Abstract: Taking a cue from remarks Kuhn makes in 1990 about the historical turn in philosophy of science, I examine the history of history and philosophy of science within the British philosophical context of the 1950s and early 1960s when ordinary language philosophy's influence was at its peak. Specifically, I make the case that the ordinary language philosophers' methodological recommendation to analyze actual linguistic practice influences several prominent criticisms of the deductive-nomological model of scientific explanation and that these criticisms are related to the historical turn in philosophy of science. I think such connections are especially clear in the work of Stephen Toulmin, who taught at Oxford from 1949 to 1954, and Michael Scriven, who completed a dissertation on explanation under Gilbert Ryle in 1956. I also consider Mary Hesse's appeal to an ordinary language-influenced account of meaning in her account of the role of models in scientific reasoning. I think there are two upshots to my historical sketch. First, it fills out details of the move away from logical positivism to more historical- and practice-focused philosophies of science. Second, questions about linguistic meaning and the proper targets and aims of philosophical analysis are part and parcel of the historical turn, as well as its reception. Looking at the philosophical background during which so-called linguistic philosophers also had a hand in bringing these questions to prominence helps us understand why.

1. Introduction

In his presidential address to the 1990 meeting of the Philosophy of Science Association, Thomas Kuhn reflects on the historical turn in philosophy of science. He says, "That's a transition for which I get far more credit, and also more blame, than I have coming to me. I was, if you will, present at the creation, and it wasn't very crowded. But others were present too: Paul Feyerabend and Russ Hanson, in particular, as well as Mary Hesse, Michael Polanyi, Stephen Toulmin, and a few more besides. Whatever a *Zeitgeist* is, we provided a striking illustration of its role in intellectual affairs" (1990, 3). At the same time this historical turn was happening in parts of philosophy of science leading to the 1962 publication of *The Structure of Scientific Revolutions*, a different shift was happening in so-called linguistic philosophy. This shift, initiated by Ludwig Wittgenstein, P.F. Strawson, and J.L. Austin, among others, was away from ideal language analysis and towards ordinary language analysis. These developments share in common a move away from understanding science and language as a set of logically related sentences to understanding science and language as consisting of practices governed by rules, habits, and conventions. Moreover, these practices resist reduction to a collection of statements and their logical relationships.

In this paper, I examine the history of history and philosophy of science[1] and situate the historical turn within the British philosophical context of the 1950s and early 1960s. Specifically, I make the case that the ordinary language philosophers' methodological recommendation to analyze actual linguistic practice influenced several prominent criticisms of the deductive-nomological (D-N) model of scientific explanation and that these criticisms are related to the historical turn in philosophy of science. I think such connections are especially clear in the work of Toulmin, who taught at Oxford from 1949 to 1954, and Michael Scriven, who completed a dissertation on explanation under Gilbert Ryle in 1956. I also briefly touch on Hesse's appeal to an ordinary language-influenced account of meaning

[1] Or, HOHAPS.

in her contributions to the historical turn. However, to keep the scope of my argument fairly narrow, the bulk of the paper focuses on fleshing out Toulmin's and Scriven's ordinary language-influenced methodological approach to scientific explanation and its connection to the historical turn.

I think there are two upshots to this historical sketch. First, it fills out details of the move away from logical positivism to more historical- and practice-focused philosophies of science during the 1950s and 1960s. The D-N model of explanation was a central topic of debate for philosophers of science and the sustained criticisms of it, bookended as they are by W.V. Quine's "Two Dogmas of Empiricism" in 1951 and Kuhn's *Structure* in 1962, deserve attention in our histories of history and philosophy of science. Second, questions about linguistic meaning and the proper targets and aims of philosophical analysis are part and parcel of the historical turn, as well as its reception. Consider, for example, debates about semantic incommensurability and whether or not the distinction between the contexts of discovery and justification is methodologically sound. Looking at the larger philosophical background of the time during which so-called linguistic philosophers had a hand in bringing these questions to prominence helps us understand the centrality of questions about meaning and the proper targets of analysis in philosophy of science at the time.

A note: My aims are to reconstruct how the D-N model was received and criticized by philosophers of science influenced by ordinary language philosophy and to explore their possible connection to the historical turn. I do not consider if these criticisms succeed.

1.1 Structure of the paper

In §2, I reconstruct Strawson's ordinary language criticisms of logical analysis as they are representative of the philosophical background in 1950s British philosophy where most of the philosophers Kuhn mentions were educated and/or taught. In §3, I consider Scriven's and Toulmin's criticisms of the D-N model of explanation, which exhibit ordinary-language-style skepticism about the ability of formal tools to shed light on philosophical issues in the sciences. In §4, I relate these criticisms to an interest in the history of science. In §5, I look at Hesse's contributions to the historical turn and her connection to ordinary language philosophy. In §6, I conclude by looking at recent appeals to ordinary language philosophers by historians and philosophers of science.

2. Philosophy of language & philosophy of science

Methodological debates about the proper targets and aims of philosophical analysis were common during the 1930s–1950s. These debates often took place between philosophers we might now think of mainly as philosophers of language and philosophers we might now think of mainly as philosophers of science.[2] In the early 1930s, Rudolf Carnap produces a series of talks and papers widely taken to represent the type of analysis favored by logical positivists. Consider the first paper in the first volume of *Philosophy of Science* in 1934, where Carnap states, "*Philosophy is the theory of science*….Philosophy deals with science only from the *logical* viewpoint. *Philosophy is the logic of science*, i.e., the logical analysis of the concepts, propositions, proofs, theories of science" (1934, 6).

[2] For papers central to these debates, see Richard Rorty (1967).

Though sympathetic to the idea that the philosopher employs some form of analysis, many British philosophers pushed back against both the aims and targets of the logical positivist conception of analysis articulated in Carnap's works.[3] In particular, many rejected what Michael Beaney calls "*transformative* or *explicatory*" analysis, of which Bertrand Russell's theory of definite descriptions is a paradigmatic example (2007, 2). In fact, according to Carnap, it was from "Russell…[that] most of us have learned the use of a symbolic language for the clarification and solution of philosophical problems" (1963, 939). Transformative, or explicatory analysis calls for taking inexact concepts and the statements in which they occur and situating them within a well-ordered formal system in which their logical properties are clear and exact. For example, Russell says the aim of his theory of descriptions is "to find a more accurate and analysed thought to replace the somewhat confused thoughts which most people at most times have in their heads" (1957, 388). Carnap's later method of explication has the same aim: "the replacement of a pre-scientific inexact concept…by an exact concept…which frequently belongs to the scientific language" (1963, 933).[4]

Among others, Strawson is a central figure in the pushback against transformative analysis in 1950s British philosophy.[5] I focus on his criticisms, rather than others, given that they are directly about the proper aims and targets of philosophical analysis.[6] On Strawson's understanding, the logical positivists are committed to the view that logic "provides a skeleton language in which the meaning of every element is absolutely precise, and the articulation of those elements absolutely clear. By using this framework, this basic linguistic apparatus, other systems of concepts can be constructed in which the mutual relationships of the parts will have just the same clarity and precision as in formal logic itself" (1956, 101). On this view, logical analysis is preferred given "the looseness, the untidiness, the shifting complexities of common speech" (102). Formal logic, though, provides a way "to construct clear models of language in which all the essential logical relations of our concepts can be made plain, while the irrelevant tangles of actual usage are cut away" (102). Translating ordinary and scientific language into a logically perspicuous formal language will help clarify what otherwise might be unclear.

Strawson acknowledges that translating ordinary or scientific expressions and statements into formal logic has its uses. However, he maintains its success often "is purchased at too high a price: the price of divorce from the conceptual realities of common speech" (104). In its place, Strawson recommends ordinary language analysis: "that of coming to understand philosophically puzzling concepts by carefully and accurately noting the ways in which the related linguistic expressions are actually used in discourse" (104).

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[3] See Stebbing (1933, 1934) for some of the earliest criticisms.

[4] Hans Reichenbach (1938, 6) characterizes successful rational reconstruction in a similar way.

[5] Strawson's (1950) criticism of Russell's theory of descriptions is particularly important. See also Strawson's (1963) contribution to the *Library of Living Philosophers* on Carnap, first written in 1954.

[6] Two more reasons: Scriven mentions Strawson's influence on his criticisms of the D-N model, and Hanson acknowledges Strawson in *Patterns of Discovery* as someone who provided "friendly guidance" (1958, ix).

For the ordinary language philosophers, the ways in which we commonly talk are already in good working order. Strawson claims "common speech is subjected to the severest of all tests for efficiency, as a medium for the expression and communication of our thoughts—the test of constant use" (103). Insofar as we are interested in "gain[ing] an understanding of the concepts and categories in terms of which we carry on our thinking", Strawson thinks "it is no good turning our backs on…actual behaviour, constructing a clockwork model form an engineer's designs and then studying that….If we want to know how they [i.e., our concepts] work, we must watch them work" (103). With this in mind, the philosopher should analyze ordinary language to understand the rules, habits, and conventions underwriting the good working order of everyday discourse.

Toulmin places Strawson's worry that whatever clarity we might achieve through explicatory analysis using a constructed language comes at the price of actually capturing, or worse, distorting what happens in ordinary discourse into a philosophy of science context. He draws attention to "the topics one finds discussed in books of logic. Induction, Causality, whether the results of the sciences are true or only highly probable, the Uniformity of Nature, the accumulation of confirming instances, Mill's Methods and the probability-calculus" (1953, 9–10). Toulmin then complains about this focus, saying, "It is not that the things that are said are untrue or fallacious, but rather that they are irrelevant: the questions which are so impeccably discussed have no bearing on physics. Meanwhile the actual methods of argument physical scientists employ are only rarely examined" (10).[7] To give an example of what he has in mind, Toulmin criticizes the ways laws of nature are grouped with other generalizations for logical purposes. He suggests that "unless one sees in some detail what the status of laws of nature in practice is, one cannot decide whether this is a proper conclusion or no" (10). He then claims, "Similarly, one can continue to write about 'Causation and its Place in Modern Science' indefinitely, if one fails to notice how rarely the word 'cause' appears in the writings of professional scientists" (10). Recalling Strawson's points about ordinary language, Toulmin claims that one cannot know if the logical analysis of science is successful in clarifying supposedly unclear concepts unless one has already looked at how science is actually conducted, including examining how scientists talk.

Scriven is also skeptical about the promise of logical analysis to shed light on problems in the philosophy of science. Scriven contrasts two approaches to questions about explanation, "context analysis" and "content analysis" (1958, 99). Drawing attention to the influence of Strawson's *Introduction to Logical Theory* in a footnote, Scriven makes clear he favors context analysis, which "regards the rigor of symbolic logic as partly spurious" since we are dealing with "already existing concepts and problems" that are used outside the logical context, and those contexts "are not governed by rigorous rules and definitions" (100).[8] This recalls a claim made by ordinary language philosophers: ordinary language has

[7] Toulmin (1958/2003, 1–2) also worries about "the science of logic" and its applicability to the ways "in everyday life, we actually assess the soundness, strength and conclusiveness of arguments."

[8] Strawson claims most treatments of logic "deal comparatively sketchily, and often rather misleadingly, with the relations between the formal systems they expound and the logical features of ordinary discourse (1952, i).

no *exact* logic—which is not to say it has *no* logic—and a close examination of actual linguistic practice makes this clear. Here, Scriven suggests that the context analyst adopt a different method, namely the method of comparison. This method compares different cases in which the meanings of the terms are clear in order to help "[discover] the function of the expression(s) being considered" (100).

Motivating this methodology in a footnote, Scriven claims "the most important argument for context analysis depends on the vast amount of information about the meaning of a term that is implicit in a well-described example of its use over and above that which can be summarized in any manageable form by a definition or set of definitions" (101). This should remind us of Strawson's claim that ordinary language has passed the stringent test of use and so likely contains much of what we might already be looking for.[9] An important parallel between Strawson's ordinary language criticisms of logical positivism and the viewpoints favored by Toulmin and Scriven, then, involves a methodological recommendation to turn from the logical analysis of scientific language to the examination of scientific and linguistic practices. Logical positivist analysis brackets these questions by distinguishing between the epistemological and the psychological, the syntactic and the pragmatic, justification and discovery. However, as Kuhn (1962/2012) argues, these are not elementary methodological distinctions, but presuppose substantive views about linguistic and scientific practices.[10] This point is behind the calls to attend to practice: only by investigating actual linguistic and scientific practices can we see if these distinctions bear out. In this spirit, Toulmin and Scriven suggest that before philosophers of science reject parts of scientific practice or historical questions as beyond the province of philosophical analysis since they do not fit neatly in logical schemas, we should attend to concrete examples from science, including history, to see if they admit of analysis. I turn now to showing how these points play out in criticisms of the D-N model.

3. Criticizing the D-N Model

In his overview of four decades of philosophical debate about scientific explanation, Wesley Salmon points out the original article in which Hempel and Oppenheim lay out the D-N model "is an outstanding example of the use of an artificial formal language for purposes of explicating a fundamental scientific concept" (1989, 35). On this model, successful explanations have the structure of valid deductive arguments in which a general law appears as one of the essential premises (explanans) for deducing the conclusion (the explanandum). Successful explanations also have true premises; a proper explanation is both valid and sound. Further, proponents of the D-N model maintain that explanations share the same argumentative structure and logical features as predictions. Prediction and explanation are structurally identical: every prediction is potentially an explanation and every explanation is potentially a prediction. The main differences between explanation and prediction are merely pragmatic, reflecting differences in the state of knowledge we happen to have at the time.

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[9] Compare with Austin's defense of ordinary language analysis: "our common stock of words embodies all the distinctions men have found worth drawing, and the connexions they have found worth marking, in the lifetimes of many generations" (1956, 8).

[10] Paul L. Franco (2018) examines similarities between Austin's and Strawson's criticisms of logical positivist analysis and Kuhn's criticisms in *Structure*.

In 1950s British philosophy, ordinary language philosophy was at the height of its influence, and, so, the D-N model was an obvious target for ordinary-language-style-criticisms. As Salmon notes, "For anyone who focuses primarily on the ordinary uses of language, the pragmatic aspects [of explanation] will be most conspicuous" (36). But it is precisely these aspects that the D-N model deems irrelevant to logical analysis and that Toulmin and Scriven think are important. I highlight three related strands in Toulmin's and Scriven's challenges. The first concerns the importance of context, especially the understanding of the audience, to determining if a successful explanation has been given. The second concerns their denial of the symmetry between explanation and prediction. The third concerns their account of the non-deductive role laws play in scientific reasoning.

I highlighted Scriven's characterization of context analysis at the end of §2.2. In line with ordinary language methodology, Scriven adopts a contextual approach that emphasizes "the meaning of terms or concepts or logical problems can only be thoroughly understood if we include a meticulous examination of the circumstances in which they occur, rather than relying on a relatively rapidly extracted formalization of their apparent internal logical features" (1958, 100). This form of analysis aims to clarify concepts and their uses by "the process of exhaustive comparison with cases where the meaning of the concepts is clear" (100). Employing this methodology, Scriven begins his 1962 paper on explanation with examples of explanations given in response to what- and how-questions. In doing this, Scriven denies that explanations are distinguished by being answers to why-questions (1962, 173–6). He then denies that explanations are in some important sense always something over and above mere descriptions. Scriven claims, "the difference between explaining and describing, does not…consist in explaining being something 'more than' or even something intrinsically different from informing or describing, but in its being the appropriate piece of informing or describing, the appropriateness being a matter of its *relation to a particular context*" (176).

A key part of the context includes the interests and state of knowledge of the audience requesting an explanation. Often, an audience requests an explanation because they lack understanding of the topic under discussion. Scriven thinks that "in order to be called an explanation at all [an exchange] must be capable of making clear something not previously clear" (175). For example, in considering whether or not complete explanations are possible, Scriven claims we must consider "the existence of a certain level of knowledge and understanding on the part of the audience or inquirer" and so "what counts as complete will vary from context to context within a field" (202). This is because "levels of understanding and interest define areas of lack of understanding and interest, and the required explanation is the one which relates to these areas and not to those other areas related to the subject of the explanation but perfectly well understood or of no interest (these would be explanations which could be correct and adequate but inappropriate)" (202–3).

Toulmin makes a similar point in a discussion about the role of the rectilinear propagation of light in explaining the length of a shadow cast by an object when the sun is at a certain height. He is particularly interested in what sort of inference takes place in such an explanation. Toulmin cautions against "jump[ing] to conclusions about the logical form," maintaining it is "[n]ot a deduction from a generalization to an instance…for, considered as a generalization, the principle is just not true: in diffraction, refraction and scattering light ceases to travel in straight lines" (1953, 25). The principle of the rectilinear propagation of

light is not simply a true generalization that serves as an essential premise in a deductive argument under which we subsume particular events. Instead, it instructs us in how to represent and explain the phenomena in question, e.g., "by drawing a straight line at the appropriate angle to the line representing the wall; that this line will mark the boundary between light and shade" and so on (27). For Toulmin, the rectilinear propagation of light provides "an intelligible model" (34–5) for representing the relevant phenomena. Here we come to the key point about understanding. Toulmin says that while "the mathematical theory may be an excellent way of expressing the relations we study…to understand them—to 'see the connection' between sun-height and shadow-depth, say—one must have some clearly intelligible way of conceiving the physical systems we study. This is the primary task of models: for know-how and understanding both mathematics and models are wanted" (35).[11]

The emphasis on understanding relates to Scriven's and Toulmin's denials of the D-N model's claim that predictions and explanations are structurally identical since they can be represented as arguments sharing the same logical features. As a first step to challenging the thesis of structural identity, Scriven says "there certainly seem to be occasions when we can predict some phenomenon with the greatest success, but cannot provide any explanation of it. For example, we may discover that whenever cows lie down in the open fields by day, it always rains within a few hours" (1962, 176). While this seems the basis for a perfectly fine prediction in everyday life, the generalization's "ability to forecast…does not constitute an understanding of a phenomenon" (177). Moreover, we might be able to explain, and hence achieve understanding of an event, without being able to predict that event, e.g., Scriven's well-known example of the collapse of a bridge, the cause of which, metal fatigue, could not have been discovered until after the collapse (181–6). In an earlier paper, Scriven makes essentially the same point appealing to a scientific example: while "the attempts by Darwin and others to encapsulate the principles of evolution in the form of *universal* laws and base *predictions* on them" fail, the explanations Darwin offers in terms of fitness had "tremendous efficacy" (1959, 477).

Toulmin raises similar objections. He begins *Foresight and Understanding*'s substantive argument asking: "'Has Science a single purpose and aim, or is it a multi-purpose activity?'" (1961, 18).[12] Toulmin considers this provisional answer: "'The better theory, idea, system, or hypothesis is one that explains more'" (21). Taking this as the aim that unites different sciences, Toulmin goes on to criticize the D-N model in all but name, putting in his sights the supposed symmetry between prediction and explanation. Fleshing out the provisional answer, Toulmin attributes the following to his unnamed opponents: "'Are not explanations essentially the means of making *predictions*?...The purpose of an explanatory science is to explain—that is, to lead to predictions; and the merits of a scientific theory are in proportion to the correct predictions which it implies'" (22).

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[11] Toulmin connects this to how a scientist "comes to understand such a principle…: he learns…in what circumstances and in what manner to draw diagrams or perform calculations which will account for optical phenomena, what kind of diagram to draw, or calculation to perform, in any particular case, and how to read off from it the information he requires" (27).
[12] Toulmin's contrasts this question with another, Wittgensteinian one: "What is sport?" Vasso Kindi (2017) discusses Wittgenstein's influence on *Foresight and Understanding*.

Toulmin criticizes this "predictivist thesis" (24) by raising counterexamples and considering possible amendments in response. So, for example, like Scriven, Toulmin claims evolutionary theory is explanatory without being predictive. On behalf of the predictivist thesis, he appeals to the postdictions evolutionary theory licenses, but worries that thinking of these as predictions spreads the ordinary notion of 'prediction' too thin (25–7).[13] That said, Toulmin grants for the sake of argument a revised thesis that "now embraces the ability to infer the occurrence of any event in question—whether it has already happened, is happening now, or is going to happen in the future" (27). His main argument against this revision does not appeal to the ordinary meaning of 'prediction,' but appeals to the history of astronomy. He argues "some of the most successful techniques for making…predictions have largely lacked the power to explain the events so forecast, having been worked out by trial-and-error and without any theoretical basis; whereas some respectable theories about the very same natural happenings have been predictively almost entirely fruitless" (27). To illustrate his point, Toulmin contrasts the predictive prowess of Babylonian astronomers with the explanatory ambitions of the Ionians, who wanted to *understand* why the heavens behaved the way that they did. On Toulmin's interpretation, the Babylonians were excellent at calculating in ways leading to accurate predictions, but failed to extend this method "to earthquakes, plagues of locusts, and other omens," and "the success and failures of their forecasting techniques remained at the time unexplained" (29). For Toulmin, while the "Babylonians acquired great *forecasting-power*…they conspicuously lacked *understanding*" (30).

In contrast, "The astronomy of the early Ionians…consisted almost entirely of speculation, theory, and interpretation, and scarcely at all of 'prediction'—either prospective or retrospective" (29). Instead of predictions, the Ionians attempted to *understand* the motions of the heavens using "all kinds of homely analogies" (29) as intelligible models. The predictivist thesis would entail that the Babylonians, who made predictions, were scientific, whereas the Ionians, who made no predictions, were not. Toulmin rejects this conclusion. For him, "Forecasting…is a craft or technology, an application of science rather than the kernel of science itself" (36). The type of explanatory understanding the Ionians were after, however, has to do with establishing ideals of natural order that are the kernel of science itself. Such ideals determine what phenomena do and do not need explaining. For Toulmin, they constitute "explanatory paradigms" (43) that guide us in tackling deviations from the ideal.

Summing up a discussion of ideals of natural order in the history of dynamics, Toulmin says, "any dynamical theory involves some explicit or implicit reference to a standard case or 'paradigm'. This paradigm specifies the manner in which, in the natural course of events, bodies may be expected to move" (1961, 57).[14] The purpose of explanatory

[13] Scriven (1962, 180) makes a similar point about postdiction and the ordinary sense of 'prediction.'

[14] In a discussion, Kuhn mentions *Foresight and Understanding*, saying he put the book aside to finish writing *Structure*. He adds, "I understand why Toulmin might have been sore at me for stealing his ideas, but I don't think I did. Let me be perfectly clear: I'm not at all sure he felt that, he has never said that. Toulmin was one of the people I had met during this trip to England at the end of my time in the Society of Fellows—I got along with him fine,

paradigms—which "are not 'true' or 'false' in any naïve sense" (57)—is to provide "a criterion for telling in what respects a body's motion calls for explanation; and what impressed forces we must bring to light if we are to succeed in explaining it" (56). Toulmin makes a similar point about laws of nature in his earlier book. Consider Snell's law: "Departures from the law and limitations on its scope, such as double refraction and anistropic refraction, come to be spoken of as anomalies and thought of as things in need of explanation in a way in which ordinary refraction is not" (1953, 79). In this sense, laws of nature are an important part of our ideals of natural order: they tell us what needs explaining and how to explain it. Recall also the rectilinear propagation of light discussed above. For Toulmin, a law of nature is not, strictly speaking, true or false, and so he denies that the best characterization of the rectilinear propagation of light is as a true generalization under which we subsume particular circumstances for explanatory purposes. Instead, the principle provides an intelligible model for thinking about light and setting up procedures for tackling problems related to its behavior.

Scriven also encourages us to focus on the functions of laws of nature in scientific reasoning and virtues they might exhibit beyond truth. For Scriven, "The important feature of laws cannot be their literal truth, since this rarely exists. It is not their closeness to the truth which replaces this, since far better approximations are readily constructed. Their virtue lies in a compound out of the qualities of generality, formal simplicity, approximation to the truth, and theoretical tractability" (1962, 212). He thinks the important explanatory function of laws is not to serve as essential, true premises under which we subsume particular facts for explanatory purposes. Like Toulmin on ideals of natural order, Scriven claims, "Laws provide a framework for events which we use as a convenient grid for plotting phenomena that may need explanation" (212). In short, laws of nature establish patterns in which we fit different events. And even though laws of nature do play "a crucial double role in the process of explanation," i.e., in establishing what needs explaining and in providing patterns for explanation, Scriven still maintains "the only ultimate element in the logic of explanation is understanding itself" (213). When it comes to the role of the laws of nature in explanation, he claims "the point is not at all that deduction from these as true premises is possible, but that these relations [expressed in the law] are the crucial factors for this inquiry, in this context, i.e., the only important ones (with respect to the degree of accuracy judged appropriate from the context, and the level of knowledge of the inquirer(s))" (213).

In summary, Scriven and Toulmin criticize the D-N model on three grounds: Its inattention to understanding, its claims regarding the symmetry of explanation and prediction, and its characterization of the role of laws of nature in explanatory reasoning. Their argumentative strategy involves examination of concrete examples of explanation, occasional appeal to the ordinary sense of 'explanation' and 'prediction', and attention to what they see as the non-deductive role laws play in scientific reasoning. In the next section, I relate these points to one possible motivation for taking the historical turn.

4. The relation to history

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he showed me around Oxford one day, but we hadn't gotten at all close. But since the time he came to the States, he and I have not gotten along very well" (2000, 297).

Summing up his account of scientific explanation, Scriven says, "It is a topically unified communication, the content of which imparts understanding of some scientific phenomena" (224). For Scriven, "Understanding is, roughly, organized knowledge, i.e., knowledge of the relations between various facts and/or laws. These relations are of many kinds—deductive, inductive, analogical, etc." (225). With these points in mind, Scriven thinks that in assessing explanations, we can ask questions beyond those involving truth, falsity, and the logical character of explanations represented deductively. He thinks we should talk about explanations as "*incorrect* or *inaccurate*…*incomplete* or *inadequate*…*irrelevant, improper,* or *inappropriate*" (200). While truth or falsity is relevant to the correctness or accuracy of an explanation, contextual considerations like the state of knowledge of the audience requesting an explanation determine if an explanation is relevant, proper, or appropriate. In drawing attention to these ways of assessing explanations, Scriven emphasizes aspects of scientific explanation he thinks the D-N model leaves out, namely, "context, judgment, and understanding" (196).

Scriven's points recall Austin's (1956, 11 n5) methodological recommendation that we ignore questions about truth or falsity "for once and for a while." For Austin, doing so frees us to focus on other dimensions of evaluation speech acts admit of, as well as the role speech acts play in ordinary discourse. In the scientific context, on Scriven's and Toulmin's account, truth and logical form are not the most important features of explanation. Scriven asks us to "notice we can talk perfectly well about 'two competing explanations' of some phenomenon in contemporary physics without feeling it improper to refer to both as explanations although only one can be true" (1962, 190) And while he grants that the use of 'correct explanation' seems to presuppose the truth of its explanans, other uses do not, e.g., explanations in fiction or explanations in historical episodes. For example, "there certainly seem to be cases where we want to say, for example, that the Babylonian explanation of the origin of the universe was basically naturalistic, without using inverted commas" (191).

In downplaying the importance of truth and logical form in favor of context, judgment, and understanding, as well as offering an account of the laws of nature that focuses on the role they play in scientific reasoning rather than their logical status as generalizations under which we subsume particular events, I think Scriven's and Toulmin's attitude towards explanation motivates an interest in history for philosophers of science. We can look to the past and see how scientists explained without focusing on questions of truth or falsity and logical form. Instead, explanations in the history of science, insofar as they are relative to the state of knowledge and interests of the persons giving and receiving explanations, are related to ideals of natural order rather than a formal schema. While we know past scientific explanations did not always feature true explanans, we can assess them within their own context: What ideals of natural order were assumed, i.e., which phenomena did past scientists feel called for explanation and which did not? What sort of understanding, relative to the state of knowledge and interests at the time, did past scientific explanations provide for their intended audiences? On what basis, besides truth and the canons of logical inference, were such judgments made?

Looking at past scientific explanations is also a way of putting into practice context analysis and the method of comparison. By examining scientific explanations in their historical context and comparing cases in which the participants involved took themselves to offer explanations, we can learn more about scientific explanation and current

characterizations of it. Relatedly, Toulmin argues that the ordinary language methods adopted by the later Wittgenstein and by Friedrich Waismann, to whom Scriven credits the method of comparison, "opened to inquiry a whole range of empirical and historical questions more radical and exciting than anyone immediately recognized" (1969, 45–6). However, the ordinary language philosopher did not take up these questions. Wittgenstein provides examples of simplistic language games and Austin's call to analyze ordinary language, specifically, "*what we should say when*, and so what we should mean by it" (1956, 7), involved considering either contrived situations or sources like case law.

For Toulmin, these methods nonetheless prompt us to consider how our linguistic practices are bound up with forms of life and how those forms of life could have been different. He says the later Wittgenstein's "little stories" were designed such that the audience was "compelled to concede that the applicability of inapplicability of our actual categories and concepts depends, in practice, always on previous human decisions, and that these decisions have become 'second nature' to us" (1969, 44). When we do this, we see our current concepts and their relationships are "not unchangeable verities but *intellectual equilibria*" (48). While this can be accomplished through ordinary language analysis, Toulmin asks, "why not go further?" (48). To fully understand our conceptual equilibria, we should examine not only current conditions on which they depend, but also ask how those conditions came into existence. So, rather than merely speculating about how our concepts ended up in their current relationships or thinking through imagined scenarios to reveal the current shape of conceptual arrangements, Toulmin wonders, "Why should we not try to base our understanding…on a wider study of how things have in fact happened?" (48).[15]

In short, to borrow from the title of his paper, Toulmin suggests philosophers of science undertake conceptual history. This involves close attention to how and why decisions about our conceptual apparatus were made. Like ordinary language analysis, conceptual history draws attention to the shape of our conceptual arrangements and the rules, habits, and conventions underlying them, as well as to how these arrangements might have been different. Providing this history offers grist for the mill of a philosopher of science interested in a contextual analysis of explanation aimed out bringing out its pragmatic features. And, I think, such a history might make more sense once one gives up on the idea that genuine scientific explanations have to be true, have to be predictive, and have to make use of scientific laws within a particular logical schema. Looking to the history of science shows the ways the state of knowledge, understanding, and forms of life at the time shape attempts to work out explanatory paradigms embodied in ideals of natural order and laws of nature.

5. Hesse and the historical turn

In talking about ordinary language methodology, Austin says, "Perhaps this method, at least as *one* philosophical method, scarcely requires justification at present—too evidently, there is gold in them thar hills" (1956, 7). At the time, some philosophers of science agreed. I have

[15] Stanley Cavell (1958, 206) Kuhn's friend and colleague at Berkeley, gestures towards this point at the end of his defense of ordinary language methodology.

highlighted the influence of ordinary language philosophical analysis on certain criticisms of the D-N model and the criticisms' connection to the historical turn. I think these connections come out most clearly in Scriven's and Toulmin's work. However, they are only one small part of the historical turn and the move away from logical positivist explicatory analysis as it relates to British philosophy in the 1950s. And, while prominent at the time, Scriven's and Toulmin's work is less influential than the work of some others mentioned by Kuhn as present at the start of the historical turn, e.g., Hanson, Feyerabend, and Hesse.[16] Moreover, I have looked at only one possible motivation for the historical turn connected to dissatisfaction with the D-N model of explanation, but there are surely other motivations. That said, here, I briefly consider Hesse's connection to 1950s ordinary language philosophy while acknowledging her important contributions, both philosophically and institutionally, extend well beyond her connection to the small corner of history I have examined.[17]

In 1963, Hesse writes a critical overview of, among other texts, the volumes from the Minnesota Center for the Philosophy of Science focusing on explanation.[18] Her overview is titled "A New Look at Scientific Explanation," speaking to the centrality of the debates over, and dissatisfaction with, the D-N model at the time. She begins by acknowledging that while "the orthodox empiricist hypothetico-deductive account of the structure of science" has made "great contributions to our understanding of science…it is no disparagement to suggest that some of their successors have fallen into a tedious era of epicyclic adjustments and over-technical elaborations which have diverted attention from the deep philosophical problems to which the H-D view provides only one among many possible lines of solution" (1963b, 98).[19]

Hesse raises these worries throughout her work in the 1950s, and sometimes, to motivate them she appeals to Waismann's characterization of the "open texture" of empirical concepts. In a discussion about whether statements admit of complete verification, Waismann—who was adjacent to both ordinary language philosophy and logical positivism—suggests, "most of our empirical concepts are not delimited in all possible directions" (1945, 122). Waismann unpacks this further: "Take any material object statement. The terms which occur in it are non-exhaustive; that means that we cannot foresee completely all possible conditions in which they are to be used; there will always remain a possibility, however faint, that we have not taken into account something or other that may

[16] Hasok Chang says Scriven's work on explanation "seems mostly forgotten nowadays" (2014, 76). Hanson's and Feyerabend's connections will have to wait to for future work. See Kindi (2017) for Wittgenstein's influence on Hanson and Feyerabend.

[17] The contributions of Toulmin, in helping establish an HPS program at Leeds and co-authoring history of science texts with June Goodfield, and Scriven, in his work at the Minnesota Center, also extend beyond this small corner.

[18] Scriven co-edited the first two volumes, and Hesse discusses Feyerabend (1962) from the second volume. There, Feyerabend appeals to a contextual theory of meaning he finds in Wittgenstein to motivate an account of incommensurability that complicates the reduction and explanation of older theories by newer theories. However, he explicitly rejects ordinary language methodology.

[19] In her review of Ernest Nagel's *The Structure of Science*, Hesse says, "it is perhaps time Ptolemy met his Copernicus" (1963a, 430).

be relevant to their usage; and that means that we cannot foresee completely all the possible circumstances in which the statement is true or in which it is false" (123). Far from taking this to be a negative feature of our empirical concepts, Hesse takes it to be essential to scientific practice. Open texture is needed to accommodate new and unexpected experiences. Hesse says, "Theories must have 'open texture', in Waismann's phrase, that is, a fringe of meaning not defined by observation, otherwise the whole meaning of the theory would change whenever it was desired to incorporate into it observations of a novel kind, and it is precisely the function of theories to assimilate such new observations without the meaning of the theories being radically altered" (1961/2005, 8).[20] Later, she says a key feature of scientific models is that they "should have the 'open texture' which allows modifications and extensions to be made as may be appropriate for the explanation and prediction of new phenomena" (23).

Here, Hesse draws attention to an aspect of the meaning and function of empirical concepts and models not easily incorporated into then-current deductivist philosophies of science. She points out that scientists do not consider the mathematical part of their theories "as if it were purely formal and uninterpreted"; instead, "they will use language about entities such as quanta of radiation, scattering, tracks of photoelectrons, and so on" (14). "In other words," Hesse says, "they still persist in talking in terms of models" (14). According to Hesse, the use of and talk about such models is a central part of scientific practice that must be accounted for. Considering "some of the mechanical models from nineteenth-century physics," Hesse says they functioned "as devices which were essential for rendering a theory intelligible and testable" (21). She then goes on to connect this point to open texture, saying, "The reason why a model such as that implied in [e.g.,] the dynamical theory of gases is not just a dispensable way of picturing the appropriate equations, is that the model can be generalised, extended and tested, and if necessary modified, as a purely formal deductive system cannot." (22).

The points about models providing a way to make theories intelligible and a guide to applying them to new phenomena, connect to the points about understanding Scriven and Toulmin make. Hesse, says, "Intelligibility is…related to the intuitive idea of explanation according to which we wish not only to correlate phenomena and to be able to make predictions, but also to *understand* their connections, and this desire in large part accounts for the long persistence of models drawn from familiar mechanisms" (27–8). In facilitating understanding, for Hesse, models play a similar type of role in scientific reasoning that laws of nature play for Scriven and ideals of natural order play for Toulmin. Models do not simply consist of generalizations under which we subsume particular events in deductive arguments. In the context of a discussion of Newton's laws of motion, Hesse notes that while one role they play is in facilitating prediction and explanation, the laws should also be understood as generalizations from empirical facts, as well as "rules [of inference] in accordance with which deductions from hypotheses are carried out" (28).

For Hesse, models, then, play a complicated role in explanatory reasoning not fully captured by the D-N model. This role is facilitated by their open texture, and to understand the central role of models more fully, Hesse thinks we need to consider the role of analogical reasoning in scientific practice. In earlier work, she rejects the idea that the use of analogies

[20] Hesse (1955, 97; 1963b, 102) make similar points about open texture.

between models and phenomena should be understood merely as "aids to discovery, having in themselves no explanatory or descriptive value" (1955, 141). Instead of relegating models to the context of discovery, she claims, "Surely the heuristic function of analogies must be regarded as an essential part of scientific theories" (141). An analysis of scientific reasoning, then, needs to go beyond logical analysis to capture the role of models and the analogies they provide in scientific reasoning. As evidenced by the central role it plays in much of her work from this period, Hesse thinks history helps in this regard. Moreover, and here is a suggestive connection to ordinary language philosophy, Hesse says, "Perhaps we may hope that the new emphasis on language will encourage philosophers to take in hand the task of clarifying the use of analogy, in science and elsewhere, to describe real relations between things" (146).

6. Concluding remarks

It is well-known that questions about meaning and reference feature prominently in Kuhn's work. Indeed, in 1990, Kuhn talks about his recent work on the notion of a lexical taxonomy that fixes the reference of the kind terms of a theory, thereby denying central claims of the causal theory of reference (1990, 4 ff.). Philosophers of language, of course, have a lot to say about meaning and reference, and historical-minded philosophy of science has a lot to say about theory change and its relevance to the meaning and reference of the terms and statements that make up scientific theories. Additionally, I have highlighted how some philosophers of science at the start of the historical turn took methodological cues from 1950s ordinary language philosophers to criticize the D-N model in their attempts to offer a more pragmatic understanding of scientific explanation. In doing so, they advanced accounts of the role of laws of nature, ideals of natural order, and models in scientific reasoning that resemble ordinary language philosophy's accounts of the rules, habits, conventions, and forms of life shaping ordinary discourse. Indeed, as the last quote from Hesse hints, there was hope that philosophers of language might have something to say about different types of reasoning employed in science and everyday life, e.g., reasoning based on analogy.

More recent work makes similar connections. Ian Hacking often appeals to lessons from Austin. He says, "Austin taught us how to study words in their sites, but despite his mastery of ancient philosophy, he examined a limited range of sites in the present" (2015, 25). In an earlier piece, Hacking relates this point to the study of history. He says, "Sites include sentences, uttered or transcribed, always in a larger site of neighborhood, institution, authority, language," and, further, "If one took seriously the project of philosophical analysis, one would require a history of the words in their sites, in order to comprehend what the concept was" (1990, 359). Hacking, then, ties the methodological recommendations of ordinary language philosophy to conceptual history much as Toulmin does. In doing so, he thinks we should follow the example of Austin, who, Hacking says, "did not analyze sentences in the sense of exhibiting their elements, but in the sense of providing an analysis of what we do with them and of what their uses are. Similarly to invoke the history of a concept is not to uncover its elements but to investigate the principles that cause it to be useful—or problematic" (360). Notice, here, the focus on usefulness, rather than truth or logical status.

Hasok Chang also encourages philosophers of science to move away from "viewing science as a body of propositions, focusing on the truth-value of those propositions and the

logical relationships between them" (2014, 67). Chang instead encourages philosophers of science to take as the target of their analysis epistemic activities, "what it is that we actually *do* in scientific work" (67), calling attention to Scriven's work on explanation as an example of someone who got this right by focusing on the *act* of explaining (76).[21] Further, Chang makes a methodological recommendation explicitly recalling Austin, "When we [i.e., philosophers of science] do pay attention to words, it would be better to remember to think of 'how to *do things* with words'" (2014, 68).

I think the history I sketched shows the fruitfulness of taking up Hacking's and Chang's methodological recommendations. Scriven, Toulmin, and Hesse looked to the role laws of nature, ideals of natural order, and models play in acts of scientific explaining. In doing so, they highlighted how scientific explanations throughout history contribute to our understanding in ways not necessarily representable in terms of propositions and their logical relationships. In being among the first to take these aspects of scientific explanation as targets of philosophical analysis, as opposed to relegating them to the context of discovery for historians and psychologists to investigate, their work sets the stage for later historically minded philosophy of science. Moreover, in de-emphasizing questions about truth and logical status of the different parts of our scientific theories, they, along with Kuhn, helped "produce a decisive transformation in the image of science by which we are now possessed" (1962/2012, 1).[22]

[21] Chang (2017) discusses ordinary language philosophers' work on knowledge-how, or knowledge-as-ability. Tying this to philosophy of science, Chang says, "my sense is that knowledge-as-ability is just as important in science as in everyday life, though Ryle's and Austin's examples tended to be taken from everyday life" (105).

[22] Opening her review of *Structure*, Hesse says, "This is an important book. It is the kind of book one closes with the feeling that once it has been said, all that has been said is obvious, because the author has assembled from various quarters truisms which previously did not quite fit and exhibited them in a new pattern in terms of which our whole image of science is transformed" (1963c, 286).

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