

## **THE DECLINING INFLUENCE OF COGNITIVE THEORISING: ARE THE CAUSES INTELLECTUAL OR SOCIO-POLITICAL?**

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A rough division is drawn between the cognitive and biomedical strand of theorising concerning cognitive processes. It is asserted that the cognitive strand is losing influence by comparison with the biomedical strand. Three types of intellectual reasons why this might be occurring are considered and each is rejected as inadequate. Three types of socio-political reasons are then proposed as important factors.

### Introduction

Consider phonological awareness and its relation to learning to read (Lieberman, Shankweiler, Fischer, & Carter, 1974; Morais, Cary, Alegria, & Bertelson, 1979), and how later theorists have assessed the significance of this discovery. “The discovery of a strong relationship between children’s phonological awareness and their progress in learning to read is one of the great successes of modern psychology” (Bryant & Goswami, 1987). “The importance of the concept of phonological awareness in theorising about reading acquisition and dyslexia can hardly be overestimated” (Castles & Coltheart, 2004). Where, though, can we place a concept like *phonological awareness* in the set of ideas and methods that constitute the scientific study of the mind? How important will it turn out to be in our future understanding of mind?

This paper will not be concerned with phonological awareness per se but with the cognitive type of theorising of which it is such a fine example. I will argue that there are two major strands – the cognitive and the biomedical – in the historical development of the set of ideas and methods that constitute the scientific study of the mind. By one major strand – the biomedical strand – I am referring to the set of concepts and methods coming from the biomedical sciences relating originally to brain anatomy and neurophysiology. A second strand – the cognitive strand – is much more disparate in its origins in that it stems originally from the philosophy of mind, logic and engineering, but historically these disciplines, especially the second two, formed the intellectual basis of the computer revolution, which in turn has helped spawn human information-processing psychology, modern structural linguistics

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and artificial intelligence.

The distinction between the two strands is not merely a matter of their historical origins. Critically there are major sociological distinctions. The biomedical sciences have the money and prestige linked to the practical importance and intellectual successes of both modern medicine and modern biology. They therefore have the potential to operate as 'big-science' in terms of the availability and size of grants, a style which is cemented by the size of the resources required by some of their practices, e.g., functional imaging. The cognitive sciences, by contrast, tend to continue the craft practice of small-scale sciences of a Kuhnian pre-paradigm type.

Intellectually, the two strands are not completely separate. Theoretically, an approach like connectionism with its roots in logic (McCulloch & Pitts, 1943), engineering (Rosenblatt, 1958), computer science (Minsky & Papert, 1969) and physics (Hopfield, 1982) derives more from the cognitive science group of disciplines. Indeed major figures in its history have come from an even wide set of disciplines e.g., psychiatry (McCulloch), mathematics (Pitts), engineering (Rosenblatt), computer science (Minsky, Hinton), education (Papert), theoretical physics (Hopfield, Amit), biophysics (Sejnowski), psychology (Rumelhart, McClelland), in quintessentially cognitive science fashion. Yet it is an abstraction based on brain anatomy and physiology. Empirically an approach like cognitive neuropsychology is inevitably increasingly strongly influenced by the biomedical environment in which it is embedded, but it is intellectually a product of the post-war information-processing revolution in the cognitive sciences. However, both areas as far as their internal social practices are concerned are much more within the cognitive strand and I will consider them as such.

Returning to the concept of phonological awareness, it combines a concept from linguistics, with a concept from the philosophy of mind. Moreover, it has been widely used in the context of information-processing models of reading (Morais, 2003). It stands squarely as one of the great achievements within the cognitive strand. The theme of this paper, however, is that the cognitive strand as a whole has been declining in its influence on the scientific study of the mind, and it addresses the issue of why this has occurred.

It is the belief of many ageing cognitive scientists that the influence of the cognitive strand is declining, relatively speaking. But is there any evidence of that? To address this properly empirically is beyond the scope of the present paper. However, consider behavioural economics, which given the credit crunch has boomed in popularity. Behavioural economics must critically depend on an adequate understanding of cognition, if it is to be of any value at all. But what fashionable discipline has it spawned – not cognitive economics but “neuroeconomics” – which tends to have little relation to the cognitive sciences (see for more examples Legrenzi & Umiltà,

2009). Or take a key conceptual framework within the cognitive strand and one that has been used to situate the concept of phonological awareness, namely the information-processing model. If one takes the journal *Cognitive Neuropsychology*, when it was at its maximum impact as a journal in the late 1980s, a survey of two years 1989 and 1990 produced a total of 28 models out of 41 papers. If one compares that journal with what is in a way its modern analogue – the *Journal of Cognitive Neuroscience* – and takes the two issues prior to the Morais Festschrift, namely August and September 2008, there were three papers with models that could be classed as cognitive out of 27 papers. One of the three models was an ACT-R type of model, the second was a very simple naming model, the models in the third paper were quasi-anatomical but related to verbal initiation and suppression. On the basis of this very flimsy set of evidence, I will make the assumption that the basic premise of the argument – that the influence of the cognitive strand is declining by comparison with the biomedical strand – is correct. I will then ask why this might be the case, treating the decline of information-process modelling as the main symptom to be discussed.

#### The decline of cognitive theorising: The intellectual grounds

In a volume in honour of José Morais it is appropriate to consider two classes of explanation – the intellectual and the broadly socio-political. There are at least three different possible intellectual grounds for the relative decline of the cognitive strand. Maybe the theoretical frameworks developed in the cognitive strand have not been supported by empirical phenomena discovered later within its claimed domain. This, in addition to the rise of competing frameworks, was what happened to behaviourism in the 1950s and 1960s with the discovery of many empirical phenomena that pointed to the existence of complex structures internal to the organism and relevant to explaining aspects of behaviour. A second possibility is that models within the cognitive strand are incompatible with the products of technically more powerful empirical procedures coming from the biomedical strand. A third possibility is that, in rather the way that Churchland (1986) argued about folk psychology, the cognitive strand is being made redundant by more sophisticated theories derived from the biomedical strand.

It is impossible to convincingly reject any of these three possibilities in a paper length piece (but see Shallice & Cooper, forthcoming). However, a major reason why the information-processing approach, in particular, should be considered a Kuhnian normal science is that it could be applied highly effectively in an empirical domain – neuropsychology – which was very different from where it originated, that of human experimental psychology.

Now consider its relation to a new and qualitatively different domain of empirical evidence – functional imaging. The interaction has indeed been much less directly supportive of cognitive concepts as the polemical stance of those cognitive scientists attacking the relevance of functional imaging evidence for cognitive theory indicates (e.g., Harley, 2004; but see also Coltheart, 2006a; Henson, 2005a). However, if we look at models and concepts that have come out of the human experimental psychology and cognitive neuropsychology traditions – such as the Baddeley and Hitch (1974) working memory model, multiple route models of reading (Marshall & Newcombe, 1973), a concept like the visual word-form system (Warrington & Shallice, 1980), the Bruce-Young (1986) face recognition model and so on, later empirical work using functional imaging techniques has generally supported the validity of the earlier concepts (e.g., Haxby, Hoffman, & Gobbini, 2000; Paulesu, Frith, & Frackowiak, 1993; Vinckier, Dehaene, Jobert, Dubus, Sigman, & Cohen, 2007; Wilson, Brambati, Henry, Handwerker, Agosta, Miller et al., 2009). Of course, there are alternative interpretations of the overall imaging evidence, but the support the technique provides for predictions from such cognitive theories and concepts is not materially weaker than that provided by, for instance, human experimental psychology evidence. Thus if we take a theoretical distinction of which functional imaging has not proved very supportive – that between familiarity and recollection in retrieval (Henson, 2005b) – then parallel fissures have appeared in the human experimental psychology support for the idea (e.g., Wixted, 2007).

Could one argue that cognitive concepts have been transcended theoretically by computational ones, in particular functional imaging ones? It is certainly the case that connectionist models now exist in the empirical domains which were originally those of information-processing models and that the models are far more technically sophisticated than their predecessors. However, to see cognitive concepts as being simply replaced by a new paradigm would be a bad mistake. First, it is often easy to see mappings between the two types of models. For instance, as far as computational models of reading are concerned, the Plaut, McClelland, Seidenberg, and Patterson (1996) model has a structure isomorphic with the Marshall and Newcombe (1973) information-processing model and the Coltheart, Rastle, Perry, Langdon, and Ziegler (2001) model has a similar relation to the Morton and Patterson (1980) information-processing model. Similarly, in the working memory domain a model like that of Burgess and Hitch (2005) can be roughly mapped into the phonological loop component of the Baddeley (1986) version of the Baddeley-Hitch model.

Secondly, with possible occasional exceptions (e.g., Rogers, Lambon Ralph, Garrard, Bozeat, McClelland, Hodges et al., 2004), most connectionist models have generally not been accepted by the field as a whole.

Indeed, it is far from clear that the sort of weak evidence that behaviour provides, by comparison with that available in 'harder sciences', is sufficient to unambiguously support a specific model. The extra power of prediction that connectionist models have when compared with information processing models does not necessarily lead to their assumptions being more solidly based empirically. Thus experimental tests between competing models are not sufficiently decisive. Further, the models generally need to contain too many unknown variables. And the empirical paradigms that can be related to the models are too diverse. Indeed, to take an influential example, the history of the different computational models of the reading process over the last 10 years shows no greater convergence than occurred for the information-processing models that preceded them (see e.g., Coltheart, 2006b; Woollams, Ralph, Plaut, & Patterson, 2007; Zorzi, 2005). Moreover, there are virtually no links between connectionist models of the artificial neural net type and the most powerful form of evidence coming from relevant brain-related data, namely functional imaging findings. And, ideologically, most artificial neural net models are looked on with suspicion by those in the biomedical camp as insufficiently reductionist. Connectionism is better viewed, at least sociologically, as deepening the theorising within the cognitive camp and being supportive of earlier approaches such as the information-processing one, rather than as being a biomedical competitor.

What then, of the third intellectual possibility, that a new science of cognition will arise phoenix-like from functional imaging findings, owing nothing to the cognitive theory that preceded it. There are a few connectionist models where the units are held to represent real types of neurons in specific parts of the brain, such as, for instance, the models of O'Reilly and Frank (2006) of working memory or that of Byrne, Becker, and Burgess (2007) on spatial memory. However such intellectual sprouts are few and far between. In general the neurally realist computational models do not compete with existing cognitive models and at least in the first of these cases, they do not relate to the very rich human behavioural data that exists. Given the massive output of empirical functional imaging work over the last 10 years it would be highly premature to presuppose that such modelling, impressive though individual examples are, could replace cognitive theorising.

Moreover, in addition to functional imaging not having led to a rash of new types of theorising as far as explanatory accounts of behaviour in cognitive experiments are concerned, functional imaging methodologies such as factorial designs and cognitive conjunctions (Price & Friston, 1997) rely on task analysis of an implicitly information-processing type. More theoretical accounts of how one might make inferences from functional imaging data to models of cognition, also make assumptions that are very compatible with information-processing concepts. Thus, Henson (2005a) uses the concept of

a function of a given region as the cognitive target for inferences from functional imaging data, and a function seems little other than the label for an information-processing “box” (see also Poldrack, 2006). And, a natural way to interpret the blobs found in functional imaging experiments is in terms of the amount of resource needed by a particular functional system (e.g., Shallice, 2003). Moreover, information-processing models of a fairly classical type have been developed on the basis of such data; an excellent example is the gateway hypothesis of the selection between perceptually-oriented processes and thought-oriented processes of Burgess, Dumontheil, and Gilbert (2007). Thus at the intellectual level there is no incompatibility between their empirical products of functional imaging methodologies and, at least, the information-processing type of cognitive theorising. It does not appear that there are strong intellectual reasons for the replacement of the cognitive strand by the biomedical one.

The decline of cognitive theorising:  
The social context of the scientific study

The social practices of the biomedical sciences and in particular those involved in the use of the technology that has most influence in cognitive science – functional imaging – are very different from those of the cognitive sciences. In practical terms the biological sciences and even more the medical sciences have had a much greater impact on the human condition than have the cognitive sciences. A consequence is that they are much better represented in virtually all relevant grant-giving bodies, and so can in turn operate in “big science” (Greenberg, 1967) style. This allows the major funding that is required to support an area like functional imaging, where teams of researchers have to be larger and need much technical support and the use of the equipment is highly expensive. The varieties of advanced technical expertise required means that collaborators are often drawn from different disciplines with each expert having a somewhat limited knowledge of the specialities of the other. This is especially the case as far as knowledge of the cognitive sciences is concerned, since it is quite outside the standard biomedical or medical physics education. In its first major decade the functional imaging experiment therefore often became of necessity much more of a political act than in cognitive science. Negotiation between investigators with limited knowledge of each other's specialities is required and publication in high-status journals was essential for the further funding necessary for future research. Given that rapid publication was critical, and that lengthy and sophisticated analysis procedures were required to produce results, this meant that even if an interpretation was not clear-cut cognitively there was

a strong pressure to publish. Typically then the paper was one experiment deep. It was common then for a functional imaging paper, as far as studies in the cognitive domain are concerned, to produce intuitive interpretations of all blobs activated without any necessity for a result to speak clearly in favour or against some well articulated cognitive theory. Such an approach differs greatly from that of a strong empirical study in cognitive psychology which copes with the weakness of behavioural evidence in involving a number of experiments, each dealing with a potential problem in the interpretation of the earlier experiments; the inferential structure gets tighter through the paper.

The functional imaging situation now is not uniform. There are, it should be pointed out, an increasing number of sophisticated and well-designed studies speaking very interestingly to cognitive theory – take for instance, Badre and D’Esposito (2007) on the different levels of control in the selection of action. However, the early political necessities in the field as far as the looseness of inferences to cognitive theory were concerned created a style of organising papers which remains and the increasing number of cognitively sophisticated papers are more than outweighed by the rash of studies attempting to investigate complex aspects of cognition, without any basis in cognitive theory. Such studies tend to use untried self-generated paradigms that aim to capture real-life interactions even social ones, without any form of task analysis of the paradigm being attempted. In the majority of such papers cognitive theorising of the type traditional in human experimental psychology tends to be replaced by a web of vague folk psychological ideas.

But the social factors that limit the influence of cognitive theorising are not just on the side of biomedicine. The cognitive sciences suffer from equally debilitating characteristics. We lack an ideology of the construction of knowledge for use in other disciplines. The cognitive sciences are much influenced in their internal intellectual organisation by their origins in philosophy. If we take psychology, for example fame came with the development of a new theoretical framework, which typically involved belittling the preceding ones – take Watson or Hull or Skinner – and a penchant for ideologically driven intellectual wars has continued in psychology and related fields, as in cognitive psychology versus verbal learning, the language wars of the 1970s in linguistics (see Boden, 2006), the battles over the past tense (Pinker & Prince, 1988) and even of the possible relevance to cognition of functional imaging data (Coltheart, 2006a). Intellectual debate is essential in a field but in addition as is obvious from the style of biomedicine itself, to produce practical applications and to influence neighbouring fields a pragmatic attempt to consolidate is also required.

Yet if we take information-processing psychology, say, which ought to be considered as Kuhnian normal science, it tends to be dismissed as prescien-

tific by the connectionists, as irrelevant given the competence performance ideology by linguists. And even its own practitioners failed to build on their achievements. Take working memory, as it was conceived of before it became biomedicalised. It had a relatively primitive but essentially productive theoretical framework – that produced by Baddeley and Hitch (1974). The framework was productive in that it worked effectively in empirical domains like neuropsychology and functional imaging, which are very different from human experimental psychology where it originated. Yet now, working memory is a quagmire intellectually. Take Miyake and Shah (1999) who in a generally excellent book had invited the main theorists in the field to write reviews orientated around a number of key theoretical issues. The editors were therefore well placed to write a definitive overview. However they also decided to be faithful to their contributors. They proposed the following “all-encompassing definition of working memory”, namely, “Working memory is those mechanisms or processes that are involved in the control, regulation, and active maintenance of task-relevant information in the service of complex cognition, including novel as well as familiar, skilled tasks. It consists of a set of processes and mechanisms and is not a fixed ‘place’ or ‘box’ in the cognitive architecture. It is not a completely unitary system in the sense that it involves multiple representational codes and/or different subsystems. Its capacity limits reflect multiple factors and may even be an emergent property of the multiple processes and mechanisms involved. Working memory is closely linked to LTM [long-term memory], and its contents consist primarily of currently activated LTM representations, but can also extend to LTM representations that are closely linked to activated retrieval cues and, hence, can be quickly reactivated” (Miyake & Shah, 1999, p. 450). Which would travel more easily to neighbouring disciplines – the idea of a central executive system and a few slave systems with buffers, the essence of the Baddeley and Hitch account, or Miyake and Shah’s version? Yet the Baddeley-Hitch account has been there to build on for 35 years!

Conceivably a third more abstract type of socio-political reason needs to be considered. Reductionism is a very powerful ideology within the scientific community even leaving on one side the practical success of biomedicine. And it is made intuitively powerful by the visually appealing and concrete nature of functional imaging displays, far from their highly abstract essence. Moreover the last twenty years have seen the death of the most influential conceptual framework of the last two hundred years for the rational social liberation of humanity as a whole from oppression, namely socialism, and the political triumph of the opposing framework – capitalist society – which has an atomised egotistical conception of the person, the consumer choosing between the goods the market provides (Schwartz, 2005). The intellectual revolutions that produced the cognitive sciences were fed by the computer



revolution, itself the product of war, but they reached their apogee in the 1960s, the period since the war where discussion of new possibilities of social organisation reached its richest level. It is not clear whether there was a link between these two phenomena of the 1960s, but if there was, then one would expect the current decade – the most arid in its liberating potential – to predispose the atomised individual scientist, part of the highly competitive system in which modern science is organised, to a completely reductionist approach to mind.

How much a new set of ideas like those involved in phonological awareness is valued depends on how the conceptual framework in which they are embedded is considered. However, over the last 20 years what I have broadly described as cognitive theorising has in my view declined in influence, by comparison with ideas derived from the biomedical sciences, as indicated by the neuro- prefix now added to various soft disciplines to make them seem more scientific. The argument has been briefly presented in this paper (but see Shallice & Cooper, forthcoming, for a much more detailed account) that this decline of influence did not arise for solid intellectually justifiable reasons. Instead the social organisation of science seems to be primarily responsible. However, here one can see three different social factors in operation. The first is the social dominance of the biomedical sciences producing a style of scientific practice that tends to ignore cognitive theorising. The second is an internal style of functioning of the cognitive sciences that prefers intellectual blood-letting to consolidation, and so fails to provide to interested scientists in neighbouring fields a coherent body of knowledge that they can use. It is possibly too early yet to assess whether a third factor – the conception of the person best fitting current science and current society – has also been important. If, though, the cognitive branch of knowledge is withering it is very likely due to the socio-political ground not to the intellectual tree itself.

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