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There has been considerable disagreement in recent years among philosophers regarding what sort of cases to examine in doing epistemology (including philosophy of science). Some have preferred to examine ordinary language and mundane situations to draw their philosophic conclusions. Yet others especially philosophers of science trained in the post positivistic period prefer to examine historical cases of scientific discovery. In this article I will address this issue, one that is in fact part of the long-standing dispute about the relation between science and common sense.

Consider the cliché that science is just organized common sense. If this cliché is false (as clichés often are), the implications for epistemology are important. For then there would be several theories of knowledge, depending upon what is known (the realm of science versus the world of the common man). On the other hand, if the cliché is true, it also has important implications for epistemology. For then (again, it would appear) we could construct an adequate theory of knowledge by merely looking at simple, mundane cases of knowledge. The choice is an important one: In epistemology, do we look at cases drawn from the history of science in all their complexity, or do we allow ourselves the luxury of concentrating on the simple ("Jones owns a Ford")? Yet, for a question of such importance, the question of the difference between science and ordinary knowledge is rarely addressed explicitly.

We really should distinguish several questions here: What is the difference between the structure of scientific knowledge (organized into theories and laws governing various domains) and ordinary knowledge (if it has a structure at all)? What is the difference between scientific method and the techniques of ordinary learning? What is the goal of scientific research (perhaps pure truth) versus that of ordinary investigation (perhaps achieving some practical end)? I will focus on just one such question. It is the central one: Is the reasoning involved in scientific discovery essentially different from that employed in ordinary discovery?

The reader may wonder at the phrase "ordinary discovery." But what I'm referring to is clear enough. Many mundane occupations involve research. If that be too lordly a designation, then say instead sustained and rational investigations which (when successful) culminate in discovery. I have in mind, for instances: the auto mechanic trying to account for a car' s rough idling; the detective trying to figure out how a jewel disappeared from a room with all exits guarded; the TV repairman trouble-shooting a defective set; the gardener trying to decide why the dichondra won't grow; the doctor trying to determine the cause of a set of symptoms; and a hairdresser trying to restore body to bedraggled hair. Is there a difference between the reasoning of the scientist and that of those other people?

C. S. Peirce for one seems to have felt there isn't any logical difference between the reasoning in ordinary and scientific discovery. One thinks of his remark:

I once landed at a seaport in a Turkish province, and, as I was walking up to the house which I was to visit, I met a man upon horseback, surrounded by four horsemen holding a canopy over his head. As the governor of the province was the only personage I could think of who would be so greatly honored, I inferred that this was he. This was an hypothesis.

Fossils are found; say remains like those of fishes, but far in the interior of the country. To explain the phenomenon, we suppose the sea once washed over this land. This is another hypothesis.

Numberless documents and monuments refer to a conqueror called Napoleon Bonaparte. Though we have not seen him, what we have seen, namely all those documents and monuments, cannot be explained without supposing that he really existed. Hypothesis again.[[1]](#footnote-1)

Peirce adds that even our belief that our memory is veridical is hypothesis, put forward to account for our feeling that it is.

But others have disagreed, feeling that scientific reasoning is qualitatively (not just quantitatively) different. It will be instructive, I believe, if we examine one such view. I choose Toulmin's account as presented in his 1960 text, *Philosophy of Science*. He may not still hold this account after twenty-five years I don't know. I would like to examine his view, and then present an observation about how we should use case studies in philosophy of science.

Toulmin begins the account by contrasting two cases of ordinary discovery with a clear case of scientific discovery, albeit of a low-level sort. He contrasts the reasoning involved in Robinson Crusoe's discovery that there was another man on his island (which Crusoe had hitherto thought deserted), and the reasoning involved in the discovery that swallows always migrate along great circles, with the reasoning involved in the discovery that light travels in straight lines.[[2]](#footnote-2)

Beginning with the ordinary Crusoe discovery, Toulmin contends that it involves (shades of Peirce) an inferential step from the observation of a fresh footprint on the beach to the conclusion that there is another person around, via the assumption that "footprints mean man." Toulmin, that is, views Crusoe as inferring:

 Footprint!

Footprints mean man.

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 Therefore, man!

Moreover, Crusoe can verify his inference by coming face-to-face with the man who caused the print. He is merely applying the old type of inference to fresh data.

On the other hand (Toulmin continues), the discovery that light travels in straight lines was *not* an inference from the data (namely, facts about light and shadows and the techniques- such as sundial construction-based upon them). He suggests several reasons why it isn't.

First, Toulmin claims that while Crusoe might meet his hypothesized person face-to-face, it makes no sense to speak of coming face-to-face with the light responsible for the shadows. "No single happening could establish the optical theory once and for all, in the way Crusoe's conclusion could be established."[[3]](#footnote-3)

Second, Toulmin feels that the scientific discovery is one that we can use profitably even if it is only approximately correct. That is, the theory of the rectilinear propagation of light is not exactly correct after all, light is diffracted, refracted, and scattered. But that doesn't stop us from profitably using the discovery to ask new and fruitful questions about light phenomena. This is not the case (Toulmin avers) with Crusoe's discovery or with the case of the swallows. The discovery here is that swallows always travel along great circles, and only that.

But the third difference Toulmin sees between ordinary and scientific discovery, a difference which surely proves that the latter involves non-inferential reasoning, is that in scientific discovery some of the key words are given new senses in the very statement of the discovery. This is not the case with ordinary discovery; the meaning of the term "man" for Crusoe doesn't change when he discovers that someone else has been walking on his beach. Yet the meanings of "light" and "travel'" do change with the discovery that light travels in straight lines.

Intertwined with his claim that the meanings of terms change in scientific discovery, Toulmin presents the claim that after making a discovery, scientists see the world in a different way, see (for instance) the shadows as effects of the blockage of light. (He also says that scientific discovery involves changing models.)

Now, Toulmin gives us quite a list of differences between ordinary and scientific discovery. The problem is that they aren't really persuasive.

Consider the first, that in the case of ordinary discovery, complete observational verification is possible, whereas such verification is never possible in true scientific discovery. This just isn't so. After all, the discovery that a bacterium causes a certain disease is surely a scientific discovery. Yet you can observe a bacterium directly.[[4]](#footnote-4)

Conversely, even if Crusoe came face-to-face with the person who made the footprint, it is deceptive to talk about instant or conclusive verification of Crusoe's conclusion, at least as far as Crusoe is concerned. He can't be sure at first that he is faced by a man, as opposed to an unusual humanoid vegetable or a local robot.

Again, the claim that ordinary discoveries such as Crusoe's or the migration paths of swallows are either precise or else are uninteresting dead-end generalizations is dubious. Granted, the cases chosen to represent ordinary discovery are less interesting than the case chosen to represent scientific discovery. But the difference is a matter of degree, not kind. Swallows often stray off course a bit; hence *that* discovery is approximate, too. Moreover, this discovery *does* lead to interesting questions: How do they stay on course? What evolutionary purpose is served by such a migration pattern? And so on. Similarly, Crusoe's discovery suggests questions of great interest to him, at least: Where did this person come from? Why is he here? What is the person’s gender?

As to the welter of claims about meaning, perceptual and model change in scientific discovery, I want to point out that the same can be said to characterize ordinary discovery at least when you go back early enough in a person' s training.

Consider this case of ordinary discovery. A young mechanic is given his first "patient," a car which has trouble accelerating from a full stop. The car can accelerate only in spurts  accelerating, then slowing, then reaccelerating. The novice, after examining the car's ignition system and carburetor, finally hits upon the idea of examining the fuel line. He finds a small hole in it, and begins thinking about whether such a hole could account for the car's anomalous behavior. Before, (we may suppose) the novice had viewed the fuel line merely as a conduit for gas. Then he acquires an insight into the system. He begins to see it as a "suction conduit" which moves the fuel by suction, more gas being sucked as the car's engine speeds up. He hypothesizes that the hole causes the saltatory acceleration. As the car accelerates, the suction increases causing air to be sucked in. This cuts down the gas flow, which slows the engine, which reduces the suction, which increases the gas, which reaccelerates the car, etc.

After discovering why the car acted as it did, the novice *sees* the fuel line in a new way.

The term "fuel line" has a new meaning for him.

Such a case may seem more like a parody than a serious counter-example to the claim that conceptual change characterizes only scientific discovery. But it seems like a parody only because we tend to forget that what we now know and the concepts we now employ were acquired very slowly. A young person making an ordinary discovery is in a very similar position to that of a scientist making a scientific one.

So in the end, none of Toulmin's putative "differences" seem to be real *qualitative* ones. Thus there seems to be no good reason to think that the reasoning involved in scientific discovery is essentially different from that in ordinary discovery, however deeper and more exciting the former might be. But are we to draw the conclusion that the philosopher can do his job just as well by examining simple cases of ordinary discovery (in the manner of those who do metaphysics by analyzing "ordinary language") instead of complicated cases from the history of science?

I think we should resist making that inference for a very important reason: Cases drawn from the history of science, despite being less accessible initially, have the surprising advantage of clarity. Let me elaborate.

It is now universally recognized that scientific research (and ordinary investigation as well) takes place in some context. This has a bearing upon how that research evolves. That is, the social, economic, technological and cultural background (not to mention the prior history of research in that domain) enters into the scientist's program.

This is no less true in the case of ordinary "research." For example, the concepts that the auto mechanic has, his background knowledge and training, prior experience, and so on all enter into the way he trouble-shoots a given car. But the relevance of background factors is much less noticeable *precisely because* it is so familiar, shared as it is by most of us.

Putting the point another way, it is easier to spot the other fellow’s presuppositions than your own. It is easier to see the complexity of reasoning in research (with the delicate interplay between background factors and new information) when the researchers are from an epistemic community different from your own. That is why introspection upon common-place investigation, and even retrospective reports by scientists about their own research, are (or at least tend to be) so much less revealing.

But something suggests itself here. If studying historical cases of scientific research is epistemologically important, not because of some essential difference between scientific and ordinary discovery, but because it is easier to detect the hidden background factors; and if such studies are inaccessible and have the drawback that the subject matter requires special training to understand what is happening in the research program; then, why not devise some approach that combines the best features of both ordinary cases (accessibility) and historical ones (clarity)?

One such compromise approach might be to study cases of ordinary investigation in times, cultures, and places far removed from our own. For instance, we could look at anthropological studies of magic and such like. (One thinks here of books like Horton's *Rationality and Magic).* But here again there may be problems of accessibility.

A better approach, it seems to me, is to design model games, with rules so clearly laid out that no questions of "presuppositions" or "background" will arise. Such model research games are now being investigated under the rubric "formal dialectic," and hold promise to be a middle course between science and common sense.

**Gary James Jason**

**Department of Philosophy**

**San Diego State University**

1. C. S. Peirce, *Collected Papers* 2.625 [↑](#footnote-ref-1)
2. Stephen Toulmin, *Philosophy of Science* NY: Harper & Row, p. 19ff. [↑](#footnote-ref-2)
3. *Ibid.* p. 19. [↑](#footnote-ref-3)
4. This claim is surely plausible, now that the theory/observation distinction as understood by the positivists has been shown to be dubious. [↑](#footnote-ref-4)