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THE FUNCTIONAL COMPLEXITY OF SCIENTIFIC EVIDENCE

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Abstract: This article sketches the main features of traditional philosophical models of evidence, indicating idealizations in such models that it regards as doing more harm than good. It then proceeds to elaborate on an alternative model of evidence that is functionalist, complex, dynamic, and contextual, a view the author calls *dynamic evidential functionalism* (DEF). This alternative builds on insights from philosophy of scientific practice, Kuhnian philosophy of science, pragmatist epistemology, philosophy of experimentation, and functionalist philosophy of mind. Along the way, the article raises concerns about the total evidence condition, requirements of certainty or incorrigibility on evidence, and accounts that restrict the type of things that can serve as evidence (to, for example, sense data, facts about particulars). DEF can also help us see the special value of novel predictions and experiments as evidence, as well as help us think about how to critically evaluate the putative evidence to determine whether it *is* evidence.

Keywords: contextualism, dynamic evidential functionalism, evidence, experiment, functionalism, inquiry, John Dewey, observation, pragmatism, predictivism, Thomas Kuhn.

Introduction

Several problems in the contemporary philosophy of scientific evidence—the experimenter's regress (Collins 1992; Franklin 1994; Godin and Gingras 2002), concerns about discordant evidence (Franklin 2002; Stegenga 2009), worries about the importance of "robust" evidence from different types of sources (Culp 1994; Stegenga 2009), and questions about "evidence for use" as distinct from evidence for theories or hypotheses (Cartwright 2006)—are dependent on a commonly assumed but radically impoverished model of evidence (or better, a family of such models). This model is impoverished in that it ignores the temporal dynamics of inquiry within which evidence plays a role, as well as the variety of functional roles for evidence within that dynamic process. Since the problems are the result of the features of the model (rather than deeper, model-independent problems, as some philosophers assume), many attempts to solve the problems amount to evasions, to patches that generate even further problems, and so on. What is needed is a systematic rethinking of the basic model of evidence underlying contemporary epistemology and philosophy of science.

Features of the traditional model are often held implicitly, causing frustration not only among philosophers but also in some areas of the social and medical sciences as well as policymaking that have been influenced by that model. Scientists and policymakers now find themselves in quandaries about how to rate evidence and how to combine it from multiple sources. Setting a framework of "evidence-based policy" is one of the latest practical problems about evidence to arise at the interface of science and policy.

In this article, I first describe the main features of the traditional model(s) of evidence, indicating the idealizations of that model which I regard as doing more harm than good. I then proceed to outline an alternative model of evidence: *dynamic evidential functionalism* (DEF); on the DEF model, evidence is:

(a) *Functionalist:* evidence is defined by its functional role(s) within a scientific inquiry.
(b) *Complex/multifunctional:* evidence plays a number of different functional roles, irreducible to any particular role.
(c) *Dynamical:* scientific inquiries are processes with a beginning, middle, and end, and this dynamical structure is relevant for understanding the roles of evidence.
(d) *Contextual:* evidence is relative to the context of the particular scientific inquiry in which it functions.

In order to lay out the model, I first have to lay out the larger model of the dynamics of inquiry in which it is embedded. Then, I will set out a detailed account of the functional complexity of evidence.

Models of Inquiry and Evidence

Before laying out the positive account of DEF, I will describe the traditional model(s) of evidence, as well as several partial departures from the tradition. These departures provide inspirational building blocks for a systematic alternative, DEF.

The Traditional, Nondynamical Support Model

The default assumptions in many discussions of evidence in philosophy of science constitute a family of theories or models of evidence that I collectively refer to as "the traditional model." I will briefly try to describe here the main features of this problematic but common, often *implicit* model of evidence. An implicit model is an organized set of assumptions that plays a role in producing various kinds of judgments and reactions that nevertheless is not explicitly articulated or acknowledged by the one who relies

on it.[1] The traditional model of evidence began life as an *explicit* philosophy or set of such philosophies, and it continues to be relied on to some degree because of the lack of a systematic alternative. Reliance on such a model seems inversely proportional to the degree to which one has attempted to grapple directly with providing a theory of evidence. In contrast to the model I will defend, the traditional model is:

(a′) *Essentialist:* evidence is defined by some *essential* property that suits it to stand as evidence.
(b′) *Monofunctional:* evidence plays *only* one important functional role—justificatory support for hypotheses or theories.
(c′) *Nondynamical:* whatever the dynamics of scientific discovery might be, they are not relevant to understanding evidence; "support" is an abstract, timeless relation between some set of evidence and some hypothesis.
(d′) *Absolutist:* a bit of evidence is evidence regardless of context; anything that isn't fit to serve as evidence everywhere isn't fit to function as evidence anywhere.

I do not consider these four characteristics to be *necessary* conditions for membership among the traditional accounts that are contrary to DEF. Indeed, I hope to contrast my model to accounts that even hold weak versions of just one or two of these theses. But the more of these features a theory or model of evidence holds, the more problematic I regard it. Most central to the concerns of this article are versions of (b′) and (c′), but I include (a′) and (d′) for the sake of completeness.

Classical empiricism is a clear example of *essentialism* about evidence (a′): evidence is all and is only impressions or sense data that are immediately given and self-validating items of experience. Certain inductive logics provide another example when they require that evidence consist in *particular* propositions about (observed) matters of fact, while hypotheses are general propositions confirmed or falsified by such evidence. Most such accounts are also *absolutist* (d′), as are any accounts that require evidence to meet a noncontextual standard of certainty.

The traditional model is *nondynamical* (c′) in the sense that it doesn't depend in any important or interesting way on the *temporal complexity* of inquiry. This is not just a matter of historical context but rather is a matter of temporal structure. Traditional models of evidence may be temporal in the sense that they consider the belief available *at a time*, or that they take into account not only evidence and hypotheses but also *background beliefs* that are known to change over time. To carry through the physics analogy,

[1] I'm using "implicit model" here in a way consistent with the use of "implicit theories" in psychology (Sternberg 1985; Dweck, Chiu, and Hong 1995) and the discussion of "nonexplicit philosophies" in Drengson 1982.

these features might be said to constitute the *kinematics* of inquiry, whereas we're interested also in the *dynamics*, that is, the causal or functional structure of the motion.

The traditional model is *monofunctional* (b′) because it defines evidence according to a single function, the "support" relation it has to hypotheses, theories, claims, and so on. As this is perhaps the central, most problematic, and most widely accepted feature of the model, one might easily call it "the *support model*." Positivist and Popperian models from the middle of the twentieth century are clear specifications of the support model, as are some Bayesian accounts of evidence ("support" being understood as verification, falsifcation, or confirmation, respectively).

On the traditional account, "support" is an abstract relation that some set of evidence (beliefs, propositions, measurement records, and so forth) holds to some further hypothesis or claim, whether the nature of that relation be logical, statistical, or formal in some other sense. Given a set of evidence and some hypothesis, we should be able to identify whether that set supports the hypothesis, and perhaps how much (at least well enough to rank-order hypotheses on the basis of the evidence). Further, we can always ask *at a time* what *the* evidence supports, and there is always a determinate fact of the matter (though we may not know what the answer is). The fact is not dynamically sensitive—that is, sensitive to where we are in a process of scientific inquiry; it depends only on what the body of evidence is (and, perhaps, background beliefs). Evidence is that which justifies, and at a fundamental level it must be more certain, more justified, more secure than that which it justifies. That is, support is a one-way relation from evidence to hypothesis. Usually, evidence must also be independent of that which it justifies, lest the justification be illegitimate because circular.

While it may appear to be a caricature to some, in its basic outlines this model captures the basic background framework for most contemporary discussions of evidence, despite explicit denials of one or more features. In future work, I hope to show the ways in which the traditional model exercises an *implicit* influence over important debates about evidence; here I will concern myself with explicit commitments to aspects of the traditional model. While Eric Barnes's (2008) account of predictivism has a form of dynamism, Barnes also assumes throughout that evidence is monofunctional (b′), and so prediction is better than other evidence because it offers "stronger" support. Monofunctional evidence is almost ubiquitous—for example, Thomas Kelly (2008) remarks: "Thus, for the Bayesian no less than for the Evidentialist, it is evidence which justifies that which stands in need of justification." According to Jim Bogen (2010), "Much of the standard philosophical literature on . . . observational evidence tend[s] to focus on epistemological questions about its role in theory testing," which is treated almost entirely as a matter of one-way support or justification relations. Bogen and Woodward (1988, 1992,

2005; Woodward 1989) have argued that the role of observational data is not to support theories but rather to generate phenomena, whereas phenomena are used to support (or disconfirm) theories. This introduces a distinction between data and phenomena as types of evidence, and a distinction between two types of functions for types of evidence: supporting theories and generating conclusions about phenomena (though in specifying the latter, Bogen and Woodward sometimes seem to collapse the distinction between these roles). This looks like a major step forward toward a more complex, dynamic framework, though the data-phenomena distinction has been quite controversial (e.g., Glymour 2000). However, the role of phenomena in reaching conclusions in inquiry is pretty much the same as the traditional account of evidence.[2]

In the basic definition, the *Stanford Encyclopedia of Philosophy* entry for "Evidence" gets things right: "Evidence, whatever else it is, is the kind of thing which can make a difference to what one is *justified* in believing or (what is often, but not always, taken to be the same thing) what it is *reasonable* for one to believe" (Kelly 2008). This is perfectly neutral between traditional and DEF accounts. The way in which evidence makes a difference to what one is justified in believing (or better, concluding, asserting, judging) does not have to be by way of a monofunctional, nondynamical "support" relation, nor must we assume that evidence has any essential properties or that the relation of support is absolute. However, that same entry by Kelly frequently assumes that the way evidence makes a difference to justification is by way of such a relation. For example, consider the explanation of the total evidence condition:

> To the extent that what one is justified in believing depends upon one's evidence, what is relevant is the bearing of one's *total* evidence. Even if evidence E is sufficient to justify believing hypothesis H when considered in isolation, it does not follow that one who possesses evidence E is justified in believing H on its basis. For one might possess some additional evidence E', such that one is not justified in believing H given E and E'. In these circumstances, evidence E' defeats the justification for believing H that would be afforded by E in its absence. Thus, even if I am initially justified in believing that *your name is Fritz* on the basis of your testimony to that effect, the subsequent acquisition of evidence which suggests that you are a pathological liar tends to render this same belief unjustified. (Kelly 2008)

It is clear that Kelly here considers justification to be a one-way relation between a body of evidence and a hypothesis.

Part of the problem is a lack of recognition of the *existence* of a model at work in philosophical discussions at all. It is quite easy to default to an

[2] See also Ronald Giere's (2006) account of model testing, which, while adding some important layers, still comes down to a one-way, linear comparison between models of data and representational models similar to the traditional account.

ingrained model when one isn't aware of the existence of the model in the first place. Such models are the source of our claims about what is "obvious," "intuitive," or "almost true by definition" about evidence, but they are nonetheless revisable or replaceable.[3] Considerations of what seems obvious should bear little weight as compared to a theory or model that is descriptively and normatively fruitful.

Dynamical Models

The temporal dynamics of inquiry have received insufficient attention among those interested in the nature of evidence. While it is popular nowadays to talk about science in terms of "practice," a generally unexplored aspect of analyzing science-as-practice is the significant impact of understanding inquiry as a process that unfolds over time, that is, the temporal dynamics of scientific inquiry.[4] I am aware of only three detailed (types of) models of the temporal dynamics of science: the Kuhnian, predictivist, and pragmatist models. The first is the class of models developed by Thomas Kuhn (1996, originally 1962) and his followers (and here I include historicist critics of Kuhn, such as Laudan [1984, 1977] and Lakatos [1970], who provide different but related models at a similar scale [cf. Matheson 2009]). This type of model discusses the career of large-scale theories, traditions, or research paradigms that govern entire disciplines or subdisciplines over a large span of time. These models, however, are so large-scale and long-term that they are not useful for addressing current concerns in the literature on the nature of evidence. By contrast, current issues do *not* deal with the evolution of theories over the long run or with the revolutionary replacement of theories or paradigms. The questions at issue—from the experimenter's regress to contemporary concerns about the role of evidence in policy—are far more local than these accounts can address, having to do with with the role of evidence in single controversies within a discipline or paradigm. To put it differently, the theories of Kuhn and Lakatos are concerned with the dynamics of *theory change*, not the dynamics of *inquiry* (where there may often be no theory change).

A second type of dynamical model is sometimes called *predictivism*. A central claim in Lakatos's philosophy of science that has also received

[3] This is one of the great contributions to philosophy of John Dewey and Richard Rorty, to show that philosophy, like science, gets at the world through sophisticated but optional and replaceable *theories* or *models*, and that often what we need is not to answer certain questions or solve certain problems but to replace the theory in which that question or problem is stated. An important related idea is that of "metaincommensurability," discussed in Oberheim and Hoyningen-Huene 1997.

[4] Wayne Martin comes close in *Theories of Judgment* (2006) when he argues that the temporal complexity of *judgment* has been ignored, though in the end he has little specific to say about what this temporal complexity looks like.

some attention in more traditional confirmation theory is the idea that *novel prediction* is particularly important, that it is the type of evidence that matters most or that novel prediction has a certain special status compared to other evidence. Clearly, if prediction is what matters, and prediction is always prospective (there are nontemporal accounts of novel prediction, of course), then evidence depends on a certain kind of dynamic relation between hypothesis and evidence. It is telling, in terms of the hold of the traditional model, how many philosophers have found great difficulty explaining the importance of prediction, or have tried to reduce the dynamical quality of prediction to standard nondynamical approaches. The predictivist account threatens to reduce its dynamic complexity, however, if it downplays too much the role of prior observation evidence. Likewise, some versions of predictivism assimilate the function of predictive evidence to "support." In such accounts, predictive evidence simply lends *more*, *stronger*, or *better* support (see Barnes 2008, 1). More sophisticated accounts of predictivism may have more in common with the DEF model than with the traditional model.

A third dynamical model of inquiry is the pragmatist model introduced by Charles S. Peirce and further articulated by John Dewey (see Peirce 1877; Browning 1994; Dewey 1991 [originally 1938]; Hickman 1998). This model works best at the more local level of particular scientific inquiries, though it has some applications at the larger scale.[5] In Peirce's original formulation, *doubt* is a necessary condition for genuine inquiry of any sort, the sort of doubt that arises when previously held beliefs and habits of action[6] fail to guide one through a particular circumstance. Inquiry, then, is the process of responding to doubt in order to fix new beliefs and habits that resolve the doubt and allow activity to continue. The temporal structure of inquiry depends on this movement from uncertainty through investigation to settled belief. Dewey adopts this basic structure,[7] supplementing it with an account of the internal complexity of inquiry, the phases of reciprocal adjustment between fact gathering, hypothesis forming, and experimental testing that lead to what Dewey calls "warranted assertion" or "judgment" rather than merely "belief."

Functionalist Theories of Evidence

Essentialism and absolutism are the aspects of the support model whose fortunes have been the worst (as mentioned above, the parts of the model need not always go together); both have been explicitly denied in various ways, and they find few defenders among contemporary philosophers of

[5] I am not implying that there are important conflicts between Kuhnian models of scientific development or predictivism and the pragmatist theory of inquiry.

[6] This formulation is redundant if we adopt Peirce's definition of belief.

[7] In Dewey's terms, inquiry is a transformation of an indeterminate/problematic situation into one that is settled.

science (though their fortunes have been fairer among ordinary epistemologists). To deny essentialism is, in my terms, to assent to *functionalism* about evidence. Functionalism is most familiar from philosophy of mind, where it is the view that what a certain kind of mental state (for example, a belief) consists in is dependent *not* on its constitution (for example, an idea in my spirit substance or a configuration of neurons in my brain) but rather on the *role* it plays in my cognitive economy, most simply conceived as its causal relationships between perceptual inputs, behavioral outputs, and other mental states.[8] For example, a belief may be caused by certain perceptual inputs and inferential operations performed upon them and on other beliefs, it may have causal relationships with other beliefs, and, when combined with desires, it may cause certain behaviors. The sum of these relationships is the *functional profile* of a belief, and, if functionalism is true, then that profile is *all it is* to be a belief. As regards its constitution, that belief could be anything, including nonextended mind stuff, a configuration of neurons, or the circuitry of a suitably complex artificial intelligence. Likewise, a certain collection of neurons might well change from belief to something else if its functional role in the mechanism changes over time.

In its basic form Bayesian epistemology is a form of functionalism about evidence. For Bayesians, evidence is the E that figures in formulae like $P(H \mid E)$ (posterior probability of hypothesis H given E), $P(E \mid H)$ (likelihood of E on H), and so on, and is used to conditionalize beliefs, calculate degrees of confirmation/disconfirmation, and so forth (see Talbott 2008 for an overview). For all practical purposes, this is *all it is* to be evidence for Bayesianism. Often, it is implicitly or explicitly stated that E must be a *statement*; however, nothing in the basic theory requires this. It is just as reasonable to suppose that a telescopic image or the results of a computer simulation can function as evidence, so long as you can assign the needed probabilities to it. Likewise, even when considering statements, nothing requires that our evidence be a statement about *particulars*, or one referring to *observational facts*. Unless one adds restrictions to the contrary, anything that gives a conditional probability for H can serve as evidence, even something more general than H itself. Bayesianism even has a primitive sort of dynamism in that it requires one to update one's degrees of beliefs on acquiring new evidence; however, it is not at all clear what sort of events in actual, concrete scientific practice instantiate this abstract operation.

[8] See Levin 2009 for an overview. As I hope is obvious, the comparison to functionalism in the philosophy of mind is merely an analogy, to demonstrate the functionalist style of explanation. Nothing in my account hangs on the success or failure of functionalism about mental states. Functionalist theories have also been given for the ontology of colors (Cohen 2009), truth (Lynch 2000 and 2001; Wright 2005; Lynch 2005), and morality (Jackson and Pettit 1995, 1996), whose fortunes are likewise independent from my account.

Other Functions Besides Support

While Bayesianism is an example of a functionalist theory of evidence (and thus an advance, on my view), it is a rather simple, impoverished one. To see this, we can return to the analogy with philosophy of mind. The simplest version of a functionalist theory of mind (so simple as often to be regarded as a *precursor* theory to functionalism proper) is *philosophical behaviorism*. On that view, the functional profiles of a mental state are specified exclusively in terms of the relationships between perceptual inputs and behavioral outputs (that is, no causal relationships are allowed *between* mental states). So, to believe that the earth is round or to feel angry just is to respond with the right behavior given some stimulus. This sort of theory of mind is now widely regarded as too impoverished to do its job—that is, to account for what mental states are. Functionalists argue that this is because behaviorism ignores the relationships between mental states.

Likewise, I will argue that the traditional model, even functionalist versions like Bayesianism, is too impoverished to do the job. In this case, it fails to provide a theory of evidence that fully accounts for the ways that evidence functions to bring an inquiry to successful resolution. It is too impoverished because it only allows for a *single* functional role for evidence, the role of supporting a hypothesis (theory, claim, and so on). By contrast, as I argue in the rest of the article, I think we can point to a number of equally important roles that evidence plays in inquiry.

This point is common among philosophers of scientific experiment. As Ian Hacking has said, "Experiments, the philosophers say, are of value only when they test theory. . . . So we lack even a terminology to describe the many varied roles of experiment" (Hacking 1982, 71). In a similar vein, Allan Franklin has argued, "Experiment plays many roles in science. One of its important roles is to test theories and provide the basis for scientific knowledge. It can also call for a new theory. . . . Experiment can provide hints about the structure or mathematical form of a theory, and it can provide evidence for the existence of the entities involved in our theory . . . it may also have a life of its own, independent of theory: Scientists may investigate a phenomenon just because it looks interesting. Such experiments may provide evidence for future theories to explain" (Franklin 2002, 1). My account goes further by enumerating the various roles of evidence (observational *and* experimental) and showing how they fit together to guide inquiry to successful conclusion.

The Dynamics of Inquiry

In this section, I outline the DEF model of evidence as an alternative that is truer to the complexities of scientific inquiry and avoids the vicious

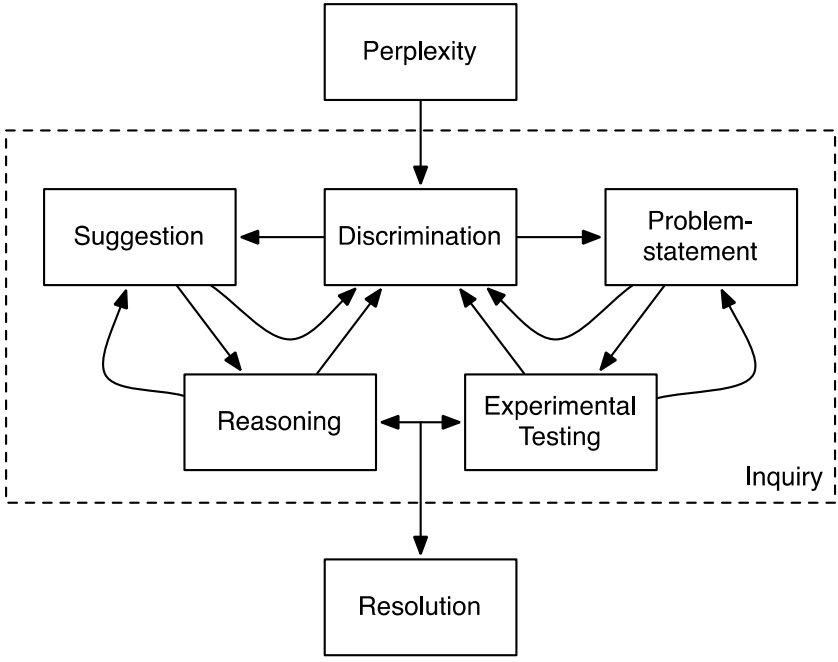

FIGURE 1. Boxology of the functional dynamics of inquiry. (Connections between phases have been simplified for clarity.)

simplifications of the support model. To begin, I will give a description of the dynamics of inquiry in which, according to the DEF model, evidence is embedded. The purpose of this section is to describe the functionalist model of the dynamics of inquiry, including the complex functional roles for evidence within that process.

In the main outlines, the dynamics of inquiry[9] can be described by a number of interlocking phases (see figure 1).

1. *Inquiry begins with a felt perplexity*. There are many types of perplexity, but they are not in general a mere state of ignorance on the

[9] This model is loosely inspired by Dewey's version of the pragmatist theory of inquiry (see Brown 2012). It is not, so far as I can see, committed to any of the more controversial pragmatist claims about truth or meaning. If the following sounds a bit like the description of "The Scientific Method" from an elementary science textbook, don't be too surprised: Dewey was influential over the shape of science education, especially in America, though his ideas have been vulgarized. Careful scholars should not consider the association a black mark against the view.

part of the inquirer. Rather, the objective state of the science—which may include theoretical frameworks and concrete models, techniques of observation and sets of data, methods of prediction and expectations of inquirers, and so on—is contradictory, confused, indeterminate, or in tension. There are conflicting tendencies within the field of investigation at the moment, a major discoordination of the practice, and this requires investigation. Hence, there are affective, practical, and objective aspects of the perplexity or indeterminacy. (Contrast perplexity with the smooth application of some theory or technique to a case with immediate success.)

2. *Discrimination. Operations of observation* must take place in order to take stock of the situation that evokes inquiry. We need to gather data on the situation that helps us begin to understand the problem at hand and the conflicting tendencies in our response to it. Prior to the interruption that begins the inquiry, the distinction between conceptual and observational materials is vague. In habitual activity, we tend to run together the facts and our ideas about them, and we behave as if there is no difference between the model and the thing. This is a reasonable and necessary way to go on, so long as no problems arise. But problem-solving inquiry requires that we discriminate (a) the factual versus conceptual materials we have to work with, and (b) features of the subject matter in question. These constitute the relevant features of the situation that has become perplexed, and are required to determine the nature of the problem and our response.

3. *Statement of the problem*. The situation must be assessed in order attempt to *formulate a problem statement* that adequately captures the given perplexity. Scientific inquiry does *not* begin with a set problem or question at which science is directed. The agenda of inquiry cannot be set by fiat. Where no genuine perplexity exists, there is no room for scientific inquiry. Where perplexity does exist, the problem cannot be accurately or adequately stated ahead of time; the statement of the problem is a phase of the inquiry itself, and it evolves as the inquiry is pursued and more adequate and sophisticated observations are made.

4. *Suggestion of hypotheses*. The first pass at determining the factual conditions of the situation, the conceptual possibilities in our theories, and the terms of the problem *suggests hypotheses* for solving the problem. Forming a problem statement and suggesting a hypothesis are coordinate activities. The former connects to the settled features of the situation in which a tension arises, while the latter connects to some possibility for further action that resolves the tension. If the factual side of inquiry pertains to what has been determined, then the hypothetical (conceptual, theoretical) side of inquiry pertains to what is possible. (This is the process that theories of *abductive reasoning* are trying to analyze.)

5. *Reasoning*. A reciprocal process of *coordination* of observed facts and theoretical-hypothetical ideas is undertaken. There are several aspects of this process that depend on each other and need not proceed linearly.

a. Background theoretical materials, well-tested models, and other conceptual resources are brought to bear on the problem at hand.
b. Hypotheses are developed by processes of reasoning to be more specific and relevant to the case at hand, to be in greater concert with more general theoretical materials, to suggest further operations of observation, and to take into account the evolving body of data and statement of the problem.
c. New observations are made in response to the evolving series of hypotheses and theoretical ideas, to answer questions posed by them, and to fill in information needed to specify the relevant features of the ideas.
d. From the set of putative evidence constructed so far, certain facts are selected or amplified as *relevant*, while others are rejected as irrelevant, imprecise, or poorly executed, or are explained away as effects of interfering phenomena that must be controlled.
e. The statement of the problem is refined to reflect the changing understanding of the situation and the evolving series of hypotheses.

6. *Experimental testing*. A series of controlled, limited, or tentative, *experimental applications* of the hypotheses are made in order to evaluate their probable efficacy in solving the problem. Earlier experiments can suggest more refined experiments, or the necessity of further articulating data and hypothesis, or the need to "go back to the drawing board."

7. *Resolution*. The aim and final product of inquiry is a judgment of how to proceed, how to resolve the perplexity that initiated inquiry. Inquiry continues until one of the hypotheses is judged to be the most warranted among the alternatives, and the alternatives have been more or less ruled out. To put it differently and more prospectively, the inquiry proceeds until a point of resolution so settled that the conclusion can be used as a reliable means to further inquiries. A judgment of warrant is a judgment about the adequacy of the hypothesis to solving the problem. Such a judgment is impossible without to some degree undergoing this process of inquiry (otherwise, it would be merely a reflexive response), and ideally the process of inquiry must be exhausted to the point that no doubt remains about the hypothesis, and the conflicting tendencies of the situation have been resolved and coordination has been restored (at least, for the moment, for the most part).

This is obviously an *idealized* picture of the conduct of inquiry.[10] It is no *a priori* imposition, however; it is informed by reflection on the complexities of the history of science and scientific practice. It is a *normative-explanatory* model, attempting to capture, explain, and make available the lessons of successful inquiries past, as well as incorporating general cognitive and epistemic considerations. The proof of this model is in its power to give us a more successful understanding of the uses of evidence and to resolve or dissolve problems of evidence that arise. If the account seems unduly simple, all the better; my main point is that almost all philosophers working on these problems are using an even simpler model, and I would be happy to entertain even more complex alternatives. I do believe that we have to make some sacrifices in the direction of simplification in order to have a usable, systematic framework, and this too has guided my focus.

Evidence on the Inquiry Model

Having laid out and explained the functional dynamics of inquiry, I can now set out the basic picture of the DEF model (see figure 2). First, in the model of inquiry I've been discussing, *functionalism* guarantees that many different types of things count as evidence: not only particular, observed facts, but also historical developments, statistical analyses, general trends, "phenomenological" laws, and anything that adequately serves some part of the functional roles of evidence and some stage of the inquiry. Second, it is important to notice the very different roles that evidence plays in the course of an inquiry. In many contemporary accounts, evidence is, if not monomodal (or essentialist), at least monofunctional: all evidence serves as a test of a theory or hypothesis, and it confirms or disconfirms it, or renders it more or less plausible, probable, or credible. On my account, evidence is not only multimodal but serves a variety of purposes:

I. *Observational evidence* serves a variety of roles related to the way that operations of inquiry depend on an understanding of the present conditions that have led to some perplexity.
   A. Through *discrimination*, it provides information about the conditions of the problematic situation.

[10] It is also worth pointing out, I think, that not only is there plenty of inquiry that isn't particularly concerned with high-level theories, there are also activities in science that do not constitute problem-solving inquiry at all—those involving education, training, exploratory "problem-finding" research, to name a few. Both of these insights are tied up with the experimentalist slogan "Experiments have a life of their own." I have little to say about the latter set of activities, except to say that they are not primarily evidence-gathering activities, except retrospectively insofar as they turn out to spur inquiry.

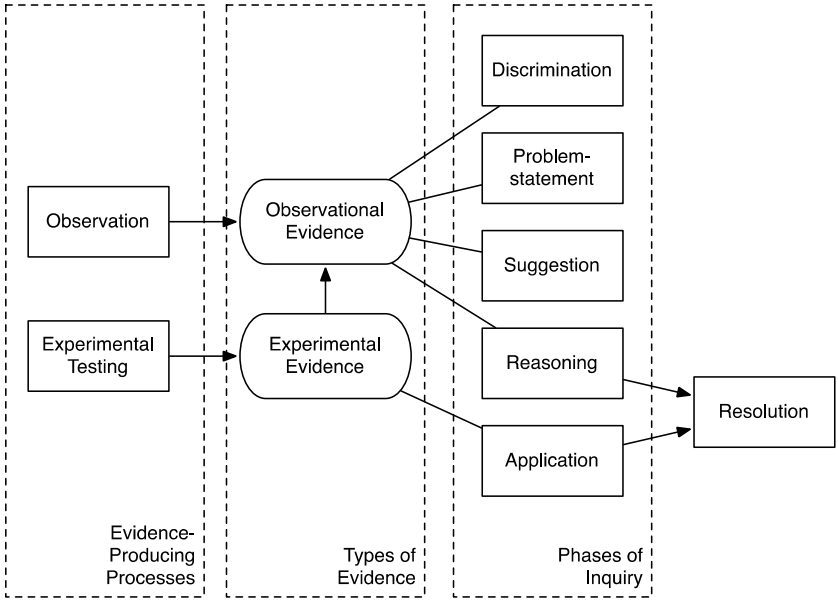

FIGURE 2. Boxology of the Dynamic Evidential Functionalism model.

B. It helps locate and state the *problem*.
C. It *guides* speculation and hypothesis formation.
D. It guides *reasoning* in order to help *eliminate*, *specify*, *clarify*, or *improve* our original hypotheses.

II. *Experimental evidence* serves the additional role of:
A. Tentative application of a developed hypothesis to check its consequences for future action and inference.
B. Generation of further observational evidence (generally of a very precise but specialized nature).

Experimental evidence in this sense, again, can be of many different kinds: not just controlled manipulations in a laboratory but also "natural experiments" that function *as if* there were a manipulation, as well as cases such as a change in public policy in a particular area whose consequences are then tracked to determine whether the application is successful. This is because it is the *functional role*, not details about the production of the evidence, that determines which evidence is experimental.

In every case, it is not some abstract or formal relation between the evidence and the hypothesis by which the evidence serves to justify the

hypothesis. The formal and symbolic is only one side of evidence. It is rather a very concrete process of transforming a perplexity into a resolution which evidence is instrumental toward, and which ultimately justifies any final judgment of the inquiry.

This model has several benefits. First, it is more faithful to the complexities of scientific practice, in that it refuses to reduce the (philosophically relevant) activities of science to judgments of which hypothesis is best supported by the body of evidence, that it makes clear the ways in which data gathering is directed toward various ends, and that it reduces some of the mystery in the process of hypothesis generation by proposing that hypotheses latch onto possibilities suggested by the facts of a particular situation. Second, it retains and strengthens the philosophical attempt to explain why scientific methods work, by describing the phases of scientific inquiry and how they work together in problem solving. Third, it provides the strongest way of responding to the various problems of the "empirical basis" (for example, epistemic status of evidence, theory ladenness, experimenter's regress), which I hope to show in future work. This more complex model of the functions of evidence can be used for a multiscale analysis of the *functional fitness* of evidence, which gives us a way of assessing the adequacy of it to stand *as* evidence.

How can we be certain that some body of putative evidence *is* evidence? For traditional empiricist accounts, the answer appeals to the incorrigible and indubitable nature of particular sense data. In contemporary accounts, the assumption is usually that evidence has a high degree of credence relative to our initial credence in hypotheses. On the DEF account, putative evidence and suggested hypotheses are both judged by their ability to be brought into mutual coordination, leading to a solution of the original problem. Many "facts" may be collected along the way and may aid in various functions in the course of the inquiry, but they may eventually be discarded as being inadequate and replaced by new facts. At the end of an inquiry, the inquirer produces a chain of reasoning from general considerations to a specific hypothesis, as well as a body of evidence in support of that hypothesis. The chain of reasoning does not represent the actual steps in the inquiry that produced them, nor does the body of evidence include every bit of data gathered along the way. Chains of reasoning and the final body of evidence are as much the conclusion of the process as the final judgment, and they are what we see reflected in ordinary scientific articles. That these final products cohere is essential, but mere coherence is insufficient: they must also cooperate to resolve the perplexity that spurred the inquiry. This is nontrivial because, as we recall, the *perplexity* is not merely verbal or intellectual but has affective, practical, and objective elements. Genuine recoordination must be achieved. Evidence functions in the complex and dynamic ways laid out above to move an inquiry toward resolution; the evidence itself

is thus evaluated in terms of its functional fitness in the process aimed at doing so.

Conclusion

My purpose in this article has been to motivate the use of more complex, temporally dynamic, functionalist models of evidence, and to provide one such model. The model is beneficial in that it provides a realistic and plausible account of scientific practice that avoids some of the problems of the gross oversimplications in traditional models of evidence; it is nevertheless sufficiently general to provide some understanding of science and some explanation of which strategies work well, and, as I will argue in future work, it provides the strongest response to problems associated with the empirical basis of science. If I have oversimplified the nature of scientific evidence in turn, all the better, since an even more complex account of the dynamical development of inquiry and the variety of evidential functions will serve my purposes just as well, if not better, so long as such an account remains manageable. With such an account in hand, the next step is to show how it can better cope with a variety of problems of evidence.

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