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What Is 'Normal'? An Evolution-Theoretic Foundation of Normic Laws and Their Relation to Statistical Normality.

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WHAT IS 'NORMAL'?
An Evolution-Theoretic Foundation of Normic Laws and their Relation to Statistical Normality[+]

Abstract: Normic laws have the form "if A, then normally B". They are omnipresent in everyday life and non-physical 'life' sciences such as biology, psychology, social sciences and humanities. They differ significantly from idealized ceteris-paribus laws in physics. While several authors have doubted that normic laws are genuine laws at all, others have argued that normic laws express a certain kind of prototypical normality which is independent of statistical majority. This paper presents a foundation of normic laws which is based on *generalized evolution theory* and explains their omnipresence, lawlikeness and reliability. An argument is presented which shows that the fact that normic laws are a product of *E*volution must establish a systematic connection between prototypical and statistical normality. This has significant implications for Millikan's evolution-theoretic conception of proper functions, to which this account is closely related.

1. Introduction. Strictly universal laws, as they are required by the deductivistic model of scientific inquiry, exist only in special areas such as mathematics or theoretical physics. Most general hypotheses in everyday life and (non-physical) sciences are *not* strictly universal but admit of *exceptions*. Their linguistic form is not "All As are Bs", formally $\forall x(Ax \supset Bx)$, but "As are *normally* Bs". Following Scriven (1959), I call these loose laws *normic* laws and represent them as $Ax \Rightarrow Bx$ (where "Ax", "Bx" denote open formulas in the individual variable x and $\Rightarrow$ is a variable-binding normic conditional operator). As the following list of examples shows, normic laws do not only dominate all higher 'life' sciences, such as biology (1), psychology (2), social sciences and humanities (3) – technology (4) is also full of them:

(1) Birds can normally fly (this was default logic's paradigm example).

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(2) People's actions are normally goal-oriented, in the sense that if person x wants A and believes B to be an optimal means for achieving A, then x will normally attempt to do B (cf. Dray 1957, 132ff; Fodor 1991).
(3) Governments normally try to keep the economy of their country intact.
(4) Turning the ignition key normally turns on the engine of my car.

In all of these examples it is *hopeless* to turn the normic law into a strict law by excluding all possible exceptions in the laws' antecedent, because the class of possible exceptions is completely heterogeneous and potentially infinite – it may contain everything 'between' cosmic catastrophes and collective insanity (the same point is made, e.g., by Hempel 1988; Rescher 1994, 14; Pietroski and Rey 1995, 84, 102).

To avoid misunderstandings, we need a clarification of our notion of "exception". With a *strict* exception to a law L I mean a true singular (basic) statement S which falsifies L (i.e., S logically entails $\neg$L). If I speak of normic laws as laws which admit exceptions, then, of course, I do not mean strict exceptions, but *loose* exceptions in the following sense: a loose exception to a normic law of the form $Ax \Rightarrow Bx$ is a true singular (basic) statement S which logically entails an *abnormal L-instance,* which is a statement of the form $Aa \wedge \neg Ba$ for a an individual constant (cf. Pietroski and Rey's 'abnormal instances', 1995, 88). Only strict laws may have strict exceptions. Normic laws never have strict exceptions, because they are not falsifiable. But they have loose exceptions.

Not much attention has been paid to normic laws in the history of philosophy (a notable exception is *Aristotle* in *Book VI* of his *Metaphysics*). In our century, normic laws were (re)discovered in the 1950's, when philosophers of history criticized the Popper-Hempel model of *deductive-nomological* explanation as being inapplicable to history, because the principles by which historians explain are never strict, but 'loose' or normic (Gardiner 1952, 124f; Dray 1957, 132, 137). This provoked an enduring debate in which, in spite of all differences, the dominant attitude was that normic 'laws' are not genuine scientific laws but *pseudo-laws*, void of empirical content, because they are not strictly *falsifiable*: by proclaiming counterexamples as (loose) exceptions, we can always protect a normic law from falsification (Dray 1957, 132; Scriven 1959, 466; Popper 1934, §20, §67; Albert 1957, 132ff).

This deductivistic attitude changed when philosophers examined more closely the nature of statistical laws. Also numerical-statistical laws of the form "r% of all As are Bs" are not strictly falsifiable (assuming the class of A's is potentially infinite). And yet they *do* have empirical content, because they may get *gradually disconfirmed* by the observation of sample frequencies which significantly deviate from the probability value predicted by the law (cf., e.g., Hays 1981, §§ 5.12, 6.8). Such significantly deviating sample frequencies constitute loose exceptions to statistical laws. Exactly the same gradual (dis)confirmation argument can be applied to normic laws – *provided* that normic laws are, or at least *imply* claims of statistical normality. More precisely: provided that $Ax \Rightarrow Bx$ implies that the conditional statistical probability of Bx given Ax, p(Bx/Ax), is *high*. I call this the *statistical consequence thesis*. This thesis would be sufficient to establish the empirical content and, hence, the scientific 'dignity' of normic laws. For then, if the number of (loose) exceptions increases in relation to the normal cases, our belief in the normic law will become increasingly weaker, until finally, we give it up.

A number of influential authors, however, have argued, that there exists no connection between normic laws and statistical majority claims. In part, these arguments came from research in *Artificial Intelligence* (AI). In this field, normic law have been experiencing a *boom* since the 80's. Special logics have been developed for normic laws (or rules) - the so-called *non-monotonic* or *default* logics.[1] At first, the statistical

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[1] Like inductive reasoning, normic reasoning is *not* truth-preserving. As a result, the rule of monotony (i.e., the rule of premise-strengthening), which is valid for deductive inferences, gets violated. For example "birds can fly, and my pet is a bird" non-monotonically implies "my pet can fly", but if we *add* the premises "and my pet is a penguin, and penguins cannot fly" this conclusion does not longer follow – it is *defeated*. For an overview of systems of NML cf. Brewka (1991) and Gabbay et al. (eds., 1994). Important philosophical forefathers are Adams (1975), Pollock (e.g., 1974, ch. 3.4) and Rescher (1976).

consequence thesis was doubted for the *technical* reason that no way was known to unify non-monotonic logic (NML) and probability theory into a coherent system. Today it is known that there exists a rather tight formal relationship: there exists a *probabilistic semantics* for NML which says, roughly, that an inference is derivable by the rules of the so-called system **P** iff it preserves (not truth but) high probability. At present, the system **P** has the broadest acceptance in the NML-community.[2]

Independent from these technical questions, severe *philosophical* objections against the statistical consequence thesis have been brought forward, by researchers in NML (e.g., MacCarthy 1986, Reiter 1987) as well as by philosophers of biology (e.g., Millikan 1984, 1989a, Neander 1991) in the context of *biological functions.* According to these objections, normic laws assert a certain kind of *prototypical* normality, which is independent of statistical majority. For example, the ability to fly is a prototypical property of birds, and this remains true *even if* – by some major disaster – the majority of birds would stop using their wings or even lose their flying ability.

So we have two kinds of normality: *prototypical* and *statistical.* What is the relation between prototypical and statistical normality? The above objections are certainly correct insofar as prototypical normality cannot be *reduced* to statistical normality. It is *more* than that: among other things, it makes normic generalizations *non-accidental* (cf. §2). I also agree with MacCarthy (1986, 91f), Hempel (1988, 25) and Millikan (1989b, 281) that we are typically unable to determine any *precise* probability values corresponding to normic laws, such as "x% of all birds can fly" or "y% of all matches light when being struck" - when?, where? All we know or assume is that these probabilities are *high.*[3] But this does not contradict the statistical consequence thesis: prototypical normality might nevertheless *imply* statistical normality, as an

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[2] System **P** covers three converging approaches: (i) *probabilistic entailment* (e.g., Adams 1975, Pearl 1990, Schurz 1997b, 1998), (ii) *preferential models* (e.g., Kraus et al. 1990, Lehmann/ Magidor 1992), and (iii) *expectation-orderings* (e.g., Gärdenfors/Makinson 1994, Rott 1997).

[3] *How* high they are, or have to be, depends on the *domain of application* - compare the breaking of a match with the crashing of a plane (cf. Schurz 1997b). A minimal acceptability condition is, of course, $p(Bx/Ax) > p(\neg Bx/Ax)$, or equivalently, $p(Bx/Ax) > 0.5$.

insufficient but *necessary* semantic condition.

The statistical consequences thesis can be justified in two different ways: (i) *methodologically* and (ii) *ontologically*. The methodological justification argues that *only* if the thesis holds, can reasoning from normic laws be *practically reliable* in the sense of a high predictive success rate (Schurz 1997b). This kind of justification has been put forward by Pearl (1988, 477-80) against the *conventionalistic* view of McCarthy (1986, 91) and Reiter (1987, 149f). The methodological justification tells us only why the statistical consequence thesis *should* hold, but not whether it *in fact* holds. What we would want to have is an *ontological* justification which demonstrates that normic laws and their statistical consequences are an *objective* feature of reality. Is there any *objective reason* for the *omnipresence* of normic laws in everyday life, in the 'higher' sciences and in technology? Or is this merely a result of our *subjective framing* of the world which is in fact too complex to be comprehensible? In the case of strict laws one usually distinguishes between *genuine laws* and mere *accidental* regularities by the fact that the former have a *unified explanation* by general *theories* (the so-called Mill-Ramsey-Lewis account; cf. Earman 1986, 87). Is such an objective theoretical foundation also possible for normic laws? In the next section I will suggest such a foundation. Its basic idea is that normic laws are the phenomenological laws of self-regulatory systems which originate from a process of evolution.

2. Evolution-Theoretic Foundation of Normic Laws. We start with the *system-theoretic* distinction between *closed* or *isolated* versus *open systems.* In closed systems, there is *no* exchange between system and environment; in isolated systems, there is exchange of heat-energy, but no exchange of matter (e.g., a gas under isothermic conditions). Only in open systems is there a continuous exchange of both matter and energy between system and environment. While closed and isolated systems are in a state of thermodynamical equilibrium, open systems are in states of stationary disequilibrium – far away from the thermodynamical equilibrium of closed 'dead' systems (cf. Bertalanffy 1979, 39f, 141ff; Rapaport 1986, 177f; Josef Schurz 1990, 234).

The laws describing systems are called *system laws.* In contrast to laws of nature (such as Newton’s total force law or special force laws), system laws are implicitly or explicitly relativized to a specification of the particular kind of system under consideration. The systems studied by physics or chemistry are, at least traditionally, closed or isolated systems. Examples of closed system laws are Kepler’s laws of elliptic planetary orbits. Typically, closed system laws are expressed as *strictly* universal generalizations. However, they are never strictly but at most approximately *true*, because no real system is completely closed. In other words, physical system laws are *idealizations* which cannot be true without employing ceteris paribus conditions and approximation procedures (cf. Cartwright 1989, 202ff, Hüttemann 1991).

In contrast, all 'higher' sciences, from biology 'upwards', are concerned with open systems, more specifically with 'living' systems or with their cultural and technical products. Very generally, *systems* are physical ensembles composed of parts which preserve a relatively strict *identity in time*, by which they delimit themselves from their (significantly larger) environment (Rapaport 1986, 29ff). For closed systems this preservation of identity is a matter of *postulate*: that our planetary system is stable is a *frozen accident* of cosmic evolution; should it ever be destroyed by a gigantic meteor then it stays destroyed forever and will *not* regenerate. But what explains the relatively strict identity of open systems, which are permanently subject to significant and possibly destructive influences of the environment? The explanation lies in the fact that all open 'living' systems have the capacity of *self-regulation*, which is abstractly described by the following conditions of cybernetics[4] (cf. Ashby 1961):

(C1) The preserved identity of self-regulatory systems is abstractly governed by certain *norm states* (in German "Sollwerte"), which the system constantly tries to approximate by its *real states*.

(C2) It does this by way of certain *subsystems* ('organs') performing the necessary *regulatory mechanisms* ('functions') which *compensate* for *disturbing influences* of

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[4] Like Rapaport (1986, 113) I understand *autopoietic self-organization* in the sense of Maturana and Varela (1992) as a generalization of cybernetics.

the environment by producing counteracting processes. Their *compensatory power* is *limited* – if the external influences exceed it, the system is *destroyed*.

The first part of our foundational enterprise consists in the following thesis: *normic laws are the phenomenological laws of self-regulatory systems.* This thesis gives us a deeper *explication* of the concept of normality involved in normic laws: it only makes sense to attribute prototypical norm states to systems with explicit or implicit self-regulatory properties. Generalizations such as "Planets of our solar system *normally* have elliptic orbits" are not normic *laws* but *accidental* normic generalizations.[5] They express mere statistical normalities, because planetary systems do not have self-regulatory capacities.

On the same reason, normic laws cannot be identified with *ceteris paribus laws.* Rather, normic laws are a certain subclass of 'ceteris paribus laws' where the ceteris paribus clause figures as an implicit normic conditional operator. Ceteris paribus laws of a different sort are used to describe closed or isolated physical systems. Because these systems do not have self-regulatory properties, their system laws need an *exclusive* ceteris paribus clause, which explicitly requires the absence of 'disturbing' factors. Examples are "Ceteris paribus, planets of our solar system obey Kepler's laws", or "ceteris paribus, rivers cut under their outside curve banks". In Schurz (2000a,b) it is argued that exclusive ceteris paribus laws of physics require a deductivistic reconstruction. They differ so significantly from normic laws of higher sciences that one should better not subsume both under the same category.

But *why* are self-regulatory systems omnipresent in our world? Where do their prototypical norm states *come from*? Why do their self-regulatory mechanisms normally work properly? Our answer is: by *E*volution (with capital "E") in a generalized 'Darwinian' sense of evolution by natural or cultural *selection*. Self-regulatory systems which have evolved by Evolution are called *evolutionary systems.* Their proto-

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[5] This accords with Flichman (1995, 41) who classifies Kepler's laws not as genuine laws but as mere uniformities.

typical (norm) states and self-regulatory mechanisms are those things which have been gradually selected in Evolution, according to their contribution to reproductive success. Due to their *limited* compensatory power, dysfunctions may occur, hence their normic behaviour may have various *exceptions*. Yet it must be the case that evolutionary systems are in their prototypical norm states in the high *statistical majority* of cases and time-points. For otherwise (with high probability), they would not have *survived* in Evolution. In this way, evolution theory explains not only why the phenomenological behaviour of evolutionary systems obeys *normic* laws – it explains also why this peculiar connection between *prototypical* and *statistical* normality exists at all. Green plants, for example, can normally perform photosynthesis. Of course it is possible that due to a catastrophic event, all green plants lose this ability. But then (with high probability), they will become extinct after a short period of Evolution. For similar reasons, governments normally try to keep their countries economically intact; otherwise they will be overthrown or will lose the next election. Analogously, electric installations normally work, for they are constructed in that way, and if this were not so, they could not survive in the economic market. Put in a nutshell, *prototypical normality and statistical normality are connected by the law of evolutionary selection*.

So far, this is a rather oversimplified explanation. But it gives us the basic idea of our second and major thesis: self-regulatory systems have evolved by Evolution, which implies by means of our first thesis that normic laws are the *phenomenological laws of evolutionary systems*. In contrast to the first thesis, this second thesis does not express an analytic truth resulting from concept-explication. It expresses a contingent truth resulting from the philosophical analysis of empirical knowledge. As a matter of fact, almost all of the self-regulatory systems which exist in our world are either living systems or their products. According to all available evidence, living systems and their products have evolved by biological or cultural Evolution. Therefore, almost all self-regulatory systems are evolutionary systems.

The evolution-theoretic foundation gives us a deeper understanding of the difference between normic laws and idealized ceteris paribus laws of physics. For the latter laws, a specification of *all* factors characterizing the ideally closed system ('and nothing else') is needed. For evolutionary systems, such a specification is neither possible nor necessary. It suffices to assume that the disturbing influences, whatever they may be, are within the manageable range of the system's regulative capacity. It is usually impossible to give an exact theoretical prediction of this manageable range. But evolution-theoretic considerations tell us that the external influences will *normally* be within this manageable range.

A related difference is the following. In physics one traditionally thinks of *complexity* as a source of *disorder* - regularities are obtained by *abstracting away* from complexities. In evolutionary systems, however, complexity is usually a *source of order* - complexity which has been selected by Evolution to stabilize normic behaviour. Ideal planets are theoretical abstractions: mass points under the influence of a centripetal force and 'nothing else'. They do not literally exist (cf. Wachbroit 1994, 587f). In contrast, normal birds really *do* exist because they are what has been selected in Evolution. When speaking of a normal bird, we do not abstract from its admirable complexity, but we *rely* on it as the cause of its normal behaviour. The idealization procedures needed for planets would not make good sense for birds: there are no disturbing parameters which, when going to zero, turn a real bird into an ideal bird which necessarily can fly and which is approximated by the real bird.

Of course, *in principle* one may always try to give a physical explanation of evolutionary systems by treating them as parts of larger closed or isolated physical systems, but in most real examples this would be a theoretically hopeless enterprise. However, there exist systems which can be fruitfully described both as closed systems of physics and parts of open evolutionary systems – namely *technical systems*. Consider the systems of electricity which surround us every day. We may consider this automatic dish washer together with its electrical circuit as an ideally closed physical system. From this viewpoint, there are thousands of possible disturbing factors which may prevent our dishes from being cleaned, and amazed we may ask ourselves why all these electrical systems can be so cheap and yet work so well. Alternatively, we may consider dish washers as part of an evolutionary system – the economic system of production and distribution of electrical products. This perspective does not give us detailed knowledge of the physical mechanisms underlying dish washers, but it gives us an explanation of their admirable cheapness and functionality in terms of gradual optimization.

There exist also self-regulatory systems in nature which have not evolved by Evolution, but came into existence by 'accident' (cf. Maynard-Smith/Szathzmary 1995, ch. 2.2). But they are rare, and their self-regulatory properties are so vulnerable to changes in external conditions that their normic regularities can hardly be called lawlike. For example, the water level of a lake has simple self-regulatory properties, but the lake has no capacity to counteract when its outflowing river is blocked, or its inflowing river lacks water. Generalizations like "normally, the lake maintains a certain level" may be seen as *transition* cases between idealized laws of physical systems and normic laws of evolutionary systems. Likewise, many normic common sense generalizations are not really lawlike. Common sense tends to conceive the presence of the "Normal conditions" on our earth in Millikan's sense (1984, 33ff) – those conditions to which evolutionary systems are *adaptations* – also as normic 'laws'. But statements such as "normally, the temperature on earth is within such and such a range" do not express prototypical normalities. It is an important aspect of evolutionary analysis that our earth, although it has brought about evolutionary systems, is *not* itself an evolutionary system, because it does not reproduce itself. Earth is vulnerable to *irreversible* devastations which in the worst case may suspend Evolution.

3. Generalized Evolution Theory. The suggested evolution-theoretic foundation is intended to apply to all 'higher' sciences, not only to biology. Therefore, we have to base it on the *generalized* theory of Evolution, which is a relatively young research program (cf. Sober 1993, ch. 7.5). It was suggested in Dawkins' concept of "memes" (1989, ch. 11) and has been further developed by various authors such as Cavalli-

Sforza and Feldman (1981), Boyd and Richerson (1985), Durham (1990), and implicitly by Millikan (1984, ch.1-2). In contrast to *sociobiology,* generalized evolution theory does not intend to explain cultural evolution by the evolution of *genes*. It assumes cultural evolution as an independent level based on the evolution of *memes*, which are complexes of human ideas and skills. However, cultural and biological evolution may *interact*, in the form of positive or negative reinforcements (Boyd and Richerson 1995, 11). Generalized evolution theory differs from earlier accounts of this sort in that it does not utilize 'Darwinism' as a mere (and often misunderstood) 'analogy' or 'metaphor', but it formulates principles of Evolution which, in spite of their abstractness, have to be taken in a literal or *technical* sense (cf. Dawkins, 1989, ch. 11). Of course, Evolution is different for biological and cultural or technical systems. But on all levels, processes of Evolution share the following three characteristics, which distinguish Evolution from other kinds of temporal developments:

(E1) a mechanism of *variation* which acts in larger populations of evolutionary systems which are in mutual competition
(E2) a mechanism of *reproduction* which leads to consecutive generations of evolutionary systems – whence variations must be heritable, and
(E3) an environment which *selects* the fittest among the variations, i.e., those with the highest reproduction rate – whence variations must differ in their fitness.[6]

To be sure, no 'Panglossian' adaptationism in the sense of Gould and Lewontin (1979) is involved in these principles. There is no intrinsic drive of Evolution to lead to perfectly adapted species of self-reproducing systems. But there *is* a *selective drive* in Evolution which favours variations which are *better* adapted to their environment than their competitors, in the sense of having increased chances to survive and to reproduce themselves. Adaptive selection is a *comparative* process: better adapted organisms can yet be imperfect in many ways. In this way, Evolution theory leaves plenty of room for the existence of selectively neutral random processes (cf. Ridley

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[6] Cf. Sober (1993, 9), who characterizes Evolution by "heritable variation in fitness".

1993, ch. 6, 7) as well as for imperfect and even dysfunctional traits which often have their explanation in their descendance from ancestor traits which were adapted to different purposes (cf. § 4). Yet, Evolution theory is high in content. Processes of Evolution are markedly different from other developmental processes (e.g., cosmic 'evolution') where some of the conditions (E1-3) are missing. Evolutionary processes do not have 'goals', but they have *directions* imposed by the selective pressure on competing variations (Dawkins 1983, 420) – each direction representing a specialized 'branch' on the tree of evolutionary descendance.

For our purpose, the question of how much 'adaptationism' is involved in Evolution theory is of less importance. What *is* important is how Evolution theory explains why normic laws are the typical *outcome* of evolutionary processes. To take a biological example, assume the genotype G of a certain phenotype of a species S mutates into a variant G* producing a phenotype P* which has a small selective advantage in the given environment. Then the *theoretical* laws of population genetics predict that after sufficiently many generations the population will be in an evolutionary equilibrium which is *independent* of the initial frequencies of G and G*, where 1-ε S-members have genotype G* with ε as a small probability due to 'abnormal' G*-mutations (cf. Ridley 1993, 107-9). The result will be the normic law "S-member normally have phenotype (or trait) P*". One also says that G* has gone to (almost-) fixation. Thereby, the small remnant of 'abnormal' variants in the resulting equilibrium is not 'superfluous' but highly important for Evolution: for example, it may prevent the Evolution of this species in later stages from being trapped in a local fitness-maximum (cf. Ridley 1993, 204ff). We should add that not *all* evolutionary processes are of such a simple kind. Examples where almost-fixation is not reached are discussed in §4. But evolutionary processes resulting in almost-fixation are very common. Accordingly, we do not claim that *all* traits of evolutionary systems obey normic laws. What we explain is why normic laws are so common for evolutionary systems.

The theoretical laws or models of population dynamics are usually expressed as purely mathematical truths, roughly stating that *if* the competing populations and their evolutionary 'forces' are such and such, *then* the final outcome will be such and such (cf. Sober 1993, 70ff). One may wonder how purely mathematical truths can explain empirical normic laws such as "birds normally can fly"? This conflict is only apparent. Of course, normic laws about *factual* evolutionary systems are only derivable from the respective mathematical truth *together* with an *antecedent premise* which asserts that the *factual* conditions of the considered evolutionary systems are closely approximated by the idealized assumptions which are hypothetically assumed in the if-part of the respective mathematical truth. This antecedent premise carries the empirical content of the explanatory premises. Note that this antecedent premise does not only contain contingent details about evolutionary 'forces'; it also entails that the evolutionary principles (E1-3) are satisfied in the first place.

Unlike laws of nature, normic laws are not *physically necessary*. Because of their dependence on accidental circumstances of Evolution, normic laws involve a considerable portion of *contingency*. If Evolution has taken place in another part of the universe, it will probably have produced species which are rather different from those on earth. Nevertheless, normic laws also contain a considerable portion of *necessity*, because of their systematic dependence on the principles of evolution theory. The *nomological* character of normic laws is also demonstrated by the fact that they support *counterfactuals* (cf. Nagel 1977, 273) such as "if this bird *were* to be hunted by a predator, it *would* fly away".

The detailed circumstances of real evolutionary processes are to a great extent unknown. Therefore, in the usual case, evolution theory will not provide a complete explanation of the normic laws of evolutionary systems, but only an explanation *schema* in the sense of Kitcher (1981; cf. also Schurz/Lambert 1994, 98). Evolution theory will usually not be able to explain why *this* particular species has just *these* particular traits, rather than other but functionally equivalent traits, because this is largely dependent on unknown contingent circumstances. But evolution theory does certainly explain why evolutionary systems have *some* traits with *some* functions performing the regulations necessary for survival and reproduction. This is sufficient for our purpose – it is exactly the theoretical foundation we are after.

The question to what extent biological and cultural (including technological) Evolution can be treated as applications of the same general principles (E1-3) is still controversial (cf. Sober 1993, 212-215). We conclude this section with a systematic comparison of these two levels of evolution. At the biological level, the *evolutionary systems* are biological organisms, and reproduction consists in the production of biological offsprings. However, the kinds of entities which 'carry' the evolution, i.e. which are directly reproduced from each other, are not the organisms themselves but the *genes* and *genotypes* (characteristic combinations of genes). To generalize these notions to all levels of evolution[7], we speak of *repros* and *reprotypes* – they correspond to Millikan's first order reproductively established families (1984, p. 23ff). Biological variation consists in mutation and recombination of genotypes. The biological reprotypes (genotypes), together with environmental conditions, produce the biological evolutionary systems (organisms) as their corresponding *phenotypes* - they are what Millikan calls higher order reproductively established families (ibid.). With a *class of evolutionary systems* we always mean a higher-order reproductively established family in Millikan's sense.

The repro(type)s of *cultural evolution* are informational 'units' and informational 'wholes' – memes and memotypes. Ultimately, they are located in human brains, or minds if you prefer. Reproduction of memes occurs by (verbal or written) information-transmission from 'teacher' to 'student'; Boyd and Richerson speak of "cultural parent-child relations" (1985, 7f). It is thereby important, that transmission of memes does not *only* go from parents to their biological offspring, but may float in all social directions: whenever one human learns from another one, memes have been transmitted. In other words, cultural evolution is "multi-parental" (Boyd/Richerson 1985, 63f). Sober (1993, 210) speaks of "type III selection models" based on "teachers having students". It was pointed out by Cavalli-Sforza and Feldman (1981) that 'hori-

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[7] Chromosomal gen-organisation and chromosomal diploidy are, of course, not generalizable.

zontal' transmissions processes of this sort must be assumed to explain the selection of certain cultural traits such as "having fewer babies". This is not in conflict with the fact that the *tradition* of knowledge and culture, i.e. the transmission of memes from older to younger *generations* of humans in a long process of education and instruction, is the most important cultural reproduction process, because only this process ensures reproduction in the long run (cf. also Boyd/Richerson 1985, 8).

The evolutionary systems of cultural evolution are *cultural 'tools' and practices* which are the phenotypic correlates of the underlying memotypes. Here we differ from Boyd and Richerson (1985, 8) who assume that cultural evolution allows no distinction between repro- and phenotypes (which has been criticized, among others, by Hull 1982). The importance of this distinction can be illustrated by way of technological evolution. In traditional accounts (e.g. Basalla 1988, 30), the technical *artifact* is assumed to be the basic unit carrying technological evolution. From this viewpoint, evolutionary principles can merely be used as metaphors (ibid., 25f), because the artifacts themselves neither reproduce themselves nor vary themselves, etc. From the viewpoint of generalized evolution theory, the reprotypes of technological evolution are the memes containing all the information necessary for the construction of technical artifacts as well as for their proper usage. It is these memes which are literally reproduced by being taught, iteratively, from teacher to student and from one generation to the next. The corresponding phenotypes, the evolutionary systems, are technical artifacts *together with* the practices of their usage. It is important see why even the phenotypes of technological evolution cannot be reduced to artifacts *per se* but must include usage practices: simply because these practices are decisive for their selection history. An example is Basalla's analysis of the evolution of automobiles (1981, 197ff). In 1900, when automobiles were mainly used for short city rides by rich people, gasoline engines were much less frequent than steam engines or electric engines. The advantage of gasoline engines in terms of higher speed and longer refilling intervals became dominant when more and more people started to use automobiles for long ride and transportation purposes. After one century of techno-economical evolution gasoline engines have become universal automobile practice.

Cultural variations occur much more frequently than biological mutations, so cultural evolution is much faster. Another difference is that variations at the biological level are 'blind'. In contrast, variations at the cultural level are mainly based on creative human *inventions*, which are not 'blind' but the result of *intentional* cognitive plans (Boyd and Richerson speak of "guided variation"; 1985, 9). Although this difference is the point of attack of many critics (cf. Hull 1982, 307f), it does not constitute a reason why the principles (E1-3) are not applicable to cultural evolution. For human inventions are fallible and imperfect, and thus, they are subject to ongoing adaptation and perfection by cultural selection (cf. also Bigelow and Pargeter 1987, 185). Although the individual cultural variations result from intentions, the long-term result of cultural evolution is *not* the result of any global intentional plan, but can only be explained by evolutionary dynamics as the long-term effect of the recursive iteration of local activities. This is also the main result of Basalla's analysis of technological evolution (1988, 211ff) which resists all explanations in terms of global goals such as 'control of nature' or 'betterment of human life'. Basalla's analysis also demonstrates the *continuity* of technical variations: contrary to what official textbooks suggest, popular inventions are almost always the improvement of forgotten previous artifacts. Watt's steam engine in 1775, for example, improved Newcomen's atmospheric steam engine dating from 1712.

Selection at the cultural level is performed by differential imitation, learning and consumption activities within relevant social subsystems. Some cultural traits are more often imitated or learned than others (Boyd and Richerson 1985, 9-11, speak of "biased transmission" and "natural selection", but like Sober 1993, 214, I prefer here the term *cultural* selection). Critics have argued that cultural evolution is non-Darwinian because it involves *autoselection*, where variation and selection are performed by the same person (cf. Hull 1982, 311). For example, the engineer who creates technical inventions is also the one who selects them, at least *before* the invention enters the economic market. Also *autoselection* does not constitute a reason why the principles (E1-3) are not applicable. What is required is only that the *criteria* of selection which determine the fitness values have some degree of independence from the competing variations. This is also the case in cultural evolution. For technological evolution, Basalla (1988) demonstrates impressively that the major selective forces are neither the inventor's original purposes nor the consumer's needs but the *economical success* of technical products. Changes in human 'needs' are usually not the cause but the *effect* of the introduction of a new technical product into the market. Similar considerations apply to other areas of cultural evolution. Of course, cultural scenarios where the selection criteria are instable also exist, for example when the fitness of variations depends negatively on their frequencies. This may lead to periodic or even chaotic fluctuations (e.g., clothing fashions, cf. §4) without any 'directed' evolution. But note that such cases do also occur in biological evolution (cf. May 1987 for an illuminating example). It is doubtful whether such processes should still be called processes of 'Evolution', because 'selection' forces which are completely instable cannot exert any selective power.

4. Why Prototypical Normality Normally Implies Statistical Normality. In philosophy of science, evolution-theoretic normality has been discussed mainly in the context of functional analysis and explanation in biology. We ignore here the question whether functional explanations are 'genuine' explanations (cf. Schurz 1999, §6) and concentrate on the concept of *proper* biological function. According to the account of Millikan (1984, ch. 1; 1989, 13) and Neander (1991, 174), F is a proper biological function of an item (e.g., an organ) X of species S iff *X's genotype was selected because X has contributed with F to S's evolutionary fitness.*[8] A crucial feature of this account is its *historical* nature. In order to discern a proper function of an organ from a mere accidental effect - e.g., the function of the human nose to smell as opposed to

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[8] This is Neander's 'condensed' version of Millikan's account. It is a special version of the *etiological* view of functions (Wright 1976). The same account is supported in Sober (1993, 84), who speaks of adaptations instead of functions.

its 'function' of supporting glasses – one must look at the *selection* history: if and *only* if the considered effect has systematically increased the fitness in the lineage of the considered species, does it count as a proper biological function.

This account seems to fit with our evolutionary characterization of prototypical normality. But there is a complication. Not all prototypical characteristics have (or had) a direct selective advantage, i.e., are proper functions in the above sense – they may also be causal *side-effects* of traits with proper functions. To take a much debated example (Cummins 1975; Bigelow and Pargeter 1987), circulating the blood is a proper function of the vertebrate heart, while the typical sound of the heart beat is a mere side effect of it – but still it is prototypical for vertebrates' hearts to make this sound. A subtle example of this sort are Gould and Lewontin's 'spandrels' (1979), which are prototypical side-effects of complex architecture which was selected for independent reasons. To cover this difference, we distinguished between *fundamental* versus *derived* prototypical traits: while the former confer a direct selective advantage, the latter are mere causal side-effects of the former. In other words, fundamental and derived prototypical traits are *common effects* of an underlying reprotype which has been selected because it produces fundamental prototypical traits. We generalize these ideas in the following *preliminary* definition of *prototypical normality* (PN_{pre}). Thereby, we use the term "trait" in a flexible, more detailed or less detailed, way – a trait may consist in a function, or in the possession of an item (e.g., organ), or in the possession of an item *with* a certain function.

(PN_{pre}) For S a class of evolutionary systems and T a trait of S-members:

(i) T is a prototypical trait of S-members iff T is produced by a reprotype R which has been selected in the history of S-members. **(ii)** If the selection mentioned in (i) has taken place *because* R produces T, then T is a *fundamental* prototypical trait of S-members. **(iii)** If this selection has *not* taken place because R produces T, then T is a (merely) *derived* prototypical trait of S-members.[9]

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[9] Because "derived" means "merely derived", our fundamental vs. derived distinction is *disjoint*.

We complete this definition with the following *co-definitions*. (1.) We call a normic generalization of the form "S-members normally have trait T" a fundamental, or a derived, *normic law* iff T is a fundamental, or derived, prototypical trait of S-members, respectively. (2.) If T is a fundamental prototypical trait of S-members and T consists in the *possession of item X with effect F*, then F is called a *proper function* of item X of S-members. In this way, we regain the concept of proper function from definition (PN_{pre}). (3.) We also apply the concept of prototypicality to repro(type)s as follows: R is a *fundamental prototypical repro(type)* of S-members iff R was selected in the history of S-ancestors *because it produces some trait T*; and R is a derived prototypical repro(type) iff R is a side-effect of some fundamental prototypical repro-(type) – e.g., by chromosomatic linkage, as in the case of *hitchhiker genes* (cf. Ridley 1993, 198). (4.) If T is a fundamental (or derived) prototypical trait of S-members, we also say that it is *prototypically normal* for S-members to have trait T in the fundamental (or derived) sense, respectively.

We can illustrate these definitions by way of some examples. It is a proper function of matches to light when struck because they have been selected for this effect. It is a prototypical side-effect of matches that their flame sometimes burns one's fingers – this is an unavoidable consequence of their reprotype. But it is not prototypical for matches to have a certain colour, etc. It is a fundamental prototypical trait of human noses to smell, and also, to stick out from the face, for these traits are, or have, proper functions. It is also prototypically normal for human noses to get cold at their top in the winter, but, of course, merely in the derived sense, as an unavoidable side-effect. It is not prototypically normal for human noses to support glasses (cf. Voltaire's "Dr. Pangloss", quoted in Gould and Lewontin 1979, 583), because human noses are products of biological and not of cultural evolution, and supporting glasses is not the causal effect of any underlying genotype. Having legs, clearly, is prototypically normal for humans (in the fundamental biological sense), but not having short or long longs, because there was no dominant biological selection for short versus long legs. Prototypical normality applies not only to species but also to higher order classes. For example, flying-ability is prototypically normal (in the fundamental sense) within the entire class of birds, although for certain (exceptional) species of birds, such as emus or penguins, *lack* of flying-ability is prototypically normal. This is not a logical conflict, but just illustrates the *non-monotonicity* of normic conditionals. Finally, casting a shadow is not a fundamental but a derived prototypical effect of trees, because it is a causal consequence of their photosynthesis-based form life that they are exposed to sun, and hence, that they cast a shadow. On the other hand, casting a shadow is not a prototypical effect of rocks or mountains because they are not evolutionary systems.

Let us turn to the main question of this section: does prototypical normality in the evolution-theoretic sense imply statistical normality? Several philosophers of science, including Millikan, Neander (1991, 182), Wachbroit (1994, 580) and Laurier (1994), have argued to the contrary. I will now try to show that, although their arguments are correct, they do not refute the statistical consequence thesis, provided the thesis is formulated in a correct way and applied to traits of the right kind. I will then give a straightforward *analytic* argument for the thesis. I will focus on biological examples, because they were central to the debate. It is clear from the preceding section, or so I claim, that similar arguments apply to cultural Evolution.

There are *two main reasons* why a trait T for which there is positive selection does not become statistically dominant in the underlying species S. *First*, it may be that the reprotype R underlying T leads to T only with low (statistical) probability, because individual acquisition of T is strongly influenced by varying (accidental) conditions of the individual's environment. In such cases, the reprotype R may nevertheless have a statistical reproduction advantage which may even drive R to almost-fixation in the underlying species S, although trait T still occurs in S with low probability. However, in such cases I think one should not consider T as a *heritable* (higher-order *reproducible*) trait – its acquisition is too strongly influenced by the environment to call it "heritable". I regard it as a conceptual element of my suggested notion of prototypicality that it applies only to (strongly) heritable traits.

For example, it is known that cultural evolution may spontaneously occur in certain populations of monkeys, as well as in other species (cf. Mainardi 1980), and there may be a genotype statistically favouring the chances of cultural evolution in these species. Yet I would not regard the possession of cultural evolution systems to be (biologically) prototypical for these species because it is not a systematically herited trait – acquisition of it depends too strongly on 'lucky' accidents of the environment. Certainly, cultural evolution is biologically prototypical for homo sapiens, and probably also for homo erectus. To take another example, there may be a slight correlation in humans between the genes determining races and the number of offspring per head, although this trait is dominantly influenced by cultural conditions. Even if this were so, one could not regard it as biologically prototypical for African races to have increased population growth. On the other hand, evidence suggests that it is culturally prototypical for poor nations to have increased offspring numbers.

Of course, one might define a weaker concept of 'heritable trait' for which a mere genetic bias is sufficient. To make this difference precise, I call a trait T to be *strongly* heritable (or: strongly higher-order reproducible) via an underlying reprotype R iff R causes T with high conditional probability p(T/R). I call T *weakly* heritable (weakly higher-order reproducible) via R iff R causally influences T with the result that R and T are positively correlated, i.e. $p(T/R) > p(T/\neg R)$, without that p(T/R) must be high. Finally, I say that a trait T is *produced* by a reprotype R iff T is strongly heritable via R, while T is merely *influenced* by a reprotype R iff T is weakly but not strongly heritable via R. While Millikan's account of proper function seems to include weakly heritable traits[10], my definition of prototypicality (PN_{pre}) is explicated in terms of 'production' and, thus, is restricted to strongly heritable traits. To avoid confusion: of course, the reprotype which merely influences a weakly heritable trait may be prototypical, but not the trait itself.

These distinctions are also relevant for cultural Evolution. If a meme M is expressed so *vaguely* that M-students interpret it in strongly different ways, then although M may statistically favour each of its interpretations, none of them counts as *proto-*

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[10] Her positive correlation requirements (p. 20, cond.3; p. 28, cond. 2) support this interpretation .

typically normal for M-students. This difference may explain why Evolution in sciences and technology with sharply formulated memes proceeds much faster than in humanities or arts where memes leave plenty of room for different interpretations.

What I will show is that the statistical consequence thesis holds for strongly heritable traits. It would not hold for a concept of prototypicality which also includes weakly heritable traits. One may correctly point out that by my definition of 'production', a statistical normality condition has entered my definition of prototypicality. Now, this is unavoidable if the statistical consequence thesis is to follow analytically. Yet my definition of production is not a petitio principii. It is motivated by independent reasons, and it by no means follows from this definition alone that a prototypical trait T must occur frequently, because nothing guarantees that the underlying reprotype R occurs with high frequency.

The *second* reason why a trait T – even a strongly heritable one – for which there was positive selection may not become statistically dominant is that besides selection in favour of the T-producing reprotype R, there was also selection against R which was strong enough to *prevent* R from going to almost-fixation, e.g., because trait T was advantageous only in certain but disadvantageous in other environments. Now, I think that in cases of this sort, neither the trait nor its underlying reprotype can count as prototypical, because of their *ambiguous* selection history. It is a second *conceptual* element of my notion of prototypicality that the *presence* rather than the *absence* of a prototypical trait or reprotype was explanatorily relevant for the species' evolutionary *performance*. It is not enough for a prototypical trait or reprotype that there was *some* selection *for* it – there must have been *overwhelming* selection for it, in the history of the given species. *Without* this condition, one may end up with the result that both the possession and the non-possession of a trait could count as prototypical for a given species – which is conceptually incoherent, because it violates minimal logical principles of the concept of normality[11] (illustrations are given below).

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[11] In holds in all NML-systems (even in those weaker than **P**; cf. fn.s 1, 2) that $Ax \Rightarrow Bx$ and $Ax \Rightarrow \neg Bx$ implies $Ax \Rightarrow \bot$, which says that it is prototypically normal for Ax to be absurd.

Our second conceptual condition resolves certain tensions between *history-related* vs. *presence-related* selection-accounts of proper functions. Presence-related accounts identify the possession of a function at a *given* time with the possession of a survival-enhancing disposition at *that* time (suggested, e.g., by Bigelow and Pargeter 1987). Millikan's argument (1984, 29) against presence-related accounts is that they cannot account for the fact that organs can be *malfunctioning*: they would force us to say, for example, that a human pancreas which is incapable of producing enough insulin does not have this biological function *any more*. What we should say instead is that this pancreas has become unable to perform *its* function – which is the function for which pancreases have been selected. This argument shows that the distinction between proper versus malfunctioning organs requires a historical account (cf. also Prior 1985, 318-20; Laurier 1996, 27f). On the other hand, *purely* historical accounts of proper functions are also inadequate. For during Evolution an organ may not only *change* the function(s) for which it was originally selected – a process called *exaptation*[12] – it may also *lose* these function(s) and become what is called a functionless *vestigial* organ, such as the human appendix. As Laurier has pointed out (1996, 33), the purely historical account of function would be forced to claim that it is still the function of the human appendix to digest cellulose because it was for this function that the appendix-ancestor was selected in ancestor species of humans. However, this violates all intuitions about biological functions.

So we need an account which combines past and present of the selection history. Kitcher (1993, 486-9) distinguishes five different versions, focusing either on the entire history, on the recent past, or on the present. His comparisons result in a slight but not conclusive preference for recent-past-accounts. Our second conceptual condition suggests solving this problem as follows: the condition of "overwhelmingly positive selection" (i) *combines* past and present of a trait's selection history, but (ii) *restricts* this history to that of the underlying species (or class) S, and does not also

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[12] For example, the original function of feathered wings in bird-ancestors was temperature regulation (cf. Millikan 1989, 44).

look on the history of the trait in *ancestor species* S', S", … of S (which is what Kitcher does). Because of (i), our account is not vulnerable to the pancreas-objection, but allows the systematic distinction between properly and malfunctioning organs. Because of (ii), our account does not need a restriction to the "recent past" – it is not vulnerable to the human appendix-objection, but allows proper functions to change or to get lost, in the tree of evolutionary descendance of species. In particular, condition (ii) implies that – unlike Kitcher (1993) and many other authors – we do not speak of the function F of an item X simpliciter, but of the function F of an item X *in an underlying class of evolutionary systems* S. The class S does not only contain all present S-members, but also all S-members in the past. In the biological case, S may not only be a species, but also a higher class, or more generally, a *subtree* of the tree of evolutionary descendance which need not correspond to any taxonomical class.

For example, the quadruped walk on land is the proper function of the tetrapodic skeleton structure in amphibians, reptiles and mammals (cf. Ridley 1993, 329f), and in the evolutionary subtree generating these classes. But it is not the proper function of the skeleton structure in their water-living ancestors, the lobe-finned fishes (Sarcopterygians), although these latter ones already possessed the tetrapodic skeleton structure and occassionally walked on the bottom of lakes with their fins (in this way, one assumes, vertebrates gradually invaded dry land). Since the tetrapodic skeleton was not selected *in* lobe-finned fishes for walking, but for swimming, walking was not a proper function but a mere side effect of *their* tetrapodic skeleton, although it *became* a proper function in their successor species, the amphibians. One also calls the lobe-finned fishes' skeleton a *preadaptation* for their land-living successor species. To take another example, the S-shape of the human spinal is unsuited for upright gait (Sober 1993, 39) – it is herited from humans' quadruped ancestors. Therefore, in homo erectus the S-shaped spinal is not a fundamental but a derived prototypically trait, although within the entire class (or evolutionary subtree) of mammals, this trait is fundamentally prototypical (which allows for exceptional subclasses such as homo erectus). Note that if we were not to restrict the selection history to *S-members* but were to refer to all *S-ancestors*, then we would have to say that the S-shape is still a proper functional trait of the human spinal, and swimming is still a proper function of the tetrapodic limb structure of mammals.

I will now show how in the light of our two conceptual conditions – strongly hertitable traits and overwhelmingly positive selection – two major types of objections to the statistical consequence thesis can be resolved.

Objection type 1: That F is a proper function of an item X of members of species S does not imply that the function F is frequently performed - it may be performed very seldom. In this sense, Millikan has argued that only a small minority of sperm perform their function of fertilizing an egg, or that only a small minority of babies (of most species) stay alive (1984, 4f, 34; 1989a, 62ff). More generally, many devices (organs) perform their functions not frequently or on average but just "often enough" (Millikan 1989b, 285, 288). Laurier (1994, 29-31) elaborates this argument, assuming a scenario where an organ of a species S performs a defence function F only in the vicinity of a predator which lives in a small part of S's habitat, so that only some S-members actually perform F – still F confers a selective advantage to S.

Although the argument is correct, it does not refute the statistical consequence thesis. Indeed, the actual performance frequencies of a functional behaviour B will often be low. But when this is the case, B cannot count as a prototypical behaviour, because one of our two conceptual conditions will be violated: either B is not strongly heritable, or B's selection is ambiguous, or both. Yet there is always something prototypical in proper functions, and this is the *capacity* to perform the functional behaviour under given circumstances C. Proper functional capacities will be possessed by almost all members of the given species, although their performance frequency may be low. Take Laurier's scenario: the question how often an S-member performs the defence function depends on the accidental circumstance of how often the predator comes into its way – this frequency is clearly not strongly heritable and, thus, not a prototypical trait of S-members. But the capacity to perform this defence function if confronted with the predator is a prototypical trait, which is possessed by almost all

S-members, as a result of their genetic constitution.

Thus I argue that proper functions should be understood as functional capacities (or dispositions), i.e. as behavioural regularities of the following form: under certain *circumstances* C a certain behaviour B is performed. B stands for the *performance* of the function F, and the circumstances C *trigger* B. These circumstances include Millikan's "Normal conditions" (1984, 33f) – *those* environmental factors with respect to which the function F is an *adaptation* (cf. Sober 1994, 84) – but they need not be frequent: even functions which are performed only *once* in an individual's lifetime can be essential for survival: e.g., breaking through the shell of the egg.

If we understand the normic laws of evolutionary systems as speaking about the *functional capacities* which have been selection in Evolution, all type 1 objections disappear: almost all sperm are able to fertilize the egg, almost all babies have the capacity to grow up, and the same applies to Millikan's example of the mouse-catching function of the claws and teeth of house-cats (1989a, 36). Of course, additional subtleties may be involved. If certain items are produced in huge numbers in order to perform a *collective* function, as in the sperm example, then – as Laurier (1996, 30f) has pointed out – we should consider the collective function to be the direct function, and the functions of the individual items to be derived functions in Millikan's sense (1984, ch. 2). For example, it is the primary function of the male genital organs to fertilize a female egg with help of spermatozoa produced in huge numbers. This does not imply that the derived fertilization function of the individual spermatozoa must be realized with equally high frequency.

An ambiguity of the reference class is involved in the *turtle-objection*: although it is a prototypical property of turtles to live to a very old age, almost all of them die within the first three weeks after birth. One may assume that the normic conditional "turtles live to a very old age" refers implicitly to adult turtles, not to turtle-newborns – in the same way as "lions have manes" implicitly refers to male lions only (cf. Pelletier/Elio 1997, 176). In this understanding, actual long life is prototypically as well as statistically normal for turtles. Alternatively, one may understands this conditional as also applying to turtle-newborns. Then this conditional can only be a true normic law if we interpret it as speaking about the *capacity* of turtles to live to a very old age, under the *circumstance* that they survive the first three weeks. For the property of a turtle-newborn to belong to the small minority which was by *accident* not caught by a predator is *not* a heritable trait, and hence not prototypical. A possible objection might be that surviving the first three weeks must be a proper function, because *reproduction* is a proper function, and turtle-newborns cannot reproduce if they don't survive the first there weeks. But the same argument applies to reproduction: of course, the capacity to reproduce is a proper function. But whether a turtle-newborn belongs to the lucky minority which will get reproduced is not a heritable trait and, thus, not prototypical.

Although the circumstances which trigger a proper function F need not occur frequently, they must occur at least *sometimes*, and with some regularity, in order to expose F to positive selection. Otherwise, F will not be systematically selected, with the result that the frequency of the underlying genotype remains low, or exhibits random fluctuation, in which cases F cannot count as a prototypical function. Moreover, the more rarely a function is performed, the higher its survival-enhancing nature must be, in order to become selected against 'random pressure'. Take again the example of Laurier: if the predator occurs with some regularity and with fatal consequences – for example, if every 5th generation it eliminates a random fraction of 30% of the species – then this is sufficient for long-term selection of the defence function F. Even if non-F-members of S may flourish some generations, their ancestors will be wiped out sooner or later. But if our predator enters S's habitat only every 1000 generations or so, F has almost no chance to get systematically selected and thus to become prototypically normal among S-members. Thus, the evolution-theoretic analysis implies that if F is a prototypical function of S-members, then it is also prototypically normal that the F-triggering circumstances occur at least sometimes and with some regularity in the average environment of species S.

To avoid misunderstanding: my claim is that the fundamental normic laws of evolutionary systems are *typically* laws about functional capacities. This claim does *not imply* that normic laws about function performances are never true – sometimes (but not typically) they are. For example, it is not only prototypically normal for vertebrates to be able to breathe, it is also prototypically normal for them to actuall breathe, because if they did not constantly breathe they would die. On the other hand, it would be misleading to say that it is prototypically normal for birds to be flying in the air, because there are many bird species the members of which spend most of their life-time on the ground in search for food and using their flying capacity only for the purpose of fleeing or migration.

This example shows us how our second conceptual condition may get violated by behavioural performances. For the evolutionary performance of most bird species, flying as well as non-flying activities are relevant. If *some* selection in favour of a behaviour B would be sufficient to count B as prototypical, the result would be that both flying (Fx) and walking on the ground in search for food (Wx) were prototypical behaviour. I conjecture that Millikan's account (1984, 33, conditions 1-3) has this consequence. But Wx strictly implies $\neg$Fx. As a result, both flying and non-flying would be prototypically normal ($Sx \Rightarrow Fx \wedge \neg Fx$), which illustrates the above-mentioned conceptual incoherence. But observe that normic laws concerning function performances are semantically ambiguous. In the above example we have taken (i) "birds normally fly" to mean (ii) "bird are normally in the state of flying", and in this meaning the prototypical normality claim is false. But one may also understand (i) as meaning (iii) "birds normally fly at least sometimes" (etc.). In this meaning, the prototypicality claim is true, and there is no incoherence between "normal birds sometimes fly" ($Sx \Rightarrow \exists tFxt$) and "normal birds sometimes do not fly" ($Sx \Rightarrow \exists t\neg Fxt$).

Let us turn to *objection type 2*. It does not concern function performances but functional capacities. Common to all variants of this objection is the following point. *It may always be the case in Evolution that, due to an environmental catastrophe or an epidemic disease, an item X of members of a species S becomes largely dysfunctional and loses its capacity to perform the function F for which it was selected. Then,* *although F remains to be the (proper) function of the disabled item X, only a minority of S-members can perform F.*[13] For example, if all of a sudden the wings of most (flying) birds lose their flying ability, then flying would still be the proper function of their wings, although almost no bird could perform this function any more.

Recall that it is an *intended* effect of historical selection accounts that organs may become disabled. In the case of catastrophes, this happens collectively. It follows that type 2 objections are correct in the following *restricted* sense: it might be the case in Evolution that at *some* times and for some species, the connection between prototypical and statistical normality is interrupted. What I will show now, however, is that – for *analytic* reasons – these cases cannot become statistically dominant in Evolution, but must remain rare exceptions. This gives me the opportunity to sharpen my statistical consequence thesis: the implication relation between prototypical and statistical normality is itself not strict, but normic, at least in the *statistical* sense of 'normic'.

Selection in favour of a reprotype R is not only needed to drive R's relative frequency high, but also to *keep* it high, because there is always a non-zero probability of dysfunctional mutations which must be continuously eliminated in order to keep R's frequency high (cf. Millikan's distinction between *building* and *maintenance* selection; 1989a, 46f). Therefore, it is sufficient for selection against an item X that X's function F *ceases to increase fitness*. So, if by a catastrophe or epidemic disease at some time in Evolution, an item X of species S loses the capacity to perform its proper function F, two cases are possible.

Case (1): Species S dies out soon after this time. Then the connection between prototypical and statistical normality was interrupted only for a minority part of S's history – *except* when species S had only existed for such a *short* period of time that the function F of item X, although there was ongoing selection in favour it, had not yet become the statistical majority among S-members. However, this situation must be extremely rare in Evolution. Of course, a scenario where almost all species, in-

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[13] See Neander (1991, 182) and Wachbroit (1994, 580). Cf. also Millikan (1984, 29) and Laurier (1996, 47, fn. 4).

cluding the selectively advantageous ones, get extinguished by a catastrophe after only a few generations, is not *physically* impossible. But such a scenario would make Evolution *astronomically* improbable, because then, with almost-1 probability, every lineage would become extinct after only a few generations. So assuming that Evolution has *not* been astronomically improbable, catastrophes causing selectively advantageous species to become extinct must have been relatively rare (cf. also Mills and Beatty 1979, 270, 278).

Case (2): Species S survives. Then we have two subcases. *Subcase (2.1):* Function F of item X is regained in S-successors. Then the period where most S-members had lost their proper function F was exceptionally short. *Subcase (2.2):* Function F is not regained in S-successors. Then selection will start to work *against* X's reprotype in S, because it has ceased to increase fitness – dysfunctional X-variants will increase in frequency and soon the majority of X-successors in the S-successors will have become vestigial traits which *no longer have* the prototypical function F. In this way, a new or modified species S* (with vestigial or exaptated trait X*) will have emerged.[14]

We are now ready to present our final definition of fundamental and derived prototypicality.

(PN) For S a class of evolutionary systems and T a trait of S-members:
(i) T is a prototypical trait of S-members at time t iff T is *produced* by a reprotype R and from T's first appearance in the S-history until time t, there was *overwhelming* selection in *favour* of R.
(ii) T is (ii.1) a *fundamental*, or (ii.2) a *derived*, prototypical trait of S-members, if the selection mentioned in (i) was overwhelmingly (ii.1) caused, or (ii.2) *not* caused, by

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[14] This argument can also handle Cummins' general objection (1976, 755f) against the evolution-theoretic account to functions. In his scenario, the function of flying is not *lost* by the birds, but it *ceases* to be *survival-enhancing*. Cummins claims that in this scenario it would still be the function of wings to fly, although there is no more selection in favour of flying. But selection against flying will soon have produced a majority of flying-disabled birds. So, functions without survival value *must remain exceptional* in Evolution.

R's producing of T.

Our previous *co-definitions* remain intact, and we add the following one: T is a fundamental prototypical trait of S-members *simpliciter* iff T is a fundamental prototypical trait of S-members at the *latest* time-point of S's existence (analogously with derived traits). Everything is now in place for my analytical argument, which applies to fundamental as well as to derived prototypical traits:

Conclusion 1: If T is a prototypical trait of S-members at time t, then there exists a reprotype R which produces T, and from T's first appearance until t there was overwhelming selection in favour of R. [From definition PN.]

Conclusion 2: If T is a prototypical trait of S-members at t and t is not a time soon after T's first appearance, *then* for most time points from T's first appearance until t, (i) T was a prototypical trait of S-members and (ii) most S-members possessed reprotype R and, therefore, trait T. [(i) from conclusion 1 and definition PN; (ii) from conclusion 1, definitions of "production", "selection", and probability theory.]

Premise 1: Most classes of evolutionary systems do not become extinct soon after aquiring a selectively advantageous trait.

Premise 1 and conclusion 2 imply, by probability theory, the final version of the statistical consequence thesis (SC):

(SC) For most classes of evolutionary systems S and times t of their existence it holds that if T is a (fundamental or derived) prototypical trait of S-members at time t, then most S-members will possess trait T at time t.

Put into a slogan: *prototypical normality 'normally' implies statistical normality*. We emphasize that the meta-normality of this slogan (the "normally" in quotation marks) is understood in a purely statistical sense – in the same way as the *meta-most* (the first "most") of the thesis (SC) is statistical. Although this meta-normic implication can hardly be taken as prototypical implication in the evolution-theoretic sense, because (presumably) there is no 'Evolution of Evolutions' (but cf. Dawkins 1989a) , it is far from being an accidental generalization: it is an analytic consequence of our Premise 1, and therefore itself 'almost' analytically true.

The formal structure behind (SC)'s "meta-most" is an *iterative* conditional probability assertion. These are possible as soon as predicates have more than one variable. For instance, we may say that most of the U.S. cities are such that most of their inhabitants have a car. Formally $Ux \Rightarrow_x (Iyx \Rightarrow_y Cy)$; the indices indicate which variables get bound by the conditional most-quantifier. In probabilistic terms, $p_x(p_y(Cy/Iyx) \geq r_1) / Ux) \geq r_2$. Thesis (SC) has a similar structure, namely: $PROTO(S,T,[t^*]) \Rightarrow_{S,[t^*]} (Sxt\ [\wedge\ t \leq t^*] \Rightarrow_{x,t} Txt)$ (with $PROTO(S,T,[t^*])$ the appropriate 2nd order sentence).

We can finally demonstrate how our analysis resolves further possible objections. For that purpose we introduce a further refinement of terminology, because the 'fundamental-derived' distinction of (PN) is not exhaustive: if R's reproductive advantage was sometimes caused by T and sometimes by T', then T and T' are called *semi-fundamental* traits.

Objection 3: How does definition (PN) allow for cases of exaptation, where a species, or an organ, acquires a new prototypical function and/or loses an old one? *Reply:* Definition (PN) admits these cases as follows. Assume (i) species S exists since time t_1 until present time t*, (ii) since time t_1 organ O of species S has had proper function F_1, and (iii) at time t_2, where $t_1 < t_2 < t^*$ and neither $[t_1,t_2]$ nor $[t_2,t^*]$ are exceptionally short intervals, organ O acquires the new function F_2 for which it is selected after t_2. We have the following subcases. (3.1): The effect F_2 did not appear before t_2 in S. Then, since there was overwhelming selection of O because of F_2 *after the time t_2 when F_2 first appeared*, definition (PN) implies that F_2 is a proper function of organ O in S-members. (3.2): F_2 was already a side-effect of organ O since time t_1. Then definition (PN) and co-definitions imply that F_2 is prototypical in the entire S-history; it is derivedly prototypical in S until time t_2, it is semi-fundamentally prototypical in *S* after t_2, and it is fundamentally prototypical in the subspecies S_2 existing between t_2 and t*. The case for function F_1 splits up into three subcases. (3.3): If after time t_2 function F_1 continues to be *one* cause for O's selective advantage, then after t_2

both F_1 and F_2 are proper functions of O in S. (3.4): If after time t_2, F_1 ceases to be a cause of O's selective advantage, but is still produced by O's reprotype as a side-effect, then at time t_2 F_1 changes its nature from a fundamental into a *derived* prototypical trait in S. (3.5): If O loses its capacity to perform F_1 after t_2, then F_1 is no longer prototypical after t_2 in S, although it *was* prototypical before t_2 in S.

Objection 4: Objection type 1 was about proper functions which are rarely performed. But the same objection may be repeated for the frequency of functional capacities. Just assume a genotype G which produces a fitness-increasing capacity F, but only in a small percentage of cases, for whatever reasons. A counterexample of this sort is constructed by Laurier (1996, 48, fn. 4). *Reply:* Then this capacity F is not strongly heritable and, thus, cannot count as prototypical. But there is still something strongly heritable and prototypical in this situation, namely a *probabilistic trait*: to possess capacity F with such-and-such probability.[15]

Objection 5: Many genes or genotypes have different phenotypic effects with different fitness values in different environments. As a result, neither the genes nor their phenotypic effects need to become almost-universal. *Reply:* Two subcases have to be distinguished. (5.1) The same genotype leads to *different traits* T_i in different environments E_i. Then the traits T_i are not strongly heritable and thus not prototypical. What is strongly heritable, then, are the conditional traits "to possess T_i in environment E_i". They are the proper candidates for being or becoming prototypical. (5.2) The same (strongly heritable) trait T, with underlying genotype G, has *different fitness values* in different environments E_i. This is the more interesting case. I agree that situations of this sort occur frequently. But they do not generally prevent almost-fixation of a trait and its underlying genotype – only in *certain cases*. The relevant subcases are treated as follows.

(5.2.1)*:* Assume genotypes G_1 and G_2 produce traits T_1 and T_2, respectively, where T_1 is advantageous in environment E_1 and disadvantageous in E_2, and vice versa with

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[15] Probabilistic traits may be ascribed to S-samples, or – assuming single case probabilities – to S-individuals. That they are statistically frequent in S is an iterative probability assertion.

T_2. As an example, the G_i are genotypes of an antelope species, T_1 are longer legs which are faster but less suited for climbing rocks, T_2 are shorter legs which are slower but better in climbing rocks, E_1 is a flat savannah environment with predators, and E_2 a rocky mountain environment. If the antelopes live in both environments, then selection of T_i will work according to *average* fitness, determined as the weighted average of the reproduction rates of trait T_i in both environments (with the average population frequencies in these environments as the weights; cf. Sober 1993, 79; Boyd and Richerson 1985, 112). Assume that in the long run, the average fitness of long legs is significantly higher than that of short legs. Then the condition of overwhelming selection in favour of long legs is satisfied, long legs are a prototypical trait of this antelope species, and their frequency will go to almost-fixation.

(5.2.2): If, on the other hand, the difference between the average reproduction rates of both traits is not high enough to beat the effect of random influences (or that of migrations, see below), the result will be equilibrium frequencies neither close to 0 nor close to 1 – a so-called *polymorphism*. Since neither of the traits are overwhelmingly selected, neither of them are prototypical and neither of them become almost-universal in the species (in accordance with the statistical consequence thesis).

So far we have investigated prototypicality for the *entire species*. It is a *different* thing to apply this concept to *populations* of a species. Usually, populations are determined *geographically*: they are that fraction of a species which lives in a certain region or environment. To determine the reproduction rates of the E_1- and the E_2-*population* of our antelope species – we call them P_1 and P_2 – one must additionally take into account the *migration rates* between them. Three cases are possible:

(5.2.3): Assume both migration rates, from P_1 to P_2 and vice versa, are high. Then migration will unify the population frequencies (Ridley 1993, 119). Selection will work according to average fitness in the same way for both populations, so that a trait either becomes prototypical in both populations, or in neither of them.

(5.2.4): If both migration rates are so low that the populations diverge from each other, then longer legs will become prototypical in the savannah population, and shorter legs will become prototypical in the rocky mountain population. Provided that neither of the two populations are exceptionally small, neither of the two traits can count as prototypical for the entire antelope species. This case is also called *multiple niche polymorphism* (Ridley 1993, 115). Note that this situation often leads to species *splitting,* in which case prototypicality of leg length (long vs. short) is restrored in the two successor species, while it is lost in the entire antelope family.

(5.2.5): Assume that the migration rates are 'moderately' high, so that they neither extinguish, nor are extinguished by, the differential selection effects. Then in each population, a constant proportion of the disadvantageous trait will be maintained against selection pressure because of continuous migration from the other population (cf. Ridley 1993, 121). Here, both populations maintain a polymorphism, which means that in neither of the two populations will one trait become prototypical and go to almost-fixation.

(5.3): Another cause of polymorphism is *heterozygotic advantage* where the heterozygotic genotype Aa is favoured over its homozygotic competitors AA and aa, as in the famous case of sickle-cell anaemia. Meiotic random splitting of heterozygotic adult genotypes will prevent homozygotic offspring from going to almost-extinction (Sober 1993, 125f; Ridley 1993, 110). None of the three diploid genotypes can count as prototypical in this case.

(5.4): A final case which causes polymorphism are traits whose fitness values are *negatively frequency-dependent:* the higher the trait's frequency, the lower its reproduction rate becomes. In nice cases, this situation leads to a stable equilibrium of two traits – a famous example is Fisher's explanation of the balanced sex ratio (Ridley 1993, 294). Again, in such a case neither of the traits can count as prototypical: it is neither prototypical for humans to be male nor to be female; but what is prototypical for sexually reproducing species is the balanced sex ratio. Less nice situations may lead to periodic or even chaotic fluctuations which endanger the survival of the entire species (cf. Sober's pollution example 1993, 97; or May's chaotic host-pathogen systems 1987, 38ff). At the cultural level, periodically fluctuating *fashions* are typical cases of negatively frequent dependent traits: what was fashionable when it was rare becomes boring when it is frequently imitated. Particular trends of fashion are not prototypical for the underlying culture – only what remains invariant throughout them.

5. Evolution-theoretic Normality versus (Ethical) Normativity. One main reason why Neander refutes the connection between prototypical and statistical normality is her emphasis of the *normative* character of 'proper function' (1991, 180f) which allows one to distinguish between 'good' and 'bad' functioning. Millikan calls 'prototypical functions' - more carefully - a *quasi-normative* concept (1984, 5). This point deserves further attention. Besides statistical and prototypical normality, there exists a *third* notion of normality in the *normative-ethical* sense. Does Neander really mean ethical normativity, when she says that "a trait *is supposed* to do whatever it was selected to do by natural selection" (1991, 183)?

If *yes*, then such a connection would be very problematic, on logical as well as ideological grounds. Possession of a prototypical function is a complex dispositional and historical - but nevertheless a clearly *descriptive* property, because it is defined in terms of the *factual* Evolution of an organism.[16] So, an inference from prototypical to ethical normality is an *is-ought-inference* – which is a logical *fallacy*, according to Hume's famous is-ought thesis and Moore's related naturalistic fallacy (cf. also Laurier 1996, 46f). In the comprehensive logical investigation of Schurz (1997a), Hume's is-ought thesis is confirmed from a modern viewpoint: it is proved that in all systems of multimodal logics, is-ought inferences (in a generalized sense including mixed conclusions) are *only* possible if one already *presupposes* certain *is-ought bridge principles* as analytically valid. But to justify is-ought bridge-principles as analytically valid is hardly possible (Schurz 1997a, ch. 11). In our context, the inference

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[16] Neader's identification of "descriptive" with "actual", i.e. "non-historical", on p. 181 does not fit the standard understanding of this term (cf Hare 1952; Schurz 1997a, ch. 1) – historical properties referring to past-and-presence are clearly descriptive.

from prototypical to ethical normality presupposes the following *e*volution-theoretic *b*ridge-*p*rinciple:

(EBP): Whatever helps to increase the fitness (i.e., the reproduction rate) of a species or class of evolutionary systems is ethically good.

The principle "EBP" can hardly be called 'analytically valid' – it is not even universally true in the contingent sense, for the following reasons. Because of the limited resources of our planet, the increase of one species' fitness is at the expense of another species' fitness. So it is impossible to simultaneously maximize the population sizes of all species. On the other hand, it is presumably ethically wrong that the unrestrained growth of one species leads to the extinction of almost all other species – even if this one species is the human species (which is the point of *environmental* ethics). The same argument applies to cultural Evolution: for example, it is impossible to simultaneously maximize the geographical expansion of rural areas and grassland. In the ideological respect, principle EBP would be a new version of Herbert Spencer's *social Darwinism* with all its political dangers (cf. Schurz 1997a, 269f).

I assume that Neander did not want to subscribe to a principle of this sort. If this is correct, then it would be interesting to hear *what else* could be meant by "normative". One might argue that there are other kinds of norms, e.g., norms of *rationality,* which are more tightly connected with prototypical normality. But we are faced with the same problem: it is an open question whether the prototypically normal traits of our intuitive cognition are also rational in the normative sense (cf. Kahneman et al. 1982 about the various systematic errors in human cognition). In conclusion, there exists *no direct relation* between prototypical and ethical normality.

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