The future of research on cognitive structure and conceptual change

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About ten years ago there was a surge in reports of probes of understanding of scientific concepts. The surge appears to have occurred spontaneously in several widely-separated countries. We now have a considerable number of fascinating studies, and enthousiasm for producing more shows no sign of diminishing. This remarkable research effort stimulates questions that themselves can be targets of research, though research of a different type from the probes themselves.

The first question I propose for study is, Why did the surge occur when it did, say about 1976 with examples of early studies being those of Za´rour (1976) and Nussbaum and Novak (1976)? At the time research was still dominated by Campbell and Stanley´s (1963) description of various experimental and quasi-experimental designs, so that most studies involved complex factorial experiments in which effects and interactions of several brief treatments were evaluated by comparing mean scores of blocks of subjects. There are three points to note about those experiments. First, the scores were from tests that were much less sophisticated than the experimental designs and the statistical manipulations to which they were subjected. Mostly they came from pencil-and-paper tests of recall of facts or performance of simple algorithms; occasionally true problems were used, and sometimes affective measures were taken. There tended to be little theory behind the choice of items. Their nature and number were determined intuitively or to suit managerial constraints such as the time available. They were marked right or wrong and cumulative scores were found by adding

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the number correct. The nature of errors and the thinking behind them were ignored. Second, the variation between scores in each cell of the experiment was ignored except as an entry in statistical tables. Often labelled 'error', it was seen as a nuisance rather than as a matter of prime importance. Third, although the design of the studies was complex, the psychological model on which they were based was simple. Usually there was not much more to the model than the notion that teaching directly determined the outcome, a model that excluded characteristics of the learners (other than intelligence, as measured by a speeded IQ test) and of the context in which they were placed and what they made of it.

In contrast to the Campbell and Stanley style of experiments, the probes of cognitive structure were simple investigations, no more than a measure without any explicit or intended treatment. However, the measures were subtle and deep. Although summary results for whole groups were reported, there was appreciation of the individual. The thinking behind people's responses was considered seriously.

Why did people make the sharp change from the experiments to the probes? In answering that question it is not enough to discover reasons for the turn from experiments; we need to know why there was a turn <u>to</u> probes of cognitive structure. After all, there are other alternatives to the experiments. Also it would be useful to know how this surge in interest in probing understanding of scientific concepts came about at much the same time in the United States, Canada, Australia, New Zealand, Britain, France, Sweden, Israel, Lebanon, Austria, West Germany and South Africa. Was it spontaneous? Or were there links that can be traced between the researchers? In answering this set of questions we need to consider another: Why did the research not appear in all countries? In most, of course, it is because of general lack of involvement in educational research, but what about an advanced country like Japan? Was there a similar occurrance there? If not, why not? If so, why is it not better known? We could all speculate about these issues, but it would be better if someone went beyond speculation and did systematic research on the questions.

Another puzzling question is, Why did earlier investigations of understanding of scientific concepts not proliferate like the ones of the 1970s? Marton (n.d.) describes probes by Swedish scholars in the first years of this century; Oakes (1945) used procedures that are so like some used in the late 1970s that Gunstone and I, for one pair of recent researchers, look like plagiarists; and although he appears not to have been interested in oedagogy, Piaget's interview work was published steadily from the 1920s on. These earlier instances change the focus of the question. It becomes, Why did not the Swedish work persist? Why did not Oakes' work stimulate similar studies from many other people? Why did Piaget have little influence on science educators before the 1970s? In commenting on a draft of this paper, Northfield suggested to me that the recent research on alternative conceptions has flourished because it is of direct interest to teachers and provides them with practical meanings for terms such as action research and schoolbased curriculum development. The relevance of the research to teachers maintains the interest of investigators. That may be so, but it would merely shift the question to: Why did not the earlier work interest teachers to the same extent?

Most of us who have been involved in probing understanding may not have the skills that are required to tackle questions like those I have been proposing. We may need to bring them before historians and sociologists. Who ever answers them, there is much to be gained from them. August Comté made an important point when he said 'A science cannot be completely understood without a knowledge of how it arose" (Comté, 1830-1842/1855, p.43). The energy with which we probed understanding carried us along for a decade: now we need to understand what we have been doing, in order to judge well what it would be best for us to do next.

Certainly we have been vigorous in our research. In 1986 my colleague Dick Gunstone checked the holding of the Monash University library for publications on cognitive structure and conceptual change. His list was not intended to be comprehensive, but it includes seven books or collections of conference papers (Archenhold, Driver, Orton & Wood-Robinson, 1980; Driver, 1983; Driver, Guesne & Tiberghien, 1985; Duit, Jung & von Rhöneck, 1985; Helm & Novak, 1983; Osborne & Freyberg, 1985; West & Pines, 1985), eleven review articles (Driver & Bell, 1986; Driver & Easley, 1978; Driver & Erickson, 1983; Fensham, 1984; Gilbert & Watts, 1983; McCloskey, 1983; McDermott, 1984; Osborne, 1982; Osborne & Schollum, 1983; Osborne & Wittrock, 1983, 1985), and more than a hundred journal articles. In addition to these are the numerous and valuable in-house publications of centres at the universities of Waikato, Leeds, Göteborg, Paris and Kiel and a large number of unpublished conference papers and dissertations. The books, articles, papers and dissertations report results obtained through a variety of probes, including concept maps, Venn diagrams, prediction-observation-explanation tasks and interviews of understanding of many topics, among the most popular being the nature of matter, energy, motion, force, gravity, electricity, life, natural selection, chemical change, combustion, floating and sinking, light and vision, and heat and temperature.

This volume of work suggests further research questions. Why were these topics chosen? Was there systematic choice? Were the researchers conscious of why they had picked each topic, were they influenced by factors that can be identified or was it all eclectic, a picking up of whatever happened to occur to them? Why, for instance, did we not focus on magnetism or sound? Perhaps someone should write to all the researchers asking why they chose the topics they did.

An interesting test of our understanding of the relation between content and prior experience would be to check whether we can take an unresearched topic such as magnetism and predict the range of alternative conceptions that we will find in a population and which conceptions will be most common. If someone wrote to us, asking for our predictions, it would be interesting to see how closely we agree with each other, why we made those predictions, and how well the predictions accord with subsequent results. Perhaps a prize could be given at next year's meeting for the most accurate prediction, and another for the most satisfactory explanation of the discrepancy between prediction and observation.

If the probes of understanding that have been made have led to principles that enable accurate predictions of common alternative conceptions, there are important consequences for curriculum development. Most curricula and teaching sequences in texts appear to be based on a tabula rasa image of the learner, and a model of learning that considers only addition of information without concern for what the learner already believes. This may not be through lack of appreciation of the importance of prior knowledge but rather because of inability to guess what ranges that knowledge is likely to encompass. Perhaps we can now make recommendations about the teaching of any topic, even those that have not yet been researched. If we cannot yet do this for unresearched topics, we need to consider whether it will ever be possible. Must we probe empirically each topic in order to list its likely alternative conceptions? It would be better if our work to date could be distilled into a theory of content.

If it should turn out that we cannot predict common alternative conceptions, there is no need for despair - the number of topics is finite, and the key ones in science are not so numerous as to make the task impossible. We would, however, need to consider what the key topics are. The knowledge that is of most worth will vary with time and place, of course. At present I would imagine that concepts like energy and ecology would be rated highly, while in other social circumstances statics or astronomy might be more important. The choice is subjective: but I see value in a debate about what knowledge is of most worth in science, for that debate could guide research and curriculum development.

A theory of content, which would include principles for predicting likely alternative conceptions, must merge with a theory of knowledge. I use knowledge in the sense of the information stored in an individual's mind, so that for me it is synonymous with cognitive structure. We need to develop further our notions of cognitive structure. I was trying to do that in an early article (Gagnè & White, 1978) and some conference papers (White, 1979; White & Gunstone, 1980), but I fear that I left my thoughts in a primitive state. Novak (1977) tried to do it, too, in refining on Ausubel's theory.

I do not have clear ideas about the form that a theory of content and cognitive structure should take. Novak's notions and mine differ over the unit for representing knowledge, he favouring concepts and I diverse smaller elements like propositions, algorithms, images and episodes; but in both cases the model is of an essentially static cognitive structure that changes only when new inputs of information are received. More likely memory is not static but kinematic, with fluid, ever-changing connections between frequently reconstructed elements. A better representation of cognitive structure might be one that captures that fluidity.

Even a better model of cognitive structure will not by itself help us to predict difficulties learners may have with specific content, nor allow ready deduction of appropriate remediation. Consider the following case described by Marton (n.d.) from a study by Dagmar Neuman on young children's knowledge of arithmetic. When a little girl was asked 'If you have four pencils in your desk and I give you five more, how many would you then have?' she answered five. When she was asked to explain her answer she raised four figures and said 'You have four,' and then as she said 'And then you have five more' she raised her thumb. From this and parallel examples Neuman inferred that children confuse names for fingers or objects with quantities. When counting on their fingers they <u>name</u> the first finger one, second two, and so on, so that the number of objects becomes the <u>name</u> of the last one in the series. Thus the little girl was interpreting the statement about being given five more as the adding of another one to correspond with her thumb. For full understanding of this sort of situation we need a theory of content as well as a theory of memory. It may be that a general theory of memory is possible but a comprehensive one of content impossible. Nevertheless, we should try to develop one.

In further development of theory we should add affective aspects to cognition. We might incorporate notions of confidence in, or certainty about, knowledge. In probing what people believe about a scientific principle or phenomenon researchers might also ask them how sure they are of the propositions they put forward, and why they believe them. Perhaps some researchers already do that. This procedure could provide insights to the problem of how to get people to alter their conceptions.

Most of our research effort so far has been directed to probing conceptions. Though, as I have said, a lot has been done, there are two ways in which this work could be extended. We have tended to work on concepts like 'life', 'force' and 'chemical change', rather than to take as targets propositions like 'the world is round', an exception that was studied by Nussbaum and Novak (1976). We may get different insights into cognitive structure if we tackle understanding of propositions. I suggest as examples 'Green plants make their own food', 'White light is made of many colours' and 'Acids neutralize bases'. The second extension is into concepts from subjects other than science. Not only would results for non-science topics be interesting in their own right, but also they would be bound to provide broader insights on the learning of science ones. The probes that have been used in science could readily be applied to concepts such as 'profit', 'crime' and 'musical form' and to propositions such as 'power corrupts' and 'English words have many origins'.

Besides probing the nature of alternative conceptions, we can study their formation and how to change them. Compared with the amount of work on probing there has been relatively little research on formation and change.

We should explore the sources of alternative conceptions. Although there has been speculation about their formation few if any researchers have asked students where they got their ideas from, nor have there been longitudinal studies tracing the emergence and development of a conception. At the very least we should alter the balance in the probes of alternative conceptions so that more are done with very young children. Also, where a person's conceptions differ from those of established science we should try to discover whether they arose from incorrect observations, different interpretations of correct observations, erroneous teaching, misinterpretation of correct information, inconsistent logic, or a different but internally consistent system of logic. In this exploration we should cast off our inheritance from experimental psychology of considering the learner as an isolated individual. Humans are social beings. We must incorporate social context in our thinking, because cognitive conceptions are socially determined.

Knowing more about how conceptions form should help us to find out how to change them. There have been attempts to change students' conceptions (e.g. Gunstone, Champagne & Klopfer, 1981; Hewson, 1982), but it turns out to be difficult. The study by Gunstone, Champagne and Klopfer (1981) is disturbing: after working intensively with 12 able seventh- and eighth-graders who initially displayed pre-Galilean views of force and motion, the researchers were almost convinced that they had succeeded in getting the students to exchange their original views for Newtonian conceptions. Unfortunately, in the final session deep probing revealed that the pre-Galilean beliefs persisted alongside the added Newtonian knowledge. As well as indicating the pedagogical difficulty of bringing about change, this result created a logical problem for research. It is straight forward to check whether new knowledge has been added, but now we want to know also whether other knowledge has been discarded, or, as it may be better to put it, whether appropriate reconciliations have been made between the new knowledge and the old. Attempts to change conceptions will not be convincing without extensive, sensitive and subtle attempts to allow learners to demonstrate the continued presence of their initial beliefs. The danger is that we could believe that some procedure is effective in promoting change of conception when all that it does is add new knowledge without affecting the old.

Why is it difficult to bring about real change in a conception? Posner, Strike, Hewson and Gerzog (1982) set out conditions that must be met before people will change an old idea for a new: they must be dissatisfied with the old notion and find the new one intelligible, plausible and fruitful. It is not as easy as it might seem to set up these conditions. Dissatisfaction with the existing idea is particularly difficult to bring about; after all, if it had not been serving well up till this point it would have been abandoned earlier. It will take a powerful new experience to overthrow all the earlier positive experience of the usefulness of the old notion.
Intelligibility and plausibility of the new idea are not so hard to establish, but fruitfulness is more troublesome. This is because people move between many contexts and can find one view fruitful in, say, a school context and another in an out-of-school context. We do that ourselves when, to use Beverley Bell's (1981) example, we change our meaning of a word like animal from scientific to common usage. Changing conceptions then becomes a matter of showing that the scientists' one is more fruitful in several if not all imaginable contexts, which might not be easy to arrange. In any

event, in probing a conception researchers might be advised to make the context clear, or to ask the subjects what they think the context is, or to vary the contexts to see what effect that has on their conceptions.

Another difficulty in bringing about change is that episodic memory is plastic; rather than the conception being changed to fit the evidence, the recollection of the evidence can be altered to fit the conception. This is well illustrated by Gauld (1986). After getting students to set up a simple circuit of a battery and globe, he asked them to choose between four conceptions of electric current, in which the current comes:

A. from one end of the battery and is all consumed in the globe;
B. out of both ends of the battery and reacts in the globe;
C. from one end of the battery, some is consumed in the globe and the rest continues back to the battery;
D. from one end of the battery, squeezes through the globe filament and all returns to the battery.

The students then had to use their chosen model to predict the relative sizes of readings on ammeters placed each side of the globe. Then the ammeters were put into the actual circuit and were read. The students had to resolve any discrepancy between their predictions and observations that the meters read the same, which meant acknowledging the accuracy of model D. Three months later they were asked again about their conception of current. The transcript for a student who initially chose model C illustrates revision of the episode:

> (model C confirmed because) the meter here was more than this one but I´m not sure what they actually read ... I think this one here was double that one ... (not model D because) that was proven wrong by the meters. (Gauld, 1986, p.52)

It is not obvious what can be done to overcome this plasticity of memory. One potentially useful approach that occurs to me comes from synectics, a detailed procedure devised by Gordon (1961) to increase the creativity of engineers. In describing how to get engineers to see familiar problems in new ways, Gordon specifies a sequence of certain states of mind that must be encouraged, such as withholding judgement and hedonistic response (of satisfaction with the solution). In attempting to bring about conceptual change we might do well to consider this notion of states of mind and should consult Gordon´s book to see how experiences can be arranged to bring them about.

The balance of our research should, for practical and theoretical reasons, shift from elucidating alternative conceptions to these unresolved problems of how to change conceptions. The practical reason is that teachers will want advice about how to promote change. We will be expected to stop having fun pointing out deficiencies of learning and to come up with solutions. The theoretical reason is that a complete understanding of cognitive structure and conceptual change demands established propositions about not only the formation of conceptions but also the tenacity with which they are held and the conditions under which they alter. Although investigating change is more difficult and tedious than uncovering alternative conceptions we should turn to it.

A potentially fruitful way of overcoming the capacity of humans to reconstruct episodes and to maintain different conceptions for different contexts is to get people to reflect on their knowledge, checking its meaningfulness and whether new information accords with present beliefs. Those acts are aspects of metacognition. Therefore it should be helpful if more research were done on metacognition. Baird (1986; Baird & Mitchell, 1986; Baird & White, 1982a. 1982b, 1984) has been active there. Unfortunately metacognition studies require considerable effort: the last two of Baird's attempts to make learning styles more purposeful ran for six months and for two years. The longer one, known as the Project to Enhance Effective Learning (PEEL), has brought about changes in teaching and learning styles, but no systematic evidence has been collected about its effect on students' conceptions. Metacognition training remains a promising but untested means of bringing students to check the relative worths of conflicting beliefs.

Sooner or later Special Interest Groups go out of existence, either by failing or succeeding. Ours could fail by descending to mindless repetition of probes of alternative conceptions or to sloppy, brief attempts to bring about conceptual change. Success is when our research is recognized as so central and important that it is no longer special but general, and the group is absorbed into the greater body. Since our aim of understanding and improving learning is central to education, we may indeed succeed. The lines of research that I have advocated, of investigating why the sharp change in research style from classic experiments to the probing of alternative conceptions began when it did, why it occurred in many countries but not all, and why the movement flourished in contrast to earlier instances; of testing how advanced is our understanding of the relation between experience and the formation of alternative conceptions; of building a theory of content and better models of cognitive structure, and merging them in a more general theory of memory that also incorporates affective components; of probing understanding of propositions as well as concepts; of extending probes to subjects other than science; of longitudinal studies of the formation and development of conceptions; of determining the conditions that foster change in conceptions; and of exploring the potential of training in metacognition for promoting good understanding; will, I trust, be the way to success.

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