

Articles

The Place of Theory in Archival Practice

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RÉSUMÉ Dans cet essai, je discute de la place judicieuse de la théorie parmi les activités des archivistes. Ma position s'articule entre deux points de vue apparemment opposés : d'une part, la négation de l'importance de la théorie dans l'étude et la pratique des archivistes et, de l'autre, l'affirmation que la théorie est, et devrait être, au cœur des préoccupations des archivistes et selon laquelle l'archivistique peut être scientifique. Bien que je conçoive que la théorie archivistique *soit* importante, en soi inévitable, je critique la notion d'une science de l'archivistique. Les tenants de la science archivistique et ceux qui s'opposent à une place importante pour la théorie partagent (largement) une même conception positiviste de la science. Ainsi, je propose ma critique de la science archivistique par un recours à la philosophie de la science. Au cours des vingt dernières années, philosophes et historiens de la science ont discrédité la notion positiviste de science selon laquelle elle transcenderait les contextes historiques, sociaux, culturels et politiques. Un point de vue plus « nuancé » a remplacé cette conception positiviste de la science. Ce mouvement permet l'apparition de nouvelles formes de théories archivistiques. La dernière partie de l'article fait quelques suggestions quant à l'allure que peut prendre cette forme de théorie.

ABSTRACT In this essay I discuss the proper place of theory among the activities of archivists. I argue for a position between two apparent opposites: on the one hand, a position that denies that there is or should be any significant place for theory in the study and practice of archivists, and on the other, the claim that theory is, and ought to be, of central concern to archivists, and that archival theory can be scientific. While I agree that archival theory *is* important, indeed unavoidable, I criticize the notion of an archival *science*. The advocates of archival science and those who deny any significant place for theory share a (largely) positivist conception of science. I therefore advance my criticism of archival science through an excursus into the philosophy of science. In the last twenty to thirty years philosophers and historians of science have discredited the positivist idea of science, according to which science is beyond and above historical, social, cultural, and political contexts. A “softer” view of science has replaced the positivist notion of science. When the positivist conception of science is abandoned, new forms of archival theory emerge. The last section provides some suggestions about how this form of theory may look.

Introduction

Recent discussions about archival theory have marked out two opposed posi-

tions on the question of the place of theory in archival studies and in archival practice. One position denies that there is (or should be) any significant role for theory in the practice or study of archives. Archival work, in this view, is a practical matter, and best left as such. Archivists have no need for theory. The anti-theoretical view has been argued most strongly by John W. Roberts, but it is no doubt a view shared to varying degrees by many archivists.¹

Contrary to this, others hold that there is such a thing as archival theory and that it has, or can have, the character of a science: archival science. Luciana Duranti defines archival science as “the body of knowledge about the nature and characteristics of archives and archival work systematically organized into theory, methodology, and practice.”² According to Terry Eastwood, a major purpose of archival theory – and of the methodology and practice that may stem from this theory – is to “treat archives in consonance with their nature.”³ An important goal of archival theory is therefore to clarify the nature of archives and to determine what flows from the nature of archives.

Archival science, according to Duranti, has a *foundational* character for practical, theoretical, and historical endeavours relating to archives.⁴ This view assumes that archives have a distinct nature and that this nature can be found in varying degrees throughout the entire history of archives. Archives thus have a specific essence, and they have a history which shows certain universally valid features, features that can be found in all or almost all human cultures.⁵ The scientific approach is meant to articulate those universal principles that go beyond any particular historical, legal, or cultural context. The Dutch manual of 1898 can thus be considered the first attempt at a “scientific archival treatise” because, as Duranti asserts, it represents “the first real effort to articulate systematically the concepts and methods that find their validity in archival theoretical ideas *with internal logic and consistency, rather than in their historical, legal, or cultural context.*” To become properly scientific, “archival science had to seek its purpose or focus within its own horizon rather than outside, and operate as a self-referential system, fully autonomous from the influences of political, juridical, or cultural conceptions.” When this is achieved, universal validity can be obtained. The “universal applicability” of the Dutch manual, Duranti observes, “was clearly perceived by both contemporary and future archivists, as demonstrated by the translations that were made for more than sixty years following its first edition.”⁶ Three features of archival theory thus are critical in order for it to be considered scientific: universality, autonomy, and internal consistency and logic.

The purpose of this essay is to explore the possibilities of a middle ground between these two positions. I argue that theory is indispensable, but that it need not, indeed *cannot* rely on something entirely independent of a historical, social, and political context. I make no particular claim to the originality of this argument: similar aspirations characterize the work, for example, of Terry Cook and Richard Brown.

Since the notion of science plays an important part in discussions of archival theory, we need to clarify this term and its relationship generally to archival theory. Much of this essay is a demonstration that science (i.e., “hard science”) does not itself exhibit the methodological rigour popularly associated with it, and is not beyond social, political, cultural, and historical influences. Science is restrained by time and place, just as any other human activity. In order to bring some much needed clarification to this and other concepts (such as “theory” and “paradigm”), I draw extensively on discussions in the philosophy of science and in other disciplines.

If archival studies are to be taken seriously as a discipline with a theoretical or philosophical basis, they must offer something beyond solutions to problems of description, arrangement, preservation, and so on. Roy Schaeffer points out that archival theory has to widen its scope, and contribute to interpretations of social changes and the ways in which these changes affect communication and the recording of information. According to Schaeffer, “It is ... the role of graduate education to develop what may be termed ‘archival thinking,’ an elaboration of an intellectual *weltanschauung* that covers *all aspects of the record and society*.”⁷ Archival theory must address wider questions relating to the nature and purpose of recorded information in its various forms.

The conclusion that emerges from my argumentation is that there *is* a place for theory in archival studies. Theory cannot be avoided, but is intimately connected to archival practice. Theory need not, however, be understood as something that conforms to a particular model of *scientific* practice, a model that is, in any case, erroneous. Theories are developed within archival practice and must be understood as a product of this practice itself. They are developed as solutions to specific problem situations or as deliberations on particular practices. To understand theories we must ask what a theory is a theory *of* or *about*: that is, which features of a specific practice is the theory a reflection of and to which problems is it offered as a solution. Theories may naturally reach beyond the specific situations that gave rise to them, but the challenge for those wishing to fully comprehend them is nevertheless to disclose the practices of which the theory was originally a part and from which it derived its meaning. For those wishing to apply the theory in their professional work, their task is to bear this in mind.

A Place for Theory?

The starting point for my discussion of archival theory is the position articulated by John P. Roberts, that is, that archivists have no need for theory. If Roberts is right, we need proceed no further. Yet, while his arguments are insufficient to dismiss all attempts to formulate theoretical reflections about archives, some of Roberts’ concerns are serious – and raise the question

whether it is possible to develop an archival theory that addresses them. When Roberts rejects theory, he has in mind a particular form of theory: a theory that takes the natural sciences as a model, and assumes that the natural sciences are characterized by specific methodological procedures and a strict set of rules governing the activity. Fidelity to these methodological principles secures (in this view) the validity of scientific research.

Roberts argues that archivists need only have knowledge of procedures and technology, the ethics of the profession, the history relevant to their archives, and of their records.⁸ The historical knowledge of their subject matter is particularly important, and it is an archivist's ultimate *raison d'être*. According to Roberts, "Archival theory does two things that are profoundly threatening to clarity of thought: it overcomplicates that which is simple and it oversimplifies that which is complicated."⁹ Because of the practical nature of archival work, theoretical reflections are unnecessary, or may be outright harmful.

I will return to this part of Roberts' argumentation after briefly considering a second reason for the rejection of theory. This second reason, offered by Roberts, is that each archival institution is unique and has its own particular concerns. He argues that "common themes in archives," taking these as being the stuff of theories, "are impossible because of the endless variability of subject matter."¹⁰ Because of this uniqueness, the concerns or problems of an archival institution cannot be addressed by general theory, only by specific knowledge and experience.

The "uniqueness argument" fails to persuade: physicians, biologists, physicists, and mathematicians deal with unique phenomena, but are nevertheless able to explain these by application of general – or theoretical – knowledge. Biologists, for example, acknowledge that all living entities are, from one point of view, unique. In fact, variation is the basis for evolutionary change. Though unique, each individual nevertheless belongs to a species, and a pattern can be discerned behind the features and development of the different species: evolution. This general theoretical framework (evolutionary theory) also forms the background for explanation of physical and behavioural features of individuals and the groups to which they belong. Similarly with phenomena within the physical sciences, individual phenomena can be explained and predicted with considerable accuracy, though the disciplines that provide these explanations – notably physics and mathematics – are of a highly theoretical nature.

There is, then, no contradiction between the generality or abstractness of a branch of knowledge and its ability to deal with or be applicable to practical, specific, and unique situations. Consequently, though each individual archives may be unique, uniqueness is not a reason to rule out the possibility of theoretical knowledge. Let us, therefore, return to Roberts' first objection.

Roberts criticizes other aspects of current archival thinking, such as some of the empirical studies conducted by various documentation strategists,¹¹ but

objects in particular to attempts to formulate a theory that “offers scientific generalizations that are neither scientific nor generally applicable,” theory which “embarks upon a search for all embracing systems and formulas.” These types of theories “labour to impose scientific precision upon a field where scientific precision is impossible.”¹² It is, in other words, against the possibility of developing an archival science and the claim that such a science should be universally applicable that Roberts objects most strenuously. This claim has, as mentioned above, been advanced by Duranti, though Roberts has in mind Frank G. Burke, who understands theory as “the development of universal laws,” contending that, “if such laws are universal and immutable, they must be applicable at all times.”¹³ Similarly, Jenkinson’s writings have a distinctive imperial flavour, and from Ketelaar’s discussion of the Dutch manual, it appears that the manual had the role for a long time as a strict guideline for archives in Holland.¹⁴

Roberts, Duranti, and Eastwood agree that a requirement of theory, especially scientific theory, is the presence of a common nature or essence. Archives, according to Roberts, have no essence, but are unique.¹⁵ Theory, understood as universal rules, is therefore not possible in his view. Accordingly, Roberts objects to the idea that archivists can rise above their social and intellectual environment, and obtain a sort of intellectual purity, a purity which for Duranti is essential for archival science. According to Roberts, “Every archivist, every historian, every asparagus farmer, is trapped by a social and intellectual milieu ... Archivists are not so much more resourceful than the rest of society that they can develop broad theories to free them from their biases.”¹⁶

One might grant, however, that the claims of archival science and the assumption of universality are a *misapplication* of theories, without believing that *all* theory is thereby to be abandoned. It might be possible to develop a more flexible theory, one that does not claim to be applicable to all archives and does not claim that all archives share a common nature or essence, but is sensitive to the variations in context and circumstances. This is, in fact, the thrust of the argument in this article. Recent developments in many areas of inquiry (history, philosophy, sociology, anthropology) are leading to the creation of theories that are of a more modest, localized nature. Though Roberts believes that he rejects theory as such, he in fact rejects only those theories that are essentialist and universalist.

What I call essentialism has a long and complex history in Western thought. The idea behind essentialism is that there is a distinctive, permanent feature that makes things what they are. Archives, diseases, biological species, works of art, human beings, and so on can then all be defined by giving the necessary and sufficient conditions that make them what they are. Essentialism dates back at least to Parmenides (around the sixth century B.C.) and Plato. It expresses the desire to find a stable core that remains the same beyond or

behind the fluctuating appearances that surround us. In the absence of a stable core or substance, essentialists assume, we can have no true knowledge of the world, since true knowledge must remain fixed. An early manifestation of essentialism is Plato's theory of forms, expressed in the notion that there is "in each case a single form for each set of particular things, to which we apply the same name."¹⁷ In spite of all the different appearances of things, and in spite of continual change, the essence or substance of a form remains unchanged. Following this line of thought, archives, for example, may have changed throughout history but their essential features have remained the same. Because of this essential sameness, general, theoretical rules, applicable in all situations may be formulated.

Essentialism has been the target of attack in philosophy, biology, literary theory, history, medicine, and other areas. A very brief summary of this criticism must suffice here. The philosophy of Ludwig Wittgenstein (1889–1951) has been a major source of inspiration for criticism of essentialism in philosophy.¹⁸ Briefly, and expressed in lay persons' terms, one can say that the major point of Wittgenstein's philosophy is to show that philosophical problems are not solved by metaphysical assumptions about essences, but by examining how we use concepts within the practical circumstances of our lives. In particular, the meaning of words and concepts are not, as in the Platonic tradition, secured by the shared forms of things to which we apply the same name, but are a matter of the ways in which we use concepts in practice. In short, our use of words and concepts is based in conventions or social customs; this, in turn, defines their meaning.

It is because Roberts, understanding theory from the essentialist standpoint, is so distinctly pessimistic not only about present theory but about the possibility of future theory that he sees it as useless. For this reason he rejects proposals that archivists branch out, so to speak, and incorporate research from other areas, and examine, for example, the social reasons for record creation or the place of archives in society.¹⁹

For Roberts, as well as for other participants in the debate (for example Burke, Eastwood, and Duranti), the prime example of theory is to be found in the natural sciences. Yet, for Roberts, because there is nothing in archival theory comparable to that within the "hard sciences," where "theory is the well-spring," archival theory is, according to Roberts, an absurdity.²⁰ Roberts sees the imposition of theory (understood as whatever is thought to characterize natural science) as an introduction of a foreign element which excludes the understanding of historical change as an intellectual factor.

But what exactly does characterize the natural sciences and can we find methodological principles there that may serve as a guideline for other areas? Discussions in philosophy of science and other areas throughout much of the twentieth century have focused on these questions. To clarify this issue as it relates to archival theory, I must, therefore, briefly examine these questions.

Scientific Theory and Methodology: Positivism

In most of the twentieth century the prime example of theory has been scientific theory, the theory characteristic of the natural sciences. In discussions of archival theories the attempt to develop what is termed an archival *science* alludes as well to this notion of theory. This is evident in the very choice of the term *science* and in the characterization of archival science as universally applicable, fully autonomous, and based on the nature of archives – a nature which is understood as little changed throughout its history, at least in its essential features. The notion of science is, therefore, critical for the understanding of the notion of theory. To examine the notion of theory as it relates to archives we need, therefore, a brief exploration of the nature of science and scientific theory.

Inspired by positivism, philosophers and scientists tried in the first half of the twentieth century to uncover principles for a scientific method that they thought all areas of inquiry should, or did in fact follow, if it was to be considered scientific.

The term positivism was initially coined by the French philosopher Auguste Comte (1798–1857). Comte's main idea was that knowledge was limited to that which could be established by experience, that is, which could be *positively* established. Knowledge was gained by description and methodological organization of facts provided by experience. In particular, Comte insisted that religious, metaphysical, or philosophical assumptions could not be part of any explanation of natural or social phenomena. Natural and social phenomena should be explained without reference to anything beyond themselves. Positivism developed and changed in the course of its lifetime, but some of the traits expressed by Comte and others, for example, John Stuart Mill (1806–1873), were shared by later positivists. Of these the following are significant:

- Science is, and ought to be, independent of morality, politics, or religion, or any other forms of discourse that do not rely directly on observation.
- Explanation and organization of experienced facts can be carried out without any reference to God, metaphysics, or values of any kind.
- Our senses provide us with direct access to the positive facts about the world, and through logical deductions we may draw inferences from the acquired information.

That is, there are such things as scientific facts and there is a scientific method to obtain knowledge about these facts. As a goal, all areas of inquiry should follow this model. Scientific explanations are superior to all other forms of explanations. In fact, explanations that do not conform to the methodological principles assumed to be characteristic of the sciences (in particu-

lar mathematics and physics) are not really explanations at all, but pseudo-explanations. Pseudo-explanations may serve moral, psychological, political, religious, or other purposes, but they are not scientific. They may not be completely false, but cognitively they are empty.

Carl Hempel's deductive-nomological method of explanation is a well-known positivist instance of an attempt to formulate such general methodological principles.²¹ Following the deductive-nomological model, an explanation derives an understanding of a specific phenomenon from general laws and information about the specific circumstances of the case at hand. An individual phenomenon is explained by being subsumed under a general law. Hempel's explanation took the form of a deduction. Hempel, and other positivists, sought to extend the principles they thought characterized natural science to, for example, historical explanations. The result was something with remarkably slight resemblance to historiography as commonly understood and practised. In general, historical explanations did not have much of a role in the heyday of positivism.²²

These reflections on positivism are directly relevant to the debate about archival theory. In Eastwood's contributions to the development of appraisal theory, he seeks to develop a theory of appraisal that is in agreement with a general theoretical understanding of the nature of archives and which has the nature of what he considers archival science. In doing this, he relies on a conception of theory which contains echoes of Hempel. Consider the following:

The difficulty of seeing clearly just what theory is becomes more difficult when we see that the method of contemplation may be deductive or inductive ... Using deduction, one generalizes, and then examines whether generalization holds in particular instances. Using induction, one examines a case to infer some general statement, and then examines other cases to confirm the statement ... Both methods rest on observation to build generalization in the interest of understanding the nature of the thing.

The generalizations themselves cannot be observed directly, but are products of thought. If the products of these thoughts are sufficiently systematic, they can be considered theory. Because the distinction "between establishing the facts of the matter and interpreting or drawing conclusions from them" is the basis for all "rational endeavour," theories can be developed relatively independent of values or of interpretations.²³ It is worth observing that Eastwood refers to theorists who are generally considered to be within the positivist tradition, for example, John Stuart Mill's father, James Mill (1773–1836), and A.J. Ayer, one of the leading logical positivists of this century.²⁴

In order to gain a clearer picture of the notions of science and theory we need, however, to look more closely at some of the criticisms of positivism, and the alternatives suggested, since they may provide guidelines for the development of archival theory.

Alternatives to Positivism

Criticism of positivism focussed on several of its aspects, but two areas were of central importance:

- the distinction between theory and observation (a distinction crucial to understanding the notion of “theory”);
- the confrontation of the positivist account of science with the history of science and with actual scientific practice in general.

In philosophy of science in this century, theory has often been defined in contrast to observation. This was particularly the case in positivist and neo-positivist conceptions of science, which held that expressions in science were either observational or theoretical. Observational sentences were reports of simple observations, for example as in a laboratory: “the pointer is now at five,” “the liquid turned yellow,” “the chick pecked at the white bead,” or “the chick did not peck at the red bead.”²⁵ A sharp distinction between theory and observation has, however, turned out to be untenable. In many cases it is not possible to separate that which is to be observed from associated theoretical terms and assumptions.

Influenced in particular by the philosophy of Ludwig Wittgenstein, the American philosopher Norwood Russell Hanson (1924–1967) examined the notions of theory and observation in his *Patterns of Discovery*.²⁶ Observation is often supposed to be the final arbiter of contentious questions. When avenues of appeal have been exhausted, we appeal to the evidence of the senses. Presumably we will at least all see the same thing. But did, for instance, Galileo see “the same thing” as his Ptolemaic opponents? Did Kepler see what Tycho saw when looking at the sky at dawn? We are inclined to say that, naturally, they did, since their retinas and their brains were affected in the same manner. But to see something is not just a physical state. It is an experience, involving expectations, assumptions, and knowledge. As Hanson put it, “people, not their eyes, see.”²⁷

Hanson argued that it will not solve the problem to say that they saw “the same” and merely interpreted what they saw differently. To make his point he introduces the kind of illustrations known from textbooks in psychology: a drawing which some will spontaneously see as a drawing of an old woman with her face turned halfway towards us, others as a young woman with her face turned away (Fig. 1), and figures similar to the famous duck-rabbit: a figure that can be seen as either a duck or a rabbit, but not as both at the same time (Fig. 2). The point is, that when we see these things we do not go through a two-step process of first seeing, and, second, interpreting that which is seen. We simply see them as either one thing or the other, and we cannot see them as both at one time.



Figure One Some will see this image as a drawing of an old woman, some as of a young woman.

To see “is to have knowledge of certain sorts.”²⁸ To see is therefore to “see that” or to “see as,” and not merely “to see.” Observation is dependent on our expectations and on the context in which we observe things.

Hanson observed that the use of the simple observational statements suggested by the positivists is extremely rare in science, and is mostly used when the experimenter is in a situation where she is very uncertain about what to expect. Furthermore, as Newton-Smith points out, “electrons” and other entities which form the core of contemporary science are not open to immediate perceptual observation. Their observation require the use of sophisticated equipment and they presuppose that one has learnt a good deal of scientific theory.²⁹ In many instances there is no seeing of the relevant kind without the mastery of training and theory. Consequently, the observed phenomena cannot be separated from theory, expectations, assumptions, and knowledge, that is, from the observer. But Hanson also noted that physicists do not form theories in the manner suggested by the positivists. Recent sociological investigations of scientific practice show that scientists reason in a manner that is highly practical, informal and, *ad hoc*, and that they seldom rely on attempts to follow specific patterns. The procedure is similar to that of a practitioner of a game who develops a sense of what is possible, what can and ought to be done, and what will work in a given situation (judging the curve of a ball, the best approach in a given situation, and so on).³⁰

A recent example of the complexity of determining methodological principles and the distinction between observation and theory is offered by Steven

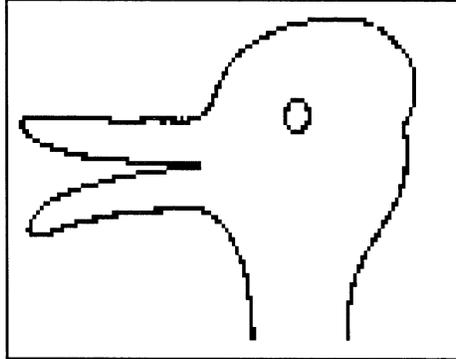


Figure Two Duck or rabbit?

Rose's discussion of his research in neurology. Rose emphasizes that science cannot be judged solely as a product but must be understood as a process (that is, practice). The results of the process are intrinsically linked to the circumstances of their creation:

Each act we make within our laboratories is dependent for its meaning on the cultural and ideological assumptions of the world which surrounds the lab, just as the lab could not exist without the technological underpinnings of machinery, chemicals, power and money which are omitted from the conventional accounts of science. And no act we make within the lab is a mere passive contemplation of nature; the products of our work themselves generate new technologies just as certainly as they generate new understanding. Laboratories have become the ideological and technological power-houses of modern society.³¹

For Rose, who works in neurology and biochemistry, the procedure for designing experiments that will shed light on the area under examination is complicated. Rose investigates chemical changes in the brain resulting from processes of learning and memory. The first task is to develop a theoretical model of the sorts of changes or patterns one might expect and, secondly, to devise experiments that might establish that these changes do in fact take place. But how can one establish that something has been remembered or learned? From a behavioural point of view this is often straightforward: a task that could not be carried out yesterday is mastered today without difficulty, or something can be recalled. But even if one does observe that a process of learning has taken place and, at the same time, some changes in the brain are detectable, how does one show (rather than just assume) that the two processes are related? According to Rose,

Measuring the shape, number and size of cells in the brain ... is, even today, with highly

sophisticated image analysis and computing systems, time-consuming, and, if one isn't careful, fraught with the danger of misinterpretation. How many of the hundreds of thousands of cells in each tiny brain region must one study to get a representative picture? How can one be sure that what is being seen and counted is "really" present in the living brain rather than an artefact, an artificial pattern generated by the techniques required to fix, slice and strain the brain tissue to make it visible? How can one scale up from what can be counted in a two-dimensional section to the three dimensions of living tissue?³²

What comes to constitute scientific fact or knowledge is, in other words, not a product of immediate observation but emerges as a result of a complicated process. The object of knowledge is determined by the prospect of success. The experimenter decides to perform experiments on the basis of an estimate of what is likely to succeed and of what he or she is likely to be able to obtain funding for. Scientific research is costly, and things that seem improbable will not be examined.³³ The practical limitations on the design of the experiment limit the knowledge which may be gained. Even at this point the result has to be interpreted, a process which, as Rose demonstrates, has many pitfalls. In the history of science there is no shortage of extravagant interpretations that were not supported by the available evidence, but were carried forward by wishful thinking and philosophical assumptions that were not clarified. The jump from a specific instance in the laboratory to general assumptions about memory must be taken with considerable care.³⁴

Paradigms

Thomas Kuhn suggested a possible alternative to the positivist understanding of scientific practice. His alternative to positivism, as expressed in *The Structure of Scientific Revolutions* (first published in 1962), emerged largely as a result of his research into the history of science. This research made him question central positivist suppositions about the nature of the scientific enterprise. His work is also worth dwelling on because it is primarily to it that we owe the prominence of the term "paradigm," a term freely, if not always accurately, used in discussions of archival theory as well.³⁵

Most scientific work takes place within what Kuhn terms "normal science." Normal science develops when a body of research for a time is sufficiently impressive to set the agenda for further research and define the limits within which this research is carried out. For more than two centuries, for example, Newton's mechanical physics dominated the discipline and seemed to provide a theoretical framework for explanation of all kinds of movement. When Newton's *Philosophiæ Naturales Principia Mathematica* was published in 1687, it represented not just a good explanation of phenomena requiring explanation, but became a model for what an explanation is and a model for every other

field of inquiry. With this work, Newton gave causal explanation a new meaning. Causal explanation became synonymous with mechanical explanation. In nature, in society, in individual human beings, mechanical explanations were henceforth sought.³⁶

In periods of normal science, that is, when a particular view of science (a paradigm) prevails, testing and research do not happen as the positivists assumed, but rather have the character of puzzle solving. In this situation, the underlying, general theory is not questioned, but phenomena are sought and subsumed within what is acceptable to the overall theory. When something appears that seems inexplicable under the paradigmatic theory, this is not a cause for rejection of the theory. (Newton's theory of gravitation was not discredited because the movements of the planets did not conform to the course that they were predicted to follow on the basis of this theory.) Instead, in circumstances such as these, other possibilities are examined. Error may have crept in, some auxiliary phenomenon (for example the existence of an unknown planet) may not have been observed, and so on. Additional hypotheses are suggested in an effort to *preserve* the paradigm as a whole.³⁷

Paradigms also determine the kinds of questions that can be legitimately asked. A paradigm serves to concentrate research into areas where results are likely to fit within the paradigm. Currently, for example, genetic explanations of behaviour, misbehaviour, and diseases have a dominant position in biology and medicine. This means that scientists in medicine search for genetic explanations rather than for, say, psychological or social explanations. The paradigm determines what counts as a problem, and from a practical standpoint decides what it is possible to obtain funding for.

Even when new approaches gain influence, this new pattern finds practical and institutional expression first and foremost before the paradigm changes: a new area of inquiry, a new subdiscipline, or maybe what is merely a new approach to an area of inquiry is given a name and a group of people becomes identified with it. The typical pattern in science is the formation of a society or organization and the creation of a journal that expresses the interests and the views of the organization or society.³⁸

When Roberts, therefore, criticizes the aspirations of archival theory for tending to "institutionalize the prejudices and fashions of the moment" and attempting to raise archivists above (in the words of Frank Burke) "their own social and intellectual environment,"³⁹ he is in fact in agreement with much contemporary philosophy, sociology, and history.⁴⁰ Scientific work is indeed not immune to the social, political, historical, and intellectual contexts in which it is carried out.

But Roberts fails to appreciate the substantial work that has been undertaken to find a standpoint which does not have universalist and essentialist aspirations but nevertheless does not mean the rejection of all theory. Theory does not necessarily entail rising above the rough and tumble of one's contem-

porary world. Along these lines, Hilary Putnam, probably the foremost contemporary philosopher of science, has concluded that there is “no such thing as *the* scientific method. Case studies of particular theories in physics, biology, etc., have convinced me that no one paradigm can fit all of the various inquiries that go under the name of ‘science’.”⁴¹ Putnam does not draw any relativistic conclusions from this.⁴² Though we have no foundations (in the philosophical sense of something beyond existing human practices and experiences) for scientific or ethical assumptions, we have criteria of reasonableness. Their internal criteria are open-ended and developing, though not as open-ended as relativists want us to believe. Putnam’s assumption is that, as a matter of fact, there is much more general agreement about principles than is assumed by the relativist, both in science and in ethics. In science and in ethics some things are not open for discussion. We do not face each other on opposite sides of an abyss of incomprehension.

The conclusion we can draw so far, then, is that theory is possible without the pitfalls feared by Roberts as an inevitable companion of theoretical efforts. There is a place for archival theory; in other words, archival theory is possible. But is it necessary? In the next section, I address this question.

Theory and Practice

The rejection of theory is supported by the assumption that theory and practice are separate entities and that, generally speaking, a practice is and can be carried out without any theory. Understood in this way, theory truly has no relevance for practice. Contrary to this position, I wish to argue that, though a practice can be carried out without any conscious theory supporting it, any practice will nevertheless rely on assumptions that can be elucidated theoretically. Moreover, theory is not only an *explanation* of practice, which then has no further influence on that practice. Once developed, the theoretical point of view influences, as previously explained, the approach to practice. The connection does not go exclusively from practice to theory, but also from theory to practice, though not solely as a set of abstractly developed principles which are then applied to practice. As Roberts points out, archivists are unable to transcend their own social and intellectual environment, and develop some “pure” theoretical standpoint. It is, however, possible to develop a reflective point of view which does not aim at transcendence, universality, or essentialism, but which nevertheless deserves the name of theory. Many of the theories referred to in the previous section (those of Hanson, Putnam, Kuhn, and Rose) exemplify such a stance.

Theoretical reflections about a discipline or an established practice typically arise when the practice or the discipline has reached a point in its development when it can no longer be continued exclusively on the basis of the already established or traditional practices. The legitimacy of a particular

practice may be questioned by someone outside that practice, or people engaged in the practice may encounter problems for which there are no longer straightforward practical solutions. Ethical concerns may raise a conflict with what is “expected” of the archivist. Conflicts or problems (anomalies) for which the traditional practice does not provide guidelines may arise. They call for reflection, perhaps for a renewal of practice. Theoretical reflections therefore grow out of the development of practice.

In this view of the relationship between theory and practice there is no choice between being either exclusively theoretical or, on the other hand, practical to the exclusion of theory. As already explained, a practice relies on a shared understanding of its object, the nature of its task, the goals to be achieved, available options, and which, if any, of the available options are permissible from an ethical, legal, or customary point of view. In this sense a practice is theoretical or has theoretical presuppositions, though the theory may not be articulated.

A practice can be carried out without the clear articulation of a theory, simply through the mastery of techniques or procedures. Not all activity is of an *intellectual* kind or requires *intellectual* activity. As Gilbert Ryle pointed out, there is also “knowing how.”⁴³ Though practice relies on theory or a broad understanding of the world (Trevor Livelton suggests the term “world view”), this understanding may not be articulated or elaborated as a theory. As Livelton says, “practice does not always employ *developed* and *articulated* conceptual knowledge,” but theory or ideas nevertheless “always and inevitably underlie archival practice, whether archivists are aware of them or not.”⁴⁴

Michael A. Lutzker, Richard Brown, and others have, for instance, pointed out that archival theory and practice relies heavily on Max Weber’s understanding of bureaucracy.⁴⁵ Weber’s conception of bureaucratic structures has become part of the theoretical baggage of the professional archivist, at least those who work in large bureaucratic structures. As Brown points out, this application of Weber effectively constitutes an archival ideology.⁴⁶ Notably, this is not surprising: modern archival theory, from the Dutch manual to Jenkinson and Schellenberg, developed largely as a response to problems of managing the massive number of records generated by the very bureaucratic structures studied by Weber.⁴⁷ It is clear, then, as this example illustrates, that political, social, and economic change can influence archival theory and practice. In fact, in a discussion of archives in South Africa, Verne Harris suggests that profound political changes, such as those resulting from the dissolution of the apartheid policy in South Africa, can alter “the very identity of archives.”⁴⁸

When a practice can be carried on without any or with only minor problems there is little incentive to engage in extended theoretical reflections. Theory typically emerges or expands when a practice bumps up against a changed reality.⁴⁹ A development or set of developments occurs which can no longer be accommodated within the prevailing practice and the methodologies devel-

oped within this practice. It is then necessary to bring to light and examine the assumptions guiding a particular practice in order either to extend it to cover the new set of circumstances or change it more radically. Jenkinson, for example, left most decisions about the appraisal and selection of records to the creators of the records. It was not for the archivist to make these decisions since they had no firm foundation for making such decisions. Furthermore, the historical records in archives would be made more accurate if there was as little interference as possible from the archivist. An archivist's primary task was then the care of the records in his or her custody. This may have been a tenable position in the early parts of the twentieth century, but particularly after 1945 the bulk of the records waiting to be brought under physical and intellectual control by archivists overwhelmed the system. Archivists were required to enter the realm of appraisal, and Schellenberg did so. When it was conceded that archivists, if only from necessity, were now required to make appraisal decisions, a new problem arose: which principles should guide these decisions? Schellenberg suggested a number of principles that could serve as guidelines for selection and appraisal, but these principles naturally then became a subject of contention. The development of documentation strategies in Canada, Germany, the United States, and elsewhere flowed from this issue. Currently the discussion of post-custodialism is in part a reaction to the development and growing use of electronic records in businesses and in governments – again a change in social practice which requires a theoretical response.⁵⁰

Similarly, diplomatics studies documentary practices because some of the documentary forms have changed over time and are no longer familiar to us. If they remained invariably the same, this would hardly be necessary. It is when things can no longer be understood and managed as “business as usual” that we call upon theory. In turn, the theoretical developments become part of a renewed practice and eventually become business as usual or “common sense.”

A practice, then, relies, if not directly on a theory, at least on some broadly perceived *Weltanschauung*. This *Weltanschauung* is present as the shared attitudes and assumptions of a society, an institution, or a profession. It is an understanding passed on from generation to generation and considered practical or common sense, but which may nevertheless, as Pierre Bourdieu writes, “instill a whole cosmology.”⁵¹ In this sense, Roberts' emphasis on historiographical understanding can itself be considered a theoretical standpoint, even though it may not be articulated as such: it is accepted as a matter of tradition or because it is part of the prevailing practice of archivists – an attitude acquired in the course of practical training. The task of the theoretician, on the other hand, is, as much as possible, to try to bring these largely submerged assumptions to the surface, to speak that which is unspoken. Through this process we become aware of how expectations, tacit assumptions, tradition,

ideology, and simple inertia (all shaped by our social and historical situation) play a large role in our interaction with the world around us.

Theory is a self-conscious reflection on a particular practice in order to bring to light the presuppositions unconsciously assumed in that practice. This view differs from the view suggested most prominently by Luciana Duranti. In her essay, "The Records: Where Archival Universality Resides," she argues that the definition of record developed in archival theory is universally valid and divorced from "cultural biases."⁵² It is, as we saw, likewise the absence of any influence from politics, law, and culture that made the Dutch manual the first scientific archival treatise.

It is, above all, on this view of the possibility of a theory free from any external influence, and therefore claiming universal validity, that the advocates of an archival science rely, drawing on a positivist conception of science, as described above. Like the positivistic view of science as liberated from myth, metaphysics, and religion, and as based on direct and unbiased observation, archival science must similarly be freed from any external influence. This science must develop "principles, concepts and methods valid for every type of archival material in any time and place."⁵³

In the view advocated in this essay, theory arises primarily from practice, and not from theory itself, not primarily from reflection on other theories, but from reflection on ways of doing. Theory and practice cannot be separated, but practices can, and often do develop relatively independently of any conscious theoretical input.

The choice, then, is not between either theory or practice, but between a practice one engages in blindly, or a practice in which one engages in the realization that the practice is constrained by factors that are not immediately obvious, but which one hopes may at least become clearer.

A New Conception of Theory

Much theory and philosophy has aspired to find a point beyond the reach of time and change, a fixed point from which we can view and evaluate past and present without clouding our judgements by biases known and unknown. This aspiration has driven many philosophers and scientists and, as we have seen, archival theorists. But attempts to find such an Archimedean point have not been successful. It is now commonly assumed that we cannot lift ourselves up to a point beyond history and society. We must accept something less than pure objectivity. In most disciplines, including the "hard" sciences, practitioners have had to come to terms with this contextual and historical nature of human endeavours, or, to use an expression from current literary theory and philosophy, with the *contingent* basis for our practices and discourses.⁵⁴

The realization of the contextual, social, and historical nature of being human has characterized much intellectual endeavour in the last quarter of the

twentieth century. We are all rooted in our time and place, and, in all our undertakings, our particular time and place leaves its distinctive marks. We may strain for understanding, and the marks yet remain hidden from view. This is especially so for features whose origin stretches into a past partially unknown or unconscious to us.

In the light of these developments, much of the debate about the place of theory in archives is misguided. The rejection of any place for theory has been shown to be unfounded. Some of the suspicion of theory was based on the assumption that archival theory must take the form that was believed to be characteristic of theory within the natural sciences. Recent examinations of scientific practices (past and present) have shown, however, that the customary positivist image of science and scientists has little ground in the social, historical, and practical reality of scientific research. The rigorous conception of natural science, characteristic of positivism in the earlier parts of the twentieth century, does not agree with actual scientific practice and with the history of science. It is now widely agreed that there is no such thing as *the* scientific method. Rejection of archival theory following this model is therefore well-founded. But this should not be mistaken for a rejection of *theory as such*. The criticism of positivism shows exactly that there are many other theoretical traditions to draw on.

If science is thought of necessity to be independent of historical and other contexts, an archival science is not possible. On the other hand, if there is nothing very special about what is often called the scientific method (other than certain forms of established practice), one is of course free to talk of a scientific method, but it no longer carries the same weight. If, in other words, being "scientific" means little more than being systematic and careful and following certain set procedures, any attempt to gain knowledge in a careful and systematic manner, including for example, the careful and systematic work of an anthropologist or a historian, may be characterized as scientific. But very little is gained by this characterization. The notion of archival *science* has, consequently, no special content. It does, however, have rhetorical appeal since science, Rose, for one, points out, has a special cachet in our society.

The paradoxical result of post-positivist developments in the philosophy of science has been the realization that there are indeed similarities between the various forms of inquiry, but the similarities arise, so to speak, from the other end of the spectrum. Hard science *is* more like history or anthropology or literary criticism or philosophy than we once thought, because all these areas can be understood as forms of interpretation and constructions of representations. None of them can make claim to strict objectivity, none of them represents a method for unmediated and unprejudiced access to the world as it is in itself, and none of them represents in isolation the full story about the object under examination. The full story will in fact never be told, but it can nonetheless serve as a guiding ideal in the unending, historically unfolding scientific enter-

prise. It is in this “softer” image of what science or systematic inquiry amounts to, that a place for archival theory should be found as well.

Theories, then, provide relatively general and abstract explanations of phenomena, but they are not answers to everything. Evolutionary theory provides a general explanation of the diversity of naturally occurring phenomena, of the rise, decline, and fall of species. It rules out certain things (for example, that the species now observable have been the same always, and that the number of species is finite), and it provides guidelines for the types of explanations sought by biologists. But it is only through the application of the general theoretical framework to the individual cases that explanations applicable to a local phenomenon will be provided. Moreover, neither biological theory nor those theories pertaining to the physical sciences may be the only type of explanation for any observed phenomena. Each may explain certain aspects of the phenomena, but these may be supplemented with other types of interpretations, for example, biochemical, psychological, sociological, or historical. Even within one narrow, specific area more than one theory may explain the phenomenon under observation equally well, depending on the purpose for which an explanation is sought.

On the other hand, when Roberts refers satirically to the “nagging puzzle of how to determine when a document is in such poor condition that it needs to be enclosed in Mylar” he misses the boat.⁵⁵ His implication is that the questions faced by archivists are so mundane that no theory is required. But theory is not about these sorts of questions, and no one would expect profound theoretical answers to such questions. No archival theory can provide the answer to Roberts’ “nagging puzzle” since it is not a theoretical question. It would, however, be an interesting theoretical question to ask why there are societies that attach such importance to written documents that they will go to great length to preserve them. For the archivist it is a given that some documents must be preserved. When faced with limited resources the archivist cannot give all of them the optimum treatment. When considerations about what to preserve go beyond determining the physical condition of the document (or whatever) they may have theoretical underpinnings.

It also follows from these considerations that, although a distinction between theory and practice can be drawn, it is not as clear-cut as Roberts (and other contributors to the debate) implicitly assume. In particular, practice is not independent of theory. All observation and practical activity requires some understanding of the nature of that activity – of what is to be measured, the various types of phenomena to be included in statistical analysis, the types of records to be retained, the purpose of archival work, and so on. Investigations and practices of any kind proceed through the use of standards or prototypes. This holds for archival theory and practice as well. Trevor Livelton’s observation that archival theory and practice are inseparable seems entirely justified.

Theory, then, becomes an examination of a practice or of practices, aimed at

articulating those general principles, ideas, or theories that give these practices their coherence – or perhaps render them incoherent. Archival theory can take its starting point in the issues raised by Roberts and Burke: What makes a society create the records that it does? What are the sociological aspects of records creation? What is history and how have historiographic trends influenced the practice of archivists? How have the practices of archivists influenced historiography? How can archivists become better aware of the biases that influence their work, and shape, for example, acquisition and retention policies? Precisely this area could form an important part of theoretical or philosophical analysis of archival practices and theories: given that archivists are not immune to various historical and social trends, can something be said about how these influences have shown themselves? How exactly do such biases find expression in archives as institutions or in the work of the individual archivist?

If theories are understood as reflections on or criticism of existing practices, and if they arise out of practice, it becomes of paramount importance to understand the exact nature of these practices in order to shed light on the underpinnings of a particular theory and to understand what the theory is about. A theory is meant to answer specific questions. The approach suggested therefore amounts to something very like a sociology of theories, or a sociology of knowledge. We need an approach that comprehends the social mechanisms that support theoretical understandings. We need to examine the specific situations that give rise to theoretical reflection and theoretical disputes.

There seem to be (at least) two ways of accomplishing that task. Similar to the “anthropological” investigations of scientific practice undertaken by, for example, Bruno Latour and Knorr-Cetina, or, in an autobiographical manner, by Steven Rose, one could simply camp out in an archives. Thus ensconced, one would observe what archivists do and write it down with as few preconceived notions of the relative importance of various tasks performed as possible. (Prejudices aside, a lunch break could be as significant a matter as a decision to acquire a large fonds.) One would ask the archivists pertinent questions, and try to draw general conclusions about the practices of archivists, how they articulate these practices, the types of reasoning that enter into decisions, the kinds of theory or ideology that motivate their work, how funding is obtained for various projects, the considerations that enter into grant applications, and so on.

This approach might well yield interesting outcomes. It would not provide a how-to archives handbook, but neither need it be necessarily reduced to a mere registration of the comings and going of the archivist. The purpose of the investigations carried out, for example, by Latour and Knorr-Cetina is clearly to provide a focus for critical reflection on scientific practice as such, and to reveal all those features of scientific practice that are smoothed over in the prevailing ideology of science. In short, it would bring to light the practical, but

submerged, unacknowledged, or denied, but nonetheless crucial features of a practice. When articulated theory is then compared to actual practice, the process would provide a source for reconsideration of both theory and practice.

Another approach is a historical-sociological examination. Sociology itself has to a large degree become historical, and history has increasingly become sociological, a sociology of the past.⁵⁶ The suggested approach to theories implies that theories must be understood in conjunction with practices. The practices that form the basis of theories must be reconstructed. If the practices originated in the past, this factor will obviously lead us into historical examination and analysis. But even our contemporary practices are shaped by their historical development. Theories and practices are of our own making, but they are made in the context of the conditions bequeathed to us by the past.

Historical examination should not be seen as separate from theoretical enquiries. As Barbara Craig has observed, “archives history is vital to the health of archival theory and practice in the future.” History should be approached not just out of antiquarian interest, but because it can shed light on contemporary concerns. Since archives are historical products, and we are part of that history, bringing that history to awareness can provide insights that make us better able to cope with contemporary concerns. Again, as observed by Craig, “the history of archives and of records-keeping gives archivists perspective – on their present situation, on their current problems, and on the future ... Knowing what has passed, we are in a much better position to access the novelty of the situation we encounter.” Historical awareness also “stimulate[s] the development of a more critical approach to our work and to the influences that [affect] it.”⁵⁷ As Livelton observes, archivists habitually bring “a certain historical sensitivity to bear on certain aspects of their work.” An archivist, he contends, must “take due notice of the social, cultural, legal, political, and administrative environment in which the documents were made and kept. Because the records involved were created over time, there is of necessity a historical dimension to the analysis required in such work.”⁵⁸

This sensitivity to context and history must be extended to the history of archives as institutions. In order to examine archives in their historical development it is not enough to scan the horizons of history in search for the term “archive” or some equivalent thereof. In order to ascertain the meaning of the term it must be understood in the historical, social, and linguistic context in which it was used. As a concept, “archive” must be viewed within the family of other concepts to which it, at the time, belonged. Were, for example, archives always differentiated from libraries and museums? What were their functions? How significant were they in the world of the time? Who controlled them? These, and a host of other questions, need to be answered in order to get a complete picture of archives in other historical epochs. The examination of theoretical matters need not always lead us into historical con-

siderations, but whenever we discuss issues of contemporary concern certain broad principles are at stake. These principles are, above all, that archives and archival theory must be understood in their practical context, and that theory and practice cannot be separated.

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Notes

- 1 See, for example, John W. Roberts, "Archival Theory: Myth or Banality," *American Archivist* 53 (1990) and John W. Roberts, "Practice Makes Perfect, Theory Makes Theorists," *Archivaria* 37 (Spring 1994). My evidence for the fact that this view is widely shared stems from conversations with archivists and archives students at the University of British Columbia. The assumption may, however, have more than anecdotal support. One of the results of a recent survey conducted by the Archives Association of British Columbia (AABC) is that most respondents do not want an increase in discussions of archival theory at the annual conferences of the AABC. One respondent comments that "I'm not sure of the value of [such] a conference for members more interested in workshops and hands-on guidance. Too much theory. We don't need to be a mini-ACA." Archives Association of British Columbia, *Needs Assessment Survey Report* (Vancouver, 1999), response to question number 99.
- 2 Luciana Duranti, "Archival Science," in A. Kent, ed., *Encyclopedia of Library and Information Science* 59 (1996), p. 1.
- 3 Terry Eastwood, "Nailing a Little Jelly to the Wall of Archival Studies," *Archivaria* 35 (1993), p. 233.
- 4 "Archival science constitutes the foundation of the *archival discipline* ... of *archival history* ... and of *archival economy*." Duranti, "Archival Science," p. 1.
- 5 See Luciana Duranti, "The Odyssey of a Records Manager," *ARMA Quarterly* (July and October 1989) and "The Record: Where Archival Universality Resides," *Archival Issues* 19 (1994), pp. 83–94. Verne Harris criticizes this view of archives and their history in "Claiming Less, Delivering More: A Critique of Positivist Formulations on Archives in South Africa," *Archivaria* 44 (Fall 1997), pp. 133–35.
- 6 Duranti, "Archival Science," p. 5. See also page 12. The emphasis on *scientific* is Duranti's. The latter emphasis is added.
- 7 Roy Schaeffer, "From Craft to Profession: The Evolution of Archival Education and Theory in North America" *Archivaria* 37 (Spring 1994), p. 31. Second set of italics added. See also Harris, "Claiming Less," and Verne Harris, "Redefining Archives in South Africa: Public Archives and Society in Transition," *Archivaria* 42 (Fall 1996).
- 8 "Much Ado About Shelving," *American Archivist* 50 (Winter 1987), p. 74, cited by Roy Schaeffer in "From Craft to Profession," p. 26.

- 9 Roberts, "Archival Theory," p. 111.
- 10 Ibid., p. 112. See also Roberts, "Practice Makes Perfect," p. 117.
- 11 See, for example, Roberts, "Practice Makes Perfect," pp. 113–15.
- 12 Ibid., pp. 111, 113.
- 13 Frank G. Burke, "The Future Course of Archival Theory in the United States," *American Archivist* 44 (1981), p. 40.
- 14 Eric Ketelaar, "Archival Theory and the Dutch Manual," *Archivaria* 41 (Spring 1996), p. 35.
- 15 "An archives is chameleon-like; it has no essence of its own." Roberts, "Archival Theory," p. 112.
- 16 Ibid., p. 116. His objection here is directed against Frank Burke's claim, but as we saw above, Duranti also believes that archival science must be independent of any social or cultural influences.
- 17 Plato, *The Republic*, p. 596a, Desmond Lee, trans. (Harmondsworth, 1974), p. 422. For a discussion of essentialism in relation to art theory see Preben Mortensen, *Art in the Social Order: The Making of the Modern Conception of Art* (Albany, New York, 1997), p. 15 ff. Essentialism in medicine is discussed in Uffe Juul Jensen, *Practice and Progress: A Theory for the Modern Health-care System* (Oxford, 1987), p. 20 ff. Essentialism in medicine, is expressed in the theoretical premises that there is a distinct boundary to be drawn between "what is unhealthy, or diseased, and what is healthy" and that "any single case of a disease has the same essential characteristics, the same nature, and the doctor identifies the disease by recognizing the essential characteristics." Ibid., p. 20.
- 18 See particularly his arguments about "family resemblance" in *Philosophical Investigations*, sections 65 ff. (Oxford, 1953).
- 19 This was suggested by Burke. See Roberts, "Archival Theory," p. 117.
- 20 Ibid., p. 111.
- 21 See Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, 1966), especially chap. 5.
- 22 Georg Henrik von Wright points out that "it seems almost an irony of fate that the fullest and most lucid formulation of the positivist theory of explanation should have been stated in connection with the subject matter for which, obviously, the theory is least suited, viz. history." Georg Henrik von Wright, *Explanation and Understanding* (Ithaca, New York, 1971), pp. 10–11. Though not in regard to Hempel, R.G. Collingwood had already noted that positivism had no place for history. In the heyday of positivism, history and the philosophy of history were neglected. Positivism's inability to account for the development of historiography was, for Collingwood, a decisive reason to reject it. See R.G. Collingwood, *The Idea of History* (Oxford, 1956 [first published in 1946]), p. 232 ff.
- 23 Terry Eastwood, "What is Archival Theory and Why is it Important?," *Archivaria* 37 (Spring 1994), p. 123.
- 24 See Terry Eastwood, "Towards a Social Theory of Appraisal," in Barbara L. Craig, ed., *The Archival Imagination: Essays in Honour of Hugh A. Taylor* (Ottawa, 1992), pp. 71–72 and 80, for references to Ayer. I argue below that the distinction between facts and interpretation, seen by Eastwood as the basis for rational behaviour, cannot be maintained. It is precisely the impossibility of maintaining this distinction that caused the demise of positivism.
- 25 According to W.H. Newton-Smith, "Observational terms were thought to be directly applicable to experience; their meaning was specified in terms of the verification and falsification conditions given by reference to possible experience. It was taken that while theory change meant change in the meaning of the theoretical terms, the meaning of observational terms was invariant under theory change. Hence, the observational vocabulary constituted a theory-neutral observational language." W.H. Newton-Smith, *The Rationality of Science* (Boston, 1981), p. 11.

- 26 Norwood Russell Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge, England, 1961).
- 27 Ibid., p. 6. About Galileo, see also Paul Feyerabend, *Against Method*, rev. ed. (London, 1988), 56 ff. Feyerabend also points out that “no single theory ever agrees with all the known facts in its domain” (Ibid., p. 39 [italics in original]). Theories are upheld even though there are known instances of observation that contradict them (pp. 39–41).
- 28 Hanson, *Patterns of Discovery*, p. 20. See also Feyerabend, *Against Method*, p. 58.
- 29 Newton-Smith, *Rationality of Science*, p. 22.
- 30 See the instances cited in, for example, Karin D. Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford, 1981), and Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (London, England, 1979), particularly chap. 4.
- 31 Steven Rose, *The Making of Memory: From Molecules to Mind* (New York, 1993), p. 8.
- 32 Ibid., p. 258.
- 33 See Knorr-Cetina, *The Manufacture of Knowledge*, for example, pp. 59, 74. See also the essays collected in Stephen Jay Gould, *The Panda’s Thumb: More Reflections in Natural History* (New York, 1980). These very readable and entertaining essays all “contravene the usual mythology about scientific practice – that facts are ‘hard’ and primary and that scientific understanding increases by patient collection and sifting of these objective bits of pure information.” Science is “a human activity, motivated by hope, cultural prejudice, and the pursuit of glory, yet stumbling in its erratic path toward a better understanding of nature” (pp. 115–16). Gould’s *The Mismeasure of Man* (New York, 1981) is a detailed demonstration of the influence of cultural prejudices on scientific practice in psychology, especially in the area of intelligence testing. For an instance of how general cultural ideas and assumptions can influence the development of science, see Gerald Holton, “Einstein and the Cultural Roots of Modern Science,” *Dædalus* 127 (Winter 1998), pp. 1–44.
- 34 According to Rose, “In what I actually do in the laboratory, I am trapped in the artefactual world mediated by machinery. I do not observe nature, as symbolized by my chicks [Rose uses chicks for his experiments], in an unmediated way. Like all scientific findings, mine are *actually* nothing but readings on meters, printouts on papers, numbers derived from machines ... which I manipulate to extract meaning and which I then endeavour to extrapolate back to stand for, to represent, deductions about the behaviour of molecules, cells and organisms in the real world.” Rose, *Making of Memory*, p. 272.
- 35 It is beyond the scope of this essay to go into detail about the developments in post-positivist philosophy of science, but, in addition to works referred to above and below, central contributions to the debate can be found in the following works: Richard J. Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics, and Practice* (Philadelphia, 1985); T.S. Kuhn, “Logic of Discovery or Psychology of Research?”; Idem, “Reflections on my Critics,” in I. Lakatos and A. Musgrave, *Criticism and the Growth of Knowledge* (Cambridge, England, 1970); Idem, “Second Thoughts on Paradigms,” in F. Suppe, ed., *The Structure of Scientific Theories*, 2nd. ed. (Urbana, 1977); Idem, *The Essential Tension* (Chicago, 1977); M. Masterman, “The Nature of a Paradigm,” in Lakatos and Musgrave, *Criticism and the Growth of Knowledge*; K. R. Popper, *The Logic of Scientific Discovery* (London, England, 1968); Hilary Putnam, *Reason, Truth and History* (Cambridge, England, 1981); Idem, *Realism and Reason* (Cambridge, England, 1983); Idem, *The Many Faces of Realism* (LaSalle, 1987); Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton, 1979); Idem, *Consequences of Pragmatism* (Minneapolis, 1982); F. Suppe, “The Search for Philosophic Understanding of Scientific Theories,” in F. Suppe, ed., *The Structure of Scientific Theories* (which gives an excellent account of the development of positivism and alternatives to positivism); Stephen Toulmin, *Human Understanding* (Princeton, 1971).
- 36 Hanson, *Patterns of Discovery*, p. 91.

- 37 See Hilary Putnam, "The Corroboration of Theories," in *Mathematics, Matter and Method* (Cambridge, England, 1975).
- 38 According to Rose, "Once a society and its journal exist, a research group has achieved a sort of corporate identity; it develops its own research traditions, standards of proof, definition of what is and what is not an important question [in short: a paradigm], and it validates them by publishing papers proclaiming them. Even more important, university departments become established in the name of the new subject and begin to produce their own graduates." Rose, *Making of Memory*, p. 41.
- 39 Burke, "Archival Theory," p. 43, cited by Roberts in "Practice Makes Perfect," p. 116.
- 40 For a recent discussion of this issue, see Lorraine Daston, "Fear and Loathing of the Imagination in Science," *Dædalus* 127 (Winter 1998), pp.73–95.
- 41 Putnam, *Many Faces of Realism*, p. 72; see also Putnam, *Reason, Truth and History*, chap. 8.
- 42 See particularly his "Why reason can't be naturalized," chap. 13, in Hilary Putnam, *Realism and Reason*.
- 43 Gilbert Ryle, *The Concept of Mind* (New York, 1960), chap. 2.
- 44 Trevor Livelton, *Archival Theory, Records, and the Public* (Langham, Md. and London, 1996), pp. 31, 35. Emphasis added.
- 45 See Michael A. Lutzker, "Max Weber and the Analysis of Modern Bureaucratic Organization: Notes Toward a Theory of Appraisal," *American Archivist* 45 (1982), pp. 119–130. Lutzker says that Weber's model of bureaucracy has been "incorporated into the professional archivist's consciousness" (p. 124). Richard Brown likewise points out that the "prevailing conception of bureaucracy and administrative organization found in archival literature continues to be grounded in the hierarchical framework of relations established by the elementary principles of Weberian sociology." "Macro Appraisal Theory and the Context of the Public Records Creator," *Archivaria* 40 (Fall 1995), p. 132.
- 46 Brown, "Macro Appraisal Theory," p. 133.
- 47 See Terry Cook, "When the Past is Prologue: A History of Archival Ideas Since 1898, and the Future Paradigm Shift," *Archivaria* 43 (1997), pp. 20–30.
- 48 Verne Harris, "Redefining Archives in South Africa," p. 15.
- 49 The situation of archives in South Africa provides a dramatic illustration of this. Though individual archivists were not always willing co-operators with the apartheid system, the State Archives Service in South Africa was, in the period of apartheid, an integral part of the apartheid system. The collapse of the apartheid system necessitated a reorientation of archives in South Africa, and has been the impetus for a flurry of theoretical discussions. See Harris, "Redefining Archives in South Africa."
- 50 As Terry Cook points out, "Archival thinking over the [twentieth] century should be viewed as constantly evolving, ever mutating as it adapts to radical changes in the nature of records, record-creating organizations, record-keeping systems, record uses, and the wider cultural, legal, technological, social, and philosophical trends in society." Cook, "When the Past is Prologue," p. 20.
- 51 Pierre Bourdieu, *The Logic of Practice*, Richard Nice, trans. (Stanford, 1990), p. 69. Beliefs guiding a practice may not necessarily be of the "head" but also be responsible for "bodily attitudes," according to Bourdieu. This is particularly emphasized by the work of Norbert Elias on the process of civilization. See Norbert Elias, *The Civilizing Process* (Oxford, 1981).
- 52 Duranti, "The Records," p. 92.
- 53 *Ibid.*, p. 6.
- 54 See, for an influential philosophical example, Richard Rorty, *Contingency, Irony and Solidarity* (Cambridge, England, 1989), Part I, "Contingency." See also Barbara Herrnstein Smith, *Contingencies of Value: Alternative Perspective for Critical Theory* (Cambridge, England, 1988).

55 Roberts, "Practice Makes Perfect," p. 112.

56 For discussions of the connection between social theory and history, see Peter Burke, *History and Social Theory* (Ithaca, 1993) and Dennis Smith, *The Rise of Historical Sociology* (Cambridge, England, 1991).

57 Craig, "Archives Theory, Archival Practice, Archives History," pp. 42–43.

58 Trevor Livelton, *Archival Theory*, p. 47.