ephemera | articles
theory & politics in organization

ISSN 1473-2866
www.ephemeraweb.org
volume 8(4): 420-432

Commensurability, Rhetoric and Ephemera: Searching for Clarity in a Cloud of Critique

Bill McKelvey

Introduction

A favourite phrase of Hollywood movie moguls is 'Any news is good news'. For academics in the modern day, this translates as 'Any citation is a good citation'. Of course, I am honoured that Norman and Pippa have spent so much time trying to fathom the meaning of my paper – 'From Fields to Science…'. Being elevated to a fearsome authority figure is a status I never dreamed of achieving. Still, I don't really believe it and will try not to let it go to my head.

More to the point, Norman Jackson and Pippa Carter (JC), perhaps unwantingly, join with good science in noting that "models contain less information than that which they model" and that models are designed to serve the purpose of the model designer. The evolutionary epistemologist and scientific realist, Jane Azevedo, captured this idea very well in the title of her book, *Mapping Reality* (1997). Physicists such as Newton made great progress by simplifying all the degrees of freedom of planets (different size, less than perfect round shape, varying distributions of lava, earth, water, trees, animals, critical theorists of all variants, etc.) down to a 'point mass'. Darwin's framing of evolution was surely simpler than we now experience it. But they got the job at hand done. If Newton, Darwin, Adam Smith, and Milton Keynes had been influenced by the modern-day over-problematizing seen in critical theory in OS, we probably would never have heard of them.

In burying every term/word in a sea of seeming random meaning-attachment, JC have lost sight of one of their few clear notions: what is a good model? Nobel Laureate Murray Gell-Mann has been talking about 'effective complexity' for two decades (1988, 1994, 2002). 'Complexity' is often defined in terms of 'degrees of freedom'. Effective complexity, then, holds that for human schema development to offer functional advantage it can be neither too simple nor too complex but, as Goldilocks put it, 'just right'. In what follows I will try to re-capture the 'just right' degrees of freedom and essence of some of the ideas obscuritized in JC's ever-ephemeral meanings. I realize that 'just right' clarity in a journal called *ephemera*, could be oxymoronic. Sin by sin I try to clarify in what follows.

My Sins

1st Sin: Un-incommensurability

I can't imagine anyone beginning a paradigm-shift discussion without starting from Kuhn (1962). Yes, in 1962 he saw it focused on physics (what else), but since then his has become the most widely read philosophy of science book (Garfield 1987) and has affected thought in most disciplines. The idea that any reader in philosophy of science would begin a paradigm discussion with that of Burrell and Morgan (1979) is beyond imagination, even ephemeral. I focus on Ch. 2 – fields – because this is the only part of Kuhn's classic that hasn't been discredited by normal-science physicists and philosophers of science (see for example, Masterman, 1970; Kordig, 1973; Bishop, 1991; Weinberg, 1998; Ladyman, 2002). Unfortunately, its legacy of incommensurability stemming from paradigm shifts, morphed into relativism, remains undaunted in most social science disciplines except, perhaps, economics.

Technically, incommensurability is a term from mathematics meaning 'lack of common measure'. As the term spread out of math, its meaning broadened to 'meaning'. I have never seen any discussion in physics supporting the idea that physicists couldn't understand different measures, no matter how weird. Of course, logical positivists, using 'correspondence theory' tried to have exact meanings extending from observation terms (measures) to theory terms; but, needless to say, at this they failed (Suppe, 1977).

Again, consider the statement: 'All generalizations are false'. JC say "it is syntactically correct, but semantically false". How it can be semantically false is beyond me. It only includes four words and the meaning of each is clear. Despite saying it is semantically incorrect JC, in fact, appear to accept it; I have to be careful here because so many of their terms have so many meanings I frequently have no idea what they mean by any specific term (shades of Derrida and Foucault?).

So, now my statement: 'The incommensurability thesis is self-refuting'. In physics, to repeat my logic, if we know enough about the terms of one paradigm to say that they are incommensurable with the terms of another, then we must know enough about the terms to render their incommensurability false. JC say it would have been ok if I had used the term 'wrong' rather than 'false'. Talk about 'baffling'! Presumably saying my statement is 'wrong' implies that said differently it would be correct. Hum. So, which of the words should be changed to make the statement correct? Perhaps they would prefer this: If a physicist knows enough about two measures to say they are incommensurable, then he/she knows enough about the measures to equate them in some way, thus making them un-incommensurable; hence self-refuting. Admittedly, I am assuming that physicists aren't totally stupid.

If the above is not weird enough, JC also say (I am tempted to put 'says') "Incommensurability, if it exists, is a 'design feature' of the model". This is beyond comprehension or belief (their terms). 'Design' is, well, design ['to form or conceive in the mind; contrive; plan' (dictionary.com)]. Why would any scientist wanting to make a contribution, wanting to be cited, wanting to become famous (but not infamous), knowingly design (conceive, contrive, plan…) theories/models to be *not* understood?

Name one physicist that by conscious design created a model to be *not* understandable by other physicists? Perhaps by mistake a model doesn't make sense. Perhaps even after trying his/her best a model, as designed, is generally misunderstood. But incommensurability by design? Hard to accept. Were Newton's *Principia Mathematica* (1687) written to confuse by plan? Was Darwin's 1859 book contrived to confuse? Did Samuelson conceive of his 1947 book as consolidating mathematical approaches in economics written to fool wgi [Sic.] were obviously going to be math-trained economist readers? Did Dawkins design his 1976 'selfish-gene' book to be misunderstood by palaeontologists like Eldridge and Gould (Eldredge, 1995). If so, it didn't work; they and thousands of others understood it instantly.

JC note that the Burrell/Morgan 'model' "does not have a time dimension over which incommensurability can be resolved". I can't think of anything that would make their model more misrepresentative and useless. Yes, it is likely that the social construction of meaning pertaining to new terms in fast moving new fields in physics, biology, nanotechnology, dark matter, neurology, whatever, could for some period of time be beyond the understanding of some scientists and their students *outside* the new circle of insiders. But, in fact, physicists look at the day-to-day idea developments in physics and totally reject the idea of paradigm shifts and incommensurability; if you don't believe me, go talk to them! To pick a classic example, when Fleischman and Pons (1989) announced their claim to have discovered cold fusion, scientists around the world figured out their measures, tried to replicate their experiments, and quickly could say exactly which of the Fleishman and Pons measures failed. In this case, the would-be new paradigm died within days – but read on below.

But of course, physics and biology, and even psychology, are 'real' sciences grounded in realism, as opposed to whatever kinds of inquiry we see in UK-style 'organization studies'. If you disagree, go read physics journals and then go read articles in *Organization Studies* (like Jackson and Carter, 1991) – or *ephemera*. Scientists are, believe it or not, smart enough to change; they read, study, and learn, they build on new ideas instantly, especially in the current world of virtual publications. JC note that Kuhn treated paradigms as pathologies that will be cured over time. The primary different between him and most physicists is that he saw the pathology lasting a long time; physicists see it passing quickly.

I said earlier that most scholars outside of UK organization studies pay little attention to the Burrell/Morgan model. Now you can see why. Is there anyone who has read their book who doesn't understand each of their four cells? *In fact, if the Determinism and Voluntarism cells are incommensurable, how could Burrell and Morgan write about both of them? That all four cells are in one book written entirely by the same two authors, virtually by definition defeats their incommensurability.* Well, ok. I guess critical theorists have trouble since they still think incommensurability applies. Is this a case of feeble-minded thinking rather than 'understanding' or 'belief'? But give a better answer, if you can!

2nd Sin: Understanding vs. belief

The mystery deepens. JC say,

> we think McKelvey is saying something else: *if we understand, or comprehend, how another argument is different to ours…then we can resolve any apparent contradictions*. This suggests that McKelvey confuses and conflates comprehension and belief…. There is…no reason at all why we should not be able to understand something at the same time as not believing it. But for McKelvey, to understand is to believe.

In the part of the foregoing quote (in their italics) they somehow read into it the idea that I think resolving contradictions in understanding automatically leads to belief. Why would resolving misunderstandings and contradictions in, say, a religion (pick any) lead me to believe any of it. Absurd!! Apparently they think I am not only authoritarian but an idiot besides. For a person, like me, who claims to be a scientific realist and is well versed in this aspect of philosophy of science, understanding and belief are steps on the way toward developing truth claims having the highest probability of truth possible.

But, let's see. I *understand* all of the history, logic, and thinking behind the idea that the Earth is flat; but sad to say, I don't *believe* any of it. In this case, 'flat-earth logic' has been shown to be false; therefore I believe the Earth is round (more or less). I *understand* the logic and wishful thinking underlying creation science, Intelligent Design, and 'born-again Christians' (I grew up with all this stuff as a missionary-kid in India), but I don't *believe* any of it since I don't see any valid research supporting the 'faith'. JC might better have said: 'understanding is independent of believing'. But it appears that their *belief* in the logic of critical theory obscures their *understanding* of anything, certainly anything pertaining to scientific realism and science in general.

The Burrell/Morgan model is the premier paradigm model? Are you kidding? Is this a case of *believing* without *understanding*? Do JC believe in incommensurability without comprehending it? JC say that "incommensurability is an issue of belief, not one of truth". What happened to understanding? Well, yes, I agree that people can 'believe' in incommensurability without asking whether it is 'true' or not. No problem here. People, even academics and especially critical theorists can 'believe' anything; truth be damned. There is no rule against believing without understanding, believing what is false, even believing when all of the facts appear to line up against the belief. We still have people believing in God and we still have the Flat-Earth Society. Some people will believe anything; people in the UK apparently still believe monarchies are still relevant…. Is this a truth? I am not in a position to say since I am from the New World – where we long ago rejected such nonsense.

3rd Sin: Campbellian Realism

Since JC show the epistemology-ontology matrix in their paper, I don't show it here. In referring to Cell 2, I said that "no one supports a position based on a realist ontology and a relativist epistemology". JC suggest that "the New Physics…not to mention Critical Realism, Critical theory, Freudian Psychoanalysis, amongst others that could be argued to fit into this cell". Yes, I agree, '*no one*' is an overstatement. There is always some nut who will take some strange position. Thus, someone could 'argue' (and some still do) that the Earth is flat. But most real scientists probably wouldn't. Even 'critical realists' who appear to be the Brit version of scientific realists (I realise I may be being charitable here), would not buy into the idea of wanting, first, to adhere to the challenges of 'scientific realism' and then to let go and accept 'anything goes' (Feyerabend, 1975) as bases for accepting what philosophers would like to think of as science-based 'truth claims'.

I mention Scientific Realism just above. Logical positivism and logical empiricism have, for philosophers of science at least, been replaced by scientific realism (Bhaskar 1975; Suppe, 1977; 1989; De Regt, 1994), which focuses on searching out 'underlying causal mechanisms'. Nothing offered by the contra-science folks would be acceptable as epistemology, much as they might hope. Water does not flow uphill. More precisely, no 'resident' of Cells 1 and 4 would support Cell 2 as offering any positive hope toward improved 'truth'; by definition no Cell 2 resident could satisfy the standards of either Cell 1 or Cell 2. Frankly, I don't see how Cell 2 can be inhabited by knowledgeable researchers.

JC mention Donald Campbell in a footnote as fitting Cell 2. I have Campbell as one of the leaders for developments in Cell 3 – what I term 'Campbellian Realism' elsewhere (McKelvey 1999). Why? Indeed, Campbell: (1) accepted the reality of individual interpretations of the phenomenal world; and then (2) accepted the reality of social construction toward a common belief in a truth claim within a social group (a scientific community in this case) – which looks like relativism and contra-science; and (3) since 1965 has accepted the evolutionary epistemological view of scientists' progression toward Popper's 'verisimilitude', i.e., his "truthlikeness" (Radnitzky and Bartley, 1987; Hahlweg and Hooker, 1989), which we now see as progression toward realism's more probable "probabilistic truth claim" (Campbell, 1965; 1974). JC's placement of Campbell into Cell 2 is ephemeral logical for sure. As Campbell put it:

> …[T]he *goal of objectivity* in science is a noble one, and dearly to be cherished. It is in true worship of this goal that we remind ourselves that our current views of reality are partial and imperfect. We recoil at a view of science which recommends we give up the search for ultimate truth and settle for practical computational recipes making no pretense at truly describing [and explaining] a real world. Thus our sentiment is to reject pragmatism, utilitarian nominalism, utilitarian subjectivism, utilitarian conventionalism, or instrumentalism, in favor of a critical hypothetical realism. (1974, p. 447; his italics)

4th Sin: Putting science to good use, heaven forbid

In the U.S., most of the people creating the organization theory/science literature are employed in business schools; of those JC mention in their paper, Pfeffer is at the Stanford b-school and Van Maanen is at the MIT b-school; I also work in a b-school. In England, perhaps critical theorists in OS don't actually work in b-schools; perhaps they work in London where people make money or in The North where they make lots of grass, cows, and sheep. But surely, some critical theory types must work in b-schools since they are doing organization studies research – well sort of research; like critical theory style research.

To beat a dead horse, there is increasing concern in America, and I think in England, about the seeming irrelevance of organizational and management research as promulgated in the journals and by members of the Academy of Management; this applies to both quantitative and qualitative research. Builders of tall buildings, ships, bridges, airplanes, and the Internet, etc., are users of engineering-school research. Doctors with sick or dying patients are users of medical research done in medical schools and as drawn in from disciplines in biology. B-schools are usually characterized as professional schools training MBAs who then become practicing managers. A growing number of b-school academics do *not* think Pfeffer, Van Maanen, and the many others I can cite (see for example McKelvey 2006) are off the track in wanting b-school research to be relevant to practicing managers. And yes, then, this includes owners, CEOs…employees and other constituents who worry or suffer if firms don't make economic rents (i.e., above industry-average profits). In any field, that profession schools (should) worry about relevant research is hardly at odds with the objectives of basic research in disciplines outside b-schools. Only critical theorists, apparently, would think otherwise.

Yes, it is true that Einstein wrote President Roosevelt a note saying theories of nuclear physics contained the makings of an atomic bomb. But, he obviously was not thinking bombs when he wrote his 1905 paper outlining relativity theory – which as I note in a footnote was *not* incommensurable with the Lorentz transformation equations (McKelvey, 2003, even though it was too much of a seeming paradigm shift to the Nobel committee to award him the Nobel Prize for it in 1921. True, then, 'Basic Science' hasn't sold its soul to generate ideas for practical benefit; but researchers in professional schools in some sense, are under pressure to do so. And of course, herein lies the relevancy problem; b-school researchers do get promoted for statistical significance, and findings mostly irrelevant to practitioners.

Given 'Agency Theory' (Jensen and Meckling, 1976), we now have CEOs with stock options making $millions; and by cleverly pre-dating their options contract (i.e., cheating) they can sell out and make $millions even as their firm goes bankrupt (Countrywide Financial and Lehman Bros. being good recent examples). Does my concern – and the concerns of all others worried about research irrelevant to practitioners – about 'relevant' research mean we all are doing so to make CEOs wealthy? Should we do the opposite – so as to keep CEOs poor – by publicizing research findings we 'believe' (understand? think?) will make firms perform poorly, thereby dashing the hopes of lower-level workers, families, and communities dependent on firms for employment and taxes, etc., while we try to flat-line stock options so CEOs don't make big bucks? If you critical theorists want to live in the Land of Illogic yourself, fine; but please try not to impute it to others. Most economies, even the UK, depend on firms offering employment, producing products, and paying taxes. Of course, CEOs make more money than others. But to say that my focus on researching so as to learn how to help firms work better is only to make CEOs richer is stupid, to say the least. In fact, I don't even consult! I am a 'left-bank intellectual'; I despise the rich! It goes back to the 'mish-kid' beginning.

5th Sin: Reflexivity

JC quote me as saying, "I do not disagree that reflexivity is present in organizations; I am just not sure in counts for very much". Of course, in principle, reflexivity exists.

Researchers well versed in research methods in b-schools have long been aware of Chia's concern that "…the researcher/theorist plays an active role in constructing the very reality he/she is attempting to investigate" (quote by JC). This concern lies at the basis of research training for any PhD student in 'good' b-schools (yes, there are always places that don't know any better). Since at least the 1950s good researchers have all worried about the questionnaire design that, once given to respondents, sensitizes them into thinking and seeing in ways they had never imagined. Even more so, good academics worry about interview protocols, case-study designs, and other researcher influences on the members of an organization that might inadvertently create behaviours that weren't present before the researcher entered the organization (see Yin 1989 for example).

Does reflexivity still exist in research here and there? No doubt. Does reflexivity "…render groundless and undecideable…" all findings about organizations? Hardly. The assumption set of modern statistical practices are surely much more confounding, meaningless, and worrisome as to impact. Of course, this is more of a problem in top American journals where quant. studies take up 80%+ of journal space. The journal, *Organization Studies*, gives some 20% of its space to quant. research. Perhaps Chia is mostly aiming his pointed remark at UK organization studies researchers. But even here, UK researchers worry constantly about case study research approaches and they are not mindless about reflexivity. I repeat, reflexivity concerns are well down the list of much needed researcher concerns. But, of course, reflexivity will never go away and could easily move up the list absent constant wariness.

6th Sin: Latour, Burrell, Pasteur, Nazism

JC quotes (whoops) quote me saying:

> The antiscience group is prone to make accusations such as Burrell's (1996: 656) assertion that modernist science (epitomized by Einstein the Zionist who was invited to be the President of Israel) caused the holocaust of 6 million Jews….

Then they quote Burrell (1996: 656)

> [In the 1960s] few saw the defining organizational form of the whole twentieth century to be the death camps of Auschwitz. Modern*ism* is about the death camps in a fairly uncontentious way even though its apologists seek to distance the likes of Auschwitz from the achievements of the modernist society…. (JC's italics).

Boiled down, I said: 'Burrell's…assertion that modernist science caused the holocaust…'. Boiled down, Burrell said: 'Modernism…its apologists seek to distance the likes of Auschwitz from the achievements of modernist society'. I used the term 'caused'; Burrell used the term 'achievements'. Let's see: if Auschwitz is a result of the achievements of modernism (which leads to Burrell's 'distancing' idea), then it must be party to whatever caused the achievements, which was modernism. Presuming that achievements don't happen without causes, then it appears that I captured what Burrell said rather more clearly than how he said it in the first place. Case closed.

7th Sin: Equating variety with diversity

Let's start with the origin of Ashby's Law of Requisite Variety. In his classic work, *An Introduction to Cybernetics*, in defining variety, Ross Ashby (1956: 124–25) pointed to the following series: '*c, b, c, a, c, c, a, b, c, b, b, a*'. He observed that *a*, *b*, and *c* repeat, meaning that there are only three '*distinct* elements' (his term) – three kinds of variety or three degrees of freedom. He also notes that order (organization) exists between two entities, *A* and *B*, only if the link is 'conditioned' by a third entity, *C* (Ashby, 1962: 255). If we take *C* as constituting an 'environment', external to *A* and *B*, then *C* can be taken as a source of order-generating constraints that helps to organize the relation between *A* and *B* (Ashby, 1956). The influence of such external constraints, gives rise to his famous *Law of Requisite Variety*, which states that "ONLY VARIETY CAN DESTROY VARIETY" (p. 207; his capitals). It holds that for a biological or social entity to be efficaciously adaptive, the variety of its internal order must match the variety imposed by environmental constraints.

Our 'updating' of Ashby's Law began with McKelvey and Boisot (2003) and then McKelvey and Boisot (2007; appeared in the Acad. Mgmt. *Best Paper Proceedings*) and most recently in McKelvey and Boisot (2009). We simply take Ashby's 'variety' ('*c, b, c, a, c, c, a, b, c, b, b, a*') and then refer to his 'distinct elements' as degrees of freedom – which are one of the typical ways of defining complexity: the more degrees of freedom, the more complex (Gell-Mann, 1994, among others). I/we don't take any particular pride in our 'modernization' or 'updating'; we are simply using Ashby's Law to offer one definition of Gell-Mann's 'effective complexity'; to wit: internal complexity is effective if it destroys external complexity. I/we are just stating the obvious: nothing more, nothing less. Thus:

> Only variety can destroy variety
>
> Only degrees of freedom can destroy degrees of freedom
>
> Only internal complexity can destroy external complexity

Following Gell-Mann, I take degrees of freedom to be the most basic phenomenological manifestation of complexity. Since Ashby was writing about 'self-organization' as far back as 1947 (first time the term was used in print), and since self-organization is a key element of the Santa Fe Institute's vision of order creation (Holland, 1988; 1995; Kauffman, 1988; 1993), our updating is surely in line with Ashby's thinking. JC quote Stafford Beer as saying, "the measure of complexity is VARIETY". Great quote! Thank you! This quote of Beer helps cement the connection between variety, complexity and degrees of freedom.

As for Allen's (2001) 'Law of Excess Diversity', JC's issue with diversity vs. variety is a tempest in a very small teapot, indeed. We now have three key terms in text: variety, distinct, and diversity. Quick definitions from 'dictionary.com' are:

> *Distinct*: "not identical; unmistakable".
>
> *Variety*: "the state of being varied or diversified".

Diversity: "the state or fact of being diverse, difference, unlikeness, variety".

Allen's point is simply that, given all of the external degrees of freedom (which, even worse, are frequently changing their nature), and given that an organization doesn't know in advance which external degrees of variety are going to cause trouble, it needs to create more internal degrees of freedom in advance than appears obvious at any given time. Whether Allen's concern is written down as variety, diversity, or degrees of freedom is irrelevant. But of course, critical theorists make their contribution by seeing shadows behind every rock; whoops, I mean every tiny stone….

Finally: come on now, JC, enter the modern world. OS could very well be the last discipline not to want to get works out and visible on the Internet as soon as possible. Physics and Biology have had highly used electronic journals for years. Most academics present their newest ideas at conferences; ideas that then appear in conference proceedings or as chapters in books. Many, if not most, initial ideas leading to Nobel Prizes in economics, for example, do not appear in the so-called 'A' journals; just to pick one, Robert Lucas's rational expectations idea first appeared in the 4th volume of a new journal. Since I mention Jenson and Meckling's very influential article on agency theory, note that it appeared in the 3rd volume of a new journal. Einstein's 1905 paper introducing relativity theory was published by his friend, Max Planck, without refereeing. Besides, what studious risk-averse referee would have accepted something as silly is wobbling time?

Oh yes, you complain about my "modernization of Ashby's Law passing…into the public channel without…peer assessment". Maybe this is my *10th sin*. But, let's see. Did we pay money to the Internet to have the 'updating' idea appear in electronic papers? *No*. Did we pay you to cite it and even discuss it in your paper? *No*. Did *ephemera* referees (well ok, reviewers) reject your paper because you cited an un-refereed virtual paper? *No*. Some people cite papers they like and some cite papers they don't like. Hence my opening line: 'Any citation is a good citation'. Think how many cites and how much fame the cold fusion guys, Fleischmann and Pons (1989), got for telling everyone about their mistake (Huizenga, 1993; Goodstein, 2002, U.S. Department of Energy, 2004; Plotkin 2005)!

Frankly, I don't waste my time looking at 'print' journals any more. I click key words into Google and then read papers that appear – hard copy or as yet only virtual. Do you really think I think I need some unknown, risk-averse referee to tell me whether or not to pay attention to the content of some paper? Hardly! Yes, I am very thankful to the one referee in my 40-year career who made five comments on a paper, one of which doubled the quality of our empirical results. I have had one other paper in which the referee improved our theory. Most of my papers, in my view, were better before the refereeing. So, besides finding new papers quickly, the best thing about Google is that one can avoid the referee process. What a concept!!! Your complaint sucks you back to the age of clanking keys in typewriters.

8th Sin: Keeping Saussure alive

This is a sin? Would that mean someone would sin by keeping me alive when that time arrives? I hate to tell you, but Cilliers cites Saussure's 1974 book, not his dead body.

Here is what I said about Cilliers' drawing on poststructuralism in my 2003 chapter titled, 'Postmodernism vs. Truth in Management Theory' (how could you miss this one?) Cilliers (p. 6) first sets out ten attributes of complex adaptive systems, and then makes his foundational argument as follows (p. 37)

> Complexity is best characterised as arising through large-scale, nonlinear interaction. Since it is based on a system of relationships, the post-structural inquiry into the nature of language helps us to theorise about the dynamics of the interaction in complex systems. In other words, the dynamics that generate meaning in language can be used to describe the dynamics of complex systems in general. Connectionist networks share the characteristics of complex systems, including those aspects described by a post-structural theory of language. It should therefore be possible to use them (or other distributed modelling techniques with similar capabilities) as general models for complex systems. These models can be physically implemented or simulated computationally.

These three points link the *poststructuralist responsible core* of postmodernism and complexity science together by virtue of their common focus on connectionism. Why 'responsible core'? Because Cilliers focuses on connectionist networks: these are real, have underlying generative causes, and are amenable to realist inquiry. But yes, one may have to go though the Campbellian Realist sequence of idiosyncratic individual perception and social construction to get past the eye of the beholder to discover the actual working network connections. I do admit to one source of possible confusion: While Cilliers and I draw on Saussure as a poststructuralist, it is possible that I presumed that more postmodernism seeped his 1974 book, based on his works but written up long after he died, than is really the case.

9th Sin: Faith in Cilliers

Paul Cilliers says (personal communication) that his original title was 'Complexity and Poststructuralism' but that the publisher thought it would sell better if 'postmodernism' was in the title rather than 'poststructuralism'. Given this, the book as written, and its use of works by Saussure, Derrida, Baudrillard and Lyotard, reflects Cilliers working from the poststructuralist literature rather than the postmodernist one. But this distinction is not terribly important here since the argument is about who supplies the underlying support for reframing organizational research into Cell 3 rather than leaving it fighting between Cells 1 and 4. Since I focus on 'contra-science' ontology to make the case, both poststructuralism and postmodernism are included. Since I also draw on Campbellian Realism and complexity science in making the argument for focusing on Cell 3, Cilliers' book is a key step in the storyline; it is he who makes the 10-point connection between poststructuralism and complexity science.

Conclusion

In principle the role played by critical theorists is important. There are a number of early postmodernist studies about what actually happens in research laboratories and how much lab power plays, social collusions, and misunderstandings contribute to what are the basic 'facts' being written up in ways that obscure and seem to create ephemeral facts rather than tell a clear story about what really happened. These studies are especially valuable because they are able to connect down to true facts, as opposed to stories created by variously motivated researchers – especially doctoral students, who are well known to produce the facts the professors want to see rather than facts really there.

On the other hand, it appears that some critical theorists could very well be doing more to confuse than clarify; more to create the incommensurability pathology than reduce it; more to obscure than clarify. Talk about reflexivity: critical theorists can easily or wilfully create wishful confusion by obscurantist attachment of weird meanings to otherwise well understood words. Whereas doctoral students may create nonexistent facts to get their professor a publication, critical theorists could easily be creating wishful incommensurability to get a publication. No wonder this journal is titled *Ephemera*.

Still, as I note at the outset, any citation is a good citation. I very much appreciate the opportunity to be forced to rethink and hopefully clarify 5-year old arguments that seemingly are open to misinterpretation. I enjoyed the style of JC's article. It was fun to read!

references

(I only include those not already in Jackson and Carter's article)

Allen, P. M. (2001) 'A Complex Systems Approach to Learning, Adaptive Networks', *International Journal of Innovation Management*, 5: 149-180.

Ashby, W. R. (1947) 'Principles of the Self-organizing Dynamic System', *Journal of General Psychology*, 37: 125-128.

Ashby, W. R. (1956) *An Introduction to Cybernetics*. London: Chapman and Hall.

Ashby, W. R. (1962) 'Principles of the Self-organizing System', in H. Von Foerster and G. W. Zopf, Jr. (eds.), *Principles of Self-Organization: Transactions of the University of Illinois Symposium*. London: Pergamon Press.

Azevedo, J. (1997) Mapping Reality: An Evolutionary Realist Methodology for the Natural and Social Sciences, Albany, NY: State University of New York Press.

Beer, S. (1979) *The Heart of Enterprise*. Chichester: Wiley.

Bhaskar, R. (1975) *A Realist Theory of Science*. London: Leeds Books.

Bishop, M. A. (1991) 'Why the Semantic Incommensurability Thesis is Self-defeating', *Philosophical Studies*, 63(3): 343-356.

Campbell, D. T. (1965) 'Variation and Selective Retention in Sociocultural Evolution', in H. R. Barringer, G. B. Blanksten and R. W. Mack (eds), *Social Change in Developing Areas: A Reinterpretation of Evolutionary Theory*. Cambridge, MA: Schenkman.

Campbell, D. T. (1974) 'Evolutionary Epistemology', in P. A. Schilpp (ed.), *The Philosophy of Karl Popper*, *Vol. 1*, La Salle, IL: Open Court.

Darwin, C. (1859/1964) *On the Origin of Species by Means of Natural Selection*. London: Murray. [A Facsimile of the 1st ed. with an Introduction by E. Mayr (Cambridge, MA: Harvard University Press)]

Dawkins, R. (1976) *The Selfish Gene*. Oxford, UK: Oxford University Press.

De Regt, C. D. G. (1994) *Representing the World by Scientific Theories: The Case for Scientific Realism*. Tilburg: Tilburg University Press.

Eldredge, N. (1995) *Reinventing Darwin*. New York: Wiley.

Feyerabend, P. K. (1975) *Against Method*. Thetford: Lowe and Brydone.

Fleischmann, M. and S. Pons (1989) 'Electrochemically Induced Nuclear Fusion of Deuterium', *Journal of Electroanalytical Chemistry*, 261(2A): 301-308.

Garfield, E. (1987) 'A Different Sort of Great Books List: The 50 Twentieth-century Works Most Cited in the Arts & Humanities Citation Index, 1976-1983', *Current Contents*, 16(20): 3-7.

Gell-Mann, M. (1988) 'The Concept of the Institute', in D. Pines (ed), *Emerging Synthesis in Science*, Boston, MA: Addison-Wesley.

Gell-Mann, M. (1994) *The Quark and the Jaguar*. New York: Freeman.

Gell-Mann, M. (2002) 'What is complexity?', in A. Q. Curzio and M. Fortis (eds.), *Complexity and Industrial Clusters: Dynamics and Models in Theory and Practice*. Heidelberg: Physica-Verlag.

Goodstein, D. (2002) 'Whatever Happened to Cold Fusion?', California Institute of Technology [http://209.85.135.104/search?q=cache:9Ro2hRZG_CEJ:www.its.caltech.edu/~dg/fusion_art.html+%22cold+fusion%22+pons&hl=en&ct=clnk&cd=10&client=safari]

Hahlweg, K., and C. A. Hooker (eds.) (1989) *Issues in Evolutionary Epistemology*. New York: State University of New York.

Holland, J. H. (1988) 'The Global Economy as an Adaptive Process', in P. Anderson, K. J. Arrow and D. Pines (eds.), *The Economy as an Evolving Complex System*. Reading, MA: Addison-Wesley.

Holland, J. H. (1995) *Hidden Order*. Cambridge, MA: Perseus Books.

Huizenga, J. R. (1993) *Cold Fusion: The Scientific Fiasco of the Century*. Oxford, UK: Oxford University Press.

Jensen, M. C. and W. H. Meckling (1976) 'Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure', *Journal of Financial Economics*, 3(4): 305-360.

Kauffman, S. A. (1988) 'The Evolution of Economic Webs', in Anderson, P. et al., op cit: 125-146.

Kauffman, S. A. (1993) *The Origins of Order*. New York: Oxford University Press.

Kordig, C. R. (1973) 'Discussion: Observational Invariance', *Philosophy of Science*, 40: 558-569.

Ladyman, J. (2002) *Understanding Philosophy of Science.* London: Routledge.

Masterman, M. (1970) 'The Nature of a Paradigm', in I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.

McKelvey, B. (1999) 'Toward a Campbellian Realist Organization Science', in J. A. C. Baum and B. McKelvey (eds.), *Variations in Organization Science: In honor of Donald T. Campbell*. Thousand Oaks, CA: Sage.

McKelvey, B. (2003) 'Postmodernism vs. Truth in Management Theory', in E. Locke (ed.) *Postmodernism and Management,* Vol. 21. Amsterdam, NL: Elsevier Science.

McKelvey, B. & M. Boisot (2009) 'Redefining Strategic Foresight: "Fast" and "Far" Sight via Complexity Science', in L. Costanzo & B. MacKay (Eds.), *Handbook of Research on Strategy and Foresight*. Cheltenham: Edward Elgar.

Plotkin, H. (2005) 'Cold Fusion Rides Again: Science Magazine Publishes More Evidence of Tabletop Nuclear Reactions' [http: //www.sfgate.com/cgi-bin/article.cgi?file=/gate/archive/2002/03/25/bltpfusio n. DTL]

Radnitzky, G., and W. W. Bartley III (1987) *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge*. La Salle, IL: Open Court.

Samuelson, P. A. (1947) *Foundations of Economic Analysis.* Cambridge, MA: Harvard University Press.

Saussure, F. de (1974) *Course in General Linguistics*. London: Fontana/Collins.

Suppe, F. (1977) *The Structure of Scientific Theories* (2nd ed.). Chicago: University of Chicago Press.

Suppe, F. (1989) *The Semantic Conception of Theories & Scientific Realism.* Urbana-Champaign, IL: University of Illinois Press.

U.S. Department of Energy Press Release (2004) 'U.S. Department of Energy will review 15 years of "cold fusion" excess hear and nuclear evidence' [http: //www.infinite-energy.com/resources/pressreleasedoe.html]

Weinberg, S. (1998) *New York Review of Books*, XLV, 15 [http: //www.cs.utexas.edu/users/vl/notes/weinberg.html]

Yin, R. K. (1989) *Case Study Research: Design and Methods*. London: Sage.

the author

The fearsome Bill McKelvey: Having rejected God, physics and mathematical syntax along with meaningless memorizing early in my 20s, I have been searching for truth from science ever since. Semi-life in a business school, surrounded by economists, nearly-autistic males, intangible phenomena – and wanting to help people make organizations work better – has complicated the search. Philosophy of science, focusing mostly on physics and biology, opened windows of delusion and challenge. The search goes on. But I see promise in elevating fractals, power laws and Pareto-based science over the misguided over-emphasis of Gaussian statistics.