Neuroscience and education: at best a civil partnership: response to Schrag

Francis Schrag refers only briefly to the paper I wrote some years ago about 'Brain-based learning' (Davis, 2004), but his observations raise some interesting and important questions about the marriage, or possibly the uncivil partnership, between neuroscience and education. I will devote a few paragraphs to addressing his criticisms of my arguments, and also make a few observations on a broader range of issues covered in his treatment.

He quotes me as saying that a brain-science perspective on learning requires the relevant states to be 'confined, so to speak, to the interior of individual learners' (Davis, 2004 pp. 27-8). On his account, I then argue that '..however extensively we investigate brain processes and states we will be unable thereby to make direct positive discoveries about learning' (ibid. p 31). Note that, in the 2004 paper, I sought to explore how far brain science might, in principle, contribute to our *understanding* of learning, and in some measure I supported the claim that neuroscience might well be helpful when looking at the consequences of *deficits* in brain functioning.

Schrag exploits my use of the term 'positive' in his objections to my argument, so I need to say something about this. I agree with much of what he says when he compares the implications for learning of potential discoveries about brain processes with the implications of discoveries about neuromuscular systems for athletic performance. Inspecting the state of muscles could just be, or so I imagine, a fairly direct way of examining the biological states of affairs that comprise the causal basis of observable muscular strength. It might also provide an indirect access to some of the necessary conditions for performance at a given level in football, rugby or cricket. So, if someone discovers a deficiency in the state of someone's muscles, they are certainly finding out about a causal factor in poor athletic performance. If they knew what drugs to provide, or what training regime to prescribe that would remove this deficiency, then it would be fair to say that their knowledge was, in some sense, a 'positive' discovery about how athletic performance might be enhanced. Similarly, it might be possible to determine that someone's brain was in a 'damaged' condition – one resulting, for instance, from a stroke, and be clear that this was a causal factor in a deterioration in cognitive functioning. Again, were medical advances to enable the damage to be repaired, this also could be said to constitute, to use my term above, a 'positive' discovery about how cognitive functioning might be enhanced.

As I argued in effect in the 2004 paper, the marriage of neuroscience and education has a sound basis where we are dealing with 'deficits'. Few would dispute that stroke patients or those suffering from head injuries are suffering from deficits. The idea of 'enhancement' begins to encounter some challenges, however, if we seek to extend the 'marriage' to some positive applications. There will be agreement about what counts as enhancing when we are dealing with athletes. In contrast, we lack a consensus about what we want a person to be able to do as a result of education, and indeed, about what counts, for instance as intelligence.

Schrag argues that, 'from the fact that some activity cannot be reduced to the performance of its "internal", bodily components, it doesn't follow that modes of intervention aimed at enhancing the performance of those components won't enhance the person's ability to engage successfully in the activity (this volume, p.?). I did not argue otherwise in my 2004 paper. There is an interesting possibility of what looks like category confusion here, however, even though I very much doubt that Schrag himself would be guilty of it. 'Enhancing' biological components is one thing, and 'enhancing' learning, abilities, and so on is quite another. If neuroscientists could deliver a drug that would

'enhance' the functioning of a Hitler's brain, it would not necessarily 'enhance' his personal functioning in any sense that most of us would support. When we speak of 'enhancement' in connection with a biological item such as the heart, it is often possible to give a precise and uncontroversial specification, informed by a scientific account of the function of the organ in question. This, of course, is *not* the case when we move to the distinct domain of discourse concerning persons, learning, knowledge, and so on.

Taking a less contentious and emotive example, we could ponder Schrag's description of Cohen-Kadosh's 2010 report concerning electrical stimulation being used to 'enhance' numerical abilities. Obviously, in order to evaluate this report, we need to ask just how 'numerical abilities' are being measured here, and, indeed, what is meant by a numerical ability in this connection in any case. Cohen-Kadosh simply include in their paper a description of a couple of (alleged) tests of numerical abilities. Their focus is on an automatic number processing ability. Something called the 'Stroop test' is, apparently, a way of measuring such an automatic ability. In that test:

> subjects are presented with two numerical stimuli on the computer screen and are required to compare the stimuli according to their physical size. The stimuli can be incongruent (e.g., a physically large 2 and a physically small 4), neutral (e.g., a physically small 2 and a physically large 2), or congruent (e.g., a physically small 2 and a physically large 4) (p. 2016)

The researchers claim that longer reaction times indicate that people are competent automatic numerical processors, while people with difficulties in this area do not delay their responses to incongruous images. Cohen-Kadosh report in the paper that the electrical stimulation lengthened the reaction times that subjects manifested when confronted with incongruous images. It was concluded from the fact that the stimulation had such a consequence, that their automatic number processing was being 'enhanced'.

Now it is not clear that educators would be necessarily committed to the value of 'automatic number processing' as described by these researchers, or, at least, not necessarily committed to their value throughout the whole period in which students learn about number. We cannot tell whether, let alone how, success on these tasks might be linked to a rich relational understanding (Skemp, 1989) of number, where individuals have good cognitive maps of the concepts concerned. In this blessed state, as and when necessary, students can think through the rationale of routines that may at times have become unthinking, thin procedures. Many maths educators will feel that relational understanding is a key aim of maths education, given that it is this type of cognitive achievement that enables mathematics to be used and applied in everyday life and the workplace, and that it is much more likely to fuel suitable motivation in the subject. There might turn out to be tensions between the possession of certain kinds of capacities to deal with numbers 'automatically' and the kind of rich understanding that fully empowers the learner of mathematics.

An Early Years teacher dealing with number might have some legitimate reservations about 'automatic number processing', and seek to diminish its cognitive influence, at least at certain points in students' development. I am not seeking to undermine the value of a whole range of 'automatic' processing as part of maths learning and performance. I am merely showing that issues of 'enhancement' remain open for educators to consider long after empirical researchers have settled the effects of electrical stimulation to their own satisfaction and according to their own measures. Educators would need to know much more about the situation before they could give it their unqualified support. From other things he says in his paper, I think that Schrag might agree with this.

My 2004 paper noted long-standing debates in the philosophy of mind about how thought contents are individuated. Schrag describes these discussions as 'arcane', and feels that he can sidestep them in his verdicts about 'neuro-education'. As a matter of fact, they have been central stage in the philosophy of mind and language since Putnam's classic 'Twin Earth' papers in the 1970s, and I believe they are extremely important for the topics with which Schrag is dealing. Internalists argue that thought content can be individuated as though it is indifferent to the context in which the individual concerned is situated. They hold that a person's psychological states supervene on that person's neurological states alone. However, externalists, supported in one way or another by the majority of philosophers working in the philosophy of mind, hold that internalism is not true for all psychological states, if, indeed for any. In their view, a significant proportion of the thought contents with which educators are concerned cannot be individuated in this way, but relate to states of individuals together with the complex sets of social practices and cultural phenomena i n which the individuals are embedded. These psychological states supervene on neurological states, together with states of affairs 'outside the heads' of the person in possession of those psychological states. Consider, for instance, an individual thinking about a £20 note. The event or events of her thinking in this way take in both neural processes and aspects of many social practices concerning money, the economy, banks, and, indeed, the intentions of others who belong to her community. It was in the context of these debates that I said, in the 2004 paper, that 'however extensively we investigate brain processes and states we will be unable…to make direct positive discoveries about learning'. This observation came at the end of a discussion of a thought experiment in which natural scientists subject a £20 to laboratory examination in an attempt to find out what it is. We know that this would be doomed to failure, and we understand why. The character of a £20 note is, of course, conceptually bound up with complex sets of social practices in the UK economy. Exclusively deploying the resources of natural science would, of course, sidestep this point, and merely discover the molecular properties of the paper used, the chemistry of the pigments, and so on. I compared the fruitless scrutiny of the paper and pigment of the note with at least some of the attempts to use neuroscience to investigate learning. Much learning is constitutively linked to aspects of culture, language and other key social phenomena. This limits the role of neuroscience *in principle.* There is a crucial difference between the sport example that Schrag deploys in his attempt to undermine my argument, and examples of cognition. It is perfectly true that, for instance, the laws of cricket are 'outside' the heads of individual cricketers, and that this, nevertheless, does not imply that medical scientists could never discover any treatments that would enhance cricket playing. However, scientists seeking the neural basis of cricket would *never* have thought that they could, so to speak, 'find' cricket processing elements in the brain. Of course, they would expect to discover aspects of neural processing necessary for the playing of cricket, but that is quite another matter. In contrast, some people, hanging on the coat tails of legitimate neuroscience have, it would seem, felt that aspects of learning can be found in the brain. Moreover, I am not convinced that researchers have always been as clear about this as they might have been. Consider just one example:

> Neuroscience has identified the brain areas responsible for language and reading, and also some of the abnormalities in dyslexia (Centre for Educational Neuroscience, UCL, 2012).

It is that term 'responsible' that is the mischief here. Schrag, commenting on a related example from the pen of Usha Goswami, feels that these ways of talking are just 'figures of speech'. I wonder. It is

unfortunate in the above example, moreover, that dyslexia is referred to as though it is clear that a specific condition answers to this label, when, in fact, this issue is still hotly contested. No one denies the existence of relevant symptoms. It is the status of claims about their causes or 'bases' that is disputed. See, for instance, Elliott and Gibbs (2008).

I want to make one final point, which does not relate to anything Schrag says about my paper. He asserts confidently that talk of brain lesions being mere 'concomitants' of an inability to recognize faces is too modest, and claims that the relevant neural states of affairs play a causal role in causing the inability. He may well be right, of course, but I suggest that he may underestimate the difficulties in pinning down how causes feature here. Certainly, neural events are necessary conditions for all psychological states, and, indeed, for symptoms that include any that are felt to manifest 'inabilities'. The notion of a 'lesion' implies some idea of 'damage', or the absence of normal neural functioning. Are scientists, let alone Schrag, in a position in which they can readily justify claims about the *direction* of the causal processes? That is to say, are their assertions that the neural events *cause* the relevant psychological events and behaviour always justified by the available evidence? In the face recognition case, how easy is it to exclude the following possibility: the very fact that certain patients stopped recognizing faces set in motion events that had specific effects on their brains? Such effects might have included the consequence that parts of the brain became 'atrophied' because they were not being used.

I have no idea how plausible that is in this particular case, but the importance of at least entertaining such a thought becomes clearer when we question the nature and direction of the relationships obtain between neural processes and cognition. There is extensive research into the *effects* on brains of a whole variety of interventions. To mention just one of a huge number that could have been cited, Kirk et al (2011) report that exercise training increases the size of the hippocampus, and, they claim, improves memory function in aging adults.

Be that as it may, it looks as though Schrag broadly agrees with my conclusions about the scope of neuroscience in education, even though he explicitly refuses to take my route. Neuroscientists, he feels, will not be 'able to guide teachers in their search for better quality curricula or methods of teaching'. That verdict certainly follows from the principled limitations that I have rehearsed above.

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