Lineages of Works: Conceptual Issues

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Abstract

Scientific works, like species, form lineages. New studies build on previous work in a variety of ways. The latter provide a basis in data and theory for new work; guide new work by suggesting ideas or methods; and act as targets of criticism and foils in controversy. In this essay, the first of several intended to constitute complete documentation of a formal account of descent among information resources, I address conceptual issues, and propose an informal model of descent.

Contents

1 Introduction

Scientists read one another's work, and use one another's results, so that one work, say, a journal publication, is a *descendant* of an earlier *ancestral* work, say, another journal publication, the results of which are used as a basis for experiment or theory in the descendant. These works form what I call a *lineage of works*. Eugene Garfield seems to have had something like this in mind when he began work on citation indexing [1]. Identifying a lineage of works amounts to identifying the development of opinion in the scientific community about the things the works in the lineage are about. The transmission of information from a parent work to a descendant is never passive. Just as a lineage of genes is transformed by mutation, natural selection, random drift, and other evolutionary processes, the information in an earlier work is transformed by creators of its descendants. Nonetheless, unlike evolutionary processes, which are not shaped by a forward-looking designer, successive works in a lineage are created with a particular purpose in mind—for instance, to deny, affirm, explore, or understand the objects and methods of study of the parent. Semantic markup, together with reading by machines and people, can be used to discover the content—and accordingly, the reality it describes—which links a work to its descendants.

This essay is the first in what I intend to be a series of essays which together form complete documentation of descent among works, including uses of its models in formal ontology. I begin, in this essay, with an account of *direct descent*, which is the relationship between a work and its immediate ancestors. This is the fundamental relationship in lineages of works. Descent across further "generations" of works is best modeled as direct ancestor-descendant relationships in which each descendant work is an ancestor to a work which is itself an ancestor to yet another work, and so on, so that a multi-generational lineage can be reconstructed as a sequence of pairs of works connected by direct descent. The development of a model of such multi-generational lineages will be the subject of the second work in this series. The third installment of the series of essays will model direct and multi-generational descent using resources from the Basic Formal Ontology and other related ontologies such as the Information Artifact Ontology. The fourth and final installment will provide methods and criteria for identifying and categorizing works in terms of their relationships of descent.

This study and its successors sketched above is aimed at modeling lineages of *scientific* works. The influence of one *literary* work on another differs in important ways from descent among works in the scientific literature, and no attempt is made here to account for the former.

The model of descent among works I develop here is intended for use in organizing texts and bibliographical references incorporated in the Darwin Manuscripts Project. This includes digital copies of Darwin's manuscripts, and, with time, will include works on evolution drawn from PubMed and OCLC's WorldCat catalog. Together with the Evolution Ontology,[1] the model of lineages I develop here will provide access to the history of debate concerning many issues concerning evolutionary biology whose origin can be traced back to Darwin's own work and which have remained controversial since—and in some cases, before—the mid-19th century. Having made this point, further discussion of the motivations for developing a formal account of lineages of works will be put off for documentation of particular projects which use the models developed in this series.

[1] The Evolution Ontology is presently under development; see appropriate links at `http://shiftingbalance.org`.

2 What is a lineage of works?

Consider two works about Darwin's finches: David Lack's *Darwin's Finches* [10] and Peter and Rosemary Grant's *How and Why Species Multiply: The Radiation of Darwin's Finches* [4]. The Grants clearly state that their work is intended to build on Lack's (see the appendix, below) and it is natural to say that the Grants' work is a descendant of Lack's. This implies the following. First, it implies that the Grants read and understood *Darwin's Finches*. Second, they formed some *cognitive attitude* toward what they read: they agreed or disagreed with Lack's interpretations; came to accept certain putative facts reported by Lack, or were unable to; had questions about what they read and understood; knew they failed to understand some parts of what they read; had their curiosity about certain points satisfied; and the like. Cognitive attitudes are unlike others because the former have to do with knowledge. The non-cognitive attitudes include, for instance, being amused or inspired by the work; being angry or thankful for it; or finding the work aesthetically pleasing or ugly. It seems clear that these non-cognitive attitudes do not establish the kind of link between "generations" of works which establish a lineage. Third, the Grants synthesized what they learned from reading Lack's works with their experience, knowledge from other sources, responses from colleagues, and so on, to create *How and Why Species Multiply*. In sum, for one work to descend from another, the creators of the "offspring" work must understand the "parent;" form some cognitive attitude toward its content; and act on that attitude when creating the descendant work.

3 Conceptual issues

The coherence and plausibility of any model of lineages of works requires the solution to two conceptual issues: one, about linguistic change (section 3.1), the other, a problem about directness of descent (section 3.2).

3.1 Cognitive continuity

The requirement that the creators of descendant works understand parent works poses a significant barrier to formulating a model of lineages of works: what establishes *cognitive continuity* between works created in different conceptual and linguistic contexts? The meaning of a term can change across time and user communities. One problem is that variant meanings, though closely related, may be incompatible with one another; another is that scientists looking back at past uses of a term or its uses in another community might be unaware of differences between variant meanings. Nonetheless, it is clear that, however attenuated the information "signal" between generations of works, scientists are strongly influenced by the works of their predecessors, even some in the remote past. If there are lineages of works, it ought to be possible to understand the nature of continuity across changes of context which occur across time spans between a work and its ancestors.

The most striking and forceful statement of problems about cognitive continuity in science appears in Thomas Kuhn's famous *Structure of Scientific Revolutions.* In this section, I explain the problem as Kuhn sees it, and sketch a solution to it, adapted from Ian Hacking [5, ch. 6]. The central idea is that, on what has come to be known as Kripke-Putnam semantics[2], the difficulties Kuhn believes to be responsible for breaks in cognitive continuity do not arise. I conclude with a necessary condition for direct descent among works which, when satisfied, assures cognitive continuity between ancestor and descendant.

Understanding across contexts: Incommensurability? Understanding is a kind of knowledge, and on traditional views, someone knows a proposition[3] P only if P is true. There are fallibilist theories about knowledge, of course; but even if some such theory is correct, there still remains the question about what, precisely, the creators of the descendant work learn from the ancestor. Perhaps the descendant's creators come to understand what the ancestor's creators believed about the world; or, perhaps they understand what would be true or what would have been true if the ancestor's creators were right about what they say in their work. These are reasonable conjectures; nonetheless, both are incorrect.[4]

In general, scientists do not actually make any attempt to put themselves in the cognitive position of the creators of past works, nor does there seem to be good reason to suppose that they are unconsciously doing so, or that their encounters with works in the history of their discipline can be reconstructed as if they were doing so. Recent literature provides examples of what are clearly unreflective whiggish readings of Darwin. In the speciation literature, there is an argument about whether Darwin was correct about the process of speciation, and in particular, about whether geographic isolation is required for it [11]. What is meant by "Darwin's theory of speciation" bears only a *prima facie* resemblance to the theory Darwin actually held. In today's context, "Darwin's theory of speciation" ought to be understood to mean something like "the sympatric theory of speciation," referring to theories of speciation developed since the 1960's according to which geographical isolation is not required for speciation. Though the theory of speciation advanced in *The Origin of Species* and in Darwin's notes does not require geographic isolation, it fails to account for how reproductive isolation develops in the absence of physical barriers between organisms in incipient species, and is not tenable today.[5]

Supposing that scientists did make such an attempt to place themselves in the cognitive context of their predecessors, there are good reasons to believe that they would generally not be able to succeed. Contexts can change significantly with the result that terms common to creators of both parent and descen-

[2] See Kripke's *Naming and Necessity* [7] and Putnam's "The Meaning of 'Meaning' " [14].

[3] The choice of propositions as the bearers of truth and falsity is not important to the point I want to make here. Any other such bearer may be substituted for propositions.

[4] The line of thought I pursue in the next two paragraphs is derived from and is similar to a line of thought pursued by Clark Glymour his discussion in the chapter entitled "Why I am not a Bayesian" in *Theory and Evidence* [3].

[5] Richard Dawkins, writing in for a broader audience, studiously avoids consideration of how earlier scientists conceived of biodiversity. He faults them for being unobservant and foolish; in light of beliefs they held, for which there were good reasons, their failure to notice what Darwin did is reasonable.

dant works can change in meaning, to the extent that the descendant's creators cannot place themselves in the cognitive position of the parent work's creators. They may have believed something the descendant's creators cannot accept due to later advances. For instance, the parent's creators may have held beliefs known by the descendant's creators to be incoherent or false. Changes in method can also be responsible for creating a significant gulf in meaning between the creators of parent and descendant works. For instance, statistical methods of measuring genetic variation and variation in phenotypes has changed drastically since the late 19th century; indeed, statistical tools such as variance and regression were invented in order to measure biological variability[6]. Interpreting data of scientists using these methods or those introduced still earlier would require understanding how their users conceived of the mechanisms of evolution and the significance of variation measured in unfamiliar ways, an understanding few scientists today possess. Most probably, the approach would be to use today's methods of measuring variation, reinterpreting the data, rather than accept conclusions by earlier scientists obtained with outdated quantitative methods.

Thomas Kuhn [8] sees this problem in uncompromising terms, so considering his view is useful for providing a clear target for counterargument. Kuhn's view is that scientific revolutions mark major changes in almost every area of scientific endeavor, including an individual scientist's immediate perception of the objects of his or her study. These changes are so fundamental, according to Kuhn, that scientists whose work is conducted in terms of the pre-revolutionary framework of science cannot understand the language used by scientists in the post-revolutionary framework—communication across the divide between post and pre-revolutionary contexts is impossible. They are *incommensurable.*

> Can Newtonian dynamics really be *derived* from relativistic dynamics? Imagine a set of statements $E_1, E_2, \ldots, E_n$ which together embody the laws of relativity theory .... From them ... is deducible a whole set of further statements including some that can be checked by observation. To prove the adequacy of Newtonian dynamics as a special case, we must add to the E_i's additional statements, like $(v/c)^2 << 1$, restricting the range of the parameters and variables. This enlarged set is then manipulated to yield a new set, $N_1, N_2, \ldots, N_M$ which is identical in form with Newton's Laws....
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> Yet the derivation is spurious, at least to this point. Though the N_1's are a special case of the laws of relativistic mechanics, they are not Newton's Laws. Or at least they are not unless those laws are reinterpreted to work in a way that would have been impossible until after Einstein's work .... [The] physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy; only at relatively low velocities may the two be measured in the same way, and even then they must not be conceived to be the same.) Unless we change the definitions of the variables in the N_i's, the statements we have derived are not Newtonian. If we do change them, we cannot properly be said to have *derived* Newton's laws .... Our argument has, of course, explained why Newton's Laws ever seemed to work.
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> ...
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> Let us, therefore, now take it for granted that the differences between successive paradigms are both necessary and irreconcilable .... The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before. [8, 101–3]

Clearly, if the meaning of terms used by scientists are incommensurable across time, lineages of works cannot extend beyond the temporal boundaries of the context on which their meaning depends.

[6]Provine [12] and Gayon [2] are useful sources on this topic.

Kuhn's descriptivism The problem is that Kuhn's understanding of natural kind terms is mistaken. The view of natural kind terms Kuhn adopts, implicitly, is *descriptivist*.

Descriptivism Natural kind term κ is used correctly to refer to an object O of kind K if and only if O has $i = 1 \ldots n$ properties P_i.

Take "water," for example. According to this analysis, someone uses "water" to refer to a sample of water if and only if the stuff to which he or she is attempting to refer consists of H_2O. In this case, the stuff must have P_1, having two hydrogen molecules to every one oxygen molecule and P_2, bonds among those molecules as are described by the currently accepted chemical theory. Accordingly, someone using "water" fails to refer in cases in which the stuff in question fails to have P_1 or P_2.[7]

The problem with this analysis is that it does not do a good job of accounting for the fact that, even if there does not exist anything with properties P_i, a natural kind term κ can still be used to refer to a sample of natural kind K. Indeed, doing so is integral to conceptual change in science. Terms are neither discarded, nor totally superseded; they are continuous with their use by earlier scientists, though their meaning shifts. Recall Kuhn's example: Newtonian and Einsteinian uses of "matter."

Definition 1 (Newtonian-descriptivist) *"Matter" is used correctly to refer to a sample s of matter if and only if s is conserved, etc., etc., etc.*

Definition 2 (Einsteinian-descriptivist) *"Matter" is used correctly to refer to a sample s of matter if and only if s is convertible with energy, etc., etc., etc.*

These definitions are mutually exclusive, that is, if the Newtonian definition is correct, the Einsteinian definition is not; Einstein's theory being better confirmed than Newton's, it is correct at present to reject the Newtonian definition, and accept the Einsteinian. From this, it would seem to follow that the Newtonian definition was never correct, because it ascribed a property to matter which it does not possess, viz., being conserved; matter does not have that property. Kuhn's extreme view is that, due to changes in conceptual framework which occurred when physicists rejected the Newtonian view of the physical world and accepted the Einsteinian, there is no sense in which a Newtonian's use of "matter" can be translated or exchanged for an Einsteinian's: they are incommensurable. This is fatal to the endeavor of modeling lineages of works. Suppose that the meanings of terms represented by tokens (sounds, printed patterns, etc.) of the same type are so unstable that as contexts change, the terms' reference changes or the term becomes meaningless. Then trying to understand how one work's influence extends across contexts is bound to fail. It would be possible to mark out periods in time during which the use of a term remains stable, and to establish a rough temporal bound at which one stable use is exchanged for another. Works on either side of this bound cannot form a lineage because terms with identical morphological characteristics appearing in both are incommensurable.

There is an alternative understanding of reference and the meaning of natural kind terms from which it does not follow that changes of context render terms used in both incommensurable with one another. Consider the following intuitive picture of conceptual change. Both Newtonians and Einsteinians successfully refer to matter when they use "matter" in Newtonian and Einsteinian senses, respectively. Nonetheless, Newtonians are *wrong* about the nature of matter, while Einsteinians are *right* about it. To account for this intuitive picture, according to which "matter" is stable across contexts, the following insight is needed: Einsteinian uses of "matter" pick out samples of stuff which are *structurally or physically the same as* the samples of stuff picked out by Newtonian uses of "matter." The conception most scientists have about the nature of that kind of stuff changes across contexts, but the use of "matter" still succeeds in referring to stuff of just that kind. This

[7] There are variants of this theory according to which correctly referring to the sample of kind K does not require that the sample have all P_i's, but rather requires it to have some subset of them, most notably those elaborated by John Searle. These theories prolong the life of descriptivism about meaning, but do not save it.

is so even if successive definitions of "matter" (or any other natural kind term) are incompatible with one another. According to the descriptivist view, someone successfully refers only if he or she identifies a sample of stuff having certain characteristics, specified by the language; if the sample fails to have those characteristics, the attempted act of reference fails. On the alternative view I am describing, successful reference does not depend upon the sample of stuff having certain characteristics fixed by the language: sameness in reference obtains across instances of a term, regardless of contexts, if the samples of stuff are structurally similar to one another. These structural characteristics are not fixed by the language, and indeed, need not even be known to users of the term. They depend solely on the nature of the stuff referred to.

In practice, the meaning of kind terms is established in something like the following way. A sample of the stuff in question is studied by scientists, who are attempting to learn something about its properties. Informally or perhaps formally, they "baptize" the sample of stuff with which they are working. There are two ways in which this "baptism" may occur. First, the term might be ostensively defined. Anatomy provides a good examples of this. Anatomical structures are identified by reference to a paradigm. "If something is like *that*, it is of the same kind K." Second, the term might be defined by what Kripke calls a "reference-fixing description." Subsequent uses of the kind term are relativized to the thing indicated by the "initial baptism:" users of the term correctly refer by using it only if they refer to a sample of the stuff which is like the stuff picked out in the initial baptism.[8]

The consequences of this analysis of the meaning of natural kind terms are illustrated by the following definition of "matter."

Definition 3 (Indexical definition) *"Matter" is used correctly to refer to a sample s of matter if and only if s is structurally the same as the sample of matter identified indexically or by a reference-fixing description by the people who first used the term.*

Presumably, "matter," or its Ancient Greek equivalent, was used by Thales, usually taken to be the first philosopher of metaphysics. He must have used something like "the stuff that everything is made of" or some similar expression to indicate the intended reference of the term for "matter." He advanced the charming—but certainly incorrect— theory that everything is ultimately composed of water [6, 80-100]. The issue of what matter really is, if there is such a thing, has been a central topic of physics and metaphysics since Thales; and although different people have had different (and incompatible) views on what it is, there is still a clear sense that the problem is to determine the nature of the stuff that everything is made of. Whatever that stuff turns out to be, it is still true that Thales and other metaphysical theorists, including physicists, were talking about that stuff, even though they had few or perhaps no correct beliefs about it. Similar accounts could be given for "gene," "species," "space," "time," "atom" or any other number of kind terms which have remained in use, some for centuries, surviving fundamental shifts in scientific theory and practice.

The Kripke-Putnam view of natural kind terms solves conceptual problems, but it also solves practical problems of categorizing information resources according to what things or kinds of things they are about. The indexical element of definition informs criteria for identifying when the meaning of a kind term changes. A kind term is introduced to the language, and may be enlarged to refer to cases not previously considered to be of the kind in question; similarly, they may be narrowed to exclude cases formerly considered to be of the kind in question, perhaps even ceasing to be used entirely as its extension is divided up among successor terms. These criteria and guidelines for their implementation will be introduced in later essays in this series.

Referential continuity A later work is a descendant of an earlier work only if the creators of the descendant work understand the earlier work; how

[8] The nature of the initial baptism and the continuity in meaning of a natural kind term raises interesting and important issues, the resolution of which is only tangential to project of modeling lineages of works. I do not believe that any of the difficulties raised by the initial baptism or other elements of the Kripkean account of meaning of kind terms I have described here are fatal to the view, though it does have its detractors.

does the analysis of reference provided in the previous section illuminate the nature of understanding across contexts for modeling lineages of works, and accordingly, form the central elements of a model of cognitive continuity across contexts? My suggestion is that the following *referential continuity condition* must be satisfied.

Referential continuity condition A work W_d directly descends from a work W_P only if both works are *about the same thing*.

This requires that the objects of study of the daughter work be the same kind of stuff studied by the creators of the parent work. The views elaborated above explain the way in which this guarantees continuity between parent descendant works across linguistic and cognitive contexts. The connection between scientists' beliefs about a given type of thing across contexts is anchored by the thing itself, which is independent of the beliefs of scientists (or anyone else) about it.

Cognitive continuity Referential continuity is not sufficient for cognitive continuity. If two works about the same thing, even if they are created in succession, the "offspring" work's creators having read the ancestral work, it need not be the case that the two works form a lineage. As explained above, it is also necessary that the descendant work's creators understand and adopt some cognitive attitude toward the content of the ancestral work. To account for this, the following *explanatory condition* is required.

Explanatory condition Let T refer to the thing which the parent and descendant are about. Then, a work W_d is a direct descendant of work W_p only if W_d's creators learn or infer from W_p some fact, statement, proposition, and more generally, have some cognitive attitude toward some content of W_P, which forms an essential part of a purported explanation advanced in W_d for some state obtaining of T or event of which T is a part.

To see how these two conditions work, consider the works of Lack and the Grants. First, there are important differences in the conceptual and linguistic backdrop against which the works were written. These include enormous changes in the way the genetic basis of variation and evolution are measured. For example, the Grants, in large degree due to their own work and the work of their students, know far more about the ecology of the Galápagos than did Lack. Mathematical modeling of evolution has changed. These changes result in subtle differences in meanings of the terms used by Lack and the Grants. Nonetheless, both Lack and the Grants work are about the same things, that is, about the Galápagos finches and their ecological setting. Because this is so, the referential continuity condition is satisfied. The explanatory condition is also satisfied. The Grants learned facts about the diversity of the Galápagos finches and Lack's interpretation of it; and the Grants formulated claims differing from Lack's in important respects and which appear in *How and Why Species Multiply* for the purpose of explaining phenomena having to do with adaptive radiation.

3.2 Directness

An adequate model of descent among works must be able to account for a distinction between direct descendants of a work, and more distant descendants. To see why this is, consider the following. The Grants, discussed above, point out that they were influenced by David Lack, presumably as a result of reading his works. The Grants' works are direct descendants of Lack's, because the Grants' knowledge of the content of Lack's work comes from those works themselves. Now, suppose that there is an otherwise obscure 19th century ornithologist whose work the Grants' have never read, but whose work is described by Lack in one of the works which the Grants did in fact read. Suppose, furthermore, that the Grants understood the obscure ornithologist's ideas, which were communicated by Lack, subsequently adopting some attitude toward those ideas which contributed to the explanation of speciation phenomena in birds; and suppose that, in virtue of being about the same thing as the Grants' object of study, viz., Galápagos finches, the referential continuity condition is satisfied. This meets the conditions sketched above, so it follows that the Grants' work descend from the obscure ornithologist's. On the one hand, this does seem to capture something essential about the relationship between Grants' work and that of the obscure ornithologist's. On the other hand, it seems clear that, because they read Lack's work but not that of the obscure ornithologist, the Grants' work "inherits" more from Lack than from the ornithologist. The best account of the lineal relationships among these works is something like the following: the Grants' works are descendants of Lack's, and Lack's is a descendant of the obscure ornithologist's, the content of whose work was reported by Lack. The Grants' works are direct descendants of Lack's works, and at least one "generation" displaced from the obscure ornithologist's.

I think there are two arguments which support the view that the Grants' works are direct descendants of Lack's, but not the obscure ornithologist. First, even supposing that Lack reported the obscure ornithologist's views accurately, they are nonetheless presented in a context created by Lack. Lack may have seen something new and different about the obscure ornithologist's work, for instance. Indeed, even if the Grant's had read the obscure ornithologist's work, it is eminently possible that they did not believe it to be in any way significant until they read what Lack had to say about it. In general, context plays such an important role in the communication of someone's ideas that descendant relationships hold first and foremost between a work and works of its readers, but secondarily between a work interpreted by another person and works by readers of the interpreter's work.

A second argument is pragmatic. If lineages of works are not reconstructed using relationships of direct descent, it will be difficult to identify any lineages of real value. Suppose I read an introductory textbook on metaphysics, which gives accurate accounts of the ideas of Plato, Aristotle, Augustine, and many contemporary metaphysical thinkers. If relationships of descent are not to be restricted to direct descent, a paper I write about metaphysics would be descended from works of Plato, Aristotle, Augustine, and so on. So would the textbook. This obscures the intellectual background which informs the ideas I express in the paper. Perhaps my understanding of Plato is derived in part from some formulations of his views in the introductory textbook; but the intellectual lineage passes through the latter most immediately, and it ought to be recognized as having greater influence than Plato himself.

There is probably no way to deduce, based only on the order in which creators of works read their respective ancestral works, whether a given work is directly descended from an ancestor. The problem stems from multiple inheritances from an immediate ancestor A as well as an ancestor of the ancestor A. For instance, consider a descendant work W_0, which is the direct descendant of W_1. Suppose as well that W_1 is a direct descendant of work W_2, and that W_0 is also. From these relationships of descent, it follows that W_0's creators read W_1 and W_2 after W_1's creators read W_2. Because such "cross-generation" inheritance can exist, it is not possible to describe relationships of direct descent among works in a complete way given only the order in which they were read. Since such a model is not possible, it must be understood as a primitive relationship which must be identified, in each case, by knowledge of the history of the works in question. For instance, the Grants learned what Lack thought about Darwin's finches as a result of their direct encounter with Lack's book, not a result of reading a summary or interpretation of Lack's work by someone else. This is known empirically, by the Grants' statements in the introduction to their book (see the Appendix, below).

4 A model of direct descent among works

Having clarified the conceptually problematic background to descent among works, direct descent among works may be described as follows.

Direct descent of scientific works Work W_d is a *direct descendant* of work W_p, about a thing T, if and only if conditions for *cognitive continuity* and *directness* are met, viz.,

Cognitive continuity W_d is *cognitively continuous* with W_p if and only if it meets the following cconditions.

Referential continuity condition Work W_d is about T, which W_p is about.

Explanatory condition W_d's creators learn or infer from W_p some fact, statement, proposition, and more generally, have some cognitive attitude toward content of W_P, which forms an essential part of a purported explanation advanced in W_d for some state obtaining of T or event of which T is a part.

Directness The creators of work W_d obtained knowledge of relevant content by reading or otherwise being caused by W_p itself, rather than from some other work $W_{p'}$ which reports the content of W_p.

The cognitive continuity condition incorporates the ideas outlined in the previous section. The referential continuity condition establishes the common reference of the descendant and parent works, across historical, disciplinary, and other changing contexts; the explanatory condition requires that the creators of a descendant work use what they learn in the parent work to formulate an explanation advanced in the former. The directness condition rules out cases in which the descendant's creators are influenced by the report of an earlier work on a still earlier work, as discussed above.

To clarify the way this model is supposed to work, consider the Lack-Grant case above. The Grants' work is an instance of W_d, and Lack's work is an instance of W_p. Lack's work is about the same thing that the Grants' is; and what the Grants learned from Lack's work is incorporated in explanations of phenomena having to do with the finches. Many other works are cognitively continuous with the Grants' work in an unrestricted sense, that is, they are like the work of the obscure ornithologist: the Grants learn about their content from Lack, but the Grants have not read the works themselves; and the content Lack communicates about these other works influences the Grants' thinking in accord with the conditions for cognitive continuity.

5 Concluding remarks

There are two chief results of this paper. First, conceptual issues are resolved—conceptual issues that, if not addressed, would leave open important questions about the plausibility of the project of modeling lineages of works. Second, an informal model of direct descent is proposed. This model provides the basis for developing a model of "multi-generational" lineages of works of the sort which connect today's literature on evolutionary biology with its history, history which remains an invaluable source of information about the issues which animate present-day controversy.

Appendix

David Lack and the Grants

Some of the scientific literature about Darwin's finches form a lineage of works. Although the lineage can probably be traced by to Darwin himself, there is a distinctive line of work on the finches which takes, as its starting point, the work of David Lack.

> [David] Lack went to the Galápagos Islands in the fall of 1938, studying the finches for three months. He spent April through August 1939 at the California Academy of Sciences and several weeks in New Jersey before returning to England in September 1939. There he analyzed his data on Darwin's finches and by June 1940 had completed the final draft of his monograph. The California Academy of Sciences, because of the war, was unable to publish the manuscript until May 1945 [9]. Before the monograph appeared, Lack had by 1944 completed his popular work *Darwin's Finches* [10], which appeared after a delay of three years [13, 406].

Peter and Rosemary Grant have created a direct lineal descendant of Lack's.

> This book is dedicated to the memory of David L. Lack, a pioneer of a field that has come to be known as Evolutionary Ecology. Sixty years ago he published his first book, entitled *Darwin's Finches*. Based on four months of field work in the Galápagos and many hours of measuring specimens in museums, it attempted to explain the ecology and evolution of an adaptive radiation made famous by Charles Darwin. It showed how it was possible to use observations of living animals to infer and interpret their evolutionary history. In doing so it established a new field of enquiry.
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> David Lack died in March of 1973. In a sense we feel that we are bearers of a torch he passed on, as our field research on the same birds began one month earlier. We had discussed Darwin's finches with him in Oxford two years before, but at that time we had no intention of studying them ourselves, and when we started we had no intention of continuing for 34 years. [4, xvii]

The Grants' also indicate some of the works from which Lack's descends, at least one of which is not acknowledged by Lack himself. "Like everyone else, David Lack was influenced by his predecessors, especially by his mentor Julian Huxley and by Robert Perkins. Perkins deserves special mention because he went to an archipelago ... to make sense of an adaptive radiation; in his case it was the honeycreepers of Hawaii" [4, xvii–xviii].

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