

To: Inquiry into Teacher Education
From: David Killingly

I welcome the inquiry into teacher training. I am an IT professional of 20 years standing, with the appropriate academic and professional qualifications. I have no involvement with or vested interest in the academic establishment, although I have had some exposure to this through the attainment of a Ph.D. in Molecular Genetics, my current candidature for a Master of Computing Studies, some years' employment in university research environments, and occasional tutoring of school and university students.

I am also enrolled in a Graduate Diploma in Education, and have completed 75% of the course. Despite some familiarity with various humanities and social sciences, I was nonetheless surprised by the poor intellectual standards and questionable philosophical underpinnings of the course, and more particularly by the lack of scientific rigour and lack of objectivity in the material presented.

So, my concern might be aptly described as that of a perplexed Dip.Ed. student, and of a life-long learner with a philosophical commitment to the pursuit and sharing of knowledge. In this submission I comment on the fifth article of the Terms of Reference, in relation to this course. In particular, I discuss course material in the form of recommended texts, and the subject matter presented through course notes.

Boylan *et al* (1985): Student Understanding about Light

Early in the Dip.Ed. course I completed an assignment for trainee science teachers. This involved replicating a study undertaken at the University. I invite the interested reader to review Boylan *et al* (1985), and assess it for scientific rigour. I consider this report inadequate at the high school level. It did not report primary data, and the survey questions were found by students (subjects and interviewers) to be ambiguous. The conclusions did not describe student understanding about light, but rather found that the students' informal terminology differed from the 300 year old formal jargon of optics. This is unsurprising, given that the formal jargon involved a borrowing and redefinition of the vernacular. The discussion made no attempt to explain this discrepancy, except as student ignorance. I was not surprised when the assignment feedback indicated that many trainees shared these criticisms, and arrived at conclusions different from those of the original study. Nonetheless, this study was provided as a model for emulation, and not as a good example of a bad example.

McInerney & McInerney (2002), Santrock (2004): Learning and Cognitive Development

A lack of intellectual acumen is evident in the choice of textbooks. Here I refer to two texts for core subjects: McInerney & McInerney (2002, *Educational Psychology*), and Santrock (2004, *Adolescence*). These books present a selection of theories of learning and cognitive development. The emphasis is on the collation and classification of the theories, rather than on skeptical analysis, or the elucidation of the processes of learning. A Web site (Kearsley 2003) recommended by the course provides the acme of this endeavour by listing over 50 different theories of learning. As presented by the recommended texts, the theories say little of substance and do not make quantitative falsifiable predictions. As admitted in the textbooks, the theories can be mutually contradictory – for example some attach greatest importance to intrinsic motivation, other to extrinsic motivation; some claim that learning is mainly facilitated by endogenous processes, while others claim that social interaction is more important.

Both texts feature the theories of Piaget prominently. It is interesting that outside of the field of educational theory, the work of Piaget does not contribute significantly (and perhaps not at all) to our understanding of learning and cognitive development; he is mentioned occasionally, generally with reverence, but nearly always in the context of errors in his observations, conclusions, or theories (e.g., Gardner 1982, Gopnick et al 2001, Pinker 1994, 1997, 1999, 2002, Marcus 2004). Gardner (1982, p.22) stated 'Piagetian theory [was] the rage twenty years ago.' In assessing Piaget, Santrock (p128) stated that '*questions have been raised about the timing of development, the four stages he identified, the usefulness of training in reasoning, and the effects of culture on cognitive development areas*': effectively everything that Santrock had reported

about Piaget' ideas was wrong. Given this assessment, why does Santrock spend so much time on Piaget? Part of the answer is given by Atherton (2003):

'His view of how children's minds work and develop has been enormously influential, particularly in educational theory . . . His research has spawned a great deal more, much of which has undermined the detail of his own theory . . .' [my bold]

So, the theory has been influential in the field of educational theory, even though it is wrong: this correlation has implications for the evaluation of both educational theory and Piaget. It appears to be a classic example of uncritical textbook propagation of canonized truth.

McInerney & McInerney (2002, p26) make the following statement about Piaget:

'As a biologist, Piaget was impressed with the way in which all species systematise and organise their processes into coherent biological systems and are able to adapt, as necessary, to the environment through processes such as assimilation and accommodation. He brought his eye as a biologist to the task of explaining the development of cognitive processes in children and introduced a conceptualisation (which sometimes intimidates the uninitiated) derived from the language of biology.'

From the perspective of a biologist, it is not clear what, if anything, this means. What does it mean for all (or even one) species to 'systematise and organise their processes'? How can one recognise a 'coherent biological system' as opposed to an incoherent one? What are the processes of assimilation and accommodation in this context? Which of Piaget's 'conceptualisations . . . from the language of biology' are of relevance in the current context of biological knowledge? As someone with biological training, what further 'initiation' would I require to make Piaget's theories less intimidating? What does 'adapt' mean in this context? I gained some insight into this last point when I read from other sources that Piaget, who was active into the second half of the twentieth century, never came to terms within Darwinian (i.e. selection-driven) evolution and has been called one of the last of the Lamarkians. It is an ironic parallel that McInerney & McInerney extract meaning from an outdated and non-instructive theory of learning, that itself incorporates a biological perspective that had been dismissed a hundred and fifty years earlier.

McInerney & McInerney (2002, p152) say about the full collection of theories of learning:

"...when these models and the strategies derived from them are combined, they have even greater potential for improving student learning. We should use a variety of models because: certain models are more appropriate to particular students needs; certain models are more appropriate to particular subjects; models can be adapted and combined to increase effectiveness; the multiplicity of school and classroom objectives requires a variety of models; and, lastly, the effective use of a variety of models enhances flexibility and the professional competence of the teacher. . ."

This statement raises several issues. Firstly, it is not clear from their text that there is any empirical evidence that these theories provide any mechanisms for improving learning. By contrast, Pinker (1997, p341) has pointed out that among American students, a decline in mathematical performance has accompanied the application of constructivist doctrines distilled from these theories. Secondly, the claim that different situations conform to the specifications of different theories undermines the claims of the theories (and the proponents who formulated them) that these are generalised theories which apply to all learning situations. Furthermore, McInerney & McInerney do not provide any information about the limited situations to which each theory might apply. If different pieces of the theories can be selected according to individual predilections, then the theory is not telling us anything we did not already know: it is not a scientific theory; it is a source of inspiration rather than a mechanism for making predictions. Thirdly, the problem with this approach, no matter what it is called, is that ultimately, the decisions of a teacher are determined not by the theory, but by their own decisions, values and doctrines. If these theories represent a craft or a source of inspiration rather than a predictive science, then intellectual honesty dictates that they should be presented as such. Presenting vague, arbitrary theories as a range of scientific options, means that one can select a particular theory to suit their own position, and then reinterpret it to justify that position; this can create the illusion amongst the uncritical that a doctrine is scientific fiat, and hence bolster otherwise unsupported beliefs. Finally, the last reason for using the theories – to enhance 'the professional competence of the teacher. . .'- smacks of image-building which would be unnecessary if the theories actually fulfilled the claimed roles.

The Santrock text concurs with McInerney & McInerney; he advocates an '*eclectic theoretical orientation*', whereby one can '*select and use*' pieces of a theory on the basis of '*whatever they consider best*' (p58). This is an interesting example of the white-coat syndrome; in this case, the application of a formal and technically-sounding appellation lends an aura of authority to what is otherwise an intellectual deception. To the critically perceptive, this is an admission that these theories have no predictive value. An analogy in biology would be to favour Charles Darwin's theory of evolution sometimes, but at other times to 'select and use' the theories of Buffon, Lamark, Erasmus Darwin, scientific creationism, intelligent design, etc. If you can decide informally that this part of a theory is true and that part is false, then the theory is not fulfilling its only purpose, which is to predictively make those same discernments for you. In science, this is called a theory which tells you no more than you already know, also known as a theory of nothing. This 'orientation' is not used in the 'hard' sciences; it is used in religion by both fundamentalist and liberal theologians, who select the parts of the Bible that they feel are inspired and instructive. The intellectual objection remains the same: if we can decide unaided what is true or false, then we don't need, and in fact cannot rely on, the Bible (or the theory) to tell us. In prosaic terms, the theories of learning could be described as a collection of homilies and platitudes, with sufficient flexibility to support a range of educational dogmas.

The NSW Department of Education (2004) Web site has referred to the arbitrary and faddish nature of these theories: 'Theories about teaching and learning come and go. One that has been doing the rounds is . . .' This cynicism is indeed ironic, given that the DET has mandated teacher education of the type presented in the Dip.Ed.

These two textbooks pay a fleeting reference to the 'information processing' theory of learning, which is presented as essentially descriptive, and makes no predictions. It was not revealed in the course that this theory is multidisciplinary 'cognitive science', which takes particular advantage of recently available observational techniques to acquire an extensive database of genetic and evolutionary mechanisms, neurophysiology, and cognitive processes (e.g., see Stillings *et al* 1995, Pinker 1997, 2002). It replaces earlier methodologies, which relied on speculative theorising with little data and few quantitative predictions. Why do the textbooks prefer to emphasise speculative and even discredited theories that make insubstantial predictions and say nothing of substance about the actual mechanisms of learning and cognitive development? I suspect this is because these theories can be easily interpreted to support any version on the constructionist doctrine that is in vogue.

The lack of intellectual rigour is reflected in the course assignments. For example, one assignment asked 'what do theories of learning tell us about enthusiasm for learning?' The real answer to this question is 'nothing', since the theories make no explicit statements about enthusiasm for learning. However, this is a leading question which presupposes a positive response, and it would be a brave trainee who was totally honest. Nonetheless, the very vagueness and malleability of the theories is ideal for such a question, since they can be interpreted to match any answer, including the course ideology. The course co-ordinator described this as a 'meaty' question; this evaluation mistakes obfuscation for profundity. A selection of exemplar answers selected by the examiners clearly indicated that they were not interested in critical analysis, but in the acceptance of the leading question and a style of creative writing suited to coffee table journalism. According to the examiners, these exemplars should provide model answers to some of the issues raised by this inquiry. I would invite the committee to obtain these exemplars from the course administration, and examine them for insights into how teaching and teacher training can be improved.

Santrock (2004): Additional Comments and Analysis

An appendix to this submission contains more detailed comments on some of the intellectual weaknesses of Santrock (2004). These comments were invited by the course co-ordinator as a result of discussions on the intellectual quality of the course and the rigour of social sciences. I have not yet received a reply.

Baumrind (1991): Socialisation Theory

An example of the embrace of ideology is provided by the course discussion of the socialisation theories of Baumrind (1991). This author carried out research over a period of thirty years, aimed at demonstrating that

parental style is a significant determinant of child performance at school. The study shows many of the weaknesses in sociological research, which would invalidate the study in hard sciences such as medical or pharmaceutical science where the result can be a matter of life or death. These include non-blind gathering of data, the inappropriate use of self-assessment and antagonist assessment, inadequate or biased sampling, confusion of correlation with cause-and-effect, and the arbitrary division of non-discrete variables (which should be measured) into discrete types (which can be counted). Baumrind (p750) disguised this last problem by a memorable piece of obfuscating pedantic formalism (also referred to as psychobabble or obscurantism): *'typological analyses often provide more meaningful information about individuals and relationships than linear analyses . . .'*. (Of course it does – or at least appears to. This is clearly one of the advantages of not measuring a variable, but using it to create groups that fit the theory.)

The persistent questioning of this doctrine by some psychologists and geneticists (e.g., Harris 1997) contributed to the eventual retraction of much of this doctrine (Collins *et al* 2000, 2001), despite attempts to save some remnants. Nonetheless, Santrock dwelt on the original tenets of the doctrine, with only an understated reference to some of the criticisms. The Dip. Ed. course endorsed the theory, by asking trainees to 'describe the four main leadership/parenting styles described by Baumrind, . . . and . . . why is it important for teachers to have an understanding of these leadership styles.' It would be a brave trainee who ignored the leading question, and pointed out the few obvious conclusions of Baumrind's data:

- First, a neglectful teacher might have a negative effect on student competence;
- Second, an authoritative teacher might affect a slight increase in student competence.
- Third, a female teacher is most likely to see a pronounced increase in student competence if they teach in an all-girl school in a higher socio-economic area with many Asian students.

The first two points may be statistical artefacts, as suggested by Harris. Many teachers intuitively have most confidence in this last effect, which incidentally was not mentioned by Baumrind.

The Philosophical Underpinning of the Course

The core components of the Dip. Ed. sit firmly in what has been called the 'Standard Social Science Model' (Tooby and Cosmides 1992), which emphasises environmental determinism, and either ignores or deliberately rejects any understanding of psychology or culture derived from the natural sciences. This model arose when there was little knowledge of molecular genetics and neurophysiology, and has continued to ignore these emerging bodies of knowledge. In its extreme form, the mind is seen as the classic 'blank slate', on which a range of features, from language acquisition, school learning, and even sexual preference and gender identity, can be written. This theory leads seamlessly to cultural relativism, which claims the special objective insight that natural scientists (and everyone else, with the implicit exception of sociologists) cannot be objective. (In this contest, 'extreme' refers to the logical extent of the claims; it is not restricted to a fundamentalist minority fringe.) Pinker (2002) reviews this doctrine, and some recent challenges to it from the cognitive sciences.

Another extreme is postmodernism; this concept is perhaps best understood as the rejection of modern values, in particular the pursuit of science. This is closely aligned with the ideology of deconstructionism; which states that all previous knowledge should be deconstructed (i.e., critically challenged), and then rebuilt from the ground up. At face value, this seems to be the same philosophy that is instilled into science students from the beginning – all knowledge should be challenged, and re-derived from first principles. In practice, however, postmodernism tends to target the natural sciences, and to reconstruct its worldviews along the lines of particular dogmas. In the process, it claims an esoteric objective insight into the meaninglessness of objectivity. The banality and non-productiveness of postmodernism have been pointed out by natural scientists (e.g., Sokal 1996a,b, Gross and Levitt 1994, Weinberg 2003). However, these logically extreme positions are nonetheless consensually mainstream, or close to mainstream.

The end result is that the social sciences, including educational theory, are influenced by a worldview which in many ways is separated from the sceptical inquiry of the natural sciences. An example is the anti-positivist school of sociology, which is advocated in the course texts (Allen 1998, Willis 1999). This view claims that the method of the natural sciences is to seek universal laws, and that this is inappropriate to the social sciences. I find this description ironic, given that the natural sciences have almost never been motivated by the search for universal laws, let alone used this as a method: natural selection, the atomic

theory, the period table, Maxwell's laws, geological gradualism, continental drift, special relativity etc. were not discovered as the result of a quest for universal laws: they emerged as the ineluctable implications of masses of data. By contrast, the history of the social sciences is replete with quests for, or the application of, universal laws, e.g., Marxism, Durkheim's functionalism, Piaget's theory of cognitive development, social cognitivism derived from Vygotsky, the general tenets of environmental determinism. Anti-positivism appears to be rejection of the method of natural sciences, without really knowing what that method is.

With this underpinning, it is no coincidence that the course presents outmoded theories of learning and development.

Conclusions

I have long been aware of undercurrents common amongst trainees and teachers that the 'Dip. Ed.' is a waste of time, irrelevant, or of poor quality. The Dip. Ed. course that I am enrolled in has made frequent references to teacher rejection of much of the training, and to the claim that most teacher training occurs after the course finishes. For example, Symes & Preston (1997, p6) state that teachers tend to renounce educational theory; they then uncritically imply that teachers are non-intellectual because of this. They do not consider alternative possibilities, such as that the theory may be flawed, or that educational theory is ineffective when it is actually applied (as in this particular case). In the course notes (Lawson 2004) states that trainee teachers often question the value of theory; without further discussion she implies that this is because trainees fail to realise that 'there is nothing quite so practical as a good theory'. Again, there is no consideration of alternative explanations, e.g., that the theory is actually not good or practical, or that the theory is poorly presented.

The poor quality of this core Dip. Ed. subject matter begs the question of what should be done? My view is that the intellectual components of the course are literally worse than useless – they are subversive, in that they give a distorted view of scientific inquiry and critical processes. This is especially relevant in the context of the training of teachers, who are in a position to transmit this disinformation to the next generation of learners. I feel that significant change is essential, and could take the following forms:

1. The current content is essentially retained, however, it is not presented as science, but rather as a craft, or a set of doctrines designed to achieve certain social and pedagogic goals. It should also be pointed out which of these ideas represent inchoate or failed science.
2. Alternatively, the psychological and sociological content of the course is simply abandoned. This would be preferable to the present distorted, subversive presentation. It would leave the graduates no less intellectually equipped than the large number of instructors who teach in universities, technical colleges, private schools, or other professional and commercial settings, where their value is not determined by the possession of a government-mandated diploma.
3. Another alternative is that the current content is replaced with the corresponding state of the art knowledge. In the case of theories of learning, this would involve relegating Piaget and a host of others to historical footnotes or obscurity, and concentrating on cognitive science, the real study of learning and thinking. Unfortunately, it is not clear that the present state of the art will help in classroom teaching. However, this is a clear statement of what is known about learning, and its applicability to the current doctrines of school education.
4. The entire course should be vetted for intellectual soundness. This is an imposing task, since classically this would involve academics sitting in judgement on themselves or each other. However, a variety of inputs into the review process would be valuable.
5. As for the Dip. Ed. as a whole, it seems that the major concern of most trainee teachers is firstly maintaining control in a classroom, and secondly, coming to terms with the curriculum content of their teaching areas. The second of these could be profitably developed at greater depth in the Dip.Ed. which may also cater for the first concern.

It is hardly coincidental that the core subject matter of the Dip.Ed. discussed here has the following confluence of features:

1. it evokes scepticism from trainees and teachers;

2. it is taught because of government mandate rather than intellectual curiosity or commercial demand;
3. it stresses sources which have diminishing or no scientific credibility or impact outside the field of education;
4. it is increasingly racked with controversy;
5. it is increasingly at variance with results from the most dynamic branches of science and technology;
6. it has been marked by a century of spectacular failures, such as Freudian theory, simplistic behaviourism (Skinner), environmental determinism (J.B Watson), feminist-inspired gender-neutrality theory, gender-reassignment therapy (John Money), and socialisation theories which have blamed mothers for conditions like ADHD and autism;
7. in the case of my Dip.Ed. experience it is staffed mainly by people whose intellectual environment is less likely to be the 'hard' sciences than the environments of primary and secondary schools.

Despite this, the assessment of the intellectual quality of a Dip. Ed. course should be done on the basis of sceptical, critical analysis as used in the 'hard' sciences: for each statement we must ask, 'is this true', and 'what does it mean in terms of quantitative predictions'. The criteria that are traditionally used to judge an expert source of opinion, such as compliance with consensus views, possession of a Ph.D., number of publications, years of teaching experience, and history of academic involvement, should not be used as the basis for judgment. I would be particularly concerned if the current drivers of the education train are used to pass judgement on themselves; I would be disappointed if the requirement for a research basis failed to recognise the different levels of rigour which apply to research in the social and natural sciences. Ultimately, this assessment requires detailed examination of the texts, the course notes and the assignment material, rather than expert advice.

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Appendix

This is my communication with a course co-ordinator, on some aspects of the Dip.Ed. course. The communication was part of a dialogue, not a formal report; hence it does not contain reference details, although most of these would have been familiar to recipient. I have removed a single word from my original communication (on p13), to avoid misquoting Santrock.

On Santrock

Santrock's 'Adolescence' has many features that detract from its value as a textbook. Here I give some examples. A case which apparently created some amusement on the forum is Santrock's example of how 'coursework [varies] in different countries' in response to cultural priorities: 'Seventh grade students in Australia take courses in sheep husbandry and weaving, two areas of economic and cultural interest to the country' (p414). This is clearly a poor example of how Australia's national priorities have impacted course work. (Note that Santrock really means how 'coursework varies **between** different countries'.)

Another example is: 'two-thirds of Asian Indian adolescents accept their parents' choice of a marital partner for them (Verma & Saraswathi, 2002)' (p17). This does not remotely resemble the wording or meaning of the source ('63% of males and 72% of females . . . preferred arranged marriages'), and may be entirely incorrect (the word 'adolescent' is not even used). Amongst my Indian friends, offspring prefer parents to arrange compatibility screenings, introductions, wedding details etc. In other words, parents do the work; offspring make the choice (if any). Here, Santrock seems to prefer dramatic effect to accuracy.

The Genetic Process (pp107-111)

In this section, inattention to detail and clumsy language suggest a lack of familiarity with the topic, e.g.,

- 'a person made of trillions of cells, each containing a perfect replica of the original genetic code' (the replica will probably be less than perfect and may well contain dozens of differences);
- 'The structure of DNA is a spiralled double chain' (DNA, like a spiral stair-case, is helical, not spiral);
- '[genes] direct cells to reproduce themselves and to assemble proteins' (genes don't do either);
- 'Each gene has its own function' (this claim is refuted by Santrock's main reference, Moore 2001, although it has long been considered simplistic bean-bag genetics by geneticists);

These criticisms range from minor to serious, and could go indefinitely, e.g., sentences 3,4,5,8,9 below:

'One of the big surprises of the Human Genome Project was the finding that humans have about 30,000 genes (U.S Department of Energy, 2001). They had though that humans had as many as 100,000 or more genes. They had also believed that each gene programmed just one protein. In fact, humans have more proteins than they have genes, so there cannot be a one-to-one correspondence between them (Commoner, 2002; Moore, 2001). Each segment of DNA is not translated in automaton-like fashion, into one and only one protein. It does not act independently, as developmental psychologist David Moore (2001) emphasised by titling his recent book the Dependent Gene . . .

The activity of genes . . . is affected by their environment (Gottlieb, 1998, 2002, 2004) . . .

In short, no single gene is the source of the protein's genetic information, much less of an inherited trait (Gottlieb 2002, 2004; Moore, 2001). Rather than being an independent gene, DNA is a collaborative gene.' (p108)

Here Santrock implies that the dependence of gene action on other genes and environment is a recently discovered phenomenon, advocated by Gottlieb and Moore and confirmed by the Genome Project. This reveals Santrock's ignorance of the topic, and his willingness to quote authors he either does not understand or can't critically assess. This is discussed below.

Santrock refers to Gottlieb extensively, yet he has no critical appreciation of the content. Gottlieb's (1998) bugbear is the 'central dogma', an insightful theory formulated by Crick (1958) to help unravel the role of the genetic material in protein biosynthesis. Gottlieb does not seem to realise why Crick used the term dogma, even though this is clear from Crick (1958): firstly, it was a bold assertion made without evidence; and secondly a dogma presents an almost irresistible challenge to scientists to provide refutation. The theory stated that genetic information flows from nucleic acid to protein in two steps: DNA -> RNA ->

protein. 'Information means here the *precise* determination of sequence, either of bases in the nucleic acid or of amino acid residues in the protein' (Crick, 1958). Crick (1970) recognised that the dogma had been misunderstood occasionally, so he emphasised that 'the central dogma of molecular biology deals with the **detailed residue-by-residue transfer of sequential information**' (opening sentence, my bold).

Gottlieb completely misunderstands this. He claimed that the infectious mechanism of prions, and the mechanism of gene repression by proteins, violated two of the forbidden information flows: protein → protein, and protein → DNA. Gottlieb is precisely wrong: neither example involves the transfer of information about residue sequences. Also, Crick (1958) explicitly excluded the transfer of conformational information which occurs in Gottlieb's first example: 'it was necessary to put the folding-up process on one-side . . . [which] reduced the central problem from a three dimensional one to a one dimensional one'. Gottlieb's misunderstanding is a schoolboy howler, but Santrock does not realise this. We could explore more, such as Gottlieb's equally fantastic misunderstanding (p793) of Crick (1982, p515), or his personal comments; however this would be flogging a dead horse. I find this paper reminiscent of the Sokal hoax, except that Gottlieb appears to be serious.

The 'nurture-nature' controversy is a popular myth perpetuated in the tabloid and coffee table presses, and in certain social sciences. In 1858 Darwin and Wallace inextricably linked nature and nurture in the concept of natural selection, which is an interaction between heredity and the environment that results in non-random differential survival of genotypes. Biology is the study of correlations and causal relationships of biological interest, without any reference to the ill-defined and uninteresting dichotomy of nurture/nature. In the words of Pinker (1997, p32), this dichotomy 'falls into the category of ideas that are so bad they are not even wrong'. Genetics and heredity are essentially studies of environmental effects as much as of genetic effects. Dawkins (1982) gave the clearest possible statement of this (without claiming priority, of course):

'Genetic variance is a significant cause of much phenotypic variance..., but its effects may be overridden, modified, enhanced or reversed by other causes. Genes may modify the effects of other genes, and may modify the effects of the environment. Environmental effects, both internal and external, may modify the effects of genes, and may modify the effects of other environment events' (p13).

As Ridley (2003) says, 'for more than fifty years sane voices have been calling for an end to the debate' (p3). Santrock seems unaware of this. Instead of discussing the causal agents of development in a meaningful way, he discusses 'nurture/nature' as an issue which is coming to a head, with Gottlieb and Moore in particular being vocal advocates for a new understanding. Gottlieb (1998) mentions the *lac* repressor as an example of this new awareness of how genetic expression is influenced by environment. Geneticists have been studying this system intensively since the beginning of molecular biology as part of their overriding interest - which is how environmental factors determine gene expression (Jacob & Monod 1957). Gottlieb seems to think geneticists did not realise that they were studying environment/gene interactions, even though it was their explicit aim, until he pointed it out 41 years later.

Moore (2001) claims that his 'book frees both individuals and society from the chains of genetic determinism' (p242). His treatment is politically motivated, polemic and distorted, and ignores much of the history of the 'controversy'. He devotes half the book to discussing environmental influences on development, but only mentions genetic influences parenthetically - even for traits that can be accurately predicted genetically, such as eye colour, phenylketonuria, and sex. It is hard to see how the book frees someone with blue eyes from the chains of their genetic propensity - presumably via a 'schoolyard fight', the counter-influence mentioned by Moore (pp60, 255). He gives no examples of genetic determinism influencing society or policy; he ignores the recent influence of environmental determinism, e.g., parental blame for dysfunctional conditions like autism, feminist-inspired ideology of gender-neutrality, and the use of enforced gender-reassignment surgery. He ignores the fact that for most of the twentieth century psychologists have espoused environmental determinism and remained ignorant of genetics (e.g., J.D Watson, gender-neutrality doctrine, socialization theory); he downplays the refutation of genetic determinism by geneticists. Santrock seems to prefer the distorted polemics of Moore to a meaningful understanding of the subject. Santrock goes on to state that the 'emerging view is that many complex behaviors likely have some genetic loading' (p111). In reality, this view had fully emerged well before this (e.g., see Wilson's 1975 summary). What is emerging now is an awareness of the futility of denying this on

the part of psychologists and sociologists. The interaction of genetic and environmental influences is a mute point, and Santrock should treat it as such, instead of proselytising the 'emerging view' with enthusiastic polemics and inadequate understanding. One reason the spurious debate has continued is that authors like Santrock are ignorant of the history as well as of the subject matter.

Santrock's treatment displays a woeful ignorance of the subject matter, avoidance of seminal authors, and a willingness to quote sources he either does not understand, has not consulted, or has misunderstood. Santrock would be well advised to consider the admonition he provides on page 73: 'Consider the source of the information and evaluate its credibility'.

Santrock on Harris (p112)

Santrock's brief discussion (p112) of the book by Judith Harris (1998) also shows his lack of intellectual rigor and his proclivity to ignore the content of his sources. Harris (1998) reviewed numerous studies which indicate that personality traits are minimally affected by parents. Santrock dismissed Harris's review without discussing the data, but by using emotive language, uncritical reference to 'a huge parenting literature', and untruths.

Santrock's six references from the 'huge parenting literature' all postdate Harris (1998), which may explain why she didn't include them in her 650 references. One of Santrock's references includes several of the main players (Collins, Maccoby, Steinberg, Hetherington & Bornstein 2000); they do not support Santrock's assertion, since they explicitly reject the pre-1990 data and concepts in bulk, and offer little new evidence. Collins *et al* (2000, 2001) reacted to Harris with a detailed re-examination of the data on parenting styles:

- they rejected most socialisation concepts and studies predating the 1990's;
- they claimed that critics, in particular Harris, mainly considered pre-1985 data, even though by my estimation approximately 80% of Harris's (1998) 650 references were less than ten years old at the time;
- they subtly omitted any reference to Baumrind's claimed correlations, or Steinberg's (1992) claimed causal relationships where parental style strongly affects child competence across a range of cultures, ages, SES, and family structures;
- they seem to have undertaken some self-education in the role of heredity in determining behaviour;
- they suggested that the field now uses a newer, more rigorous methodology. One of their two exemplar references is Steinberg *et al* (1994). This study contains numerous flaws, of which three in particular (separately or together) render it worthless: firstly, the data was not collected double- (or multiple-) blind; secondly, parental style was not measured directly using standardized tests, but by interviewing the children; thirdly, 64% of the data was omitted because they did not fit the expected outcome. These 64% may represent a new parental style which promotes child competence more effectively than authoritarianism; alternatively, they may be unrecognised authoritative parents who produced low competence children. This cannot be determined from their report.

The watered-down correlations claimed by Collins *et al* deal with competence in the school environment, and a one-dimensional parental style of authoritative versus non-authoritative. Their main support comes from extreme conditions which say nothing about parental styles, i.e., complete parental deprivation of Romanian orphans, the immediate aftermath of divorce, troupes of genetically abnormal monkeys, the transition from home to school.

Santrock states '*Harris's description of peer influence do not take into account the complexity of peer contexts and developmental trajectories*'. This implies that he has not even read the book; Harris discusses both aspects across a range of ages and cultural settings, and with evolutionary and functional perspectives. This breadth of coverage is not found in any of Santrock's sources that I have been able to check. Santrock continues, '*Harris is wrong in saying that parents don't matter. For example, in the early child years parents play an important role in selecting children's peers and indirectly influencing children's development (Baumrind, 1999)*'. Firstly, Harris does not say that parents don't matter; she says '*these early relationships are essential, not just for normal social development, but even for normal brain development*' (p153), and Chapter 14 is devoted exclusively to this. Secondly, Santrock's supporting reference (Baumrind, 1999) is not only one of the least credible - Pers. Comm. - but it does not describe any new

data; it is an 'unpublished review' of the previous edition of his textbook, suggestive of passing remarks like 'Nice book', 'Thanks', 'By the way ...' exchanged in the corridor. Third, the claim is in fact part of Harris's thesis, in particular, that in many areas parents most effectively influence their children by manipulating environmental variables such as school, cultural and social context, and access to peer groups. In contrast to Santrock's flimsy comment and supporting reference, Harris discussed this point over several pages (pp335-338), with more substantial references.

Santrock's vehemence is interesting, given that Harris's conclusions do not conflict with Baumrind's and Steinberg's earlier generalisations. Harris's conclusions refer to single-dimensional, adult personality traits, as measured by psychology tests; the other authors claimed correlations between parenting styles and offspring competence. His reaction is even stranger when you consider that Harris's proposals are consistent with the case that Santrock has developed over several pages. He states that 'common rearing, or shared environment, counts for little of the variation in adolescents' personality or interests' (p111), and 'heredity and environment operate together . . . to produce a person's intelligence, temperament, height, . . . and so on' (p112). This is also Harris's claim, and she points out that several of Santrock's favoured authors, including Baumrind, have totally ignored one of these components: heredity. The real irritation to Santrock may be that Harris has highlighted the shoddiness (acknowledged by Collins *et al*) of much of the research on parenting styles, or that she is challenging a favourite ideology.

It is interesting that both Harris and Santrock are authors of textbooks. Harris takes a critical (i.e., skeptically enquiring) approach to the textbook data, appears to be familiar with the material and the sources, and writes in an unambiguous, understandable style. Santrock devotes many pages to evolution and genetic issues (p8, p26, p29, pp104-113); his concepts and terminology are amateurish and confused, and his familiarity with the material and his sources is poor.

Santrock on Baumrind (pp318-320)

Santrock devotes several pages to Baumrind's definition of parenting styles and their effects on children. He adds two caveats: (1) that 'parenting styles do not capture the important themes of reciprocal socialization and synchrony', and (2) that 'many parents use a combination of techniques' (p320). The first caveat is a euphemistic admission that children have significant effects on parents' behaviour, and that this obscures causality; given this, one might expect Santrock to devote equal space to discussing offspring effects on parents, but he doesn't. The second caveat essentially means that variance has not been determined when parental styles have been measured; this should raise questions about the techniques used to measure parenting styles, although Santrock himself does not do this.

Harris raised additional concerns, including a question of reliability: data were not collected blind or double blind. This would completely invalidate the study in 'hard' sciences, for example medical research, where the reliability of the results has 'real' consequences. A second issue about reliability is that apparent correlations could be artefacts resulting from the sample sizes, statistic methods, and levels of significance used; according to Harris, it required some massaging of data before researchers like Baumrind or Maccoby actually uncovered correlations. Harris questions the validity of interpreting these correlations as parental effects on children. First, the contribution of heritability was not extracted from the correlations. Second, in later studies, parental style was determined by interviewing the children; clearly, this method could only establish a correlation between children's competence and what they say about their parents, instead of between competence and parental style. Third, the correlation between parental style and child competence seems to be dependent on cultural context, sex, or type of relationship (mother-son, etc); this is an interesting result, which really implies that child competence is more dependent on these other factors, than on parental styles. However this was not explored. A concern not raised by Harris is that parental style is a complex attribute, and that it cannot (or at least has not) been measured via an objective standardised text. As mentioned previously, many of these criticisms have been recognised by Collins *et al* (2000). Despite these issues, Santrock awards the theory of parental styles and influence uncritical support, more appropriate to an ideology than a science.

Theories of Development

Santrock presents 'theories of development' (ch2, ch4) as a scientific study, yet his presentation is far from scientific, even though some of the material may be. An obvious point is that his overview totally ignores physical development. I suspect that this is because of the difficulty of integrating the different concepts of 'theory' used in the natural and social sciences. His discussion of physical development (ch3, after the overview) is a description of what actually occurs, i.e., the theory of physical development is the generalized, textbook description of actual phenomena. By contrast, his overview of development is essentially a classification of theories of cognitive development, rather than a study of the subject matter itself.

Santrock begins by discussing psychoanalytical theories. These have long been recognized as non-predictive, difficult to test, and of dubious clinical value, i.e., they are non-scientific. Santrock's evaluation is quaint in that he lists what he sees as good and bad points, with no discussion of these points. Evaluating a scientific theory by looking at areas of strength and weakness is probably as valid as deciding a person's innocence or guilt by a vote, or voting to decide if a person is god as was done at Nicea. A substantial list of failings probably indicates that a theory requires wholesale replacement rather than fine-tuning. These theories aren't useful models of reality; they are doctrines and hence necessarily misleading. Why has Santrock elevated these doctrines above their rightful place as historical footnotes? What function can they serve, except as bad examples?

Next Santrock discusses Piaget, a perennial favourite of education instructors. His evaluation (p. 128) consists of a scorecard of pluses and minuses. The minuses include that Piaget was wrong about the 'timing of development, the four stages he identified, the usefulness of training in reasoning, and the effects of culture on cognitive development areas.' In other words, Piaget's observations were unreliable, and everything he said about development (at least as presented by Santrock) is wrong. Santrock discusses each of the minuses without refuting them.

Santrock's pluses include 'Piaget has been giant in the field of developmental psychology,' and 'Piaget was a genius when it came to observing children'. Neither statement constitutes a scientific evaluation; both are strange given that (according to Santrock) Piaget's theory was wrong, and his observations were unreliable. Other pluses include concepts: 'assimilation, accommodation, conservation, hypothetical-deductive reasoning'. It is not obvious from Santrock that these and other concepts (schema, cognitive framework) are rigorously defined, or that they can be unambiguously recognized or quantified. For example, what is the definition of schema, that would allow all schemas to be named described, catalogued, and henceforth unambiguously recognized in the same way as is done for subatomic particles, atoms, muscles or bones in the human body, and even abstract entities like biological species, and languages? These concepts seem to be sufficiently flexible to fit many observations; this is a theory that doesn't tell us anything more about a phenomenon than it assumes, i.e., it is a theory of nothing. If these concepts really were unambiguous and recognizable and useful, then (1) Piaget would probably not have made basic errors in his observations or in his theory; (2) there would be inescapable consensus on the value of the theory; (3) it would have led to conclusive testable predictions. Compare these with some other concepts: the atomic model, natural selection, the speed of light, particulate inheritance; these concepts are so clearly defined that they have led to irresistible, overwhelmingly useful theories. Apart from his gigantic stature, his observations, and his concepts, Santrock doesn't award any other pluses to Piaget.

Santrock describes three predictions of Piaget's theory. First, 'the adolescent's mind is not a blank slate. Adolescents come to school with their own ideas about space, time, causality, quantity, and number'. This predication is not obvious from Santrock's description, and in any case would be a non-discriminatory postdiction: there could be many reasons why children know something about time, space, causality and counting before reaching the age of twelve. The second prediction is that 'the best way to nurture [adolescents'] motivation to learn is to allow them to interact with their environment in a spontaneous way.' This is an implication of Piaget's theory, as described by Santrock; but it is hardly rocket science, nor is it a prediction of any value. How do you measure the interaction - how can you be sure that a teacher who allows a lot of interaction is applying Piaget's theory, whereas a teacher who allows a little bit of interaction is not? What level of interaction is consistent with the theory? If the parameters cannot be

measured, then the prediction cannot be tested; hence we do not have a scientific theory, but rather an ideology. The third implication mentioned by Santrock is that 'instruction might be too formal and too abstract from a developmental point of view'. This prediction suggests that an unquantified effect might occur; it may be interesting, but it is not spectacular endorsement of the theory. It is interesting to compare these predictions of Piaget's theory with the types of predictions made by other theories: general relativity and quantum mechanics make predictions which are quantitatively accurate within current limits of precision (1 in 10^{14}). Theories that do not make deterministic predictions can still be spectacularly useful, e.g., some predictions of plate tectonics and evolution have been earth-moving (e.g., mountain building, and continental drift respectively).

Santrock asks 'has [Piaget's] theory withstood the test of time?' This is an interesting question, given that the theory is wrong, the underlying observations were inaccurate, and the implications are vague. Wider reading tends to deepen the paradox. Writing in 1983 and 1993, Gardner stated 'Piagetian theory [was] the rage twenty years ago' (p22). He rejected Piaget's results as wrong, inaccurate, or incomplete, but curiously claimed to retain his methods. It is perhaps telling that he didn't specify the methods, but presumably he is referring to observation, a scientific tool hardly unique to Piaget. Gopnick *et al* (1999) present a detailed account of infant cognitive development, but only mention Piaget reverentially as a historical footnote; Pinker (1994, 1997, 1999, 2002) produced extensive tomes on development with minimal or no debt to Piaget.

A partial answer to Santrock's question is that Piaget's claims are consistent with the overwhelming Western social ideology of the second half of the twentieth century, which has focused on liberation, personal development and fulfillment. Also, the predictions are vague enough to fit a wide range of situations. Atherton (2003) gives another answer:

'His view of how children's minds work and develop has been enormously influential, particularly in educational theory . . . His research has spawned a great deal more, much of which has undermined the detail of his own theory . . .' [my bold]

So, the theory has been influential in the low-key intellectual activity of educational theory, even though it is wrong: this correlation has implications for the evaluation of both educational theory and Piaget. Piaget has been influential because uncritical, unknowledgeable textbook writers have featured him in their textbooks, which in turn has led other textbook writers to include him. Educators perpetuate this by complying with the dictates of the prevailing textbooks and ideology. The NSW Department of Education (2004) Web site has referred to this faddish approach: 'Theories about teaching and learning come and go. One that has been doing the rounds is . . .' As a result, Piaget's writings are canonized; e.g., 'remember from . . . Piaget's theory . . . that many adolescents begin to think in more abstract and idealistic ways' (Santrock, p169). If we know this at all, we know it from watching them; we don't know it from Piaget's theory, because according to Santrock the theory is wrong. So, Piaget (at least as presented by Santrock) is famous for being famous.

I point out again that it is not necessarily Piaget's theory, but rather Santrock's presentation, which does not make sense. Despite this, the paradoxical presentation is not necessarily ineffectual. Firstly, many trainees and even trainers may be no more inclined to critical thought or intellectual rigor than is Santrock; hence they may accept the presentation without even being aware of the paradox. Secondly, even though Santrock could not be used as the basis for critical or rigorous understanding of the topic, a trainee can effectively use the textbook as the basis for an assignment, because it presents an opportunity to demonstrate creative writing, ideological compliance, and an understanding of Piagetian concepts in terms of experience (which is the reverse of science).

Theories of development and learning make enough clear statements to show that they are contradictory. For example, Piaget describes development as the unfolding of innately determined stages; in contrast, Vygotsky describes development as the sum of incremental changes induced by a subset of environmental factors. Bronfenbrenner contradicts both of these by claiming that development involves a wide range of hierarchically related environmental influences. It is not clear from Santrock that Bronfenbrenner says anything useful about development, or that his ideas constitute a 'theory'. According to Santrock, this model simply classifies the environment into seemingly arbitrary categories; is not clear why, or what greater understanding results from this classification. For example, why is this more useful than classifying

the environmental influences into green and non-green things? This question cannot be answered from Santrock. It is hard to see how Santrock's description could contribute to a meaningful understanding of development, e.g., a student may memorize and even recall in an exam that an adolescent's exosystem may include her mother's workplace, but how does this make a meaningful contribution to the student's understanding of adolescent development? On the plus side, it indicates compliance with canonical received wisdom.

Finally, Santrock describes an 'eclectic theoretical orientation', whereby one can 'select and use' pieces of a theory on the basis of 'whatever they consider best' (p58). Quasi-formal language does not make this scientific; rather, it is an admission that these theories have no predictive value. An analogy in biology would be to favour Charles Darwin's theory of evolution sometimes, but at other times to 'select and use' the theories of Buffon, Lamarck, Erasmus Darwin, scientific creationism, intelligent design, etc. If you can decide informally that this part of a theory is true and that part is false, then the theory is not fulfilling its only purpose, which is to predictively make those same discernments for you. In science, this is called a theory which tells you no more than you already know, which is not a useful theory, or is a theory of nothing. This 'orientation' is not used in the 'hard' sciences; it is used in religion by both fundamentalist and liberal theologians, who select the parts of the Bible that they feel are inspired and instructive. The intellectual objection remains the same: if we can decide unaided what is true or false, then we don't need, and in fact cannot rely on, the Bible (or the theory) to tell us.

Brain Development and Education (p98)

Santrock rejects the relevance of brain-based research in a few paragraphs (p98) that include flippant value judgments without assessment of evidence, low-profile references, and what at first sight is a monumental (indirect) misquote of Sousa (1995) (careful reading suggests that this is poor editing, rather than a misquote). In contrast, he devotes several chapters to various psychosocial theories even though the two areas of inquiry are possibly equally as valuable or worthless to educational theory. Santrock's assessment of brain-based research could just as easily be applied to Piaget's theories, as presented in this volume; Table 1 provides an alternative reading, with references to neuroscience replaced by references to Piagetian theory. Why does Santrock reject one with superficial coverage, yet advocate the other even though he has indicated that it is wrong? By dismissing neuroscience, Santrock is continuing a long tradition in psychology of ignoring the physical basis of their subject matter, much as psychology has remained ignorant of genetics for most of the twentieth century.

Table 1: Santrock's text (p98, left column) has been rewritten in the right column, with references to neuroscience replaced by references to Santrock's presentation of Piaget's theory. It is not clear which version is more realistic.

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| <p>Numerous claims have been made that elementary and secondary education should be brain-based. Some journalists have even suggested that educators should look to neuroscientists for answers about how best to teach children and adolescents. Unfortunately, such bold statements are speculative at best and far removed from what neuroscientists actually know about the brain (Bruer, 1999). We don't need to look any further than the oversimplified hype about logical 'left-brained' individuals and creative 'right-brained' individuals to see how easily the relevance of neuroscience to education has been exaggerated (Sousa, 1995).</p> <p>One common misapplication of neuroscience to education is the idea of a critical or sensitive period - a biological window of opportunity when learning is easy, effective, and readily retained. However, there is no neuroscience evidence to support this belief (Bruer,</p> | <p>Numerous claims have been made that elementary and secondary education should be <i>psychology</i>-based. Some journalists have even suggested that educators should look to <i>psychologists</i> for answers about how best to teach children and adolescents. Unfortunately, such bold statements are speculative at best and far removed from what <i>psychological</i> scientists actually know about <i>learning and cognitive processes; in fact there are scores of competing theories of learning</i>. We don't need to look any further than the oversimplified hype about <i>innately programmed developmental events</i> to see how easily the relevance of <i>psychological</i> science to education has been exaggerated (<i>see p128</i>).</p> <p>One common misapplication of <i>psychological</i> science to education is the idea of critical or sensitive <i>stages - different periods of cognitive development</i> - when learning of <i>specific tasks</i> is easy, effective, and readily retained. However, there is no <i>psychological</i> science evidence to support this belief</p> |
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1999). One leading neuroscientist even told educators that although children acquire a great deal of information during the early years, most learning likely takes place after synaptic formation stabilizes, which is after the age of 10 (Goldman-Rakic, 1996).

(*see p128*). One leading *textbook writer* even told educators that *although children acquire a great deal of information during the early years, most learning of abstract concepts can only occur after puberty (see pp123-127)*.

Even if it has limited applicability to educational theory, neuroscience can give insight into the various psychological theories which themselves are applied, rightly, wrongly, imprecisely or ineffectively, to education. An obvious example is brain lateralisation, rejected uncritically by Santrock and misguidedly by one of his sources (Bruer, 1999). Lateralization indicates that abilities are modularised, and differ between individuals. This implies that education should cater for a range of abilities and perceptual styles. Whilst any application beyond this minimal claim would be 'oversimplified hype' as claimed by Santrock, this implication is at least as definite and precise as the implications that Santrock extracted from Piaget.

Neuroscience reveals that the brain is not a single, general-purpose learning organ; instead, different abilities are associated with different parts of the brain. This is indicated by brain scans and by natural experiments where localised lesions remove specific abilities, e.g., the use of nouns or of verbs, the control of a hand, arithmetical abilities, the ability to recognise faces, musical abilities, memory etc. One implication is that there are many capabilities, and that different tasks involve different neurological pathways which may each require different teaching and learning methods. Hence the various generalized theories of learning (Piaget, information processing, Vygotsky, humanist, etc.) are destined to remain generalized, vague and watery. This is intuitively obvious, and is probably why most trainee teachers reject the various theories of learning as useless oversimplifications without formally knowing why: it's because they are.

Several psychologists have incorporated the 'modular brain' approach into theories of multiple intelligences. Of course, this approach is correct; however, the ability of Gardner, for example, to disentangle only seven, or eight, or eight and a half intelligences reveals the difficulty of attacking this problem from a psychological view; evolution is pragmatic, and the grouping of skills by neurological pathways will not necessarily reflect our functional classification. For example, musical and mathematical abilities seem connected in some fundamental way which does not correspond to how we separate these in the cultural world, e.g., into different educational faculties or even different institutions, different functional roles in society, different educational approaches etc. The real theory of development and learning will come from a detailed understanding of the neurophysical development of specific task-related pathways, i.e., from a basis of 'hard' sciences such as neuroanatomy, genetics, medical science, beneath an interpretive platform of psychology. The real theory will describe how the brain can perform or learn multiple tasks (more than the 8 ½ suggested by Gardner), many of which will be enumerated and tied to specific brain circuitry. This circuitry will develop, but it will not be limited to four stages, as per Piaget: each circuit will have its own number of stages and its own dependencies on other circuits; the circuits will have hierarchical and sequential arrangements reminiscent of Piagetian stages. The real theory will describe how each circuit matures via a series of protein synthesis events (i.e., gene expressions), each of which is triggered by internal or external environmental stimuli, which in turn may be dependent on gene expression and environmental events that occurred during earlier stages (i.e., development is the epigenetic result of 'nurture and nature', using the antiquated terminology). Once in place, the circuitry will be subject to modification, or learning, via environmental influences, in particular language, social interactions, and tool use as suggested by Vygotsky. This modification will probably involve altered synaptic connections, and with enough analysis could be described as a complex set of stimulus-response-reinforcement interactions, for what it's worth. You could even analyse the external environment in much greater detail than Vygotsky, e.g., as per Bronfenbrenner, though again *a priori* it's not clear that this would have any value. Detailed knowledge of the internal environment would reveal factors which could be interpreted as internal motivation, resulting in development and learning which could be described as self-fulfillment as per humanist theories, and so on. Current theories of learning are crude descriptions; the real theory will emerge as a detailed description of reality which could be presented in the same style that Santrock uses to describe physical development in Chapter 3, i.e., a straightforward, generalised description of what happens.

Summary

These examples show that Santrock misquotes and misunderstands his sources and material. His selection, evaluation and presentation of material are uncritical and inconsistent. No doubt this selection is an example of the 'eclectic theoretical orientation'. This 'orientation' is overtly non-scientific, and is indistinguishable from religious or ideological decision processes. The net result is that much of the book is ideological rather than scientific.

Santrock claims to present the scientific study of adolescence. He may attempt this, and much of this inquiry may in fact be scientific; however, in many subject areas his presentation and evaluation is so lacking in intellectual rigor (i.e., is unscientific) that it makes me disinclined to accept his projected authority in any area. This is paradoxical, given that he discusses the scientific method and also the concept of critical thought; however, the paradox disappears if you look these in more detail.

The textbook is subversive, because it creates a false impression of science and critical intellectual inquiry. Ironically, this could be particularly important in the present context of a large body of mature-age students, many of whom may have lost the questioning of youth, but have become highly motivated receptors of dispensed canonical wisdom. Many students may have a critical capacity similar to that of Santrock, or of the course administration which chose the book, or of the general population, which endorses fantasies such as astrology, religion and political rhetoric, which prefers entertainment to information, and which considers personal opinion, whether informed or not, to be a valid path to truth. Questions on the forum like '*how would you define "critically discuss..."*,' or '*Does a critic only criticise, or is he/she allowed to praise?*' indicate that at least some students have progressed through the education system to the postgraduate level without knowing the meaning of 'critical'. These at least are susceptible to Santrock's disinformation.

I perceive the aim of the Dip.Ed. course as to provide trainee teachers with heuristic knowledge which has an established value and a sound intellectual basis. According to sources encountered during this Dip. Ed. program (and also every pre- or in-service teacher that I have ever talked to about this subject), trainees and teachers do not have much confidence in the ability of Dip. Ed. courses to meet this aim. For example, Symes & Preston (1997) state that teachers tend to renounce educational theory, then uncritically imply that teachers are non-intellecutals because of this. They do not consider alternative explanations, such as the possibility that teachers reject the theory because it is 'bullshit', or because it is poorly presented. Similarly, Lawson states that trainee teachers often question the value of theory; without further discussion she implies that this is because they fail to realise that 'there is nothing quite so practical as a good theory'. Again, she does not consider alternative explanations, i.e., that the theory is actually not good or practical, or that the theory is poorly presented.

Does this subject (EEL491) have an intellectual basis? If it does, then this should be reflected in all of the course material, including the textbook. Alternatively, if the subject does not have an intellectual basis, then it should explicitly acknowledge this, and recognise that the subject matter is founded in craft, ideology or doctrine. To claim that an ideology, or an uncritical treatise like Santrock, is science is misleading at best. The aim of the course is clearly not primarily intellectual - it is not to train people to be critical or to be Oscientists; it is to help people to become schoolteachers. It may be that this is best achieved by transmitting ideological compliance to those whose job is to transmit the same compliance to the next generation. What do the course assessment procedures reveal about this? Do the assignment and exam questions promote and assess compliance, or critical thinking? How do the examiners distinguish the two? These would be interesting questions to pursue, given the quality of the text chosen for the subject.

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