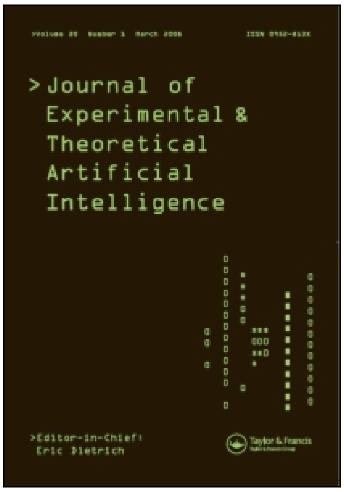

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COMMENTARY

Peirce's abduction and cognition as we know it

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1. Introduction

Charles Sanders Peirce explained the dilemma: direct experience is concrete, described experience is abstract. As a result, one cannot talk about the simplest damn thing: neither a spring flower in bloom, the sunshine that illuminates it, nor the child that delights in its beauty, except by performing abduction. To describe or to explain, to know or to believe is to abduct the complexity of the concrete, in the ordinary sense of kidnap. In its place are left practical and useful, but inevitably narrow presumptions and preferred abstractions.

Even children make sophisticated abductions. Theirs is a coherent Gestalt-like understanding of the world, even when particular coherent abductions grossly misrepresent the world (Kloos 2007). In all cases, immediate reality is distorted in abduction and cannot be ransomed. No investment of time, energy, material or vocabulary can be sufficient to reinstate concrete immediate reality. 'The truth is that the whole fabric of our knowledge is one matted felt of pure hypothesis confirmed and refined by induction. Not the smallest advance can be made in knowledge beyond the state of vacant staring, without making an abduction at every step' (Peirce quoted in Brent 1998, p. 72).

Peirce's insights motivated the original pragmatism, which spawned contemporary pragmatism and pluralism, although Peirce dissociated himself from such and renamed his own programme pragmaticism. How could it have ended otherwise? The philosophy itself recommends endless looping self-questioning and recapitulation, and insinuates subtle unspoken differences of opinion even as we concur. You will always in some way disagree with me, even as we shake hands in agreement or co-author a scientific paper. That we may always disagree is the fuel of creative and productive collaborations, the grist of the grinder for change and discovery.

The present collection of three articles, all espousing pluralism, seems to channel Peirce and his contemporaries in the fact that they disagree so profoundly on the actual basis for a pluralistic cognitive science. Edelman (this issue) offers to set the ontological cornerstone for a house of pluralism as he equates cognition with computation. Jilk, Lebiere, O'Reilly, and Anderson (this issue) celebrate a pragmatism grounded in common principles, which derive from simulations using production rules and neural nets, and by recognising that no single abstraction captures human activities in their entirety. Dale (this issue) argues for an integrative and transactional pluralism in the sense that one may need

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different, apparently inconsistent, intellectual tools or instruments for the different jobs at hand.

Dale's position is the most innovative, compared to most cognitive science, as it ends in a kind of metaphysical pluralism, if these two terms can stand together without oxymoron. I will explain why I disagree with Edelman's argument, why the otherwise positive suggestions of Jilk and co-workers, may not go far enough, and why Dale's most radical suggestion may already be our fate.

2. Edelman's ontology

I respect scientists who take strong positions, as does Edelman (this issue). You know where Edelman stands because he spells out his personal gamble so clearly, and his strong position allows arguments that have some hope of resolution, whether by falsification, synthesis or transcendence. Edelman's position does not sit easy with pluralism, however, insofar as the present article is representative. Even as he argues for a kind of pluralism, he asserts the truth of his basic assumption – that cognition equals computation – so pluralism itself must be had on these terms. At one time this assertion might have gone unquestioned but many cogent arguments now exist for why cognition cannot be equated with computation.

2.1. *Biological intelligence*

Freeman (2004) explains why computation gives the wrong account of biological intelligence for instance. As the present articles note, a key strength of computational models lies in their capacity to mimic syntax, regularities in the ordering of symbols. However, the crux of biological intelligence is meaning or semantics, and semantics does not reduce to syntax, a point of fact that Peirce also intuited. Thus, we may acknowledge the proven utility of computational modelling even as we accept the absolute failure of computation to articulate meaning. Moreover, because this failure originates in the key strength of the computer metaphor, it is considerably the wrong metaphor from which to constitute a theory of cognition. Put simply, computation is the wrong tool for the job of meaning.

As Freeman discovered, biological intelligence already equals meaning at the early periphery of the central nervous system. In olfactory cortex, for instance, biological intelligence is conveyed in coordinated travelling waves of neural activity and meaning is conveyed by the pattern of these travelling waves. Thus in olfaction, the dominant mode of perception in most animals, the meaning of an odour is expressed simultaneous with its perception.

For example, a rabbit can be conditioned to respond to the smell of banana oil. Subsequently, as it inhales banana oil, the rabbit will exhibit a travelling wave of neural activity across its olfactory bulb. The wave structure is a complex emergent pattern of amplitude-modulated activity. Neurons fire repeatedly at particular amplitudes, within a radially symmetric spatial-pattern of phase modulation, with fixed phase velocity in all directions. Significantly, the pattern entails the meaning of the banana oil to the rabbit. Consequently, when the conditioned stimulus *banana oil* serves as an unconditioned stimulus, as the same rabbit acquires another conditioned stimulus *sawdust*, the banana-oil pattern stays with sawdust.

Already, at the olfactory bulb, the peripheral outpost of sensory cortex, neural activity pertains to meaning. Neural activity conveys what the smell means behaviourally, and is not simply a representation of smell. Parallel findings have been corroborated for other senses as well (Freeman 2000), findings that serve this argument as widely established evidence that cognition is not simply computation. The meaningful dynamics of the central nervous system 'differ considerably from what we consciously and explicitly consider as mathematics' (von Neumann 1958, p. 81).

2.2. *Information theory*

Juarrero (1999) also explains why computation fails to give a successful account of meaningful intentional behaviour. The crux of the argument concerns whether a philosophical or scientific approach can successfully distinguish intentional from unintentional behaviour – a *wink* from a *blink*, in her terms. Leverage for her argument comes from a habit of thought that conflates efficient cause and algorithmic information flow. For example, any algorithmic theory can be rewritten as staged feedforward chains of inputs (causes) and outputs (effects), which conflate efficient cause and algorithmic flow.

But information theory is not about causes in this exclusive sense. It emphasises choices among probabilistic alternatives, collapsing sets of alternative outcomes, so-called symmetry breaking. The consequence when behaviour originates in cascades of choice points is insurmountable uncertainty. Each choice point conceals the impetus of the choice, for instance, whether the outcome was originally intentional (Juarerro 1999). Information (effects) cannot be tracked backward in time from behavioural outcomes to their origins in intentions or stimuli.

It is telling, in this case as well, that failure originates in yet another key strength of the computer metaphor. The digital level of a computer can be rendered in its entirety. One can write down a complete state description of the machine at the digital level. But no 'natural objects seem to be of this nature. The computer is really a physical instantiation of a model. We know a model can compute, but can it live or think?' (Bell 1999, p. 2015). Again, the metaphor of explicit computational states is simply the wrong metaphor for the job.

Juarrero (1999) goes on to speculate that actions originate in meaningful cognitive contents, and contents function as meaningful constraints. Temporary constraints reduce degrees of freedom for choice to insure that a choice is shaped towards the prescribed end; where the ends, in this sense, are intended meaningful behaviours. The emphasis on meaning over syntax, and constraints over causes, to understand intentional behaviour, picks up indirect support from psychology's repeated failures to distinguish intentional behaviour empirically.

There are not now, nor have there ever been, reliable empirical criteria to distinguish so-called controlled (the *wink*) from automatic (the *blink*) behaviour (Goldstein 1939; Fearing 1970; Tzelgov 1997; Prochazka, Wolpaw, Clarac, Loeb, and Rothwell 2000). Nonetheless, the distinction remains widely salient in the theory and practice of psychology. In line with Juarrero's (1999) argument, this perpetual mismatch between theoretical goals and intuitions, *versus* what one actually observes, comes from tightly laced habits of thought and straightjacket methods that seek exclusively efficient causes (Van Orden and Holden 2002).

2.3. *Coordination not computation*

The arguments of Freeman (2004) and Juarrero (1999) assume behaviour is meaningful and not simply the meaningless ordering of behavioural elements. Thus, the challenge to understand meaning cannot be ignored to favour simpler problems of syntax (Rosen 1991; Turvey 2004). We require alternative habits of thought to get past these and other arguments against cognition as computation. Thus, at this time in cognitive science, given known failings, who would now risk their short career on the computational gamble in which Edelman binds his thinking? Computational models are wonderful tools, they mimic much behaviour after the fact, and they are failed metaphors for cognition.

Fortunately, other conceptual tools are in play with which to understand cognitive activity, especially contemporary ideas about coordination and synergy (Haken 1983, 1984; Kelso 1995). In this way of thinking, I claim, the primary and original function of embodied, embedded language and cognition is to coordinate human activity at all scales, with each other, with other animals, and with the complex and changing contexts and environments that define the human niche (Abram 1996).

Indeed brains, bodies and people express synergetic coordinative structures across many (or all) spatial and temporal scales of activity. These include circadian rhythms that emerge from molecular interactions (Harrisingh and Nitabach 2008), the broadly coordinated rhythms of the brain (Buzsáki 2006) and cognitive performance (Gilden 2001), synergies among speech articulators (Kelso, Tuller, Vatikiotis-Bateson, and Fowler 1984) and semantic synchrony of conversation partners (Dale, this issue), coordinative structures among limbs (Kelso 1995) in posture (Balasubramaniam, Riley, and Turvey 2000) and otherwise across the body (Turvey 1990), entraining limb and body movements across individuals (Schmidt, Carello, and Turvey 1990), in conversations (Shockley, Santana, and Fowler 2003), and the coordinated joint actions of teams. Even a weekend with a novel can be construed as delayed coordinations, across time and space, between reader and writer and other readers.

In the cyclic coordination of living processes, at some point, the phenotype chooses 'what is read from the gene and what is done with it' (p. 2017); spending the weekend in a library, instead of procreation, will alter the pattern of gene expression accordingly, for example (Bell 1999). Living processes never live in isolation and coordinations among processes span levels and frustrate the functionalist agenda. The hardware is the software; the software is the hardware (Davia 2005). Consequently, causal entailments run up in scale and down in scale no matter the particular scale at which we enter the organism. In this world of complex possibilities, I would not risk the pleasure of work to sustain a simpler worldview that must either exclude or reduce the importance of these facts. Cognition does not equal computation.

3. Jilk and co-workers' pragmatism

Jilk et al. (this issue) describe gains made by juxtaposing alternative models of the same phenomena. They propose a complement to the typical competition between theories, idealised in critical experiments. In this pragmatic complement, different models may instantiate legitimate alternative descriptions.

Alternative descriptions are useful. They set different priorities and emphasise or ignore different aspects of phenomena. They may even differ about which parts of phenomena are left outside of an account, as in a theory of water behaviour that would over emphasise molecules only to fail to explain waves. Alternative descriptions underscore why, though it can be practical to act as though one's theories or beliefs are true, it is a mistake to forget the practical basis of so acting. Juxtaposed alternative models are one protection against this mistake.

For instance, the original ACT-R model assumed that the rational level of explanation was strictly separable from mechanistic levels of explanation (Anderson 1990). Rational in this sense refers to choosing the algorithm that optimises adaptation of behaviour (from the algorithms that are available and within the limits of biological wetware). ACT-R was inspired by Marr's (1982) functionalist proposal to divide theoretical labour among levels of analysis, as one would do when thinking of the behaviour of a computer. If cognition is computation, then the algorithmic software description, at the rational level of analysis, can be made separately from the mechanistic description of hardware, consistent with Edelman's (this issue) computational ontology, for example.

However, collaboration between the ACT-R and Leabra research teams revealed deep mathematical and conceptual parallels between emergent representations in Leabra's neural networks and subsymbolic mechanisms in ACT-R. The two kinds of architectures inform each other through similar, sometimes substitutable modular organisation and functionality. In the end, Jilk et al. (this issue) propose a synthesis of ACT-R and Leabra, nicknamed SAL, in which labours of control *versus* representation are divided between the two. Already their proposal foregrounds deep questions, that neither approach answers, such as the origin of symbols, while their integration makes progress on fronts as daunting as the symbol-grounding problem.

3.1. *Design principles*

While reading Jilk et al. (this issue), I was reminded of my collaboration with Greg Stone. Stone taught me a similar kind of pragmatism. His concern, at the time, was the almost exclusive practice of seeking binary oppositions between models and theories, the concern of Newell (1973) described in the introduction of Dale (this issue). Inspired by Peirce, Stone's goal was an explicit and productive theoretical method. For example, theoretical progress could be had by close attention to design principles, which express shared patterns among theories, models and characteristic empirical patterns (Stone and Van Orden 1993, 1994).

The idea of design principles is well illustrated by the pragmatic approach of Jilk et al. (this issue). Induction of design principles circumscribes sets of possible formal models that satisfy common functional criteria. Nevertheless, there exists an inevitable tension between characteristic empirical patterns, chosen to satisfy empirical intuitions, *versus* formal patterns, chosen to suit theoretical intuitions. For instance, described empirical or formal patterns entail abductions that may fail or evolve in the future. This tension yielded many arguments between Greg and me in which he overemphasised formal patterns and me empirical, or maybe it was the other way round. More important, this tension implies that fidelity to a large body of data in no way guarantees that a theory or model is valid (Duhem 1954; Einstein and Infeld 1966; Quine 1961).

The latter point goes the natural next step from logical points made in the target articles. For instance, the articles all note that discrete symbolic dynamics and continuous dynamics can be behaviourally interchangeable, that data cannot make this discri-mination. This fact was made salient to me in the 1990s when I attended two different courses in the same mathematics department on non-linear dynamics, both called chaos theory. Each was taught from a different perspective. Briefly, one emphasised discrete incremental time and iterative functions (e.g. Peitgen, Jürgens, and Saupe 1992) of which Turing machines are a special case. The other emphasised continuous time and systems of non-linear differential equations (e.g. Strogatz 1994). Both sufficed to explicate common proofs and the geometry of non-linear dynamical systems.

3.2. *Effect = structure*

The articles suggest this and more profound lessons of abduction. Peirce previously realised that intuitive *a priori* beliefs about how systems work, though incorrect, might yield useful theories and models (see also, Lakatos 1970). Newtonian mechanics is one such example, false but useful. Thus when the utility of working assumptions is evaluated (however, utility or progress or truth is conceived), one must look at the larger enterprise, one must examine the aggregate of collective work at a larger scale, larger than the workings of individual investigators.

For instance, presently, at the collective scale, cognitive science appears to me to be theory rich and data poor. There is no shortage of data *per se*, but most data collected express the same dynamics, changes that increase or decrease a mean value for instance. Nonetheless they are segregated across a morass of different models and theories. Despite common dynamics, countless idiosyncratic task-specific models are proposed to explain effects of the same sort. This practice has been questioned, of course, as the 'effect = structure' fallacy, for example (Lakoff 1987).

In the fallacy, effects are deemed transparent to causal cognitive structures, but only after they are segregated according to the cognitive task they come from. Task distinctions themselves reflect experimenters' intuitions about cognition (Shallice 1988). In other words, data do not create the primary distinction between models, intuition does. Consequently, differences among intuitions cannot be resolved, and they tend only to grow in number and without bound (Van Orden, Pennington, and Stone 2001; Kloos and Van Orden 2008).

Bell (1999) was led to similar concerns albeit from different data perspectives in molecular biology, neuroscience and artificial intelligence. I'll begin with the central dogma of molecular biology. The dogma is that genes make proteins and not the other way around. But 'the central dogma of molecular biology is wrong!' (p. 2017, see also Neumann-Held 2001). Bell constructed the analogy that follows to envision what really goes on at the level of genes and proteins.

> It is as if there was a bookish town (a cell) with a central library (the genome) and people (proteins) who came in to read short sections here and there, share with each other what they had read, and use the knowledge to build and change the town. Who is controlling here – the townsfolk or the library? (Answer: neither.) (Bell 1999, p. 2017).

This analogy was intended to facilitate thinking about feedback processes in genetics. Ubiquitous feedback processes also constitute brain structure and Bell (1999) also describes phenomena of control and neural firing to facilitate thinking about feedback in this domain (i.e. molecular control of firing sensitivity, cell body control of dendrite activity, eye movement control of retinal activity, local electric fields and diffusion gradients of nitric oxide control of neural firing, delivery of energy in blood flow and so on). In the end, he dispenses with the idea of a neural level for cognitive 'computations' (despite the usefulness of the descriptive anatomical level, in its own right). Neurons are not feedforward information channels.

3.3. *Perception and action*

Regarding AI, Bell discusses perception and action and laments that we do not yet 'have a mathematical theory of the perception-action cycle' (p. 2014) as 'universally useful for characterising cyclic systems as Shannon's information theory is for characterising . . . feed-forward systems' (p. 2015). Thus, one reason scientists fall back on feedforward thinking is simple ignorance of the appropriate formal and conceptual tools to analyse complex feedback systems, even though some tools already exist (Riley and Turvey 2002; Riley and Van Orden 2005). As a consequence, another generation of cognitive scientists may retreat to Marr's (1982) emphasis on feedforward computation of perceptual representation, for example, ducking the real challenge. If so, another generation of study will end predictably in the multiplication of specialised (not explained), irreconcilable (not testable), perceptual facts (not processes) (Uttal 1990).

Perhaps, instead, we may see a full-blown paradigm shift, the first for cognitive science and the first for psychology, I would argue. We may retain the utility of computational modelling but for the new purposes of complexity science. The shift that I anticipate would move us past endless parsing of behaviour into cognitive components, a *morphologically reductive strategy*, towards a focus on emergent causal properties of coordinations among, and within, brains, bodies and worlds, *a strategically reductive strategy*. Although both strategies pursue ultimate causes of cognitive activity, the latter strategy includes a more comprehensive inventory of causal mechanisms.

4. Dale's metaphysics

In this final section, I hope to clarify my claim that Dale's (this issue) intuition pumps truly point to a different metaphysics. Why? Because Dale's 'approach . . . may also recommend integrating . . . [apparently] competing theories in meta-theoretical frameworks that would sustain their coexistence' (p. 2). In other words, while some contradictory descriptions may turn out to be more false or less false, others may require complementarities.

I hang this argument on complementarities because to acknowledge complementarities is to accept pluralism in the strongest sense that Dale describes, with implications for the nature of reality. For example, complementarities were first met within quantum physics and, while cognitive science is not simply physics, it presently confronts challenges at the same fundamental depth that complementarities posed for physics – namely, the fundamentals of measurement.

4.1. *Complementarities and emergence*

Complementarities refer to equally true but qualitatively different descriptions that are in conflict, that contradict each other. For example, complementary descriptions in quantum mechanics include the famous duality of the electron, which appears sometimes as a particle and sometimes as a wave. I expect the electron example will be significant for cognitive science. Cognitive science also confronts complementary properties of performance measures, which I will describe.

In physics, wave or particle behaviours are emergent products of measurement paradigms. Measurement paradigms express collective principles of organisation in the subatomic realm, and collective principles of organisation are the source of physical laws. 'The myth of collective behaviour following from law is . . . exactly backward. Law instead follows from collective behaviour, as do things that flow from it, such as logic and mathematics' (Laughlin 2005, p. 209).

I claim likewise that measurement outcomes of cognitive activity are emergent and that cognitive science confronts complementary outcomes as close to its foundation as those that motivated quantum physics. For instance, repeated measurements of the same cognitive performance, taken iteratively from the same individual, will fluctuate in a fractal wave, lacking a stable mean or variance. Chris Kello claims that fractal waves are universal properties of within subject, carefully controlled, repeated measurements (Kello, Beltz, Holden, and Van Orden 2007; Kello, Anderson, Holden, and Van Orden 2008).

Nevertheless, repeated measurements of the same behaviour, taken independently from different individuals (at different times for example), can yet congregate around a stable mean value or central tendency, with defined variance. Thus, measured human behaviour exhibits complementary properties in *between* versus *within* subject behaviours, those of *datum* and *fractal wave* (Van Orden, Kello, and Holden, 2008). Although energetic resistance remains as to whether fractal waves are substantial features of human behaviour, replications abound.

4.2. *Metaphysics and fractal behaviour*

I trust that arguments about fractal behaviour will sort themselves out. Too many cognitive scientists agree that to acknowledge only the facts that one finds convenient is incompatible with science. Consequently 'sooner or later [instances of this practice get] swept away by the forces of history' (Laughlin and Pines 1999, p. 30). So that is not my present concern. Of interest here is the very fact of the protracted sorting of things out. After all, the first reports of fractal behaviour were not greeted with enthusiasm and they have not yet resulted in dramatic changes in cognitive science.

Sorting an issue out implies intellectual conflict that may extend over lengthy periods of time – years, decades, longer. Concerning fractal waves, the conflict is so far about whether fractal waves are real and, if so, how seriously we should take them (compare Wagenmakers, Farrell and Ratcliff 2005 with Thornton and Gilden 2005). But to argue about what is real and what exists is to argue metaphysics and ontology – philosophy. Scientists who would wash their hands of philosophical questions inevitably persist in their own private and unexamined metaphysics. They can be brilliant – philosophy is not a prescription for doing science – but they (we) will remain wholly (partly) ignorant nevertheless about the abductions that deliver their beliefs.

Reluctant modesty is a good companion to knowledge and belief. 'The first step toward finding out is to acknowledge that you do not satisfactorily know already' (Moore 1998, p. 50, quoting Peirce). We rely instead on temporary and incomplete metaphysical and ontological assumptions to make sense of what we do. Dale (this issue) prefers to dispense with ontology, but I believe this difference between Dale's argument and my own is only a matter of shading. Both Dale and I see utility in unearthing assumptions, but I perhaps more trustingly expect that many ways in which contemporary assumptions are incorrect will eventually be made explicit.

We naturally will remain significantly ignorant about cognition, as we do of every subject matter, which is the clear lesson of history and abduction. And yet, reality is an infinite and patient teacher; we will eventually be less wrong or more usefully wrong, than we are at present. Thus when Dale suggests 'a focus on the emergent characteristics of complex non-linear systems', I suggest he also states his current metaphysical position – one that I presently share.

5. Conclusions

I included some personal details in previous sections to suggest how I arrived at beliefs similar to the bold proposal of Dale (this issue). I also wanted to segue my personal belief that little or no weasel-room remains for feedforward and computational habits of thinking about cognition. The possible solutions these habits present are well and rigorously understood, as are well-known failures.

What is yet missing, however, is an alternative habit of thought, widely familiar, and equally formulaic in its body of procedures for doing cognitive science. Complexity theory may provide such an alternative, but it has so far been applied by only a few experts, who have racked up stunning empirical success in a few challenging domains. Across cognitive science altogether, however, complexity theory has so far mostly, plainly and incrementally accrued confirmation of *a priori* assumptions (e.g. strongly non-linear behaviours, fractal behaviours and catastrophe flags, for example, Hollis, Kloos, and Van Orden 2008, is a review).

Every systems theory and research method comes with *a priori* assumptions, so confirmation is reassuring. Empirical verification of assumptions licenses conceptual tools that might not otherwise be justified (e.g. chaos theory, fractal geometry, catastrophe and bifurcation theory). And yet, the population of experts still remains relatively small. The broader understanding of cognitive activity from a complexity perspective waits to be mapped out. I gamble with Dale (this issue) that we will achieve this broad understanding, both of the kinds of solutions offered, and where the limits lie. In other words, I hope for an inventory of success and failure like what has been achieved for computational accounts. Towards this goal cognitive science will more fully confront the inherent complexity of meaningful cognitive activity.

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