Australian Journal of Teacher Education

Volume 8 | Issue 1 Article 4

January 1983

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There Is No Such Thing As A Non-Theoretical Basic Empiricism

According to the empiricist tradition, science starts with observation. The observer should in an unprejudiced fashion faithfully record what he sees, hears, etc., about him. From his (admittedly) limited number of observation statements, the laws and theories of science can be established by generalization. Chalmers summaries the view when he writes,

> If a large number of A's have been observed under a wide variety of conditions, and all those observed A's without exception possessed the property B, then all A's have the property B. (1976,5)

There are more sophisticated versions of the empirical account, but the core of them is still covered by the above descriptions.

But a significant problem derives from what is to count as "a wide variety of conditions". What is to count as a **significant** variation in conditions which will allow us to decide that all A's have the property B? Unless **un**important variations are eliminated, the number of possible variations in which we could test for B is infinite. The answer is that the experimenter uses his current **theoretical** understanding of the situation. But this of course is to admit the Trojan Horse of prior theory; it shows the key role that theory must play **prior to** observation. As Hanson says, all data are already theory-laden. I think that this general point about prior theory can be generalized right across the activities of life. Clearly it is the case in education. Understanding of educational situations and activities does not begin with observation; it begins with present values and current theory of a commonsense or more sophisticated sort, dictated by the history of educational ideas.

It is interesting to note the persistence of the view that non-theoretical observation can occur, that there can be an impartial observation language. Misunderstanding of the place of theory in the sense I am using the word is I think widespread. Let me use the views expressed in a recent textbook in the empiricist tradition, called *The Science Game*, to make my point. Its authors recommend that their views be implemented by practitioners of behavioural science.

They grant the importance of theory. But their view of theory is significantly different from the sense I am using here. Their view of theory sees it as a kind of shorthand or summary. Thus they write,

> The important point is that theories are simpler than the data they are designed to represent. Theories are built (1) by squeezing some parts of experience together — — all blacks, all smokers, all Southern Democrats — — and (2) by ignoring or omitting some information, such as the differences that exist among blacks, among different smokers, among different Southern Democrats (1978, 197).

Their view of theory is as a sort of general view which allows human beings to deal with a total situation in less than its total complexity. It is true that this feature is an important aspect of all theory and of all general knowledge. But concentration upon this meaning of 'theory' takes attention away from theory in the sense of something prior to observation — — a sort of logically-necessary aspect of our 'way of looking at something'. And because the authors approach their work firmly in the empiricist tradition of science **starting** with observation, they then talk about the possibility of a **theory-less** observing computer. They write,

> We propose that the most important reason for bothering with theories is that we have no alternative. We would have an alternative, if, like some wondrous computer, we saw, heard, and felt everything, if we had a massive and unlimited memory file showing each bit of information separately and permanently, and we could draw at will from our memory file any information we wished for examination (Agnew and Pike, 1978, 197).

The quotation is a logical-nonsense. Their view of theory as a sort of generalizing summary leads them to believe that it is merely contingent, human **empirical** limitations which make the situation what it is. They fail to grasp that though their view of theory as a merely empirical necessity due to our intellectual limitations truly describes one important sort of theory, the situation is always underpinned by logically-necessary theory of the other sort. Not even some "wondrous computer" could **just observe**. Observations are **logically** drawn up into categories. Categorization is a logically-necessary part of what gives **meaning** to observations. So it is not just in their sense of 'theory' that theory is empirically necessary. It is also the case that some sort of theory, in this sense of prior assumptions, prior orientations, ways of grasping hold of the world, would be logically-necessary even for "some wondrous computer . . (which) . . saw, heard, and felt 'everything' ".

So I am supporting the claim that there is no pristine, neutral observation language, which is ontologically-privileged, and which can serve as a starting point for science. There is no presupposition-less observation or approach.

How Can We Tell When A Theory Is An Improvement On Its Predecessors?

So science does not begin with observations and observation statements. It begins with theory. And observation statements do not provide a secure basis for science as they depend in their turn for their security, on theory. The scientific revolution of the 16th and 17th centuries was not achieved by a search for "the facts" and by developing generalizations from them, but by a slowly evolving development of better theories. The question that thus arises is, How can we know when one theory is better than another?

One answer (which derives from the work of Popper: 1968, 1969) which has received considerable support, is that a theory must be framed in such a way that it is **potentially falsifiable.** To be potentially falsifiable is to allow in the way that the theory is stated, for empirical claims which can count against its truth. The theory must not be so stated that no conceivable empirical happening could falsify it. If a theory stated in this way stands up to all the tests we can throw at it and survives unfalsified, then for the time being it is viewed as the best theory we have and can be allowed to enter the scientific pantheon. It becomes part of science at that point in time. But once it is falsified, it must be rigorously dismissed. Thus we learn from our mistakes, and science can be viewed as a continuous evolution of better and better approximations, as one potentially falsifiable theory succeeds another.

I believe that there is much that is acceptable in this view. In fact some extremely eminent scientists, such as Sir Peter Medawar and Sir John Eccles take this view of science. Firstly, one very important point that it is that this emphasis on potential falsifiability makes us conscious that scientific explanation must never be allowed to **remain** metaphysical. (Unlike those critics who complain that science doesn't really **explain** because it doesn't show **why** something is the case, but merely shows

that something is the case, I see scientific explanations as genuine **explanations** because they state **that** something is the case, in a way which is always potentially falsifiable. Whereas, it is a characteristic of metaphysical doctrinal and religious systems (which the critic sometimes has in mind as being explanatory) that they are usually compatible with things happening in any way whatsoever).

Secondly, stress on falsifiability emphasises the importance of describing systems, situations and experiments in such a way that they can be **replicated** by others in other places and times. ". . . the institutional life of science is designed to submit the observations put before it to rigorous international and cross-temporal criticisms: it demands that these observations take a form which will allow of such criticism" (Passmore, 1978, 84).

But one problem is that Popper takes too uncomplex a view of falsification. For if all observation statements themselves rely on theory, as I claimed at the beginning, then there will be a problem about how we know when an observation statement has actually performed its job of falsification. As Lakatos says, on the approach to better theory which I have just outlined:

> The **assumption** is that there is a natural, psychological borderline between theoretical/or speculative propositions on the one hand and factual or observational (or basic) propositions on the other . . . This assumption is complemented by a **demarcation criterion**: only those theories are 'scientific' which forbid certain observable states of affairs and therefore are factually disprovable. Or, a theory is 'scientific' if it has an empirical basis (1971, 97, 98).

But because of the theoretical basis of observation, there are complications for both the assumption and the criterion. The assumption makes too simple a distinction between theory and fact. For falsificationists like Popper argue that **acceptance** of theories is always tentative, whereas the **rejection** through falsification, is decisive. But now we see that because rejection depends on observation and observation statements are always themselves theory-laden, then they too are always potentially in error. Since theory-acceptance is always tentative, observation statements which presuppose theory must always be tentative.

Indeed the history of science is replete with examples of the rejection of observation statements rather than the rejection of the theories with which they conflict. Flamsteed's corrections of his astronomical observations in the light of Newton's theory is one of the more famous examples. And there are also difficulties with the demarcation criterion which argues that the difference between what is scientific and what is not is that scientific claims forbid prior to the fact, certain states of affairs. But of course, a theory can often be protected from falsification by deflection of the critical observations onto some other part of the initial conditions, auxiliary assumptions, instrumentation, etc. In fact there are plenty of good historical examples which show what turned out to be the quite legitimate deflection of criticism onto features other than the theory proper.

Given these logical difficulties, can we then ever rationally eliminate a scientific theory, or is it merely a matter of Kuhn's psychological shifting? (1970) The answer is yes, if we see a more sophisticated falsificationism in terms of a temporary set of conventions which can themselves be discarded. And in fact, a real test of a theory is not a two-sided struggle between a theory and an experiment or observation. Real tests of theories in history are three-sided contests between various experiments, observations and rival theories (Lakatos, 1970, 129). And because of this complexity, some of the most significant tests result in confirmations rather than falsifications.

A more sophisticated falsificationism regards a theory (T1) as falsified only if another theory (T2) has these features:

(1) T2 has excess empirical content over T1, i.e. it predicts (or postdicts) novel facts, facts improbable in the light of, or even forbidden by T1.

(2) T2 explains the previous success of T1, i.e. all the unrefuted content of T1 is contained (within the limits of observational error) in the content of T2; and

(3) some of the excess content of T2 is confirmed. (Lakatos, 1971, 116).

Thus on this view, Newton's theory was falsified by Einstein's because it predicted such features as the bending of light rays near large masses, a fact not even conceivable in Newton, was able to explain all of Newton's successes (as a special limiting case of Einstein) and various aspects of

Einstein's theory were confirmed, e.g., the bending of light during Eddington's 1919 eclipse experiments (Lakatos, 1970, 124). It is still the case then that observations remain importantly at the base of scientific progress, and ". . . learning about a theory is primarily learning which new facts it anticipate(s)" (Lakatos, 1970, 123). For instance, Newton's theory predicted the return of Halley's comet, the existence and course of Neptune, the bulge of the earth. "What really count are dramatic, unexpected, stunning predictions" (Lakatos, 1980, 6).

But the progress of science is concerned not merely with one theory's being superceded by another; it is also concerned with developments within a theory itself. Lakatos stresses the point by calling a successful theory like that of Newton or Einstein, a 'scientific research programme'. And it is the programme as a whole which develops. Such a programme has a 'hard core'. Any challenge must be directed elsewhere than the hard core by those who wish the programme to develop. They must use their ingenuity to work out auxiliary hypotheses which form a protective belt around the hard core and redirect any challenge at these. Such moves can be seen to be successful if by directing the challenge elsewhere than at the hard core, novel predictions continue within the theory. Lakatos points to Newton's programme as an exemplar of this interpretation, in which he and his followers, ". . . turned each new difficulty into a victory of their programme" (Laplace, 1796, quoted in Lakatos, 1971, 133). In Newton's case the challenge was diverted away from his three laws of motion and his law of gravitation (Lakatos, 1970, 23).

Particular scientists thus demonstrate firm convictions about the phenomena which nature can yield and about the ways in which these may fit the theory, and to understand a science is indeed to gain, ". . . a deep commitment to a particular way of viewing the world and of practising science in it" (Kuhn, 1970, 358). And it is fortunate for the development of science that scientists do show tenacity, otherwise too many good theories would be still-born (including even Newton's). But the good scientist also agrees that anomalies do exist, although they cannot be easily explained at the moment. But for the theory to remain rational, it must not be allowed to become sacrosanct like a dogma or doctrine.

Further conceptual tools, which parallel much of what has already been said are provided by Feyerabend's ideas. Feyerabend points out that much of the actual development of science came about through the evolution of what in the first instance were largely unsupported theoretical speculations which were actually contradictory of then well-accepted scientific claims and supposed scientific facts. One of the most famous examples of this was the evolution of heliocentric theory. As he says,

> The Aristotelians could quote numerous observational results in their favour. The Copernican idea of the motion of the earth, on the other hand, did not possess independent observational support, at least not in the first 150 years of its existence. Moreover, it was inconsistent with facts and highly confirmed physical theories. And **this** is how modern physics started: not as an observational enterprise **but as an unsupported speculation that was inconsistent with highly confirmed laws** (Feyerabend, 1968, 13)

Feyerabend goes further than Lakatos and argues that better science comes about only if we continue to work with several theories at the same time and only if we deliberately try to construct alternatives.

> You can be a good empiricist only if you are prepared to work with many alternative theories rather than with a single point of view and 'experience'. This plurality of theories must not be regarded as a preliminary stage of knowledge which will at some time in the future be replaced by the One True Theory . . Theoretical pluralism is assumed to be an **essential feature** of all knowledge that claims to be objective (Feyerabend, 1968, 14).

Furthermore, some facts will emerge **only** if we use alternative approaches. Feyerabend discusses the issue of Brownian motion. Brownian motion is the irregular, zig-zag motion of the solid particles suspended in a colloid (solid in liquid, solid in gas, etc.) caused by the collisions between such particles and the molecules of the fluid in which they are suspended. As Feyerabend points out, the existence of Brownian particles refutes the second law of thermodynamics. As such it provides a fact relevant to the acceptance or rejection of that law. He asks whether the relationship between the law and the particle could have been discovered by a merely observational investigation of the consequences of the law and the theory of thermodynamics. And he suggests that on such an approach it could never have been shown that Brownian motion refutes the second law, for " . . a 'direct' refutation of the second law that considers only the phenomenological theory and the 'facts' of Brownian motion is impossible" (28). In fact, the refutation was achieved very

differently. It was achieved, ". . . via the kinetic theory and Einstein's utilization of it in the calculation of the statistical properties of the Brownian motion" (29).

Indeed some theories quite restrict observation, and in extreme cases almost replace it. Sometimes, even the phenomena to be "explained" are decided by the theory. Thus, as was mentioned earlier, theories and presuppositions of one kind or another always play their part in scientific observation and development. So the good scientist is he who continually subjects his ideas to alternatives.

Feyerabend's approach therefore emerges as a specially sophisticated sort of falsificationism which sees science as progressing only through the deliberate construction of alternatives which can indicate inadequacies in both theoretical and factual claims. As he says, ". . neither 'facts' nor abstract ideas can ever be used for defending certain principles come what may" (1968, 39).

What emerges from this second action then, is the crucial place that alternative theories can occupy. Alternate theories need to be developed as counterweights to those which appear to be most acceptable at present.

How Can We Achieve Improved Teacher Education Theory?

What are the implications in all this, for theory in teacher education?

Though natural scientific understanding and progress are a paradigm of human endeavour, I am of course not suggesting that the theories which we offer to student teachers are just a form of applied natural science (though some may be). I have pointed to the importance of alternative theories in the development of natural science; and, that the aspects of natural scientific development just discussed seem to be significant may be an argument for developing parallels of various sorts, as long as we continue to keep in mind the significant differences in the situations.

It is clear that we cannot easily bring about the falsification of say, behaviourism by pointing to the successes of Freudianism, **in the way that** Newton was falsified by Einstein's theory. That took decades anyway. But at least we can try to be openminded and eclectic, and we can deliberately cultivate alternative theories as explanations of activities, as Feyerabend suggests. And as the natural scientific case shows we must never expect to produce "The one true theory". Even more so must we not expect this to arise in the teacher education area. And at least, consideration of natural scientific theory opens our minds to **possibilities.**

Feyerabend's approach is I think crucial in teacher education. For when people become familiar with a theory they often begin to see the world in its light. (That is a metaphor; it is close to being literal). The person incorporates the concepts and categories and explanatory principles of the theory into his world view, and he "dwells in them" in such a manner that he is often no longer consciously aware of their use. Some teacher educators are so immersed in their behaviourism or pragmatism or existentialism or systems theory that they see only those aspects of educational situations which are revealed by their theory. As Polanyi puts it:

> A theory is like a pair of spectacles; you examine things by it, and your knowledge of it lies in your very use of it. You dwell in it as you dwell in your own body and in the tools by which you amplify the powers of your body (1975, 37).

But of course the opposite of theories as pairs of spectacles is theories as blinkers. Too commonly theories become just that. Hence the need forteachereducatorsand student teachers to encounter numerous theories. Teacher education theory just has to move forward (in Lakatos's terms) in an "ocean of anomalies", by using diverse theories.

It is not possible for most of us to actually try to **construct** alternative theories, but it is possible to familiarize ourselves with alternatives. Feyerabend's and Lakatos's views provide argument for this approach. And with a slight shift their views may indicate the good sense of working within one theory but being as eclectic as possible. In teacher education theory we have had for several decades alternative approaches from psychology, sociology, philosophy, etc., and alternatives within these. But it is doubtful if most teacher educators have used these theories to throw critical light on one another. Indeed, when for instance, philosophy has been used in this manner, its comments have often been ill received.

I have in mind the theory of curriculum objectives rubbing against the newer theory of rules and procedures for curriculum; the presentation to students of a wide range of different psychologies; the return of historical studies as a counterweight and contrast to sociology; the fruitful clash of modern analytic philosophy of education with the older "philosophical schools" approach; and so on.

And just how far do we **range** in our Feyerabendian approach to the use of theories? I recently encountered an argument in aesthetics which drew a distinction between justification in the sphere of art and justification of purposive activity (Beardsmore, 1971, 10) which seems to me to have application in our distinction between education and training. I think this sort of thing is a reminder that to understand the human condition demonstrated in really educational activities, we need to look not just to science of the positivistic sort, but to science of the European Geisteswissenschaften sort, and to the humanities.

What is more, underlying all intellectual striving there always lie the theories and presuppositions I have already pointed to. If we have a narrow intellectual base, then we bcome victims of our presuppositions rather than using them as tools for understanding. We therefore need to look out for theoretical presuppositions in teacher education theory. For instance, do researchers into moral education really grasp the complexities of what they are trying to deal with? It seems to me that they beg enormous questions, and a close examination of their presuppositions is what is called for, **before** any activity of so-called research occurs. This is because morality is a complex mixture of knowledge, feeling, habit, action, thought and judgment. At least John Wilson, despite some problems, was going in the right direction when he broke moral education down into his components of Phil, Emp, Gig, Krat, Dik, etc. (Wilson, 1967, 1970).

Of course it **may** be that natural scientific type explanations are all that is needed in order to understand education. And there are some sophisticated and distinguished exponents of this viewpoint. But certainly the claim has not been **demonstrated** to be the case. And it ought not just be **assumed** to be the case. For that is scient**ism** — — the view that tries to convince us that scientific type explanations and descriptions are all that human beings can achieve if they are to understand.

Now it is true to say that if something cannot be analyzed and classified and measured, then it eludes science. But it is only science that it eludes. That it eludes science does not mean that it cannot be understood. Indeed there seem to me to be definite limits to the scientific approach. With scientific methods you can find out only that which your methods and instruments are themselves capable of finding. If we proceed on the basis of scientific postulates methods measurement and quantification, then we may not be able to detect thoughts, feelings and purposes, hopes and fears or measure various amounts of these. But this does not show that such things do not exist or have no meaning or importance. There can be many interpretations of a thing, or a person, each true **as far as it goes**.

Certainly we must use rigorous methods. But rigour is not confined to the methods of science. In teacher education we require rigorous application of our total cognitive processes, which try to interpret the meaningfulness of situations. Actions and meaning interrelate with feelings, thoughts, expressions. Human beings who are being educated are not just rule-following, but also rule-**making** entities. We need to pursue understanding in the sense of sympathetic insight into the mental life of others. Indeed, where attempts are made to reduce psychology and sociology to what can be directly observed and statistically evaluated, a whole range of human experience and meaning is lost, and human beings are trivialized: men seek recognition as well as food.

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