

# *Complexity, Complexity Reduction, and 'Methodological Borrowing' in Educational Inquiry*

NOEL GOUGH

*La Trobe University (Australia)*

*Complex systems are open, recursive, organic, nonlinear and emergent. Reconceptualizing curriculum, teaching and learning in complexivist terms foregrounds the unpredictable and generative qualities of educational processes, and invites educators to value that which is unexpected and/or beyond their control. Nevertheless, concepts associated with simple systems persist in contemporary discourses of educational inquiry, and continue to inform practices of complexity reduction through which researchers and other practitioners seek predictability and control. In this essay, I examine a number of theoretical, practical and historical dimensions of complexity reduction in education and their implications for inquiry and action. I focus in particular on the ways in which some education researchers have reduced the complexity of the objects of their inquiries through 'methodological borrowings' from other research endeavors, such as borrowing a version of 'evidence-based' research from medical science, and borrowing the 'triangulation' metaphor from surveying.*

## Complexity in the sciences and education

Complexity is a heterogeneous assemblage of concepts and metaphors arising from studies of complex systems in a variety of scholarly disciplines, including the sciences. Acknowledging the significance of complexity has transformed many of these disciplines but, as the following brief and partial history demonstrates, education research may have lagged behind a number of other fields in recognizing the implications of complexity for conceptualizing the objects of its inquiries. I emphasize that this history is partial because the scholarly literature to which I have ready access is predominantly Euro- and/or US-centric and limited to works available in English. Readers who work in other cultural traditions, and are familiar with relevant scholarly literature in languages other than English, must make their own judgments about the extent to which my arguments apply to their circumstances.

From Newton's era until the late nineteenth century, Western science focused to a large extent on determining the material structures of simple systems (see, e.g., Casti, 1997), and many scientists worked in the types of laboratories that provided the physical models for school and undergraduate teaching laboratories throughout the twentieth

century and beyond – laboratories equipped with apparatus associated with the stereotyped image of a scientist described in Margaret Mead and Rhoda Mettraux's (1957) research as 'a man who wears a white coat... surrounded by... test tubes, bunsen burners, flasks and bottles' (p.386). Subsequently – and especially since the development of integrated circuit technology and microprocessors – many scientists turned their attention to the informational structures of complex systems, such as protein folding in cell nuclei, task switching in ant and bacteria colonies, the nonlinear dynamics of the earth's atmosphere, far-from-equilibrium chemical reactions, and other objects of inquiry that lend themselves to investigation through computer simulations. Although studies of complex systems were foreshadowed in some scientific specializations as early as the 1870s – such as Willard Gibbs' pioneering research on multiphase chemical thermodynamics (see Weaver, 1948) – the terms 'complexity' and 'science' began to be linked explicitly in the 1940s, especially in fields such as systems biology and cybernetics (see Castellani, 2009; Castellani & Hafferty, 2009). Because complexity is a characteristic of many networked systems, it has also been a focus for inquiry and speculation in the social sciences, humanities and arts; noteworthy examples include studies of complex dynamics in literature and science (e.g., Hayles, 1990, 1991; Porush, 1991), syntheses of insights from computational theory and postmodernist philosophy (e.g., Cilliers, 1998, 2005), and explorations of constructs of emergence in organizations and management (Goldstein, 1999).

As Paul Cilliers (2010) points out, 'there is no coherent "complexity theory" which will unlock the secrets of the world in any clear and final way' (p. vii). A number of authors prefer to speak of 'complexity' rather than 'complexity theory', arguing that complexity is not necessarily (or exclusively) a theory, but might also be understood as an ontology or methodology (e.g., Biesta & Osberg, 2010). Nigel Thrift (1999) suggests that complexity is a rhetorical hybrid that takes on new meanings as it circulates in and through a number of actor-networks and, as it encounters new conditions, generates new hybrid theoretical and rhetorical forms. He further suggests that complexity signals the emergence of 'a new structure of feeling in Euro-American societies, which frames the future as open and full of productivity' (p. 31). In this sense, complexity invites us<sup>1</sup> to understand that many of the processes and activities that shape the worlds we inhabit are open, recursive, organic, nonlinear and emergent. It also invites us to be skeptical of mechanistic and reductionist explanations, which assume that these processes and activities are linear, deterministic and/or predictable and, therefore, that they can be controlled (at least in principle).

William Doll (1986, 1989, 1993) was one of the first education scholars to explore the theoretic and practical implications of reconceiving curriculum, teaching and learning by reference to concepts associated with chaos and complexity theorizing in the natural sciences.<sup>2</sup> Doll drew on Ilya Prigogine's work on dissipative structures in far-from-

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<sup>1</sup> I do not intend my use of terms such as 'us', 'our' or 'we' to imply that I am speaking *for* others. The 'us' and 'we' to whom I occasionally refer are those *with whom* I imagine I am having the conversation to which this essay contributes. They are likely to be readers with whom I share both an identity and responsibilities as a higher education worker, with particular reference to our interdependence in a common global political economy and our shared occupancy of an increasingly global public space of discourse and representation in which we deliberate together to decide our common futures (see Fraser, 1993).

<sup>2</sup> Others education scholars who explored chaos and complexity in the 1980s and early 1990s include Daiyo Sawada and Michael Caley (1985), Catherine Ennis (1992), Bill Green and Chris Bigum (1993) and me (Gough, 1991). Some of these early studies focus almost exclusively on chaos theory, which explains one cause of apparently complex behaviour in a dynamical system, namely, the sensitivity of some systems to variations in initial conditions. Although chaotic systems are deterministic, they are not predictable, because small differences in initial conditions (such as those resulting from rounding errors in numerical computation) can produce widely

equilibrium thermodynamic systems (see Prigogine & Stengers, 1984) to argue that concepts such as emergence – which occurs when a system of richly connected interacting agents produces a new pattern of organization that feeds back into the system – should encourage us to acknowledge the non-linear, unpredictable and generative characteristics of educational processes and practices. Prigogine’s research demonstrates that irreversible processes in open and far-from-equilibrium chemical systems can give rise to increasingly higher levels of organizational complexity. In Jaegwon Kim’s (1999) words, such systems ‘begin to exhibit novel properties that... transcend the properties of their constituent parts, and behave in ways that cannot be predicted on the basis of the laws governing simpler systems’ (p. 3). Osberg and Biesta (2007) refer to this type of emergence as ‘strong’, thereby distinguishing it from what Mark Bedau (1997, 2008), among others, calls ‘weak’ emergence, which includes surprising events in natural systems (such as unexpected weather conditions) that can be explained deterministically by reference to the system’s prior state.

### Education and the ‘trailing edges’ of simple systems science discourses

Complexity offers an alternative to modeling education on simplifications of industrial systems, such as the so-called ‘factory’ model of schooling inspired by Frederick Taylor’s (1911) principles of ‘scientific management’, which remained a powerful force in educational administration and curriculum studies, especially in the USA, until at least the late 1960s. For example, the textbook *Fundamentals of Curriculum Development* (Smith, Stanley, & Shores, 1957) included a chapter titled ‘Curriculum development as educational engineering’, and George Beauchamp (1968) similarly devoted a chapter of *Curriculum Theory* to ‘curriculum engineering’, in which he characterized school superintendents, principals and curriculum directors as the ‘chief engineers in the curriculum system’ (p. 108). Taylor’s emphasis on designing industrial systems to achieve specified products is reproduced in the objectives-driven curriculum models of Franklin Bobbitt (1918, 1928) and Ralph Tyler (1949), and is presently manifested in outcomes-based approaches to higher education curriculum, many of which are informed by John Biggs’ (1996) influential principle of ‘constructive alignment’.<sup>3</sup> Bobbitt, Tyler and Biggs represent curriculum as a simple, tightly coupled system in which it is both possible and desirable to closely align what students do in order to learn with intended learning outcomes and how they are assessed.

Many curriculum theorists opposed the crude instrumentalism inspired by Taylor’s principles,<sup>4</sup> but others championed an alternative version of ‘scientific’ management of curriculum by selectively appropriating concepts from the nascent science of cybernetics – the study of systems which understand both humans and machines in terms of information processing. For example, in the 1980s, David Pratt (1980) and Francis Hunkins (1980), among others, borrowed concepts from cybernetics to support

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divergent outcomes. Complex systems are not predictable because they are not deterministic; self-organisation *emerges* from a multiplicity of interactions.

<sup>3</sup> For example, Chris Rust (2002) uses the term ‘constructive alignment’ to characterise ‘a paradigm shift... in the espoused rhetoric of higher education... in much of the English-speaking world (including the UK, the USA, Australia, New Zealand and South Africa)’ (p. 146). I find it puzzling that Biggs’ recycling of principles that have been in circulation for nearly a century (in the discourses of curriculum inquiry with which I am familiar) can be understood as part of a ‘paradigm shift’.

<sup>4</sup> Prominent critics of mechanistic curriculum models include the ‘deliberative’ curriculum scholars influenced by Joseph Schwab’s (1969, 1971, 1973) germinal essays on ‘the practical’, together with the authors (and their affiliates) represented in William Pinar’s (1975) edited collection, *Curriculum Theorizing: the Reconceptualists*.

assertions such as: 'the cybernetic principle... permits rationalization of the total managerial activities related to maintaining the program' (Hunkins, 1980, p. 324).

In the years since Norbert Wiener (1948) coined the term 'cybernetics', it has developed as an interdisciplinary science that interprets the interrelationships of organisms and machines in terms of concepts such as feedback loops, signal transmission, and goal-oriented behavior. Cybernetics is a contested and ever-changing conceptual territory and there is no singular 'cybernetic principle'. Thus, Pratt's (1980) attempts to apply 'a cybernetic perspective' to the problem of 'managing aptitude differences' raise critical questions about which cybernetic principles (if any) should apply to the 'scientific management' of education:

The problem of maintaining consistently high achievement from a group of learners who differ in aptitude and other characteristics can be seen as an instance of the general question of how a system with variable input can be designed to produce stable output. Phrased in this way, the question lies squarely within the field of cybernetics, the study of self-regulation in systems (p. 335).

Pratt uses temperature regulation in a building to exemplify a simple cybernetic system, and temperature regulation in the human body as an example of a cybernetic system 'found in nature'. An unexamined assumption in Pratt's argument is that curriculum systems and cybernetic systems should be 'designed to produce stable output'. His choice of examples assumes that 'stable output' is an inevitable product of self-regulation in nature and, therefore, that such stability is also a desirable product of curriculum work – and that modeling curriculum on cybernetic systems can help us to achieve that stability.

However, homeostasis is just one among many key concepts that have circulated in the discourses of cybernetics at various times. Katherine Hayles (1994) points out that during the period from (roughly) 1945 to 1960, two conceptual 'constellations' formed that were in competition with one another:

One of these was deeply conservative, privileging constancy over change, predictability over complexity, equilibrium over evolution. At the center of this constellation was the concept of homeostasis, defined as the ability of an organism to maintain itself in a stable state. The other constellation led away from the closed circle of corrective feedback, privileging change over constancy, evolution over equilibrium, complexity over predictability. The central concept embedded in it was reflexivity, which for our purposes can be defined as turning a system's rules back on itself so as to cause it to engage in more complex behavior. In broader social terms, homeostasis reflected the desire for a 'return to normalcy' after the maelstrom of World War II. By contrast, reflexivity pointed toward the open horizon of an unpredictable and increasingly complex postmodern world (p. 446).

In Hayles's (1994) brief history of three waves of cybernetics since WWII (see Figure 1), reflexivity displaced homeostasis as a key concept in the period from 1960 to about 1972, after which the emphasis shifted to emergence, with interest focused 'not on how systems maintain their organization intact, but rather on how they evolve in unpredictable and often highly complex ways through emergent processes' (p. 463). Hayles emphasizes that concepts such as homeostasis and reflexivity do not disappear altogether but linger on in various ways and may exert an inertial weight that limits the ways in which newer concepts are deployed – in Stephen Hill's (1990) words, these concepts constitute the 'trailing edge' of conceptual change in cybernetics.

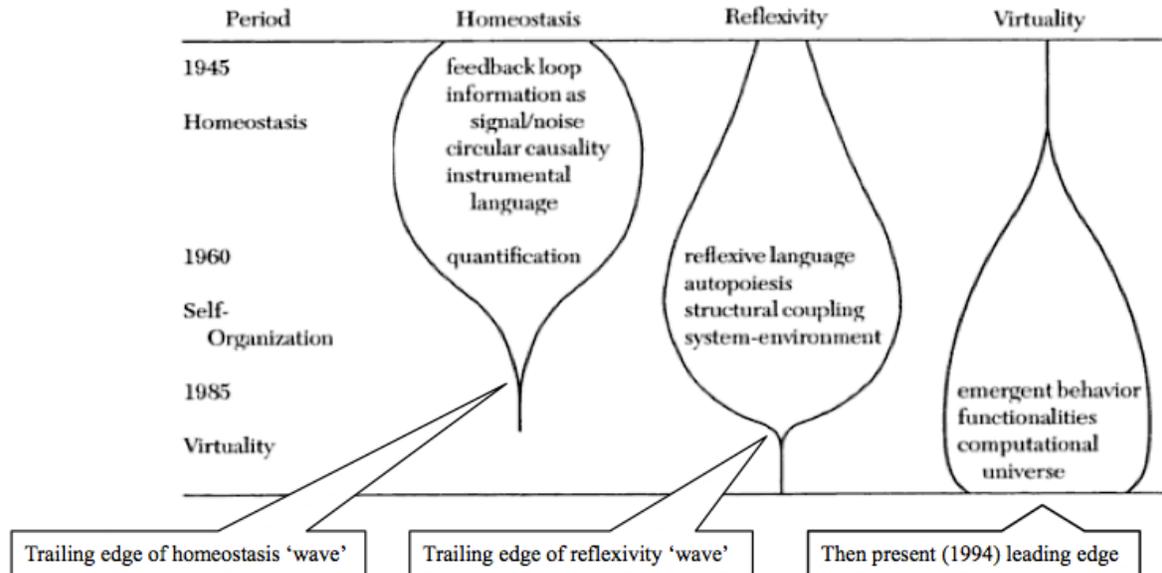


Figure 1: Three waves of cybernetics since World War II  
(adapted from Hayles, 1994, p. 444)

In regard to Hunkins' and Pratt's selective borrowings of cybernetic principles, I believe that it is reasonable to ask why such well-regarded curriculum theorists, in 1980, continued to privilege homeostatic self-regulation two decades after it had ceased to be generative in the field of cybernetics – a concept that by then was part of the 'trailing edge' of developments in cybernetics. If these theorists were interested in the implications of cybernetics for educational theory and practice, why did they not look towards its leading edges by exploring reflexivity, emergence and self-organization? I speculate that, unlike cyberneticists, educators faced few compelling challenges to the deeply sedimented conceptions of 'natural' order to which Pratt alludes – order as stability, predictability, and equilibrium. Such conceptions of 'natural' order are pervasive in many disciplines and often have persisted as very lengthy 'trailing edges' in their popular representations, as has been the case with what we can quite literally call the 'textbook ecology' received by many undergraduates in US colleges and universities for more than 50 years.

During the post-World War II period, under the leadership of Eugene Odum, the US version of systems ecology privileged the concept of the ecosystem as a stable and enduring emblem of 'natural' order, epitomized by the dominance of the 'balance of nature' metaphor which, as Kim Cuddington (2001) argues, 'is shorthand for a paradigmatic view of nature as a beneficent force' (p. 463). Environmental historian Donald Worster (1995) argues that Odum's (1953) textbook, *Fundamentals of Ecology*, 'laid so much stress on natural order that it came close to dehistoricizing nature altogether' (p. 70). He also notes that during the 1970s and 1980s 'the field of ecology... demolished Eugene Odum's portrayal of a world of ecosystems tending towards equilibrium' (p. 72). For example, the studies collected by Steward Pickett and Peter White (1985) deliver the consistent message that the very concept of the ecosystem has receded in usefulness and, to the extent that the word 'ecosystem' remains in use, that it has lost its former implications of order and equilibrium (see also Pickett, Kolaska & Jones, 2007). Similarly, Andrew Jamison (1993) points out that systems ecology contributed very little to the solution of environmental problems and, by the late 1970s, new evolutionary approaches had become increasingly popular among ecologists, 'so that systems ecology today is only one (and not even the most significant one at that) of

a number of competing ecological paradigms' (p. 202). Nevertheless, Odum's ideas have endured as a trailing edge of conceptual change in ecology, epitomized by the publication of a fifth edition of *Fundamentals of Ecology* (Odum & Barrett, 2005) three years after his death.<sup>5</sup>

Gregory Cooper (2001) observes that in areas such as population and community ecology, 'the balance of nature idea ... has worked in the background, shaping inquiry', but that it has also been argued largely on conceptual rather than empirical grounds (p. 481). In this light, it is significant that Robert Ulanowicz's (1997, 2007, 2009) empirical work – which includes network analysis of trophic exchanges in ecosystems, the thermodynamics of living systems, causality in living systems, and modeling subtropical wetland ecosystems – emphasizes that chance, disarray and randomness are necessary conditions for creative advance, emergence and autonomy in the natural world.

The above snippets from the history of education theorists' selective borrowings of scientific concepts and principles point to the need for two related types of caution. Firstly, if we apply scientific understandings to educational inquiry, then we should ensure that these understandings constitute the current leading edges of the relevant field or discipline, rather than recycle the abandoned or outmoded concepts and principles that constitute their trailing edges. Secondly, we should be cognizant of the risk that privileging scientific explanations might be interpreted as reifying a one-way relationship between 'natural' order and human affairs. I see no reason to exclude the invocation of nature as a ground for judgment, but I cannot assume that descriptions of the physical or 'natural' world are prescriptions for social life. I agree with Andrew Ross (1994) that 'ideas that draw upon the authority of nature nearly always have their origin in ideas about society' (p. 15). Recommendations for educational decision and action cannot be justified by reference to Prigogine's Nobel prize-winning studies of far-from-equilibrium chemical systems or to Ulanowicz's research on emergence in ecological systems (for which he received the 2007 Ilya Prigogine Medal). Rather, the value of such studies to educators is immanent in the new 'structures of feeling' they provide – the new concepts, metaphors and forms of social imagination that might emerge from their deployment in educational discourses-practices.

Conceptual change in the disciplines of cybernetics and ecology invites educators to be suspicious of a simple systems rationality in which educational policies, directives, incentives and disincentives function as homeostatic devices, regulating the diverse inputs of students, teachers and researchers by bringing them within closed circuits of corrective feedback in order to maintain stability and equilibrium.

### Complexity, complexity reduction and 'methodology borrowing': some issues and implications for educational inquiry

Complexity invites us to understand our physical and social worlds as open, recursive, organic, nonlinear and emergent, and to be cautious of complying with models and trends in education that assume linear thinking, control and predictability. I will focus here on two such trends. First is the increasing emphasis over the past two decades in measuring educational 'outcomes', exemplified by international comparative studies such as the Trends in International Mathematics and Science Study (TIMSS), the Progress in International Reading Literacy Study (PIRLS) and the OECD's Programme for International Student Assessment (PISA). These studies, and their translation into comparative/competitive 'league tables' that are assumed to provide information about how national education systems perform relative to those of other countries, are used by

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<sup>5</sup> The second edition (coauthored with Howard Odum) was published in 1959 and a third edition (sole-authored) in 1971. According to Odum's biographer, Betty Jean Craige (2001), his textbook, *Basic Ecology* (1983), 'was actually the fourth edition of *Fundamentals of Ecology*' (p. 191).

national governments to inform educational policies, often accompanied by the rhetoric of 'raising standards'. A second and related trend is the increasing emphasis on making education an 'evidence-based' practice by seeking causal links between measured educational 'inputs' and the measurement of outcomes. Advocates of evidence-based education, such as Robert Slavin (2002), argue that educational inquiry should be modeled on the types of scientific research procedures, exemplified by large-scale experimental randomized controlled field trials, that produced the 'progressive, systematic improvement over time that has characterized successful parts of our economy and society throughout the 20th century, in fields such as medicine, agriculture, transportation, and technology' (p. 16).

Complexity offers us a way to think about relationships between inputs and outcomes that does not impel us to seek evidence of causal relationships between them. Complexity suggests that educational processes ought to be characterized by gaps between 'inputs' (policy, curriculum, pedagogy) and 'outputs' (learning). In Biesta's (2004) terms, these are not gaps to be 'filled' but sites of emergence. As Jeffrey Goldstein (1999) writes, emergence 'refers to the arising of novel and coherent structures, patterns, and properties during the process of self-organization in complex systems' (p. 49). In other words, what we have previously imagined to be 'outcomes' or 'products' – knowledges, understandings, individual subjectivities, etc. – emerge in and through educational processes in unique and unpredictable ways. As Biesta (2009, p. 40) argues, education contributes not only to qualification (the transmission of knowledge and skills) and socialization (the insertion of individuals into existing social, cultural and political orders), but also to processes of subjectification – of becoming a subject – or what he previously referred to as the 'coming into presence' of unique individuals (Biesta, 2006, p. 49).

Rethinking education as emergence potentially destabilizes the instrumentalist rationality that, as it were, 'programs' educational systems (and the agents/agencies within them) to privilege orderly and predictable processes culminating in stable output (see Gough, 2010). However, this potentiality is undermined by a politics of complexity reduction which is not unique to education and educational research. Most forms of inquiry deliberately reduce the complexity of the objects of their inquiries and/or the data they produce in one or more ways – these may be prospective (e.g. limiting the number of initial variables) or retrospective (e.g. backwards selection of particular trajectories). But acknowledging complexity should dispose us to ask questions about how complexity reduction is achieved and, perhaps more importantly, who is reducing complexity for whom and in whose interests. If our knowledge interests are in prediction and control, as in much medical science, then reducing the complexity of the object of inquiry might be defensible. For example, medical scientists seeking a vaccine against malaria reduce its complexity by limiting the initial variables they study. They deliberately – and some might say defensibly – limit their investigations to those aspects of malaria that portray it as a 'natural' entity caused by protozoan parasites and spread among humans by mosquitoes; the variables they study are those that they see to be pertinent to producing a physicochemical solution to the malaria problem.

But malaria is much more complex than this: outbreaks of malaria in particular places and times result from numerous complex interactions among parasites, mosquitoes, humans and various social, political (often military), administrative, economic, agricultural, ecological and technological processes. The complexity of malaria as an object of inquiry is more apparent if we see it, as David Turnbull (1989) argues, as a political disease 'resulting from the dominance of the Third World by the colonial and mercantile interests of the West' (p. 287). Acknowledging that malaria-as-an-object-of-political-inquiry is more complex than malaria-as-an-object-of-immunology also has implications for education. In most Western school curricula, malaria is mentioned only in biology courses, where it frequently is used as an example of the roles

of some organisms in carrying disease. The intended learning outcomes are usually little more than students being able to recall a number of biological 'facts' such as: the protozoan parasite's lifecycle; explanations for the symptoms of mosquito bites (itchy swellings) and malaria (chills and fevers); and reasons for Western travelers to some tropical locations to take precautions against contracting the disease. But such a reduction of malaria's complexity is not defensible from my standpoint as a science educator committed to 'the ambiguous struggling through and with colonial pasts in making different futures' – to quote Helen Verran's (2001, p. 38) characterization of postcolonialism. I can see no worthwhile educational purpose for bringing Western students' attention to malaria as an object of Western scientific inquiry unless we also alert them to the massive human tragedy of millions dying of malaria in the West's tourist destinations and, moreover, that this is a tragedy that Western nations have the resources (but not the political will) to ameliorate.

In the remainder of this essay, I focus on reductions in complexity that result from what I call 'methodological borrowings' from other disciplines or specializations. I offer three cautionary tales that demonstrate how complexity in specific areas of inquiry has been reduced by borrowing concepts and/or tools from the methodological frameworks used in other areas. I draw a number of my examples from environmental education research because it is an area of curriculum inquiry in which I have professional interests and responsibilities.

*Cautionary tale 1: environmental education researchers borrowing from (constructivist) science education research*

An examination of the history of environmental education research in particular times and places provides several examples of the ways in which researchers have reduced the complexity of their objects of inquiry. For example, as I argue in greater detail elsewhere (Gough, 1999a), the majority of research reports published in *The Journal of Environmental Education* (JEE) up to the late 1990s appear to be modeled on some of the more positivistic forms of science education research. I am not alone in reaching this conclusion: Ian Robottom and Paul Hart (1995), who reviewed environmental education research (and summarized other reviews of that research) from the 1970s onwards, argue that the predominant approach could be characterized as 'applied science in nature' (that is, purportedly experimental), 'objectivist', 'instrumentalist', and 'behaviorist' (p. 5). In this approach, environmental education researchers reduced the complexity of learning by limiting the variables they included in their inquiries to observations of 'objective' behavioral change. They deliberately ignored 'subjective' conceptual activity.

I suggest that environmental education researchers should exercise caution when 'borrowing' concepts and/or methodologies from other disciplines. Objects of inquiry in environmental education research are not identical to objects of inquiry in science education research and, moreover, may be complex in different ways. Research in mathematics and science education for several decades has been strongly influenced by 'constructivist' approaches to learning. The constructivist position is that 'knowledge is not transmitted directly from one knower to another but is actively built up by the learner' and is also informed 'by a view of scientific knowledge as socially constructed and by a perspective on the learning of science as knowledge construction involving both individual and social processes' (Driver, et al., 1994, p. 6). Much of this research has focused on conceptual change and educational strategies for dealing with students' 'conceptions' (e.g., Driver, 1989), 'preconceptions' (e.g., Clement, 1982) and 'misconceptions' (e.g., Novak, 1987).

Bruce Munson's (1994) attempt to adapt constructivist science education research to the purposes of environmental education research provides an example of complexity

reduction that is particularly pertinent to the brief history of complexity theorizing in education I provide above. Munson draws on the literature of conceptual change in science education to explore ways of dealing with students' 'ecological misconceptions'. Munson equates 'misconceptions' with 'scientifically incorrect interpretations and responses to problems' (p. 30) – a more dogmatic formulation of 'misconceptions' than those offered by science education researchers such as Clement et al. (1989), who see them as 'students' ideas that are incompatible with currently accepted scientific knowledge' (p. 555). Munson argues that because 'ecology forms the foundation for environmental education', research on students' 'ecological misconceptions' should 'provide useful insights for environmental educators' (p. 30). Thus, Munson ignores any contestation over what knowledge might be 'foundational' for environmental education and also neglects contestation within the field of ecology itself. Rather tellingly, Munson begins by quoting Eugene Odum's (1977) view that 'we are abysmally ignorant of the ecosystems of which we are dependent parts' (p. 1289). Despite paying lip service to the proposition that 'the field of ecology has improved considerably over the last 16 years', Munson alleges that 'environmental educators could still use Odum's quote to express our concerns and beliefs about the public's understanding of basic ecological concepts' (p. 30), an assertion which may reveal that Munson himself is 'ignorant' of post-Odum ecology. Munson's phrasing suggests that 'basic ecological concepts' are somehow stable and enduring (even 'natural') rather than being constantly changed and reformulated by the ecologists who construct them.

Munson's foundationalist assumptions about the existence of 'basic ecological concepts' match Odum's foundationalist view of stability in nature, and his apparent ignorance of post-Odum ecology suggests that many of the 'basic ecological concepts' to which he refers might themselves be 'misconceptions' – insofar as they are incompatible with currently accepted scientific knowledge – and his appropriation of the conceptual change discourse of constructivist science education research is thus little more than an elaborate rationale for replacing students' 'misconceptions' with his own.

Also, in Munson's schema, subjectivities are as stable as ecosystems: 'misconceptions are stable elements of an individual's conceptual framework and highly resistant to change' (p. 33), a proposition that is indeed supported by a great deal of the conceptual change research in science education (although it is also possible that much 'evidence' of conceptual stability is an artifact of the researcher's assumptions of a stable subject). This kind of assumption leads Munson to interpret the literature of conceptual change in science education in a way that seems to take reductionism to new extremes of absurdity:

If educators view misconceptions as completely individualistic, they will find the task of teaching for conceptual change overwhelming. However... some studies have found that the vast majority of individuals hold a limited number of misconceptions (Driver et al 1985). This suggests that a finite number of ecological misconceptions exist. Such a conclusion should be encouraging to environmental educators and environmental curriculum developers. (p. 34, my emphasis)

Munson's suggestion 'that a finite number of ecological misconceptions exist' is logically absurd (a finite sample does not prove the existence of a finite population) and somewhat perverse – I cannot understand why any human being would accept finite limits to human imagination as an a priori principle (and history would seem to demonstrate that humans have a limitless capacity to generate concepts that might in retrospect be judged to be erroneous in some way).

Although conceptual change research in science education involves particular kinds of complexity reduction, which might or might not be defensible, Munson's 'borrowing' of conceptual change and 'misconceptions' research for environmental education involves further layers of complexity reduction that cannot be defended. At the very least, as contestation about the ecosystem concept illustrates, there might simply be too

few (if any) uncontested conceptions in environmental education that can function as reference points for 'desirable' conceptual change.<sup>6</sup>

*Cautionary tale 2: education researchers borrowing from 'evidence-based' medical science*

As already noted, the idea that education should be or become an evidence-based practice is now a widespread and uncritically taken-for-granted assumption in many countries. This assumption appeals to many education researchers because they are often quite legitimately concerned with exploring 'what works' to achieve desired purposes. Thus, Kristan Cockerill's (2010) evaluation of a community water education program quite rightly seeks evidence of its effectiveness in informing community members about water availability and management. However, there are other instances of education research where seeking evidence of 'what works' reduces the complexity of the issue under investigation in ways that produce simplistic – and thus almost meaningless – conclusions. For example, a number of environmental education researchers have sought to determine the significant life experiences and formative influences on the development of environmental awareness.<sup>7</sup> Thomas Tanner (1998) characterizes this research as 'studies which aim to identify formative influences' in the lives of 'adults committed to environmental quality' (p. 365). Elsewhere he writes:

The rationale for such research is simple: if we find that certain kinds of early experience were important in shaping such adults, perhaps environmental educators can, to the degree feasible, replicate those experiences in the education of the young (Tanner, 1998a, p. 399).

The naivety of assuming that 'what worked' for us environmentally responsible adults could or should be replicated for our and other people's children is exposed in a number of subsequent commentaries and critiques of significant life experiences research (Dillon, Kelsey & Duque-Aristizábal, 1999; Annette Gough, 1999; Noel Gough, 1999b). However, my point is that this type of research exemplifies a particularly obvious form of complexity reduction, namely, the attempt to produce 'evidence' of causal relationships between a particular category of inputs ('formative influences' in the early years) and outcomes ('adults committed to environmental quality').

In the UK the push for evidence-based education arose partly in the wake of David Hargreaves' (1996) Teacher Training Agency lecture and subsequent publications (e.g., Hargreaves, 1997) in which he draws an analogy between teaching and medicine, claiming that 'the knowledge-base of teachers is less rich than that of doctors' (1997, p. 410).<sup>8</sup> Other UK educational researchers were quick to point out why evidence of 'what works' is an inadequate basis for educational thought and action. For example, Elizabeth Atkinson (2000) explores the ways in which theories, rather than evidence, provide an essential infrastructure to teachers' day-to-day thinking and practice, and compares 'the restrictive effect of a focus on "what works" with the opportunities offered by

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<sup>6</sup> I have further reservations about adapting 'misconceptions' research to environmental education. In particular, I question the ways in which some researchers identify some conceptions as 'misconceptions' and the assumptions they make about the relationship of privileged conceptions (such as 'scientific' knowledge) to action (or dispositions to act). See, for example, my detailed critique (Gough, 1999a) of Joy Palmer's and others' (1995; 1996) studies of the development of environmental knowledge and concern in children from pre-school years onwards.

<sup>7</sup> See, for example, the various contributions to *Environmental Education Research*, 4(4), 1999, a special issue on significant life experiences guest edited by Thomas Tanner.

<sup>8</sup> In the USA the reauthorization in 2001 of the Elementary and Secondary Education Act (commonly known as the 'No Child Left Behind' Act) has explicitly promoted randomized research designs and implicitly promoted clinical trials as the only legitimate educational science. In so doing, the law seeks to remodel education research in a medical mode.

postmodernism for broadening the scope, purpose and interpretation of the research of the future' (p. 317).

More recently, Biesta's (2007) criticisms of the idea of evidence-based practice, and the ways it has been promoted, focus on the tension between scientific and democratic control over educational practice and research. Biesta examines a number of assumptions underlying evidence-based education, including the extent to which education can be compared to medicine and the role of knowledge in professional actions. Biesta (2009) further notes that many of those who champion evidence-based education argue that the only acceptable evidence is that which can be produced by large-scale experimental studies (such as randomized controlled field trials) and careful measurement of the correlation between 'input' and 'outcomes'. He also emphasizes the restrictions that evidence-based approaches place on the role of research in educational practice and the ways it distracts us from more important deliberations on the purposes, functions and directions of educational processes and practices.

As Gary Thomas (2010) points out, the mere use of the word 'evidence' is often taken to be enough to clinch an argument. Thomas affirms that 'we all use evidence of many kinds and forms and the more of it we have, the more confident we can be', but he also asserts that 'we should be cautious about claiming that we have better evidence than someone else' (p. 15). He provides two very clear examples of the abuse of 'evidence-based' claims in reporting both educational and medical research.<sup>9</sup> In each case, researchers selectively adduced meager evidence and transmuted it into 'unequivocal' evidence that supported their predetermined theoretical positions. Thomas also considers how evidence is understood in another practice-based profession, namely, law:

I stroll through the second floor of the Gower Street branch of Waterstone's<sup>10</sup> and I happen upon the law section. In one of those delightful moments of serendipity my eye is caught by a bank of shelves containing books on evidence. Not one shelf, but a whole bank of them, and each one on aspects of evidence...

It became humbly clear to me that lawyers approach the notion of evidence with more finesse, deliberation and care than I have ever done. (Inevitable, really, since they have been thinking about evidence for millennia, rather than since 1998.) They have caressed it, nurtured it, problematised it, taxonomised it. They raise issues about its nature: whether it is direct evidence, circumstantial evidence, documentary evidence, collateral evidence, confession evidence, witness evidence (including the definition of 'witnesses'; the oppression or competence of witnesses). They muse about silence, hearsay, testimony, affirming evidence, character evidence, expert evidence.

They ponder over standards of proof, reverse burdens of proof, standards within standards, presumptions of fact, persuasive presumptions. They worry about bias, corroboration, privilege and interest, admissibility, cogency, prejudice, relevance.

So for lawyers, evidence is a fragile thing. It is not a boulder to be thrown into debate (pp. 14-15).

Rather than accepting the proposition that educational researchers should follow the example of evidence-based medicine – or, rather, Western medicine – we should perhaps also consider the implications of adopting other understandings of evidence. As Thomas indicates, we might usefully consider conceiving evidence in terms of legal

<sup>9</sup> Matthew Weinstein (2004) offers an insightful critique of randomized experimental designs by drawing on the narrative of a human subject participating in a medical randomized experiment to raise questions about the extent to which such designs secure the goals that the 'No Child Left Behind' legislation claims they will: validity, rigor, and replicability.

<sup>10</sup> Waterstone's is a UK-based chain of around 300 bookshops. Its main academic branch is located on Gower Street, near University College London, and is the largest academic bookshop in Europe.

studies, but we could also consider other disciplines and cultural referents, such as traditional Chinese medicine, divinity, game studies, journalism, irenology, Islamic economic jurisprudence, media studies, silviculture, risk management, psychophysics, or even disciplines that only exist in speculative fiction, such as therolinguistics (Le Guin, 1984).<sup>11</sup> What counts as evidence in these discourses-practices? What else informs decision-making in them? What might be their analogs in education? How would educational research informed by these analogs differ from current orthodoxies?

*Cautionary tale 3: social science researchers borrowing 'triangulation' from surveying*

A relatively recent development in educational inquiry is the crusade for 'mixed methods',<sup>12</sup> some of whose advocates go so far as to represent it as a new 'paradigm'. For example, Burke Johnson and Anthony Onwuegbuzie (2004) assert:

Mixed methods research is formally defined here as the class of research where the researcher mixes or combines quantitative and qualitative research techniques, methods, approaches, concepts or language into a single study. Philosophically, it is the 'third wave' or third research movement, a movement that moves past the paradigm wars by offering a logical and practical alternative (p. 17).

I would prefer to move beyond the paradigm wars by relocating to what Patti Lather (1991) calls the 'post-paradigmatic diaspora' (p. 121), but there is a further difficulty with mixed methods, namely, the uncritical appropriation of 'triangulation' – that is, 'seeking convergence and corroboration of results from different methods and designs studying the same phenomenon' (Johnson & Onwuegbuzie, 2004, p. 22) – as a major reason for conducting research in this way.

I have recently reviewed a number of manuscripts submitted to Environmental Education Research that use the 'triangulation' metaphor as if its meaning was unproblematic and uncontested. For example, one author writes: 'The accuracy of the coding of the drawings was triangulated by the verbal data from the interviews and group discussions'; another writes: 'The researcher shared and discussed the data and interpretation so that it could be reflected and triangulated to enhance the reliability of the data analysis process'. Neither author offers any additional description or explanation to justify how triangulation works, what it does, and why it might be justified in their respective research activities. Similarly, Cheryl Lousley's (1999) account of her critical ethnographic research with four urban, multicultural secondary school environment clubs includes a sole mention of triangulation without further explanation: 'The validity of the research results can be augmented through triangulation of methods and analysis' (p. 296). Again, Mauri Åhlberg and Vuokko Ahoranta (2002) offer no explanation of how they deployed triangulation other than simply asserting: 'Triangulation among complementary methods and data sources produced consistent conclusions' (p. 128). More puzzling is Julie Ernst and Martha Monroe's (2004) single reference to triangulation which appears only in the abstract of their article: 'Interviews of students and teachers were used in the classic sense of triangulation' (p. 507).

Other authors offer a little more explanation of how they understand triangulation and how they use it in their research. For example, Daniel Shepardson et al. (2009) refer to their 'process of independently constructing categories and then reaching consensus' which, they claim, 'provided a degree of triangulation, reducing the influence of bias and subjectivity and increasing the validity of our analysis and interpretation of the

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<sup>11</sup> In Le Guin's (1984) short story, therolinguistics is the study of animal languages.

<sup>12</sup> The relative recency of 'mixed methods' is indicated by the dates of such publications as the first *Handbook of Mixed Methods in Social and Behavioral Research* (Tashakkori & Teddlie, 2003) and a new *Journal of Mixed Methods Research* first published in January 2007; see <http://mmr.sagepub.com>

results' (p. 555). Regula Kyburz-Graber (2004) explicitly equates triangulation with 'using multiple sources of evidence (data collection and interpretation)' (p. 58).<sup>13</sup>

Norman Blaikie (1991), who was a land surveyor for 16 years before turning to sociology, gives a clear and detailed description of the concept of triangulation in surveying, navigation and military strategy, and its subsequent appropriation by the social sciences. He argues that 'triangulation means many things to many people and... none of the uses in sociology bears any resemblance to its use in surveying' (p. 131). He also points out that 'triangulation' of social worlds make sense only if the researcher works within a 'positivistic frame of reference which assumes a single (undefined) reality and treats accounts as multiple mappings of that reality' (p. 120).

Alexander Massey (1999) builds on Blaikie's critique to demonstrate that some researchers have mistakenly assumed that the ontological and epistemological bases of certain sociological activities are the same as those underpinning the triangulation methods used in surveying. The result of this philosophical and methodological confusion is that in studies that use mixed or multiple methods, many misleading and invalid claims are made in the name of triangulation. Massey identifies seven common logical errors underpinning methodological triangulation, and concludes that its conceptual basis is flawed 'to such an extent that generations of researchers and readers have lost their way through their very attempts to improve sociological "navigation" techniques' (p. 195). The same might be true for education researchers who have uncritically embraced mixed methods.

I speculate that the prior disciplinary histories of many environmental education researchers might dispose them to see the triangulation metaphor as common sense. For example, sampling techniques such as line transects in botany reinforce the legitimacy of a surveying methodology.

### Not a conclusion...

I share Susanne Kappeler's (1986) antipathy to the conventional ways of concluding a text:

I do not really wish to conclude and sum up, rounding off the argument so as to dump it in a nutshell on the reader. A lot more could be said about any of the topics I have touched upon... I have meant to ask the questions, to break the frame... The point is not a set of answers, but making possible a different practice (p. 212).

So I end with a question rather than a conclusion: how might understanding our worlds and selves as open, recursive, organic, nonlinear and emergent make 'a different practice' possible for educational inquiry?

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<sup>13</sup> In the literature of environmental education research, Marcia McKenzie (2005) provides one of the few examples of a critical appraisal of the utility and defensibility of the triangulation metaphor.

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## About the Author

Noel Gough ([n.gough@latrobe.edu.au](mailto:n.gough@latrobe.edu.au)) is Foundation Professor of Outdoor and Environmental Education at La Trobe University, Australia. His current research focuses on the diverse implications of globalization, internationalization and multiculturalism for education, and on refining poststructuralist research methodologies in education, with particular reference to curriculum inquiry, environmental education, and science education. See <http://www.latrobe.edu.au/education/about/staff/profile?uname=NGough>