Dickins, Thomas E. ORCID: https://orcid.org/0000-0002-5788-0948 and Dickins, David W. (2002) Is empirical imagination a constraint on adaptationist theory construction? (Commentary). Behavioral and Brain Sciences, 25 (4) . pp. 515-516. [Article] (doi:10.1017/S0140525X02320090)

Final accepted version (with author's formatting)

This version is available at: https://eprints.mdx.ac.uk/9465/

Commentary on Andrews, Gangestad and Matthews

Abstract – 56 words
Main text – 1201 words
References – 96 words
Total text – 1353 words

Is empirical imagination a constraint on adaptationist theory construction?

Thomas E. Dickins
Brain and Cognition Research Group
Division of Psychology
Nottingham Trent University
Nottingham NG1 4BU
United Kingdom
00 44 (0) 115 848 5618
thomas.dickins@ntu.ac.uk
http://ess.ntu.ac.uk/Dickins

David W. Dickins
Department of Psychology
University of Liverpool
Bedford Street South
Liverpool L69 7ZU
United Kingdom
00 44 (0) 151 794 2950
dickins@liverpool.ac.uk

Abstract
Andrews et al present a form of instrumental adaptationism that is designed to test the hypothesis that a given trait is an adaptation. This epistemological commitment aims to make clear statements about behavioural natural kinds. The instrumental logic is sound but it is the limits of our empirical imagination that can cause problems for theory construction.

Andrews, Gangestad and Matthews (AGM) have proffered a form of instrumentalism that renders adaptationism the experimental hypothesis, and exaptation, constraint and spandrels the null hypotheses, in a universe of only four sources of design. They provide evidentiary criteria that can be used to determine possible adaptations and in order to think about their useful metric an example is offered:

Stimulus equivalence (SE) involves the formation of derived relations between a set of stimuli which resembles a mathematical equivalence set. Such sets exhibit the properties of identity, symmetry and transitivity (Sidman et al, 1982). In the behavioural sciences the interest is in the emergence of these relational properties spontaneously, in the absence of formal reinforcement or informational feedback, after a minimum number of trained links between the stimuli have been established, typically by employing an arbitrary matching-to-sample procedure. For example, two 3-member classes of A1-B1-C1 and A2-B2-C2 might be formed as follows. First A1-B1 and A2-B2 relations would be trained, by means of informational feedback. The first stimulus mentioned in a pair - such as A1 - denotes the sample stimulus and the second - B1 - constitutes the correct comparison stimulus, following A1, to choose from the array of comparison stimuli (here just B1 and B2). In a series of individual trials, participants learn to select B1 from this array when A1 has been presented, and B2 when A2 is the sample. Then B1-C1 and B2-C2 are similarly trained, the Bs now serving as samples and the Cs as comparisons. Any kind of stimuli can be used in this paradigm, and the relations between them are usually purely arbitrary, to be learned within the experiment, and independent of prior experience.

Formation of an SE class requires satisfying conjointly, in unreinforced tests, the 3 criteria listed above. **Identity**, such as selecting A1 when A1 and A2 are presented as comparisons after A1 has served as sample - is normally assumed in humans. Presenting B1 as a sample with A1 and A2 as comparisons constitutes a test of **symmetry** as the subject has to pick A1 from an array, inverting the trained relation. Other tests of symmetry would be B2-A2, C1-B1 and C2-B2. Presenting A1 as a sample with C1 and C2 as comparison tests **transitivity**, to achieve which the subject has to choose C1, and similarly for A2-C2. Finally presenting C1as sample, with A1 and A2 as comparisons, and the subject expected to choose A1, constitutes a combined test of symmetry and transitivity.

Although most laboratory animals can acquire the basic trained relations of arbitrary matching-to-sample, the general consensus within the field is that the ability to form SE classes is peculiar to humans. Consequently some theorise that SE emerges as a by-product of learning to name (Horne and Lowe, 1996), others that SE is a necessary precursor to language (Dickins and Dickins, 2001). Both camps see a fundamental relationship between symbolic behaviour and SE because symbols are arbitrary representations tied to classes of objects, events and states of affairs. In this way a symbol and its relata constitute an SE class. In this paper we will regard any putative SE *mechanism* as a rudimentary symbol machine. Symbols have uses not only in language, but also in arithmetical processing, mathematical reasoning etc. It is not inconceivable that SE had some cognitive benefit for our species. How might we apply AGM's metric to further this conception?

Although SE appears to be human-specific, Tonneau (2001) argues that SE might be a form of functional equivalence (a term used to refer to a group of stimuli which share the same behavioural function, either because they share a common training history, or because of some other kind of transfer of function between them), which is seen in other species. If this is true it could push our interest further back in phylogenetic time. Alternatively, SE might represent an exaptation of an original functional equivalence mechanism. Comparative data must be used in conjunction with other criteria such as *special design*, which is the most cogent criterion according to AGM. But special design does not remove the possibility that the trait resulted from an exapted learning mechanism. However, AGM argue that the property of domain-specificity, if demonstrated, might lend some weight to an adaptationist hypothesis. Developmental specificity indicates a biased outcome for the mechanism involved, and therefore a specific selective story. This is not straightforward for SE because it is empirically difficult to test pre-linguistic infants on a matching-to-sample or related paradigm and there are few ontogenetic studies of this ability. Horne and Lowe (1996) claim that early word learning instils SE, but the absence of pre-linguistic data renders this no more than a speculation. But, even if we could overturn this empirical limitation, would we still be able to invoke domain-specificity as a useful criterion?

If an SE *mechanism* is a rudimentary symbol machine SE might have been co-opted to linguistic, mathematical and other symbolic behaviours. The use of the term co-opted is deliberate - if SE is the bedrock of such behaviours, those elements that differentiate, say, linguistic symbols from general abstract SE classes could be the product of later evolutionary innovations, i.e. language-specific mechanisms. *As such, any apparent exaptation of function might not lie within the original mechanism at all but within the subsequent processing of its output by new mechanisms evolved for highly specific functions.* In other words, a linguistic symbol, at its most basic, is part of an SE class and the same is true for a mathematical symbol, and so on. The SE mechanism is still just producing SE classes as "before" but this time in a different domain. And this is critical, for the notion of domain-specificity used here is of a reasonably coarse grain, for SE classes can be formed between any kind of stimuli but possibly only under certain conditions. Words are more than rudimentary symbols, having grammatical properties endowed by language-specific mechanisms. However, when we see linguistic behaviour we are also seeing SE behaviour and it is this that presents us with a problem. The predominance of high-order symbol crunching mechanisms might make the telling of the developmental story about SE empirically intractable, just as linguistic and mathematical effects might mask SE effects in the lab. This might only be because of the reliance upon informational feedback in current empirical scenarios.

We are making a point about exapted learning mechanisms. AGM argue that a learning mechanism will initially be selected within a particular problem-space. This mechanism can then be exapted to output different functions leading to the mechanism producing either the old and the new function or only the new. However, looking at the problems facing the SE investigation we potentially have an original learning mechanism, for a relatively broad problem domain, with an output that has subsequently become the input for novel cognitive mechanisms. The function of this SE mechanism has not changed but its outputs might have been parasitized. None the less, it might well be that the possession of an SE mechanism set the initial conditions for language evolution etc. The question of the adaptive status of a putative SE mechanism still remains - a problem that perhaps represents the limitations of our empirical imagination.

References

Dickins, T.E., & Dickins, D.W. (2001) Symbols, Stimulus Equivalence and the Origins of Language. *Behaviour and Philosophy*, 29: 221-244

Horne, P.J., & Lowe, F.C. (1996) On the origins of naming and other symbolic behaviour. *Journal of the Experimental Analysis of Behaviour*, 65 (1): 185-241

Sidman, M., Rauzin, R., Lazar, R., Cunningham, S., Tailby, W., & Carrigan, P. (1982) A search for symmetry in the conditional discriminations of rhesus monkeys, baboons and children. *Journal of the Experimental Analysis of Behaviour*, 43: 21-42

Tonneau, F. (2001) Equivalence Relations: A Critical Analysis. *European Journal of Behaviour Analysis*, 2: 1-33

Acknowledgement

The authors are grateful to Keith Laws for his useful comments.