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ABSTRACT

This commentary discusses reasons that lead scholars to feel entitled to make judgements in areas where they have little or no expertise. Three current reports about the future of online learning are considered, issued by the Global Learning Council, Massachusetts Institute of Technology, and the Gates Foundation. Two of these publications contain contradictions and confusion that may simply be due to lack of awareness of distance education (DE) and online learning research. The third report gives good evidence but has been criticized for being too academic. The conflicting standards and criteria evident in such writings may be due to institutional pressures to promote policies and principles despite lack of supportive evidence, and to weaknesses in the DE literature itself. As major institutional policies about online learning are likely to be based on the recommendations of such reports, it is important for DE specialists to challenge them where appropriate.

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Out with the old

'Much research has been done on online learning.' Alternatively: 'not much research has been done on online learning.' These familiar openings of student essays commonly provide little evidence in support of their claims, sending a clear signal that their writers are unaware of how much research has actually been done. Recently, the 'not much research' option has received prominent support in the distance education (DE) literature, despite the immense amount of research published in journals such as this one for half a century. The failure of particular educators to acknowledge the history of their ideas when laying claim to them has been analyzed in previous articles by this writer (Baggaley, 2014a, 2014b, 2015). The substance and terminology of flipped learning, for example, were not invented in the last decade as per Sams (2010) claim that he and Bergmann 'were the first to flip their classes'; and the tendency to ignore and even to disparage previous academic studies is not only to be found in the writings of junior researchers, but also occurs at the highest academic levels: witness the statements made on behalf of the Global Learning Council (GLC), a recently established group including seven university and corporate presidents (Global Learning Council [GLC], 2016; O'Neill, 2013). Ignoring the vast best-practices literature of online learning of DE in both the West and Asia (Belawati, 2010; Jung, Wong, Chen, Baigaltugs, & Belawati,

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2011), Hunter Rawlings, President of the Association of American Universities, quoted by O'Neill (2013), stated that 'there is very little good research on the best forms of online learning' and 'there are no good studies of what constitutes bad online pedagogy.' The GLC (2016) has since expressed the opposite view:

> An extensive body of research in learning science and related fields illuminates the mechanisms that underlie robust learning. It's well established that applying learning science to the design of technology-enhanced learning leads to enhanced learning outcomes.

As with its 2013 statement, however, the GLC's 2016 report provides minimal literature evidence for this view.

In parallel with the GLC report, a Massachusetts Institute of Technology (MIT) report (Willcox, Sarma, & Lippel, 2016) discusses the role of online learning in higher education in more detail. It summarizes the major areas of educational research, including cognitive psychology, neuroscience, social sciences, educational technology, and assessment; and, unlike the GLC paper, it supports its review with a copious list of 261 endnotes including 247 references to books and articles, 12 (unnamed) web sites, and 2 comments. Although the report's title indicates that online education is a major focus, the 28-page body of the report actually devotes only 3 pages to details about that topic, beginning with 'Much has been written about the impact of online education' (p. 13). The term *distance* receives only 5 mentions in the report, in uncorroborated student-like phrases such as 'distance education goes back many decades' (p. 2), and 'Distance education has existed in the United States in various forms for more than a hundred years' (p. 13). For actual details of DE's history and scope, the report relies on just one of its 247 citations (Matthews, 1999), and cites only one other publication in a DE journal (Jaggars, 2014). For the MIT writers (p. 14), it appears that online education began with the online publication of MIT's curricular materials in 2002 (Diamond, 2003). Bates (2014), by contrast, shows that the theory and practice needed by online education had their origins in ancient Greece, with numerous scholarly updates ever since.

A third report, edited by Siemens, Gasevic, and Dawson (2015) and sponsored by the Gates Foundation's MOOC Research Initiative, compensates for the neglect of the DE literature by the previous two reports. 'Preparing for the Digital University' is a 6-chapter, book-sized discussion of the literatures of DE, online learning, blended learning, and assessment, with particular emphasis on the future of MOOC research. The report's research team surveyed 339 relevant articles in 10 DE, educational technology, and educational computing journals, and selected for discussion 37 articles satisfying the meta-analysis source selection criteria of Bernard, Borokhovski, Schmid, Tamim, and Abrami (2014): that is, the studies had to be peer-reviewed, focussed on DE and higher/adult education, with a systematic approach, and with a criterion for the selection of primary sources. Notwithstanding this academic rigour, the Siemens et al. report has been criticized by Downes (2015a) as 'a really bad study' (¶ 2) focussing on 'a small spectrum of academic literature far removed from actual practice' (¶ 2). Once accused of not being academic enough (Lange, 2011–2013), Siemens, Downes' former colleague in promoting the MOOC concept, is now accused of embracing academic ways in order to make 'a name for himself in the academic community' (Downes, 2015b; ¶ 3). Rancorousness of this kind is unhelpful in such discussions and, to Siemens' credit, his response (2015) has served to reduce it. As Van Valkenburg (2015) has indicated, the exchange has drawn attention to the fact that many current MOOC researchers 'are (not completely) aware of the history of online education from before 2012' (¶ 3).

Shaky foundations

What to believe—that there has been very little good research into online learning? that there has been no research into the best practices of online learning? that there is an extensive body of such research? that there were no worthwhile online learning studies before 2002 or 2006? and that academic studies of online learning cannot be both rigorous and useful in practice? These assertions may sound improbable and even tongue-in-cheek, but they are made in all seriousness by current well-known writers in relation to the three reports discussed above. One thing the reports have in common is that they are all in-house publications or blog postings. Siemens has been criticized before for his dependence on unreviewed blogs in promoting his *connectivism* notion (Lange, 2011–2013)—although Lange's comments were in turn trivialized as a mere personal attack (Mackness, 2011). But the advantage of using traditional academic outlets for one's views is that, through peer-reviewing, confusions and contradictions that they may contain can be resolved before they see the light of day. Neither of the GLC's opposing conclusions about online learning would have been likely to appear in a peer-reviewed DE journal, for neither was based on evidence.

The GLC's 2016 report sidestepped this problem by stressing that much research has indeed been done but not applied. To rectify this, the report proposes that a 'supporting social-technical infrastructure' (p. 1) should now be established in educational institutions, enabling a 'conducive culture' (p. 1) in which technology-enhanced learning (TEL) can thrive: that is, an environment in which TEL recommendations will actually be read. But once again, the report makes no reference to the kinds of structure urged by scholars and institutional service units over the years as means to overcome institutional 'structures of indifference' to TEL (Spratt, 2008); and in neglecting to discuss precedents, the GLC report fails to explain how its own proposed infrastructure is likely to succeed where others have not. Are the GLC's members actually more aware of the learning sciences literature than they appear? As they are drawn from pure science, computing science, business and law backgrounds rather than from education or social science backgrounds, and as no citation in their 2016 paper predates 2004, the answer is probably not.

As with the GLC report, the MIT report recommends increased institutional awareness of TEL through interdisciplinary collaboration and the creation of 'thinking communities' (p. x) to champion educational innovation. The main agents in this transformation process, the report recommends, should be 'learning engineers' (p. x). The terms *learning engineering* and *learning design* have recently become popular, says the report, although its definition of these terms is limited to a single reference to the work of the American scholar Herbert Simon (1967, p. 25). The learning engineer's responsibilities, stated Simon, should be to work with faculty members in designing learning experiences:

> Concrete demonstrations of increased learning effectiveness, on however small a scale initially, will be the most powerful means of persuading a faculty that a professional approach to their students' learning can be an exciting and challenging part of their lives. (p. 77)

In the absence of detail about developments of Simon's idea since he offered it 50 years ago, the MIT report leaves its reader to infer that its first implementation will be at MIT. Those who fill the new learning engineer positions will be:

> passionate about education, and must be aware of the latest research from at least several of the numerous fields of learning science. They must be *simpatico* with learners and have good instincts for teaching. They must be prepared to work with teachers, administrators, and students. They must be … (p. 26)

and the list goes on, covering all the skills and virtues seen in the job descriptions of educational technologists, instructional designers and educational computing specialists throughout DE's history. The report's decision not to explain the learning engineering and learning design terms in detail is acknowledged in a footnote and suggested as an item for future research:

> While we do not explore the implications underlying these different phrasings here, as at MIT we view design as one of the many capabilities of a well-trained engineer, the differences in underlying assumptions between these two labels are worth exploring. (p. 39, footnote 243)

Responsibility for making these definitions will presumably fall to the learning engineer, consistent with his or her duty to read the literature for which colleagues (e.g., the report's authors) evidently have no access or time owing to their work in other disciplines. For this purpose, the learning engineer could start with the extensive literature influenced by the knowledge engineering concept, including educational technology, instructional design, and the *Journal of Data and Knowledge Engineering* launched in 1985. To justify the originality of its learning engineering principles, MIT's learning engineers should also explain how they differ from those of cybernetics (Wiener, 1954) and epistemics (Goldman, 1978; Longuet-Higgins, 1977). The learning engineer's literature studies should then go on to examine the updated review of institutional DE quality assurance measures by Latchem (2016).

The MIT report's lack of awareness of the relevant literature is further illustrated by errors in analysis and discussion. Fewer than half of its citations on a central topic of the report, educational technology (pp. 13–15), were published before 2006, and the report is explicit in explaining why. Efficient online learning, it suggests, began at that time with the launch of the Khan Academy in 2006 (Wikipedia, 2016) and with the MOOC of Siemens and Downes (2008). Previous online learning literature thus appears to be of no consequence: 'MOOCs bring new advances in scale and the degree of interactivity, sophistication, and personalization to distance learning' (p. 2). This conclusion overlooks the fact that many MOOCs have been criticized for having the opposite qualities: a lack of efficient interactivity (LaBonte, 2012), a loss of personal teacher-student contact (Mackness, Mak, & Williams, 2010), and the use of primitive methods such as the grading of students by each other (Rees, 2013). The MIT's neglect of the DE literature in its work has been discussed in detail by scholars including Bates (2013), Daniel (2013), Naidu (2013), and Romiszowski (2013)—literature summarized by Baggaley (2014a). But in its 2016 report, MIT continues to ignore these criticisms, and limits its references to negative attitudes about MOOCs to those in a single newspaper article (Hartnett, 2013) concerning objections to the approach at San Jose State University. The main justification for MOOCs, as far as the MIT report seems to be concerned, is that 'students have voted with their feet' about them, and have enrolled in them in their millions (p. 2). The common 85–95% MOOC dropout rates (Jordan, 2015) is not mentioned. The report also incorrectly attributes the coining of the MOOC acronym to Athabasca University and Canada's National Research Council (p. 14) rather than to a University of Prince Edward Island writer (Cormier, 2008), and the invention of the *flipped classroom* term is ascribed to Khan (p. 6) despite its use at least 6 years earlier (Baker, 2000).

The MIT report does give special attention, however, to the role of aeronautics perspectives in educational development (p. 7, 25). Unsurprisingly, two of the four lead researchers in the project, including the first author, are aeronautics and astronautics specialists.

Building sandcastles

What makes scholars in one discipline feel entitled to assess the contributions of another discipline without apparently knowing anything about it, and in the process to ignore the criticisms of approaches that they endorse (e.g., MOOCs)? In part, this selective mentality can be due to the pressures of an institutional party line, and it is certainly not exclusive to MIT. MIT's selective attention to criticisms of MOOCs, however, may at least in part be explained by its commitment to creating them. To date, MIT has run over 90 courses involving the platform of the MOOC provider edX since co-founding that company in 2012 (p. 14). The usurping of another discipline's ideas may also be due to the age-old wish of academic careerists to build a castle in the literature as a personal legacy. A common defence for relabeling pre-existing ideas is that it is not plagiarism but a strategy for focussing new attention on them. Thayer (2016) cites this justification, with a certain degree of rhetorical exaggeration, in defence of recent *flipped learning* accounts:

> [Flipping] is changing the discourse about education. I would call it 'reframing', a well known and effective strategy for producing change. And, the fact is that flipping is generating excitement about innovating in education with technology the likes of which we haven't seen since the advent of the writing slate.

Stimulating new interest by reframing others' ideas is fine, but not without attribution.

It is also tempting to suppose that academics overlook previous literature because their knowledge of it ended when they ceased to be formal students of the discipline. This would certainly explain why the current memory for previous scholarship in the literature is so short. To accept this hypothesis, one would have to believe that academic colleagues maintain a collective fiction that they are up-to-date with the more recent literature. That and that corollary would not be easy to test, for asking colleagues about their academic reading would not necessarily yield reliable data. An easier test would be to correlate the dates of writers' literature citations with those of their formal training. The online biographies of writers and editors responsible for the reports reviewed in this article, for example, indicate that relatively few of them received any formal training in relation to online learning, on which basis the problems in their reports are not surprising. Analyzing the sources that writers cite can also reveal the breadth of their awareness of publications on particular topics and from specific countries. The MIT report, for instance, is top-heavy with references to American sources, and further content analysis would reveal whether or not the coverage of DE by journals specifying Australia, Britain, Canada, China, Turkey, and other regions in their titles, has led to a range of different national perspectives in the field.

In addition, confusion about the origin of ideas can result from poor editing and reference formatting in the literature. An interesting example is the manner in which writers and editors commonly mishandle the work of the Russian theorist Vygotsky. Vygotsky's work on social learning theory has been widely cited in the general educational literature if not frequently by DE writers (Gredler & Shields, 2004; Naidu, 2015); and the Russian original of his book *Mind in Society* was reissued in four editions. In the 1960s, students were lectured about Vygotsky (1930), after which references to Vygotsky (1978) became common. In the three reports reviewed in this article, Vygotsky is mentioned once in the MIT report though without a literature reference (p. 6), and twice by Siemens et al. (2015, pp. 16, 184) in connection with *Mind in Society*. One of the latter citations refers to the book as the first edition (1980) and the other to a previous edition (*sic*) in 1978. Siemens (2006) has previously referred to

Vygotsky (1989) though provided no source for this, nor is one listed in the UNESCO bibliography of Vygotsky's work (Ivic, 1994).

But Vygotsky died in 1934, and has lived on in the literature like an old relative who the family pretends is still alive in order to collect his social insurance. References to the Cole et al. edition of *Mind in Society* (1978) rarely cite its original publication date (1930–1934), and the edition itself includes references to work conducted decades after Vygotsky's death, giving the erroneous impression that he was posthumously influenced by celebrated scholars who were actually influenced by him. Draper (2013) has chided writers for flaws in their citation of Vygotsky's work, and for their carelessness in allowing new readers to assume that his ideas are the product of modern thinking rather than having been well developed 80 years ago. Let us call this confusion the 'Forgotsky Effect.'

In the DE literature, the opposite has occurred (U. Bernath, personal communication, July 24, 2016), with classic writings by DE scholars being cited rather than the authors' updated versions (Holmberg, 2003; Peters, 2010). This oversight fails to do justice to the authors' most recent thinking, although it is easier to forgive; for it can result not only from carelessness in citing the work but also from publishers' inadequate promotion of successive new editions as significant advances rather than mere reprints.

Hypotheses such as these would explain why memories of previous scholarship are not only short but are frequently reinvented by new writers, with or without a conscious intention to plagiarize. The online learning literature, for example, is currently revisiting questions that were examined in detail by educational television (ETV) researchers from the 1960s to 1980s. Following early evaluations of individual MOOCs (Fini, 2009), writers have proceeded to classify different kinds of MOOC (Morrison, 2013), and to distinguish between MOOCs aimed at different kinds of audience (DeBoer, Ho, Stump, & Breslow, 2014; Liyanagunawardena, Adams, & Williams, 2013). This progression of ideas is identical to the evolution of ETV research, which began with comparisons of TV and other media in the 1960s and 1970s, as reviewed by Schramm (1975), and evolved to the more elaborate aptitude-treatment-interaction studies of Cronbach and Snow (1977) and Salomon (1979). The ETV research tradition is not acknowledged in many modern DE writings. In this context, it is therefore refreshing and welcome to see one online learning writer make a full admission of the flaws in his previous work. Sams' ideas about flipped learning were criticized for their neglect of the previous literature by the current writer (Baggaley, 2015) in an article later cited in a review of one of Sams' books by Gwilym (2015). Whether or not influenced by the current writer, Sams (2016) has since made the following public statement:

> Much that I have done has been underinformed, underresearched, and underdeveloped. I have concluded that I have a lot more to learn if I expect others to continue to look to me for advice about how to operate their classroom. I am grossly deficient in my understanding of historical and current learning theories and practices, and it would be irresponsible of me to continue on my current trajectory. (¶ 2)

If today's generation of online learning researchers were to pay respect to the lessons of history as admirably as this, they could save time and money by cutting directly to the most salient questions of the day, rather than sweeping the old solutions aside in the kind of 'gale of creative destruction' described by Schumpeter (1942, p. 83). If today's researchers add solid foundations to their work, their competing claims may survive as useful landmarks, but when castles are built on sand a new tide comes in and washes them away.

Conclusions

Three recent reports about online learning have been examined. Two of the reports, from the GLC (2016) and the MIT (Willcox et al., 2016), contain numerous flaws and false premises, and ignore the theory and practice accumulated in distance education, educational technology and other disciplines over 50 years. These reports should be regarded as major embarrassments to the institutions that sponsored them. The third report, (Siemens et al., 2015), serves as a good model for more disciplined analysis and reporting. Sadly, the problems of the first two reports are symptomatic of those observed in modern society generally. This article has been written during the 2016 US presidential campaign, in which incorrect statements are being made almost daily, plagiarisms are denied, proved and still denied, and libellous insults are issued of the most extreme kind. Those responsible evade their critics by saying that they should desist and move on to other matters—a response that satisfies a large portion of the public that is ignorant of the facts or unwilling to hear them. Nichols (2014) argues that this malaise stems from a lack of social gatekeepers. There was a time, he points out, when participation in public debate 'required submission of a letter or an article, and that submission had to be written intelligently, pass editorial review, and stand with the author's name attached' (¶ 15); and the current commentary suggests that this is as much as a problem in current DE thinking as in politics. Nichols makes his point more directly still, recalling the Dunning-Kruger Effect (Kruger & Dunning, 1999) whereby individuals overestimate their expertise and develop a 'confidence of the dumb,' dismissing those who disagree with them and demanding proofs and literature evidence that they are not prepared to give for their own positions:

> We are witnessing the 'death of expertise': a Google-fueled, Wikipedia-based, blog-sodden collapse of any division between professionals and laymen, students and teachers, knowers and wonderers—in other words, between those of any achievement in an area and those with none at all. (¶ 4)

But the online learning statements reviewed in this article represent the thinking of prominent educators and are intended to influence future DE directions. As such they should not go unchallenged. They make one point that is absolutely true: that previous (unspecified) work has not been applied in practice. But their common solution—that faculty members and learning engineers' should now cooperate in 'thinking communities' to read and apply the previous work—has been applied many times before. Perhaps it would be more to the point to propose that institutional administrators—for example, the presidents and senior executives comprising the GLC—should also be charged with reading the learning sciences literature from now on, so that they can justify their decisions to accept or reject its recommendations rather than ignoring them with the 'confidence of the dumb.'

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Jon Baggaley is Emeritus professor at Athabasca University, Canada. He has taught at universities in England and Canada, and is the author of books including *Dynamics of Television* (with Steve Duck, 1976) and *Pyschology of the TV Image* (1980). The evolution of DE over the last century is examined in Baggaley's book *Harmonizing Global Education* (2012); and his 10-hour multimedia history of its research and evaluation methods is at: https://vimeopro.com/baggaley/home/

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