set of primary reinforcers, and their general properties, for example in terms of deprivation, as ultimately shaped by natural selection. Some individual differences in primary reinforcement, as a matter of mere intra-species genetic variation/naturally selected individual differences is also true (and not taken into account in Skinner’s old Darwinian view). However, the notion that “what is learned is often an important determiner of what genes are fed back into the gene-pool” is actually an implicit reference to the Baldwin Effect, at least as I interpret it, which I have dedicated a whole chapter to. While I agree with

Plotkin and Odling-Smee that the Baldwin Effect gives considerable insight into the “interconnectedness” of biological evolution and learning, I do not see how it *negates* Skinner’s proposal about selection in phylogeny, ontogeny and culture. Rather, I use the Baldwin Effect (see sub-chapter 3.4) to *elaborate* on and *enhance* Skinner’s theorization. While Skinner is perhaps overly simplistic, his critics here seem to see overly anxious to point out complexities.

### 3.3 Primary and secondary reinforcement

The concept of primary and secondary reinforcers plays a central, absolutely decisive role in my thesis, and I will here begin to explain how and why.

In ontogeny consequences selecting behavior have a role equivalent to that of fitness in phylogeny. Any consequence can function as reinforcement, given that it increases the probability of a response reoccurring. The role of operant conditioning in ontogeny, as postulated by radical behaviorism, is equivalent to the role of natural selection in phylogeny, as postulated in evolutionary biology (Skinner, 1981). To build a successful, reliable, theoretically rigid notion of ontogenetic selection, special attention must be given to the overt selective mechanism proposed for that mode of selection. Differential reproductive success, fitness, the overt selective mechanism of phylogenetic evolution itself explains the very central workings of biological, phylogenetic evolution. Greater fitness means greater distribution of the heritable traits carried by the parent organism(s). The understanding of fitness promotes a clear, simple understanding of phylogenetic evolution and natural selection.

Fitness is also a very clear, concrete and understandable concept: The number of offspring an organism has, and, relative to the reproductive population it is part of, its relative number of offspring compared to other members of the population. Of course the calculation fitness ultimately extends over theoretically infinite amounts of generations: Counting the amount of children, grandchildren, grand grandchildren and so forth. Fitness can be best understood as the amount of offspring surviving to reproductive age, that a parent organism produces, the amount of resulting

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grandchildren, grand-grandchildren, and so on, continuing into future generations. In each generation one will be able to measure fitness by the amount of children, grandchildren, grand-grandchildren and so on, relative to the parent.

Without (understanding of) this simple, overt selective mechanism, evolutionary biology may not have been as easily discernable, demonstrable, reliably replicable and ultimately not as reliably applied as a theory to describe the world, as Darwinian theory has proven to be. To establish a reliable theory of ontogenetic selection, identifying mechanisms and properties that fill much the same role as fitness benefits from being done.

That property is reinforcement (Skinner, 1981, 1976). Naturally then, defining and understanding reinforcement as clearly and coherently as possible is important in achieving the above stated goal of creating and maintaining a clear, usable theory of ontogenetic selection. The definition most commonly used to define reinforcement is what I refer to as the *functional definition*: An event is reinforcing if it increases the likelihood or actual frequency of repetition of a response that it follows. If such an event amounts to an introduction of a stimulus or event it is, by definition, *positive reinforcement* (Skinner, 1976).

*A positive reinforcer strengthens any behavior that produces it: a glass of water is positively reinforcing when we are thirsty, and if we then draw and drink a glass of water, we are more likely to do so again on similar occasions. (Skinner, 1976, p. 51)*

Conversely, if the event amounts to a withdrawal of a stimulus it is, by definition, *negative reinforcement*.

*A negative reinforcer strengthens any behavior that reduces or terminates it: when we take off a shoe that is pinching, the reduction in pressure is negatively reinforcing. (Skinner, 1976, p. 51)*

Challenging these definitions is not the point, as they are, again, by definition correct, as a matter of functional relationships between behavior and environment. However, while they are probably fine for most laboratorial, empirical work, that is, for most behavior analysis, they are, in my opinion, insufficient for a broader theory of

*extensional* ontogenetic evolution—ontogenetic evolution extending phylogenetic evolution, in the sense that operant conditioning is a naturally selected, phylogenetic adaptation.

Just as we can readily identify and comprehend the role of number of offspring plays in transfer of frequency of a heritable trait to the next generation, we must know and be able to readily identify what reinforcement *is*, beyond its equivalent *functional* role of transferring greater frequency potential for a given behavioral response to the future.

Knowing the structural properties that facilitate reinforcement is one potential solution. Particularly, knowing the functioning of the neurological mechanism(s) that facilitate operant conditioning can provide a non-tautological account of reinforcement. The problem is that the neurophysiology of operant conditioning is mostly unobservable, and probably largely theoretical, for now. Also, even if the covert, neurophysiological mechanism of reinforcement was used as definitional anchor, one still lacks an understanding of how some consequences become *selecting* consequences historically. The functional definition, apt for laboratorial situations where actual functional relations can be established, provides only a semi-tautological or tautological account of how this happens, when experimental evidence, for various reasons, is not available. One of Chomsky's more legitimate criticisms of Skinner's attempt at accounting for verbal behavior was that the term reinforcement often amounts to a tautology (Chomsky, 1967), although, in my opinion, not to the dramatic extent that Chomsky proposes, and by no means rendering Skinner's theory useless, as Skinner has support in the functional, experimental definition. Chomsky seems also to have forgotten that natural selection<sup>7</sup> is often subject to a very similar charge of tautology, without thus collapsing as a theory.

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<sup>7</sup> Adaptive traits are selected on the basis of their differential reproductive value, but what makes an adaptation adaptive is per definition its reproductive value.

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To establish how consequences *become* reinforcing—that is, *selecting* consequences—an account of the evolution of reinforcers themselves is needed. Operant conditioning is itself a phylogenetic adaptation, and accordingly some reinforcers have evolved by phylogenetic evolution. These reinforcers are by definition primary reinforcers. Primary reinforcers are *unconditioned, non-contingent* reinforcers. They are thus principally products of phylogenetic selection.

*It is commonly said that a thing is reinforcing because it feels, looks, sounds, smells, or tastes good, but from the point of view of evolutionary theory a susceptibility to reinforcement is due to its survival value [I suggest the term reproductive value, see sub-chapter 3.2] and not to any associated feelings. (Skinner, 1976 p. 52)*

In the course of the lifetime of a person, however, lots of events and stimuli will be reinforcing, not all of them gaining their status from a history of natural selection. Those events and stimuli are by definition *secondary* reinforcers. To non-tautologically define their status as reinforcers, we must trace their historical relation to primary reinforcers, from which they gain their power. While a history of natural selection non-tautologically accounts for the evolution of primary reinforcers within a given species (/reproductive population), the ontogenetic history of the individual member of a species *extensionally* accounts for the evolution of secondary reinforcers. In other words, it is beneficial to attend to the ontogenetic evolution of secondary reinforcers in the individual organism, extending the phylogenetic evolution of primary reinforcers in its species (ancestral populations). When this is done properly, non-tautological accounts of potentially all instances of reinforcement can be achieved. Thus the tautology-problem regarding reinforcement is essentially solved, at least on a theoretical level.

For the purpose of understanding the co-control of behavior by phylogenetic and ontogenetic contingencies, and better understanding and conceptualizing ontogenetic evolution itself, a definition of reinforcement that forms an extensional bridge from phylogeny to ontogeny is needed. Operant conditioning itself is a phylogenetic adaptation or adaptations. It has itself been selected for its reproductive value. As with all ontogenetic adaptations (learning), operant conditioning results in lasting

changes in the phenotype beyond phylogenetic adaptation, within and limited to the lifetime of the organism (thus Lamarckism is in no way suggested). When a part of a species' behavioral repertoire (for example related to particular musculature) becomes sensitive to consequences it produces, it will plausibly be open to all or most types of primary reinforcement that is evolved in the species (for example reaching out for an edible fruit using one's hands).

### Phylogenetic versus ontogenetic maladaptivity in reinforcement

On the one hand, each type of reinforcement is selected for its past reproductive value and thus the behavior of the organism should be understood in terms of adaptation to ancestral environments. For example, the availability of sugar in processed forms, in huge quantities compared to hominid ancestral environments, itself makes sugar a maladaptive reinforcer today. A lot of this maladaptivity transfers to ontogenetic selection of behavior: Seeking out and consuming sugar (candy, cakes, pastry, soda drinks and so on) is reinforced at a potentially alarming rate (resulting in some cases in such problems as obesity and type 2 diabetes). The maladaptivity continues as high sugar substances with low nutritional value provide stronger reinforcement than natural high sugar substances with less concentrated amounts, like fruits, berries and some vegetables. This amounts to *differential* reinforcement (a parallel to differential reproductive success in phylogeny). Furthermore, the very strength of reinforcement of substances with unnaturally high concentration of sugar steadily selects new ways to acquire such substances, for example using sugar in many dishes, eating dessert, having soft drinks with non-sugary meals, and so on. In short, when a primary reinforcer turns maladaptive, maladaptivity may tend to extend into ontogenetic maladaptation, resulting in potentially devastating effects. Whether a reinforcer is phylogenetically or ontogenetically maladaptive is a matter of some convention. If the current ontogenetic environment is rearranged so as to resemble the ancestral environments in which the primary reinforcer was initially phylogenetically shaped, then that primary reinforcer will essentially regain its adaptive status. Thus the evolutionary nutritionist will frequently refer to "stone age"-diets and environments. However, rearranging relevant aspects of "stone age"-environments today is possible

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to some degree, and more importantly, is not necessarily the *only* way to arrange adaptive distribution of, say, sugar.

Accordingly, whether the effect is truly and ultimately maladaptive in nature, relies largely on the arrangement of ontogenetic contingencies. It is possible to successfully remove or limit availability of maladaptive reinforcers from the environment. It is possible to arrange differential reinforcement in favor of natural high sugar substances (fruits, berries, etc.). And most importantly, it is possible to use the “maladaptive” reinforcer adaptively, a classic example being parents who will not let the child have desert before she has finished the more nutritional main meal, dinner. It is still possible to shape a host of functional, adaptive behavioral repertoires using the “maladaptive” reinforcer. Take for example a child with autism being taught to speak with small bits of chocolate, or tokens, which can eventually be traded into candy rewards. The white sugar-rewards are not very healthy in themselves, but the repertoires they are used to shape can make the difference between an able and a disabled, underdeveloped individual.

The crucial point here is that ontogenetic extensional adaptation ensures the possibility of fostering adaptive repertoires based on maladaptive reinforcement. Processed, high sugar food is unhealthy food, but it nevertheless frequently shapes adaptive repertoires. For example, a working man who is fond of chocolate and sweets, eating all too much of it, may nevertheless be partly reinforced by these in his work, as work is exchanged for money, and money is exchanged for these unhealthy, but reinforcing edibles. In this example, maladaptive primary reinforcers give strength to a secondary reinforcer, money<sup>8</sup>.

Maximum maladaptivity is only reached when maladaptive reinforcers (in isolated terms) are used to shape maladaptive behavior (also in isolated terms). The heroine abuser is a perfect example. His behavior is shaped by a toxic substance that bypasses

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<sup>8</sup>Actually money may partly be a primary reinforcer, depending on whether genetic dispositions to be reinforced by token rewards has evolved in humans. For the sake of simplicity I am assuming that money is principally a secondary reinforcer.

natural means of reinforcement, and commonly learns to new ways to steal, lye and cheat and ultimately has his life destroyed, as a result. What is seldom thought of is that a government could use that, or similar, maladaptive reinforcers to shape adaptive behaviors in the addict, like maintaining a day job, basic self-healthcare and a home (Iguchi, Stitzer, Bigelow, & Liebson, 1988 describes a similar study where methadone was used to reinforce abstinence from unauthorized drug use, contingent upon submitting urines testing negative for illicit substances.)

In today's society the heroine dealer shapes the behavior of the heroine addict instead. There is seldom any differential reinforcement of comparable strength available to the addict (lest he has a social network of tremendous abundant resources), and thus his dysfunctional lifestyle persists. Note that I am not making normative suggestions here (i.e. e., suggestion for social reform). I am certainly no advocate of governments using drug-reinforcers to control the behavior of its citizens—rather I am engaging in hypothetical problem solving in a local and very special circumstance. The point is to illustrate the difference between phylogenetic and ontogenetic maladaptivity in reinforcement. Even a vile reinforcer like heroine can be used to shape the repertoires of a model citizen (actually it would probably extremely effective). Nonetheless, again, without moving into normative territory, the goal of any society would typically be to use adaptive reinforcers to shape adaptive behavior. That would imply that the behavior analyst should use fruits and berries, if possible, rather than candy and chocolate, when for example shaping a verbal repertoire in a child with autism, although the choice would be of lesser priority in such a task.

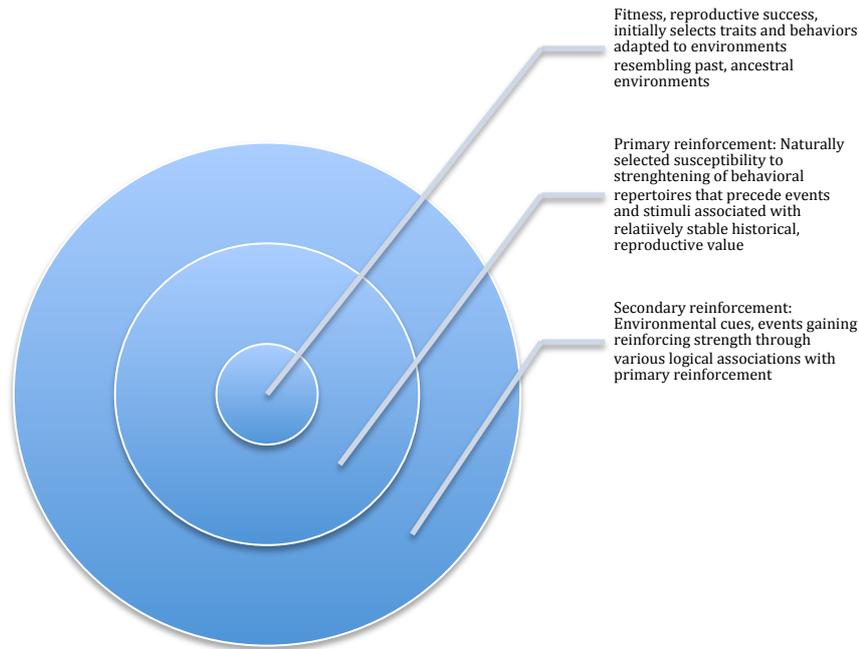
### ***Reinforcement as phylogenetic adaptation***

Classical conditioning gives the organism a capacity to acquire some new adaptive repertoires during its lifetime: A response can apply to new stimuli for which it was not originally phylogenetically selected.

However, it is weak compared to the awesome adaptive power that results when contact with a vital resource selects the preceding response from a pool of (random)

behavioral variation. Not only are responses that are useless in a novel environment weeded away. Entirely new responses, adapted to *current*, not ancestral, environments that reliably lead to vital resources are established during the lifetime of the organism.

To increasingly complex organisms and environments, however, primary reinforcement does not hold. There are intermediate goals, chains of responses necessary to reach the resources associated with reproductive success. I am tempted to say that there is *an increase in the logical complexity* involved in acquisition of vital resources. *Secondary reinforcement*, as I prefer to define it, is any selecting consequence that has acquired its status (“power”) by some logical relation to a primary reinforcer, *during the lifetime of an organism*. Secondary reinforcers have two defining characteristics then: They *evolve* during the lifetime (in contrast primary reinforcers that appear during the lifetime of an organism rather “*develop*” as a matter of maturation, which is more heavily phylo-/genetically governed: The obvious example is adult sexuality, which develops during puberty in humans). I use the term *evolve* deliberately, to denote to *evolution*. The evolution of secondary reinforcers is a part of general ontogenetic evolution, but the principal ontogenetic evolution is selection by consequences—reinforcement in the case of ontogeny—as described by Skinner (1981). Secondary reinforcers gain their status and strength through some manner of logical connection to primary reinforcement, for example through classical conditioning.



### *Subchapter summary*

The important basic premise that has been established in this chapter is a defense against the tautology-problem related to reinforcement. Although there are many possibilities (some good, some bad) for anchoring reinforcement non-tautologically—neurophysiology (brain mechanisms corresponding to reinforcement), mentalist hedonism (pain and pleasure) and common sense (reward and punishment)—I opt for anchoring the definition in fitness and phylogenetic evolution. Skinner already did this for primary reinforcers, by noting that they acquire their power from natural selection (Skinner, 1976, 1981).

This is achieved by postulating an ontogenetic evolution of secondary reinforcers, extending the phylogenetic evolution of primary reinforcers.

To summarize, *two* intermediate goals towards seamless understanding of the relation between phylogenetic and ontogenetic contingencies is achieved in this chapter: (1) A non-tautological definition of potentially all selecting consequences (reinforcement) in phylogeny, and in the same vein (2) a method of tracing the complete evolution of secondary reinforcers, from their ontogenetic evolutionary history, back to primary reinforcement, and from primary reinforcement back to their natural selection in the phylogeny.

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The key is primary reinforcement. Part of my thesis is that all reinforcement can be traceable to a relation, through primary reinforcement, and ultimately to natural selection (fitness). In other words, all selecting consequences should potentially be shown to have a logical relation to *how it relates to historical fitness*. For example, how does riding a bike, steering a helicopter or learning to play guitar relate to primary reinforcement and thus back to historical, ancestral reproductive value? Presumably *chains of secondary reinforcers* leads to a smaller pool of, or a single, primary reinforcer(s), typically more closely associated with reproductive value. The logical relation between these chains built during ontogeny, finally going back to primary reinforcement, which has in turn shaped in the evolutionary history of the species, makes for a seamless, extensional understanding of the relationship between phylogeny and ontogeny. I urge the reader not to confuse this logical relation between fitness and reinforcement with the idea that all reinforcement is necessarily adaptive or promotes fitness—reinforcement is related to ancestral, historically stable reproductive value, but not all behavior that is reinforced is adaptive (ontogenetic maladaptation), nor are all primary reinforcers adaptive in the contemporary environment (phylogenetic maladaptation).

### 3.4 The significance of learning in phylogenetic evolution—the Baldwin Effect

In the preceding chapter I established a non-tautological definition of reinforcement especially for theoretical purposes, which rests on natural selection of primary reinforcers. I also established what can be illustrated as “an evolving tree” of secondary reinforcers during the lifetime of the organism.

Dennett summarizes the Baldwin effect as follows:

*What Baldwin discovered was that creatures capable of “reinforcement learning” not only do better individually than creatures that are entirely “hard wired”; their species will evolve faster because of its greater capacity to discover design improvements in the neighborhood. This is not how Baldwin described the effect he proposed. His temperament was the farthest thing from behaviorism. (Dennett, 1995, p. 79)*

Dennett seems to find behaviorism well suited to account for the Baldwin Effect (BE) for the occasion. However his introductory account of it is in my view unsatisfactory and does not capture the essence and explanatory power and theoretical simplicity of BE. Dennett describes BE as a process by which organisms can benefit from being close to a “Good Trick” (Dennett, 1995), rather than possessing it. “Good Trick” is Dennett’s favored term for adaptations. I think it is better to describe the Baldwin Effect in terms of not only discoverability of adaptations, but realization, and to what degree potential adaptations are translatable into actual adaptations. A potential phylogenetic adaptation may be useless to the infant phenotype and also the maturing phenotype beyond infancy. Lets say for example some members of our pre-bipedal hominid ancestors had a mutation which gave them musculature that enabled them to stand erect for short amounts of time. If standing erect was able to come under operant control, say, within the very first generations of that mutation, then that mutation could potentially become instantly adaptive, also within the very first generations. Standing erect in itself would be next to useless, but if standing erect would occasionally scare away predators, enable dominance within the tribe, enable reaching for fruits—all possible consequences that could plausibly appear by chance and (per definition) be repeated more readily once reinforced—. Within a generation standing erect could be shaped from “next to useless” into a powerful multi-adaptation (naturally selected but realized through operant conditioning), depending on how many operant classes (types of reinforcing consequences) the behavior was a part of (I have mentioned reinforcement such as food stuffs, dominance, scaring away predators). The result, of course, would be greater fitness for members that 1) had the genetic mutation and 2) were disposed to have the traits and behaviors enabled by that genetic mutation come under operant control.

### *First cycle*

As members with the standing erect-mutation would have greater fitness, there would be greater frequency of that mutation in future generations of the population. Now, if social learning (i.e. e., imitation, modeling, observation) is also evolved in the population, various ontogenetically adaptive practices involving standing erect could spread in the population, with less dependence on the actual operant contingencies that originally selected the practices.

I have now described the first level or cycle of the Baldwin Effect. Here natural selection is enabled to pick up—select—a potential phylogenetic adaptation. What makes phylogenetic adaptations like the one in the example *adaptive* is ability to learn to use them and operant learning is powerful, because it includes a newly evolved trait or uncommitted pool of behavioral variation in potentially very many operant classes, each tending to lead to historically stable adaptive goals (food, shelter, reproduction, and so on).

### *Second cycle*

In the second cycle of the Baldwin Effect there is selection for innate repertoires or as I personally prefer to say, *pre-learned* repertoires, replacing the costly learning curve for acquiring learned repertoires: Lets say that many phylogenetic *potential* adaptations require a long period of learning to become functional and thereby *actual* adaptations. If many adaptations take this form then the organism will be preoccupied with long learning periods, throughout life but perhaps typically in the early parts of the lifecycle (even perhaps leading to “long childhoods”, such as is indeed the case with humans). For one, this leads to relatively long periods where adaptations are partly or wholly ineffective. Secondly, the organism has less time and resources directed at novel ontogenetic adaptations, instead being preoccupied with having potential phylogenetic adaptations shaped into actual adaptations.

This presumably opens the population to selection for pre-learned repertoires—further phylogenetic shaping of the initial *potential* phylogenetic adaptations. The result is that organisms eventually are born with adaptations that require lesser

subsequent ontogenetic shaping. In other words, there is phylogenetic selection for adaptations that require less learning time to become functional.

The most essential feature of the Baldwin Effect is thus that ontogenetic adaptation guides the shaping of phylogenetic adaptations. An organism requiring only a short learning period to utilize a given phylogenetic adaptation has better potential fitness than one requiring a long learning period. In light of the Baldwin Effect we can thus hypothesize that many “instinctive”, innate—*pre-learned*—repertoires of contemporary species and populations were once learned repertoires in their respective ancestral populations. The nest building of birds that cannot possibly have been due to learning, operant or otherwise, may have been learned in more primitive forms in ancestral bird populations. In the nest building of some contemporary bird populations operant conditioning may play only a small role. For example, some consequences that are produced in the nest building process may still serve to keep the repertoire in check, even occasionally producing more effective methods of gathering, stacking and preparing materials. There is no reason why there should be selection for entirely fixed repertoires that cannot still be enhanced by learning. There are no plausible contingencies of fitness that would provide such selection. Rather most pre-learned repertoires will benefit from keeping an open end towards further optimization by learning. This is especially true for phylogenetic adaptations that depart from their original, past adaptive function and take on new functional roles.

What is important to establish is that the Baldwin Effect provides theoretical simplicity and enhanced explanatory power to traditional adaptationism. It is not an unnecessary layer of explanation on top of traditional Darwinism. Neither does it replace Darwinism in any way, since it does not breach its original principles in any way. I also think that an emphasis on operant conditioning enhances the plausibility of the Baldwin Effect. When there is a flexible connection between fixed consequences (primary reinforcement) that are more or less securely linked to phylogenetic history of greater fitness and new behavioral disposition that arise from genetic mutation (for example the ability to stand tall), the possibility for adaptive outcome is greatly enhanced. The Baldwin Effect and operant conditioning provide a

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sort of complementary theoretical coherency to each other. One argument for the basic and continued importance of operant conditioning in many or most advanced species is that it played (and continues to play) an important role in *facilitating* the implementation of many potential phylogenetic adaptations. It is said that most genetic mutations are “harmful”—maladaptive—to the organism. With the Baldwin Effect presumably most still are, but far less than if organisms somehow had to be born with instantly adaptive traits. I think a minority of phylogenetic adaptations has a history of instant adaptation. Mostly those phylogenetic adaptations that appear as part of a subsequent phylogenetic shaping of traits are plausibly instantly adaptive—where there is a phylogenetic, rather than ontogenetic, history of adaptation. I imagine ontogenetic adaptation must typically be in place to “nurture” a “hopeful” genetic mutation, especially where the genetic mutation does not build on top of a history of phylogenetic adaptations involving earlier genetic mutations.

### **3.4.2 Baldwinian Selection for Innate, Pre-Learned Repertoires**

Often the Baldwin Effect will presumably result in phylogenetic shaping of relatively complex pre-learned repertoires. Rather than developing as a blank slate of unshaped, behavior-general neurology, the infant brain will come with many advanced behavioral dispositions (considering the long evolutionary history of our species). Some of this neurology may be more or less fixed, while some of it may combine pre-disposition with plasticity. The plasticity is likely linked to common modes of learning: Classical conditioning, operant conditioning, imprinting, etc. At least that is a hypothesis that I would like to promote. I think the possibility that ontogenetic adaptation builds on phylogenetic adaptation is not only very plausible in light of the Baldwin effect, but is also a good working hypothesis for understanding how learning and biological evolution co-work. Learning is too often seen as opposed to “nativity” or innateness, or alternatively one proposes “combination” of learning and innate “structure” and “development”. This is a typically *structuralist* view, which I think is unfruitful—it is structuralist because “biology” is identified with the “organism’s structure”, while learning is identified with the “environment”. This is actually not

only unfruitful, but utterly false. Learning is an organism-environment relation and cannot be identified with the environment as cut off from the organism.

The rule, rather than the exception, I think, is that learning is crucial for realization and development of pre-learned, innate repertoires. Learning seldom takes on a role that is neatly isolated from innate, pre-learned repertoires. Recall my definition ontogenetic adaptation—learning—as simply lasting adaptive changes to organism's physiology based on contact with the environment during ontogeny (adaptation is here defined broadly as adjusting to or responding to the environment. Maladaptive or neutral repertoires can indeed also be learned). The world simply is not cut into naïve and ultimately false structuralist categories.

### **3.4.3 Baldwinian Selection for New Primary Reinforcers**

I now turn to what I think is a most important potential role for Baldwinian selection. As I have mentioned, in the second cycle or stage of the Baldwin Effect there is selection for decreased cost in learning. Learning is thus typically replaced with more elaborate pre-learned repertoires, but, importantly, it can also be replaced with more effective learning. One way in which learning can be made far more effective is if new types of reinforcement evolve that are *associated* with already established forms of primary reinforcement.

An organism whose behaviors are reinforced by consequences involving nutritious food consumption benefits from learning new ways to find and consume such food. However, having to stumble into nutritious food to learn new ways to acquire it can turn out to be a slow and costly learning curve. When a genetic mutation makes behaviors associated with finding food themselves reinforcing (for example modes of hunting and gathering behavior) the organism can minimize the learning curve. The second cycle of the Baldwin Effect does not necessarily need to constitute pre-learnedness, even though it is certainly also beneficial. It can also constitute more effective and faster learning. A typical cue for phylogenetic selection of primary reinforcers can be hitherto conditioned reinforcers (but it is in the nature of non-Lamarckian phylogenetic evolution that such connections are loose, anyway).

Organisms that engage in a costly learning curve of classical conditioning for acquiring appropriate conditioned reinforcers will tend to have lower fitness than organisms that have pre-learned such reinforcers. Of course a pre-learned reinforcer is per definition a primary reinforcer. Thus the result is that organisms may acquire a large set of associated primary reinforcers through phylogenetic evolution.

When human children play, they may be actually reinforced by behavior associated with more directly adaptive types of reinforcement. They acquire many adaptive repertoires, or prerequisites of adaptive repertoires, without doing what adults consider useful (i.e. e. functional, adaptive) work. Of course children also take many cues from their cultural community, for example through imitation.

## 4. A modern synthesis for ontogeny

This chapter draws upon and is inspired by Cleaveland's article (Beyond trial-and-error in a selectionist psychology, 2002) on distinguishing between codical and material units in ontogenetic selection. Cleaveland distinguishes between overt behavioral responses and associations; the latter of which is what is actually subject to strengthening by ontogenetic selection (2002).

I have now established an understanding and definition of reinforcement, which theoretically traces any reinforcer logically back to its phylogenetic origin (3.3) and provided a synthesis between modern evolutionary biology and radical behaviorism, with respect to intra-population selection, as opposed to group selection, and an understanding of how individual genetic differences play a central role, both as the source of variation in the modern view of natural selection, and as a vital source in individual behavior (3.2). I've broken down Skinner's assignment of ontogenetic and phylogenetic contingencies to individual and species behavior respectively, and instead indicated a viewpoint where there is more of a seamless extension between phylogenetic and ontogenetic contingencies.

However, modern evolutionary biology offers a very significant synthesis, which puts genes and phylogenetic contingencies in a systematic and explanatory complementary relation to each other: The modern evolutionary synthesis (Sterelny & Griffiths, 1999). Meanwhile psychology, at the level of ontogeny, has made little progress in defining how behavior and the organismic structure relate to each other. Skinner drew some informative sketches, distinguishing between the causal statuses of the organismic structure and the ontogenetic contingencies shaping it (Skinner, 1976).

*An organism behaves as it does because of its current structure, but most of this is out of reach of introspection. At the moment we must content ourselves, as the methodological behaviorist insists, with a person's genetic and environmental histories. (Skinner, 1976, p. 19)*

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Skinner managed to depart from the everyday phenomenological perspective that our actions originate within us, that we do things because of how we feel, what we want and turn attention to the role of the environmental histories, first of all to the role of the environment in ontogeny, being a psychologist, but also, as we have seen, to the role of the environment in phylogeny (Skinner, 1966, 1981).

### *What is reinforced?*

The replicator-interactor distinction in modern evolutionary biology illustrates a difference between the performance that is subject to selection—the interactor, a potentially adaptive trait—and the actual, selected, transferred component—the replicator(s), a gene, allele (a gene variant), or a set of genes.

*Replicators are things that are copied into the next generation: they form lineages of things with the same structure. Interactors (or vehicles) are entities that exist in each generation of a copying cycle and interact, more or less successfully, with the environment. (Sterelny & Griffiths, 1999, p. 55)*

Similarly for ontogeny, one may ask what is actually reinforced in operant behavior. Is it a behavior that is selected, strengthened, or is it actually a neurological association that has been strengthened and reinforced?

Distinguishing between replicators and interactors in ontogeny implies that it is not the actual observable behavior that is reinforced, but an aggregate, an enabler or producer of behavior. It also then becomes more obvious that contingencies are not copied to an inner environment (though conceptually and analytically from a behavior analytic standpoint), because the replicative component is part of the contingency. The organism is changed by contingencies, but the contingencies are not copied or stored in the organism (Skinner, 1976; 1990).

This enabler is the organismic structure in general, and the brain and central nervous system in particular, including mental states. This has the potential to define a systematic unit of behavior and thus contribute to a clarification of what is selected as distinguished from and in relation to the selecting consequence, something that further benefits anchoring and defining reinforcement and behavior reinforced.

	<b>Brain as behavior replicator</b>	<b>Brain as behaving (what the organism/brain does)</b>
<b>Ontogeny</b>	Neurological, synaptic associations; associants (Cleaveland, 2002)	Private contingencies and non-private contingencies; remembering (itself an effect of contingencies but enables private re-arrangement of contingencies—similar to what Dennett calls “inner environment”)
<b>Phylogenetic parallel (illustratively important)</b>	Genes as replicators; genome as central replicator component	Genome as part of the phenotype; genes as part of (read: Genome as phylogenetic adaptation)
<b>Legacy contingency</b>	Stimulus-organism-response (S-O-R)	Discriminating stimulus-response-reinforcing stimulus (SD-R-SR)

*Tabell 4-1 Ontogeny: Brain as behavior replicator versus brain as behaving*

## 4.2 Popperian organisms

Dennett argues that humans are not merely Skinnerian creatures, but have evolved capacity for preselection of behavior (Dennett, 1995). He labels organisms that have this capacity for preselection Popperian organisms. He explains,

*Unlike the merely Skinnerian creatures, many of whom survive only because they make lucky first moves, Popperian creatures survive because they're smart enough to make better-than-chance first moves. (Dennett, 1995, p. 375)*

I agree with Dennett that humans are indeed Popperian creatures. I hold, however, that Popperian organisms are a brand of Skinnerian organisms. They largely derive their Popperian capacities from the actual environment. It is unclear whether Dennett also holds that Popperian organisms are a brand of Skinnerian organisms, but judging

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by his dismissiveness of Skinnerian theory (1995, p. 376), one should think not. However, he discusses whether pigeons were ever proven by behaviorists not to be Popperian (p. 376) (as if behaviorists were at any time trying to prove that the pigeon was “Skinnerian”). He also discusses whether a range of species may be considered purely Skinnerian (1995).

Dennett also seems to underestimate the role of social learning and in the particular case of humans, verbal instruction. Human infants are rarely subject to unlucky first moves, as they are brought up in a teaching, corrective, verbal, and not to forget, protective community. Furthermore, if they were to make any “first moves”, no preselective capacity would be likely to save them. Human infants seem to rely heavily on care, guidance and instruction. Baldwinian selection, as I have noted, goes some way in explaining this: Human infants must tune and actualize a host of potential phylogenetic adaptations by subsequent ontogenetic adaptation.

Baldwinian selection of primary reinforcers also favors an outcome that is similar to preselective capacity, but simpler: By phylogenetic evolution stimuli that are associated with other primary reinforcement and punishment eventually themselves become primary reinforcers and punishers (by a process which slightly resembles the evolution of secondary reinforcers in ontogeny). So a child will not have to suffer the punishment of falling down hills or stairs. The sight of heights will already punish it. To the extent that this constitutes preselection, it takes place in phylogeny and extends to ontogeny as a matter of operant conditioning, which is indeed successful on the first trial.

Though these are good alternative explanations, and I think sometimes better explanations than Popperian preselection (in the example of the infant’s reaction to heights), I do agree with Dennett on important aspects. The question is not whether humans can do selective trials on the environment privately and covertly—they can, obviously—but, first of all, which environment the trials are performed upon. Secondly, a rhetorical question, where do behaviors, whether a matter of thinking or

actual operation, perform their ultimate function? Has thinking ultimately evolved to perform behaviors on an inner environment, or the actual environment?

I think it is safe to say that thinking, even in humans, is most of all a premeditative measure for acting upon the actual environment. Functions like memory may allow us to act upon properties of the environment that are no longer present. A child may remember touching a hot stove, or the instruction not to do so, and in both cases avoid future accidental encounters with the stove. Though memory allows the effect of contingencies to be prolonged, and for us to operate silently upon features of the environment (for example re-experience a prior encounter) no longer present, the effective contingencies stem from the *real* environment, not an inner environment. Memorization, a slightly indistinct mode of learning, coincides with operant conditioning—both memory and the capacity of operant conditioning are of course phylogenetic adaptations. The resulting behavior can thus be explained in terms of natural selection and operant conditioning.

Dennett envisages a library of information constituting our “inner environment”. The fact that we so easily forget features of the environment suggests rather that our remembering is selective—*selected* to be precise, under an operant regime. We remember things that are useful, or functional to remember, and the best measure of functionality we have is reinforcement. It may of course be that we have yet *another* preselective capacity of selective remembering, but then we are approaching a leaking regress which rapidly adds unnecessary theoretical complexity.

While I do not challenge the complexity of human behavior, and the complexity of the structure proximately causing those behaviors, I do challenge that it should have to be matched with an “equivalent” theoretical complexity. It seems to be the tendency that since human behavior is so complex it has to be matched with complex theory. Must we really propose an inner environment to *explain* human behavior? Does memory constitute an inner environment; is it a copy of the actual environment? If it is a copy of anything, it is a copy of the initial structural changes caused by ontogenetic contingencies (thus memory is indeed a type of ontogenetic

adaptation/learning). Do those structural changes necessarily have to be copied? Is it not plausible that a preservation of the initial structural change will do? When the person or organism remembers, it is then a matter of re-behaving—re-activation of that changed structure. Accordingly, when something is forgotten it is (proximately) because those structural changes having waned, that is, lost their initial composure, for example by lack of strengthening (reinforcement) (ultimate/private cause).

We are accordingly Popperian organisms controlled by the actual environment, not by an inner environment, though exactly what constitutes an “inner environment” is somewhat uncertain. It is absolutely clear that humans internalize: All ontogenetic adaptation, including memory and reinforcement, is per definition internalization—by the structural change resulting from contingencies. Memories resemble earlier experience/behavior, earlier contact with the environment. If the initial experience/behavior is a presentation of the environment, the memorization of that experience is a re-presentation. Memorization is thus a matter of re-behaving in the absence of the events and stimuli that caused the initial experience/behavior.

Phylogenetic evolution can only stably select for innate conception of equivalently stable, recurring features of the environment, like for example basic physics, isolation of distinct events and stimuli, and some anatomical features of ancestral phenotypes.

The question of whether we operate upon an internal environment is not of great importance, at least not to my thesis. The important point is that any internal environment does not replace the actual environment. To be able to operate upon internalized features of the environment constitutes a natural and plausible phylogenetic evolution of operant conditioning. However, the principal source of contingencies remains in the actual environment. When we learn new skills we tend to operate upon the actual environment, whether it is learning to row or riding a bicycle. We are subject to contingencies in the actual environment. If we already had internalized the environment and its contingencies, we could very well learn to ride bicycles, control helicopters, do break dancing and making elaborate dishes by simply sitting passively and thinking about it. It is appropriate to ask whether

phylogenetic evolution would favor copying of the actual environment to an inner environment, when the actual environment is readily available—always. Novel contingencies of the environment have to be “copied” during ontogeny. That they should have to be ineffective until copied to an inner environment, where only then they become effective is not likely. Advancement of the memory capacities of a species is more likely to constitute a *prolonged*, and thus potentially more adaptive and functional, effect of the contingencies—phylogenetic adaptation that facilitates more advanced memory *coincides* with an operant regime. Rather than internalizing the contingencies, what happens is that the effects of the contingencies are preserved more systematically and over a longer time span.

Lets say a person vividly remembers a car-crash in which he was involved. Whenever he drives under similar conditions (discriminating stimuli) he recalls this event. He thinks about this event, recalling what went wrong, how he lost control of the car on an icy lane, for example. He recalls his responses but changes some features. In the silent trial a discriminating stimuli prompts an appropriate response based on his experience. He sees the icy parts of the road, but this time around, in the silent trial, he slows down, and approaches the icy lane with due caution. His silent trial is successful (and thus privately, negatively reinforcing). Is it right to say that the contingencies of the driving accident are copied to his inner environment, or that the effect of those contingencies are enhanced by naturally selected memory capacity?

In a way the contingencies are “copied”, but only as a matter of the silent trial—remembering, re-experiencing the event, with some variation in response (stopping the car in response to the icy parts of the road). Thus, again, it is more precise and correct to say that the silent re-behaving—thinking about, remembering the event—constitutes private contingencies. Thus the contingencies have been *reproduced* as a matter of behaving (remembering, thinking), not copied to or stored in an inner environment.

Being a Popperian creature does not necessarily mean self-sufficiency in very early stages of the lifetime—humans, and many other animals, are after all *raised* for self-

sufficiency. Humans especially, with our long childhood, do not become fairly self-sufficient operators upon the environment until relatively long into ontogeny. As far as preselected self-sufficiency goes, it is afforded by natural selection, by evolution of appropriate responses to stimuli and events that have appeared fairly stably in parts of phylogeny. Baldwinian selection ensures that more such events and stimuli that are related to fitness do appear stably during phylogeny (for example being reinforced by hunting and exploration as opposed to just finding and eating food).

Being a Popperian creature *does* mean that one can act silently upon features of the environment one has been subject to during ontogeny and thus perform some amazingly adaptive pre-selection of responses prior to features of the environment that resemble contingencies that one has previously been subjected to, either by direct experience, imitation, observation or verbal and non-verbal instruction. Considering that we may be able to do potentially infinite amounts of successive silent trials, successively building upon each other, there is quite a distinction between the Popperian and simpler “Skinnerian” (Dennett, 1995) organism—all fairly compatible with the radical behaviorist (i.e. e., *Skinnerian*) framework.

The main points proposed in this sub-chapter are that an “internal environment” does not replace the actual (ontogenetic) environment and that we are indeed Popperian organisms, as proposed by Dennett. However, Popperian organisms are essentially advanced Skinnerian organisms, fitting more or less seamlessly into the framework of operant psychology and more broadly, radical behaviorism.

## 5. Seamless causal relationship between biological evolution and culture

### 5.1 Culture

It is possible to give a good account of the seamless extension of ontogeny from phylogeny in terms of compatible causal categories. Radical behaviorism is already strongly disposed towards this possibility. All I have done so far is to explore that possibility, make some suggestion for further use and development and try to sort out some associated problems. With regards to pure philosophy of science the implication is initially a more seamless link between psychology (of learning) and (evolutionary) biology. Accounting for culture includes further linkage with anthropology, as Skinner briefly outlines in “Consequences” (1981). What I envisage is even more broadly a breakdown of the common barrier between the social (and even humanist) sciences and natural sciences. Psychology is the key because it is properly the science of the ontogeny of the individual organism in relation to the social and nonsocial environment. In this regard behaviorism is more *exemplary* than *necessary*. For psychology to achieve this, ontogenetic adaptation, learning, almost certainly needs to be taken seriously. Psychology cannot be a discipline, which adds nothing to its biological base sciences but talk of mind. Mental states can be studied with great success in the fields of neurology, genetics, and evolutionary biology, without ever having to mention the word “psychology”. A type of ultimate *causation* should be peculiar to psychology, and even if we grant mental states the most generous causal status that natural science can possibly afford, they are not ultimate causes of behavior. Mental states simply are not ultimate causes in a world of approximate universal causation.

Nature-nurture, or genes and environment, does not do us any favors in sorting out how culture relates to the ontogeny of the individual person or organism, but “environment” nevertheless clearly, but implicitly, includes culture, at least in the case of humans. This chapter is mainly about sorting out what culture *is*, in relation to ontogeny. Though I see great potential for behaviorism on that account, this is a part

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where Skinner is less than clear, making only preliminary suggestions and some outright mistakes (1981). I will try to sort this out fairly completely.

## 5.2 Defining Culture

As I define culture, its minimum prerequisite is *social learning*. Thus I reject the notion that language somehow defines culture, though it certainly characterizes human culture. Culture *is* I contend, the transfer of learned responses (often but not always ontogenetic adaptations) within and between the generations of a population and also between populations. As these learned responses manifest in more than one organism, they may properly be identified as *practices*. A practice is a response that is transferred between organisms by social learning. A practice is also a behavioral equivalent to the more popular term *meme*, within roughly cognitivist co-evolutionary theory. While memes are typically held to reside in the “mind”, practices are readily identifiable as observable behavior. A *practical* translation of meme may be silent trials—*thinking*—associated with a given practice, or actual stored information that can potentially be used to communicate, teach or otherwise transmit a practice: An actual tool, a blueprint for a tool, a recipe for soup, a videotaped dancing lesson, a song on a compact disc, etc. I believe that we can certainly internalize even detailed experiences with a practice and this may potentially be identified as a meme, but I think it is better to conceptualize it as some defining logical properties of a practice, which we recall significantly by behaving in ways we did when we originally were exposed to that practice.

## 5.3 Kin-Selection versus cultural selection of altruistic behavior

One of the successes of the operant psychological account of culture is that maladaptation is easily accounted for. A cultural group may reinforce maladaptive behaviors in individuals of the group in favor of the welfare of other group members. Examples are a group’s promotion of ideas like heroism, martyrdom, patriotism and self-sacrifice for greater good to certain individuals of the group, or group interests.

Evolutionary biology already has a good theory of persistent maladaptive behavior of the individual at the benefit of the group: Kin-selection. An organism has more genes in common with relatively related organisms than with relatively unrelated organisms of lesser kinship. Thus the organism will transfer its genes to the next generation, not only by itself reproducing, but also through the reproduction of its kin. Accordingly organisms, including humans, tend to bestow resources on other persons according to closeness of kinship (Sterelny & Griffiths, 1999): One's own children, children of female sibling (greater certainty of kinship), children of male sibling, and close kin in general, tribe, nation, historically reproductively isolated population (race), species. It is all a matter of *relative* kinship the way I see it and understand kin selection.

Nuclear family is favored over extended family, which again is favored over local community or tribe, which again is favored over national, ethnic, racial in-group in general, which again is favored over out-groups as such, which again is favored over other non-human species.

Old Darwinian operant psychology sees kin-selection as a matter of “species behavior” (Skinner, 1981; 1990). I suggest instead that kin-selection extends to ontogenetic contingencies in the form of moderation of primary reinforcers. We are thus more reinforced by bestowing resources on close kin as opposed to lesser kin. A mother's behavior of bestowing resources upon her child—feeding it, may be extraordinarily reinforced, which enables her to more effectively learn new ways to support the child. Recall that primary reinforcement is defined in relation to historical fitness.

I do not know what kind of behavior Skinner attributed kin-selection to in ontogeny. Perhaps a respondent repertoire or a simple genetic endowment, which could readily be modified by contingencies of reinforcement. This is an interesting theory which does not seem implausible at all, but still I would like to suggest the possibility that kin-selection is in fact embedded in primary reinforcement. Either way, since genes themselves are unobservable to organisms, some observable phenotypic features identifying kin are likely learned by a process of imprinting, or similar.

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## 5.4 Cultural Evolution

Cultural evolution seems to me to emerge as a very realistic, plausible phenomenon, in light of operant psychology. When Skinner's original, slightly misplaced parallel to phylogenetic group selection and species-selection, focusing on all of the practices in *cultures*, as opposed to *practices* fairly independent of enclosing cultures, operant culture theory starts to make sense too. Practices spread as a function of being reinforcing to learn, practice (maintain) and transmit (either by explicit teaching, imitation, simple verbal, written or otherwise symbolic rule-governance or observation). Explicit teaching distinguishes itself among others in that the teacher (for example parent, tutor or peer) is reinforced by the resulting behavior shown in the learner and that contingencies, rules, imitational and observational routines are explicitly arranged for the learner. To put it simply, we often learn from people who are not engaging in any form of explicit teaching directed at us. It may be someone we see doing their own routine, without much regard for our observation of them.

The practices rely on being reinforcing to cultural members for spreading, in contrast to memetic theory, where memes. They will thus tend to be adaptive but need not to be. It is important to point out that cultural evolution follows as a major extension of the "problem solving" engaged in by natural selection. Like I have noted earlier, through operant conditioning, culture enables shaping of behavioral repertoires, *means* adapted to the contemporary social and nonsocial environment, meanwhile reaching adaptive *goals* of fairly stable historical reproductive value (primary reinforcement). Accordingly, the alpha male of the modern society is a CEO or a rock star, rather than a tribe chieftain (a hypothetical tribe chieftain mental module, as could be proposed as a parody of cognitivist evolutionary psychology, would probably be seriously maladaptive, unless flexibly applying some goals which can extract suitable behaviors from the contemporary environment, similar to operant conditioning).

Culture is absolutely a product of natural selection, first significantly through the evolution of operant conditioning and finally through imitation, observation,

modeling, and partly verbal instruction, etc. (constituting the transfer of behavioral variation from organism to organism). Recounting Skinner:

*Ultimately, of course, it [the role of operant conditioning in ontogeny and culture] is all a matter of natural selection, since operant conditioning is an evolved process, of which cultural practices are special applications. (Skinner, 1981, p. 502)*

Practices mutate as a matter of becoming part of the repertoire, and thus behavioral variation, of new individuals. Recombination of behavioral repertoires, as well as slight errors in transmission (for example in the teaching) of a practice is parallel to “mutation” (on the interactive, phenotypic level) in phylogenetic evolution. Practices may also combine with other practices or behavioral variation to form a synthesis, a slight parallel to sexual recombination. For example prototypic dancing may hypothetically have evolved, in a culture, independently from music and rhythm, but at some point formed a synthesis. Today, at least, it is uncommon to dance unaccompanied by rhythm and music. Music functions as a discriminating stimulus setting the occasion for dancing and tends to make it more reinforcing.

Since cultural evolution, like phylogenetic evolution, spans across generations of organisms, it is also in a sense “phylogenetic”. I may actually have erred in exclusively using the term phylogenetic evolution for organic, Darwinian evolution of genetic transfer and inheritance, but I hope it will suffice to say that cultural evolution has both “vertical” and “horizontal” transfer, whereas Darwinian evolution is only vertical. Cultural spread to other living members in cultural in- and out-groups, both younger and older, as well as subsequent generations (offspring, offspring of others). Darwinian evolution works only vertically, and one-way: To subsequent generations of newborn offspring. Darwinian evolution does however have a horizontal equivalent, wherein a gene or set of genes may spread across (a) population(s), across geography, but not without already transferring vertically.

## 5.5 Cultural evolution is a creative process

One of the most, if not the most, important reason for considering a cultural theory of operant selection is that it tells us why cultural evolution is a creative process. Some of the confidence behind sociobiological propositions that genes alone determine culture stems from the notion that natural selection is the only known non-intelligent creative process.

Practices that are differentially reinforcing compared to other practices presumably spread faster, more consistently. The same behavioral variations that play a part in the operant learning of the individual make up a variation in cultural evolution. Some practices rely on individual differences, be they genetic or a matter of acquired, secondary reinforcers during the lifetime, thus there may be cultural practices that attract a certain segment of the population, like sports, hobbies and other interests. Memes fail to account for this well-known phenomenon because memetics fails to identify reinforcement (or in mental terms, the pleasure and pain of the bearer of the meme) as a selecting consequence in cultural evolution. Instead memetics is centered around the idea that memes, or ideas are parallel to an organismic, Darwinian evolution. The obvious difference is that organisms replicate on account on their survivability and ultimately reproductive means, while ideas are host to individuals who utilize ideas as part of a functional, adaptive repertoire. Thus memes will be measured, not merely by their own merits of reproductivity, but by their functional adaptive value to their host, and as I have hypothesized, reinforcement is the premier measurement of function in ontogeny.

### 5.5.1 Creative cultural institutions

Some cultural institutions represent creative processes of their own. The most admirable examples are natural science, through the hypothetic-deductive scientific method, and the capitalist free market. In the latter, enterprises that provide reinforcing goods and services to the largest mass of people are in return reinforced in proportion to that provision. Although all human cultures are evolutionary, some are

more consistent with the principles of creativity—the culture that blends conservatism, history and creative change, the renaissance culture.

Rather than to view these creative cultural institutions as entirely unrelated processes of variation-selection-retention, they may be viewed as different expressions of cultural operant selection. In science, for example, the reinforcing consequence is refined notions of truth, verification or non-falsification (a mix between primary reinforcers and logically complex secondary reinforcers). In the market money, of course, or other exchange commodities, is the reinforcers. Money is a secondary reinforcer associated with anything it can reliably be exchanged for, and probably the most significant type of secondary reinforcer in modern society.

It is my opinion that creative cultural institutions are all more general expressions of cultural operant selection. In science, for example, the reinforcing consequence is refined notions of truth (a mix between a primary reinforcer and secondary reinforcer), discovery, and perhaps most importantly association to application of gained knowledge (secondary reinforcement by association to anything from technological convenience to health benefits).

## 6. Conclusion

In the preceding chapters I presented Skinner's radical behaviorist perspective on the natural selection and operant conditioning as modes of selection in phylogeny and ontogeny respectively. I argued that Skinner subscribed to an older group selectionist view of Darwinism, which I considered to have influenced his theory of how phylogenetic and ontogenetic selection negatively. Rather than separate behavioral repertoires in terms of species behavior, shaped by natural selection, and individual behavior, shaped by operant conditioning, I argued for a perspective that acknowledges a multitude of phylogenetic adaptations relevant to human psychology. Meanwhile I sought to further illustrate the intimate connection between phylogenetic selection and learning, by discussing and examining the Baldwin Effect.

I also emphasized that ontogenetic selection extends phylogeny, in the sense that primary and secondary reinforcement can both be logically linked to their phylogenetic history. In that sense all behavior shaped in ontogeny can be traced back to a relation to ancestral reproductive value, while still not making the mistake of naively explaining naturally selected behavior with reference to present adaptive value.

In chapter 4 I took the argument for a less artificial distinction between what behavioral repertoires are shaped phylogenetically and ontogenetically further, by arguing that operant conditioning is indeed, like Dennett (1995) suggests, merely one of many adaptations, but that it is an adaptation of unequal importance, due to its special function-estimating role in ontogeny, similar to that of fitness in phylogeny, and to its historical adaptive significance.

Meanwhile, and in a related manner, distinguishing between interactors and replicators in ontogeny enables a potential understanding of how operant conditioning shapes neurological associations as replicating components of behavior. These neurological associations may in turn be part of plastic properties of naturally selected adaptations (either with or without a significant history of Baldwinian

natural selection, as described in the chapter on the Baldwin Effect). The idea is that phylogenetic adaptations are further shaped, realized, enhanced and actualized through ontogenetic adaptation.

One of my major points concerning our understanding of psychology is that we do not need to explain complex behavior by mediating variables, by reference to neurological structures (S-O-R). We can rather explain complex behavior as a set of neurological adaptations governed and shaped by ontogenetic contingencies extending phylogenetic contingencies. The state and structure of these neurological adaptations at any given time is a proximate cause of behavior, which observation is not necessarily needed to study the behavior-environment interactive component of behavior.

Understanding psychology fields in terms of different causal categories may help enhance the cooperation between them. I think psychology eventually will align its fields along similar lines as I have described. Whether it will be behaviorist or not, there will eventually be a psychology of ultimate ontogenetic causation, extending phylogenetic which is distinguished from fields of studies which explain behavior in neurological, cognitive or mentalist terms. I suppose it probably will consider more directly or seamlessly how the cultural and non-cultural environment shapes neurology.

If behaviorism is once again going to dominate psychology, it might as well become a “neurobehaviorism” in all possible fruitful ways. There is a need to align behaviorist terms consistently with neuroscience, in terms of behavior replicator units and the internal mechanisms of reinforcement. Insights in neuroscience and behavior analysis should complement each other.

The operant regime I propose, lends itself from the notion that operant conditioning is a special form of measurement of function, parallel to that of fitness in phylogeny, even in an incredibly advanced organism like humans. Dennett (1995) challenges behaviorism in the most appropriate manner. Why would natural selection favor operant conditioning over thousands of other phylogenetic adaptations? The answer, I

suggest, is that operant conditioning is one of many adaptations indeed, but not necessarily an equal among others. The best argument for the sub-ordination of other adaptations under an operant regime is that operant conditioning performs has a history of unequaled adaptive function. That history is the selection of behavior adapted to novel environments, which produce consequences of stable historical reproductive value.

Neo-Darwinian radical behaviorism, as I have put it forth is both a theory and a sketch for a philosophy of science. All the more important that it (hypothetically or actually) stands the trial of empirical judgment. I prefer that my proposed neo-Darwinian radical behaviorism leads a Popperian faith, be it falsification or not. If it turns out to be inconsistent with empirical data, then it will be falsified and thus retain its status as an honorable attempt, and pave way to better approaches and better theories. Every true evolutionary theorist, one could say, is ready to falsify his own theory in the Popperian spirit.

Cleaveland is highly critical of selectionist psychology often being held to be synonymous with operant conditioning (2002). While I may not share his worry I find it conceptually interesting and probably best to think of operant conditioning as one specific expression of ontogenetic selection generally. Internal, domain-specific mechanisms of selection, if identified, are not operant conditioning, because operant conditioning refers to operation upon the environment, and silent operation upon properties of the environment no longer present, for example through memorization.

I think operant culture theory has many qualities I do not see elsewhere. It seamlessly connects biological evolution and culture, while nevertheless demonstrating the autonomy of culture and cultural evolution. This is true also for other approaches to Darwinian culture theory, but these seem to lack a defining selecting consequence, parallel to fitness. Thus the explanatory power of operant cultural theory is at an advantage, and makes perfect sense of why culture can carry so much maladaptive behavior, while still clearly being a Darwinian product of historical net adaptive value.

Unfortunately there are many important subjects that I did not manage to discuss and take into account properly, for example consciousness and mental states. I think it is important to have a clear theory of consciousness and mental states. I think consciousness and mental states can be discussed fruitfully within a system theoretic framework of brain-as-replicator. I think the approach of thinking about mental states as functional rather than physical states is incomprehensible—for something to be functional in a physical world it has to be physical. Mental states are probably physical properties with a functional value, but to what extent it makes sense to isolate their functionality from their neurological embodiment, is an important question. For example, the mental world connects millions of stimuli seamlessly together, but then again, isn't that what the brain already does? At least it is good to know that there are always interesting subjects—new or largely unvisited terrains—within philosophy to delve into.

The final evaluation of my thesis is that it is somewhat successful considering the circumstances. I think I have managed to show, although less powerfully than I had hoped, that operant conditioning is worth at least considering as a defining functional-historical pattern in ontogeny and in cultural evolution. I also successfully argued that the classical radical behaviorism is inconsistent with modern evolutionary biology, but only to an extent where adjustments renew its relevance. Meanwhile, I am fairly confident that distinguishing between proximate and ultimate explanations, and interactors and replicators of behavior, in ontogeny is fruitful. I was to a lesser extent able to convincingly show that culture theory benefits from a distinction between replicator and interactor. I also think that it is of great benefit to view the relationship between biology and psychology more in terms of history; the lifetime of organisms extending the evolutionary histories of its species, rather than in terms of the mind-body dichotomy. All in all I drew a dim sketch of a *philosophy of interdisciplinary science of behavior*, but a sketch nonetheless.

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