John Stuart Mill (1874, 208) defined induction as the operation of discovering and proving general propositions. William Whewell (in Butts, 1989, 266) agrees with Mill’s definition. Is Whewell therefore assenting to the standard concept of induction, which talks of inferring a generalization of the form “All As are Bs” from the premise that “All observed As are Bs”? Does Whewell agree, to use Mill’s example, that inferring “All humans are mortal” from the premise that “John, Peter and Paul, etc., are mortal” is an example of induction? The surprising answer is “no”. How can this be? How can there be a dispute between Mill and Whewell about the nature of scientific induction if they agree on the definition?

On my reading, Whewell has an entirely different understanding of the term “general”. For Whewell, the proposition “All As are Bs” is not general if it is a mere juxtaposition of particular cases (see Butts, 1989, 163). Rather, for Whewell it is necessary that (Butts, 1989, 47) “In each inductive process, there is some general idea introduced, which is given, not by the phenomena, but by the mind.” The proposition is constituted of facts and conceptions which are, “bound together so as to give rise to those general propositions of which science consists”. For Whewell “All humans are mortal” is not general in the appropriate sense because there has been no conception added. Whewell insists that in every genuine induction, “The facts are known but they are insulated and unconnected . . . The pearls are there but they will not hang together until some one provides the string” (Whewell, quoted from Butts 1989, 140-141). The “pearls” are the data points and the “string” is a new conception that connects and unifies the data. The “pearls” in “All As are Bs” are unstrung because “All As are Bs”, though a general proposition in the sense that it applies to all relevant instances, does not connect or unify the facts; or as Whewell puts it, it does not colligate the facts. Thus, Whewell’s view of induction is not the standard philosophical view of induction.

Whewell distinguishes “colligations” from what are commonly thought of as inductions to make the point that it is only the former that, by virtue of the mental act of introducing conceptions, genuinely connect or unify facts. To make this point, he gives an example that illustrates the subtle ways in which our perceptions are intertwined with our mental conceptions, an example that appeals to our intuitions even in simple cases where that interaction might not be so obvious at first glance.

When anyone has seen an oak-tree blown down by a strong gust of wind, he does not think of the occurrence any otherwise than as a Fact of which he is assured by his senses. Yet by what sense does he perceive the Force which he thus supposes the wind to exert? By what sense does he distinguish the Oak-tree from all other trees? It is clear upon reflexion, that in such a case, his own mind supplies the conception of extraneous impulse and pressure, by which he thus interprets the motions observed, and the distinction of different kinds of trees…. The Idea of Force, and the idea of definite Resemblances and Differences, are thus combined with the impressions on our senses and form an indistinguishable portion of that which we consider as the Fact. (Whewell, Quoted from Butts, pp.123-124)

So while Mill and Whewell agree that inductions produce propositions that are more general than the premises from which they are generated, they differ on the meaning of “generality” and therefore disagree about the nature of the inductive process.

Does this dispute make a difference in scientific examples? Whewell and Mill argued over Kepler’s discovery of the elliptical motion of Mars. Kepler started with observations of the
position of Mars relative to the sun at various times.¹ The observations might be represented as points scattered around the sun, from which Kepler inferred that Mars’s orbit is an ellipse.² As in any example of curve-fitting, the data points are the pearls and the curve is the string (see ch. 2).

Could this example be understood as an example of induction in Mill’s sense? Whewell and Mill can agree that the conclusion of the inference is that “All positions of Mars lie on ellipse $b$”, where $b$ is the name of a particular ellipse. So, in this example, the predicate A is “is a position of Mars” and B is “lies on ellipse $b$.” But Mill has to say that the data are of the form “at time $t_1$ Mars lies on ellipse $b$, at time $t_2$ Mars lies on ellipse $b$, …, and so on.” Notice that for Mill, the predicates that appear in the general proposition also appear in the description of the data.

On the other hand, Whewell considers the data to contain no mention of the ellipse $b$, or any ellipse, so “lying on ellipse $b$” is a new conception that colligates the data. The data are “at time $t_1$ Mars is at position $x_1$ at time $t_2$, Mars is at position $x_1$, …, and so on.” For Whewell, the facts and the conception are then bound together so as to give rise to those general propositions of which science consists. So, Kepler’s conclusion is general in the sense that the general conception of an ‘ellipse’ is “superinduced” upon the facts, and is not a “mere union of parts” or a “mere collection of particulars.” (Butts, 1989, p. 163.) That is why Whewell sees Kepler’s inference as a colligation and therefore, a genuine induction.

Against Mill, Whewell (1849, p.//) says that “the fact of the elliptical motion was not merely the sum of the different observations, is plain from this, that other persons, and Kepler himself before this discovery, did not find it by adding together the observations.” When Millians see this quote, they tend to interpret it wrongly as saying that an inductively inferred proposition is not merely the sum of the observations because it covers new instances. But Whewell does not agree that the observations contain, or determine, the conception of an ellipse. So, Mill and Whewell have substantively different views of Kepler’s inference.

We have already seen that the mortality example is an induction for Mill, but not for Whewell. But are there cases that Whewell would see as inductions but Mill would not? Such examples are possible, for Mill insists that “any process in which what seems the conclusion is no wider than the premises from which it was drawn, does not fall within the meaning of the term” (p.210). So imagine that an economist wants to explain the inflation rate of the Soviet Union, and uses the known inflation rates for all the years from 1917 to 1990. For Whewell, this can count as a genuine inductive inference if the explanation introduces a new conception; maybe the concept of price control. On the other hand, Mill would be forced to say that there is no induction because there are no new instances of the proposition, due to the unfortunate disintegration of Soviet Union in 1990. I think that intuition is on Whewell’s side in this instance, for the induced proposition does have implications about what the inflation rate would have been in 1991 had the Soviet Union survived. For Mill this is not enough because this is no testable prediction.

Thus far: Whewell introduced the term “colligation” to refer to the process of conceptualizing observational data. This is the essential part of induction for Whewell and he used the terms “induction” and the “colligation of facts” interchangeably. Mill agreed with most of what Whewell had to say about colligation, but viewed this as a process that occurs separately from and prior to a genuine induction.

This issue is especially salient at a time when various versions and extensions of hypothetico-deductivism, such as Popper’s falsificationism, logical positivism, and even Bayesianism, generate so much controversy. Whewell’s philosophy of science presents an

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¹ For a description of the method Kepler used to obtain these data, see section 5 of Forster (1988), or the appendix of Hanson (1973).
² The example considers only Kepler’s inference to his first law.
alternative to the traditional philosophy of science, which are all inspired by Mill’s view. The
main disputant was Kuhn (1970), who argued as a historian of science that traditional philosophy
of science does not fit the actual practice of science. Kuhn preached the view that Millian
philosophies of science should be replaces by science studies, which look at how scientific
communities interact and evolve—the idea being that there is no higher standard in science than
the assent of the relevant scientific community. This view is highly influential today, even
amongst philosophers of science. It is interesting to note that Whewell is also a historian of
science, who unlike Kuhn, talks about the internal dynamics of science—the evolution of ideas
and the tests of hypotheses. While Whewell had a lot to say about the psychology of scientists,
he also talked about the confirmation, or tests, of theories. What he says about the tests of
hypotheses might be commandeered for the purposes of resurrecting a more traditional
philosophy of science that is not subject to Kuhn’s criticisms.

Is the debate between Mill and Whewell more than a terminological dispute? Is it
substantive? When added to claims about justification, the dispute is clearly substantive. For
Whewell claims that the colligation is an essential to the consilience of inductions, which is
essential to the justification of scientific theories. Mill disagrees.

Colligation, for Mill, is a part of the discovery process, or the process of invention, whereas
induction is relevant to questions of justification. Whewell’s characterization of induction, Mill
objects, belongs to (what we might call) the ‘context of discovery,’ and Mill thinks that Whewell
confuses them. Accordingly Mill (1874, 222) charges that “Dr Whewell calls nothing induction
where there is not a new mental conception introduced and everything induction where there is.”
“But,” he continues, “this is to confuse two very different things, Invention and Proof.” “The
introduction of a new conception belongs to Invention: and invention may be required in any
operation, but it is the essence of none.” Abstracting a general proposition from known facts
without concluding anything about unknown instances, Mill goes on to say, is merely a
“colligation of facts” and bears no resemblance to induction at all. Whewell, of course,
disagrees.

True, Whewell does think that mental acts are essential features at every stage of scientific
progress, and that mental acts are essential to invention and discovery. But to say that they are
essential to discovery does not imply that they are not also essential to justification. So, Mill has
no good reason to accuse Whewell of confusing invention and proof. In fact, Whewell makes a
clear distinction between invention (i.e. colligations) and justification (i.e., Consilences of
Inductions).

Whewell distinguishes four tests of scientific hypotheses. By ‘instances’ he is referring to
empirical data that can be fitted to the hypothesis in question:

1. The Prediction of Tried Instances (used in the construction of the hypothesis).
2. The Prediction of Untried Instances;
3. The Consilience of Inductions; and

For Whewell, ‘induction’ refers to the process of applying or constructing a scientific hypothesis
to explain some empirical phenomenon. For example, Newton explained the motion of a
terrestrial projectiles in terms of gravity, and he also explained the moon’s motion in terms of
gravity. These are two different phenomena and their explanations can be seen as involving two
difference inductions. Whewell also used the term ‘colligation of facts’ in place of ‘induction’. A
consilience of inductions occurs when two, or more, colligations of facts are successfully
unified in some way. Newton’s theory of gravity applied the same form of equation to celestial
and terrestrial motions (the inverse square law), and in the case of the moon and the apple, both
colligations of facts made use of the same adjustable parameter (the earth’s mass).
Consequently, the moon’s motion and an apple’s motion provide independent measurements of
the earth’s mass, and the agreement of these independent measurements was an important test of
Newton’s hypothesis (see ch. 1 for more detail). This test is more than a prediction of tried or
untried instances. It leads to a prediction of facts of a different kind (facts about celestial bodies
from facts about terrestrial bodies, and vice versa).

The consilience of inductions is accompanied by a convergence towards simplicity and unity
because unified theories forge connections between disparate phenomena, and these connections
may be tested empirically. So, a theory needs to be unified in some respects for there to be a
successful consilience of inductions. Simplicity and unity are necessary conditions for the
consilience of inductions, but not sufficient. A theory like ‘everything is the same as everything
else’ is highly unified, but not consilient. As Einstein once said, science should be simple, but
not too simple.

In the Novum Organon Renovatum, Whewell (1858) speaks of the consilience of inductions
in the following terms:

We have here spoken of the prediction of facts of the same kind as those from which our
rule was collected [tests (1) and (2)]. But the evidence in favour of our induction is of a
much higher and more forcible character when it enables us to explain and determine
cases of a kind different from those which were contemplated in the formation of our
hypothesis. The instances in which this has occurred, indeed, impress us with a
conviction that the truth of our hypothesis is certain. No accident could give rise to such
an extraordinary coincidence. No false supposition could, after being adjusted to one
class of phenomena, exactly represent a different class, where the agreement was
unforeseen and unanticipated. That rules springing from remote and unconnected
quarters should thus leap to the same point, can only arise from that being the point where
truth resides.

Accordingly the cases in which inductions from classes of facts altogether different
have thus jumped together, belong only to the best established theories which the history
of science contains. And as I shall have occasion to refer to this peculiar feature of their
evidence, I will take the liberty of describing it by a particular phrase; and will term it the
Consilience of Inductions. (Whewell, quoted from Butts, 1989, 153.)

Contemporary philosophers of science, including Popper and Kuhn, have failed to place any
special emphasis on the consilience of inductions, partly because I believe that they have not
understood it.

For Whewell, the addition of a new conception is the essential feature of any induction, and
is therefore essential to any consilience of inductions, and therefore essential to the justification
of theories, since consilences provide the most important evidence for the truth of our theories.
Whewell never confuses discovery and justification; a colligation of facts is never fully justified,
confirmed, until it “jumps together” with other colligations of facts.

Whewell does not think that all inductions are fully justified at their inception. “Real
discoveries are . . . mixed with baseless assumptions” (Whewell, quoted from Butts 1989, 145).
That is precisely why Whewell considers a consilience of inductions necessary to provide
additional evidence of a hypothesis’s truth. The truth of a hypothesis, for Whewell, remains
suspect even if it appears to accurately predict new instances of the same kind (as in the
mortality example and the Kepler example). That is unless, or until, it passes his most rigorous
test by leading to a consilience. What Mill seems to accept as justification -- that a hypothesis
successfully predicts instances of the same kind -- is only one of several tests in Whewell’s
methodological schema. In fact, Whewell, by requiring increasingly more difficult tests insists
upon far more justification for a theory than does Mill.

The example about the Soviet economy plays on this difference, for while the inductive
proposition makes no new predictions (test 2 is impossible), it could be tested by its consilience
with other inductions. The beam balance example (ch. 3) is another pertinent example. For UNIFIED and COMP do roughly the same on test 2, but UNIFIED achieves a consilience of inductions that SIMP does not. Likewise, Copernicus argued for the heliocentric view by pointing out that his theory predicted relational facts like the correlation between the position of the sun and the retrograde motion of the outer planets. This prediction arises from the fact that Copernicus’s theory is more unified than Ptolemy’s. The Copernican theory achieves a consilience of the inductions pertaining to the apparent motions of the planets taken one at a time that is not matched by Ptolemy’s theory. Yet the published version of Copernicus’s theory has more circles than Ptolemy, and does not achieve more accurate ‘next instance’ predictions (both theories perform badly on test 2).

In place of the consilience of inductions, Mill talks about the deductive subsumption of lower level empirical laws under more fundamental laws, which is a well-known part of hypothetico-deductivism. Whewell’s account of consilience gets around the common objection that deductive subsumption is too easy to satisfy. For instance, hypothetico-deductivism tries to maintain that Galileo’s theory of terrestrial motion, call it G, and Kepler’s theory of celestial motion, K, are subsumed under Newton’s theory N because N deductively entails G and K. A common objection is that the alleged deductive relations hold only ‘approximately’, but there is a more serious problem for this view. The problem is that G and K are also subsumed under the mere conjunction of G & K, so subsumption by itself does not capture the idea that N is more unified or consilient. Fortunately, Whewell’s view does not have that consequence because consilience makes essential reference to the fact that Newton successfully colligated facts about motion using the added conceptions of force and mass.

There is a secondary dispute between Mill and Whewell, which is seen to be a red herring in light of this analysis: It doesn’t matter whether conceptualization occurs prior to an induction as Mill insists, or during the induction as Whewell maintains. No matter which view is correct, the important difference is that conceptualization plays an essential justificatory role for Whewell, but not for Mill.

However, there is another closely related issue that is relevant. Mill claims that the property of “lying on ellipse $b$” is determined by and read from the data themselves. If this empiricist view of concepts is correct, then it is hard to see why conceptualization should matter to theory comparison, because all competing theories will be on the same footing. According to Mill (1874, 216, Mill’s emphasis):

 Kepler did not put what he had conceived into the facts, but saw it in them . . . A conception implies, and corresponds to, something conceived: and though the conception itself is not in the facts, but in our very mind, yet if it’s to convey any knowledge relating to them, it must be a conception of something which really is in the facts . . .”

Whewell does not deny that the regularities in nature exist before we perceive or conceive them; he does not reject the claim that the orbit of Mars was elliptical before anyone knew that to be true. Rather, Whewell thinks that Kepler placed the data “in a new system of relations with one another” was not determined by the data themselves. The interpretation of the data is theory-dependent. This is especially clear in curve-fitting examples, which Whewell talks about in some detail (see Butts, 1979, 211-237). According to Whewell, “the Colligation of ascertained Facts into general Propositions” consists of (1) the Selection of the Idea, (2) the Construction of the Conception, and (3) the Determination of the Magnitudes. In curve fitting, these three steps

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3 This is a phrase from Butterfield (1949, 1-7) that Kuhn (1970, p. 85) quotes in order to explain what he means by applying his duck-rabbit metaphor to how scientists’ view of data changes across scientific revolutions. We believe that there are many affinities between Whewell and Kuhn, but that is another topic.
correspond to (1) the determination of the Independent Variable, (2) the Formula, and (3) the Coefficients. (See ch. 2.) In the Kepler example the independent variable is ‘time’. The data are observations of Mars at various times, and the aim of the induction is to characterize all positions as a function of time. The second step introduces the conception of an ellipse. At that stage, the claim is that the orbit of Mars is some ellipse, without saying which ellipse. In the third step, the family of ellipses is fitted to the data, and the measured parameters (or coefficients as Whewell calls them) are those characterizing the best fitting ellipse. This is ellipse $b$, and this third step yields the specific claim that all points on Mars’ orbit lie on ellipse $b$. There are two important points to notice. First, the data first enter the process in step 3, but this process makes no sense unless the formula is already fixed because the “best fitting curve” means “the best fitting curve in a family.” If a different formula were chosen, then the resulting orbit would not be an ellipse. Moreover, it is always possible that a different family, or formula, could yield a curve that fits equally well. So, there is no sense in which the data determines the formula. Mill’s idea that the data is “all observed positions of Mars lie on ellipse $b$” has no logically or historically basis.

What Mill misses is the important distinction that Whewell makes between the idea(s) that are used to express the facts and the conception(s) used to colligate the facts. As we have seen, on Whewell’s view, all facts are mind-laden to the core. But the data are laden with ideas, which are a special class of conceptions, like time, space, and number. These are needed in step 1 to determine the independent variables. It may be that, for Whewell, ideas are determined by the data, but they are not to be confused with the new conception that must be added in step 2. There is no sense in which the conception used to colligate facts is determined by the data.

Mill’s view is especially implausible in other examples like Newton’s argument for universal gravitation (see Forster 1988, section 5). Mill would have to say that Newton saw the masses and forces in the data prior to the induction. In other words, Mill would have to assume that Newton observes that the planets are at the positions they would have if Newton’s theory of gravitation were true. But that is a consequence of the inferred proposition, and not part of the description of the data on which the inference is based. We do not need to labor this point because it is now widely accepted that Mill is wrong about the idea that theoretical quantities are induced directly from the observations. It is widely accepted that the observation of theoretical quantities, if it is referred to as observation at all, is theory-laden (c.f. Kuhn’s duck-rabbit analogy).

To represent Whewell as interested only in the psychology of discovery or to characterize 19th century empiricists like Mill as denying to the mind any role in the development of scientific knowledge, is to oversimplify the deep philosophical differences between Whewell and his critics (e.g., Metcalfe 1991). To do so also downplays Whewell’s innovative, albeit controversial, contributions to the problem of confirmation. We have seen that there are fundamental and important differences between Mill’s and Whewell’s philosophy of science and that the nature and the substance of those differences are not merely terminological. Nor are the differences as obvious as they may seem at first glance. That is why contemporary writers tend to misunderstood fundamental aspects of Whewell’s epistemology, which leads to a confused reading of his philosophy of science.
Further Reading


