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STIMULUS EQUIVALENCE:
A LABORATORY ARTEFACT OR THE HEART OF
LANGUAGE?

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in partial fulfilment of the requirements for
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Abstract

This thesis surveys some of the implications of the presented collection of publications, all of which address the phenomenon of stimulus equivalence. Stimulus equivalence (SE) is first operationally defined in terms of Sidman's trio of criteria: symmetry, transitivity, and reflexivity (Sidman & Tailby, 1982). Then some of its main features – the phenomenon of delayed emergence, the effects of nodes, and the influence of properties of the stimuli used, including nameability and meaningfulness - as exemplified in the empirical studies presented, are evaluated in the light of recent literature. The variety of ways in which SE classes may be formed are described, and the question of when SE relations take effect – during the training of the base relations, or subsequently, or only in the course of unreinforced testing for derived relations – is discussed. The effects of nodal number in multi-nodal linear classes are examined and contrasted with those in serial learning. Some methods of chronometric and protocol analysis, as developed in some of the collected studies, are described, and the outlines of a model of SE class formation they might help to form is presented. The role of naming and of language in general is discussed as a sufficient route to SE class formation, but not one that is perhaps necessary for its laboratory demonstration. The role of SE in the opposite direction, in the ontogeny and phylogeny of language, is considered. Here, besides learned speculation, more empirical studies are awaited, of children, and some new developments in comparative cognition. Highlights are described of the few brain imaging studies implicating SE, following the pioneering empirical study and the earlier review in the presented collection. The survey ends by again extolling the relevance of Tinbergen's (1963) four levels of explanation in behavioural biology to see the phenomena of SE in appropriate perspective.

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A: DEFINITION OF MAIN TERMS

What is stimulus equivalence?

If a set of stimuli which have no prior relations or connections are formed into a minimal number of associative pairs by some kind of training, in such a way that each stimulus is either directly or indirectly connected with every other stimulus in that set, they may come to exhibit the properties of equivalence. This means that direct connections can now be demonstrated between stimuli which have only been indirectly connected in training. Also, if a new function is now established for one of the stimuli, such as making it serve as a discriminative stimulus for a new response, the other stimuli in the set will spontaneously acquire the same function without any training (that is they display ‘functional equivalence’).

Stimulus equivalence (SE) operationally defined

Following Sidman & Tailby (1982) participants are typically **trained** using the so-called¹ “matching-to-sample” (MTS) procedure. Here, in a simple example, on each of a series of separate trials, the participant is first presented with a single “sample” stimulus. This is followed by two or more “comparison” stimuli, selection of one of which will be consistently reinforced. Thus, in the simplest possible case, if stimulus A1 is the sample, and stimuli B1 and B2 are the comparisons, selection of B1 (and not B2) will be reinforced. Contrariwise if A2 is the sample, B2 is the “correct” comparison. Technically these are two instances of **conditional discrimination**, where the B stimuli are items in a simultaneous discrimination, and which to choose is determined by the presence of one or other conditional discriminative stimuli, i.e. the A “sample” stimuli. Sufficient such training results in the associative pairs we can denote as A1~B1 and A2~B2, where the symbol “~” indicates some kind of relation, not yet specified. Similar **trained relations** are subsequently, or simultaneously,

¹ The term “matching” has been borrowed from its use in “identity matching”, where a sample e.g. A1 has to be matched with an identical stimulus A1 among the other comparisons, as distinct from “arbitrary” MTS (as here) in which there is no necessary pre-experimental resemblance or other relation between A1 and B1. A pigeon would not necessarily be able to generalize identity matching to new stimuli which a human would do with ease. However a pigeon would learn either kind of matching over a comparable number of trials (Wright, 2001): for humans identity matching is by far the easier. Comparisons with pigeons, without necessarily being concerned with possible homologies, may expose hidden assumptions implicit in a behavioural context contrived in the laboratory.(see also Section I)

established between one stimulus from each such pair and another stimulus, giving 2 further trained relations, e.g. $B1 \sim C1$ and $B2 \sim C2$.

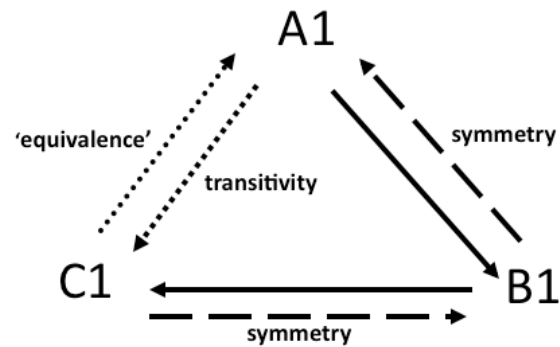


Figure 1: One ABC training class is shown, with the solid arrows indicating the direction of the sample-comparison trained relations. The interrupted arrows represent untrained relations which may emerge if tested for, without reinforcement.

Then **test** trials (without reinforcement) are given for “derived” or “emergent” relations, as follows:

- 1) **Symmetry** (dashed lines). This refers to the inversion of a sample~comparison relation, e.g. C1 is now presented as a sample and B1 is the correct comparison, or B1 is a sample, and A1 the comparison.
- 2) **Transitivity**. Here A1 is the sample, C1 the comparison.
- 3) **Reflexivity**. This would be shown by having a stimulus both as sample and as one of the comparisons, e.g. A1 as sample and A1 and A2 as comparisons. Systematic selection of A1 would define an $A1 \sim A1$ reflexive or identity relation. (not shown in Figure 1).

These three kinds of test comprise the criteria for membership of the stimuli in a common equivalence class or set. In our simple example, if they were all passed, there would be shown to be two 3-member stimulus equivalence (SE) sets:

$$A1 \equiv B1 \equiv C1, \text{ and } A2 \equiv B2 \equiv C2.$$

Here the symbol “ \equiv ” denotes an equivalence relation. These three criteria are precisely those which constitute the definition of an equivalence set in mathematical set theory (Stevens, 1951). In practise, in humans, the operational criterion of reflexivity is seldom applied, but rather taken for granted. Thus a **single test of “equivalence”** can be given which combines symmetry and transitivity, such as the C1-A1 relation.

Larger equivalence classes can be formed with more linking stimuli or **nodes** (B is the only node in our simple example) with more complex, not necessarily **linear** structures like our introductory example. Most of the experiments in the current collection involve uni-nodal or multi-nodal linear classes, but **many-to-one (or comparison-as-node)** classes (based on an AD, BD, and CD training structure) are described in *dwd*: 17, and **one-to-many (or sample-as-node)** classes (based on AB, AC, & AD training) in *dwd*: 15.

B: THREE PHENOMENA FROM EARLY STUDIES

In the first papers (*dwd*: 1, 5) in these collected studies on **equivalence class formation (ECF)** three phenomena became apparent which stimulated much of the subsequent research:

- a) “**delayed emergence**” (the gradual reduction of errors² and decrease in the reaction time (RT) of correct responses over repeated test trials) ;
- b) the effects of **nodality** (or nodal number, i.e. the number of nodes intervening between a sample and comparison, sometimes called ‘nodal distance’); and
- c) the effects of the **nameability and/or meaningfulness of stimuli** on ECF.

² The terms “correct” response or “error” refer respectively to choices in accordance or not in accordance with the SE relations as defined by the experimenter.

In our first study (*dwd: 1*) the MTS procedure was used to generate six 3-member ABC classes. Trained relations, symmetry, transitivity and equivalence were simultaneously tested, in two phases. In addition to accuracy (the proportions of correct responses compatible with equivalence) we measured the **reaction time (RT)** of each correct choice (the time from the onset of the comparison stimuli to the response). Different types of visual stimuli were used for different groups. Group 1 participants were given 'pre-associated' or semantically related icons (such as three examples of 'heavenly bodies', or three 'plants'). Group 2 were given 'unassociated' (arbitrarily combined) but individually meaningful and nameable icons. Group 3 were given 'abstract' stimuli, contrived or derived from obscure exotic scripts, not likely to have much meaning or to evoke a name. Participants in Group 1 showed universally low error rates and short RTs across all four types of test. Groups 2 and 3 showed higher error rates and longer RTs on tests of symmetry and trained relations, and initially higher scores still (roughly twice) on tests of transitivity and equivalence, the first indication of a nodal effect (here the B stimuli were the **nodes** which in training had separate links with both sample and comparison, but were absent from the screen in AC or CA tests). It was expected that the node would need to be retrieved from memory on initial tests involving a transitive relation. While in the first round of testing the error rates were similar for Groups 2 and 3, in the second round they diminished more for icons than for abstract stimuli. RTs were lower for icons than for abstract stimuli in both phases of testing.

In a third experiment all participants were given abstract stimuli and were trained and tested as before. Before training however they were taught names for the abstract stimuli, those in Group 1 being given **individual names** for each of the 18 stimuli, and those in Group 2 given a total of six **common names**, each the same for all three stimuli in one of the six ABC training classes. Individual names made no difference to the pattern of results for errors and RTs which were like those in the previous experiments, but the pattern for group names resembled that for pre-associated stimuli in Experiment 1, viz low scores on both measures which were equal across all types of test. It was as if training with a group name had established stimulus equivalence (SE) classes perhaps

even prior to the initial matching-to-sample (MTS) training. However this last comment anticipates the discussion in Section I about the role of “naming” in ECF particularly in relation to the work of Horne & Lowe, (1996,1997), and the experiments in the last paper in the collection, *dwd: 17*, below.

In the next study (*dwd: 5*) class size was expanded to five, with six 3-nodal linear ABCDE classes. AB, BC, CD, and DE trained relations were established in successive training episodes, in each of which a multistage “errorless” procedure was used: the number of comparisons was gradually increased from one (the correct comparison only) to six, with a criterion of 19 correct responses out of 20 having to be reached prior to each expansion. All four types of test were then given in combination in four batches of randomly ordered trials. In Experiment 1 only 2-nodal and 1-nodal tests of transitivity and equivalence were given, together with tests of BA, CB, DC and ED symmetry and tests of the original trained relations which were characterized as “zero-nodal”. This enabled an overall pattern of test pairs which avoided the possible scaffolding of specific multi-nodal relations by means of previously tested relations which might have been formative steps along the same “pathway”.

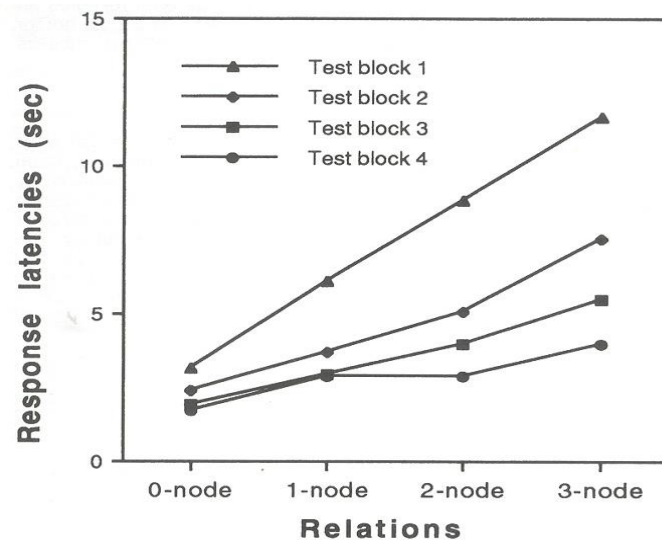


Figure 2: The relation between mean response latencies (=RT) and the number of nodes (“0” for tests of symmetry and trained relations, and 1-3 in tests of transitivity and equivalence) for four repetitions of the block of tests.

In Experiment 2 all possible relations were tested. Though participant numbers were small and there was considerable individual variation in the rate and extent to which equivalence relations were established, statistical comparison confirmed the following effects. The proportion of “correct” responses (those compatible with SE) decreased, and the RT (see Figure 2) for correct responses increased, as a function of an increasing number of nodes, from zero to 3. There was no significant nodality effect on the RTs of incorrect responses. Nor was there any significant effect of directionality, viz trained relations versus symmetry, or transitive relations versus equivalence (symmetry + transitivity) relations. Over repeated tests accuracy increased and RTs diminished, lowering the slopes of these two, largely linear accuracy x nodality and RT x nodality functions (though only the latter interaction was statistically significant).

Further discussion and analysis of nodality effects follow later (Section G).

C: FUNCTIONAL EQUIVALENCE

Another important feature of SE relations is that of the **functional equivalence** of the members of an SE set. If one of the stimuli is set up as a **discriminative stimulus**, that is to say a stimulus in the presence of which a particular response is reinforced (and not reinforced in its absence), the other stimuli in the set, without any further training being required, will also now function as discriminative stimuli for the same response. This kind of spontaneous **interchangeability** between members of an SE set is an important property, relevant to comparisons between SE and language, and the possible ontogenetic and phylogenetic interrelations between the two, discussed below.

The phenomenon of functional equivalence was explored in the context of avoidance learning in *dwd:15*, and was addressed in *dwd:17* which was a replication and extension of a study (Canovas, Debert, & Pilgrim, 2014) in which it seemed that first establishing functional equivalence may have subsequently given rise to SE relations. An alternative account was given in *dwd:17*: it was proposed that the discrimination training was tantamount to a MTS procedure

using a **many-to-one (MTO)** (or comparison-as-node) **training structure** so that SE relations may have already existed between the stimuli prior to the transfer-of-function training subsequently given by Canovas et al, 2014.

D: VARIOUS PROCEDURES LEADING TO EQUIVALENCE CLASS FORMATION

In addition to MTS there are various other procedures by means of which specific relations can be established between stimuli in pairs, which, with the pairs interlocking, can also result in equivalence class formation (ECF).

Pavlovian or **respondent conditioning**, in which one pair member is presented first and is followed on every trial by the other member, has been shown by Leader and her colleagues to lead to SE when subsequently tested for using MTS (Leader, Barnes, & Smeets, (1996); Leader, Barnes-Holmes, & Smeets, (2000); Leader & Barnes-Holmes, (2001); Smeets, Leader, & Barnes, (1997). In several studies in the present collection (*dwd*: 13, 14, 16) respondent-type trials were used successfully for the basic training in “study phases” which alternated with go/no-go or “same-different” trials (see next paragraph) in the “response phases”. Here testing for SE relations also used same-different trials. **Go/no-go procedure** – In this, following presentation of the sample, there is only one “comparison” (a term justified if one conceptualises the test as a succession of trials, some with a correct and some with an incorrect comparison stimulus) in which there are two alternative responses, such as “yes/no”, Fields et al, 1977; or “same”/“different” (Cullinan, Barnes-Holmes, & Smeets, (2001). In the *dwd*: 12, 13, 14, & 16 studies no reinforcement or informational feedback was given on the same/different trials, but on the alternating respondent phases the correct comparison was revealed so that the criterion of mastery in the response phases was readily achieved³.

A problem with these go/no-go procedures is that a participant may construe as correct only comparisons to which they responded “yes” in the particular context of training, and they may not carry this response over to

³ This entire procedure was introduced with the primary aim of providing test trials with one stimulus only needing to be fixated upon, to eliminate the extra “noise” likely to be involved in visual scanning of an array of comparisons in conventional MTS during fMRI measures of neural activation in *dwd*: 18, unlike the previous studies (*dwd*: 8 & 10: see also *dwd*: 11 and Section K below).

stimuli having a potential equivalence link with the sample in the different context of testing. An example of this occurred in a pilot study for *dwd*: 18 (Dickins, 2008): given tests of CA equivalence after AB and BC same/different training about half the participants (9/16) soon came to respond according to SE, but the remaining seven participants consistently responded “different” on all equivalence trials. When a further group of participants were given the additional explicit instruction immediately prior to testing that two stimuli previously associated with a common third stimulus should be treated as “same”, as well as those that had been directly connected in training, they all (8/8) rapidly and consistently conformed with equivalence.

ECF would seem to consist of (at least) **two components**, knowing what to do (the SE **algorithm** as it were) and **remembering** the taught associations. With regard to the former there is no inherent logic leading from trained relations to SE. It would depend upon the interpretation of the relation ‘~’.

“If $A \sim B$ and $B \sim C$ ”, it *does not* follow that “therefore $A \sim C$ ” (for example).

The case may be different for the relation “>”, if this can be instantiated behaviourally in some way (see account of **transitive inference** in **serial learning** in Section H below).

“If $A > B$ and $B > C$ ”, it *does* follow that “therefore $A > C$ ”.

Exemplar training is a way to initiate the algorithm, synonymous here perhaps with the term **relational frame (RF)**, which Hayes, Barnes-Holmes, & Roche, 2002 construe as an operant (defined in what some might see as a rather over-inclusive way), and shaping this up for appropriate application. Exemplar training seems to have been the key factor in enabling a Californian sea lion to pioneer ECF in a non-human species (Schusterman & Kastak, (1993, 1998), a claim not accepted by all researchers in SE (see Horne & Lowe, (1996). Memory would still be the other limiting factor. Stimulus properties found by Fields, Arntzen, & Moksness, (2014) to increase the likelihood of ECF (especially since they used a linear training structure which compared with MTO and OTM has a low yield⁴), included salience, nameability, meaningfulness, and distinctiveness from other stimuli. These are all familiar in cognitive psychology for their effects on memory. (See for example the von Restorff effect – if one item in a list is

⁴ “yield” is defined here as the proportion of participants reaching an acceptable criterion of ECF

made conspicuous, such as one nonsense syllable coloured red while the rest are black, recall of that item is enhanced independently from the list, but recognition of it is increased only in the context of the list: e.g. Fabiani & Donchin, (1995).

Finally explicit verbal **instruction** can substitute for purportedly pure operant (or respondent) laboratory procedures, including the establishment of the baseline relations (Eikeseth, Rosales-Ruiz, Duarte, & Baer, (1997), or given just prior to testing can raise the likelihood of ECF following conventional training, as we have seen (*dwd:19*). Also a participant's **prior knowledge** of the SE literature, or in some cases acquaintance with mathematical set theory (found in odd instances by both Lanny Fields (pers.com.) and in *dwd:17*), may facilitate ECF.

E: WHEN DO SE RELATIONS TAKE EFFECT?

Given that the baseline relations have been trained, at what point do the derived relations of symmetry and transitivity become available? Do SE classes exist before they are tested for? Despite the objections of some, e.g. McIlvane & Dube, (1990), that such a question is inconsistent with behaviour-analytic theorizing, Doughty, Leake, & Stoudemire, (2014) argued that it is useful as a heuristic, as advocated for example by Zentall, (2013), provided that the problem is analyzed in terms of basic behavioural concepts. When **delayed emergence** is encountered (*dwd:1,5*; Sidman, Kirk, & Willson-Morris, (1985) this suggests that testing itself is playing a part in the formation of SE relations. However the not uncommon immediate emergence, or the manifestation of fairly complete SE relations on initial tests, are compatible with a prior origin in some cases. Conceivably derived relations might be formed as the trained relations are being laid down, or during some process of consolidation between training and testing. How could we know this? Can SE relations be demonstrated in any other way than by MTS tests?

Doughty et al, (2014) trained one set of relations, and then either tested for SE, or not, before training a second, conflicting set. The latter were tested and SE relations dependent on these were established and then extinguished. A demonstrable **resurgence** of relations based on the earlier set of trained

relations was then found, but only if these had been previously tested. Doughty et al, (2014) also deployed two other methods of testing participants for these SE relations: (i) having them **sort** all the stimuli (presented on separate cards); and (ii) getting them to assess the **degree of relatedness** between probe pairs of stimuli. Both types of test confirmed the absence of SE relations if they had not been tested for earlier.

A screen-based **sorting** method was also developed (*dwd: 14, 16*) and compared (*dwd: 16*) with card-based methods reviewed by Fields et al, 2014. Both kinds of sorting seemed likely to offer adventitious opportunities for the participant to compare and contrast possible associative links, both those remembered from trained relations and those emerging from being clumped together in the test, similar to what may well be the case during MTS testing, and likely to promote ECF.

Sorting tests can be seen as **recognition** tests, but participants can also be asked to **recall** stimuli vocally, or write them down or even draw the stimuli “as they come to mind”. Some participants recall many or all of the stimuli, whilst others, despite good performance on other measures, may recall surprisingly few. Now the order in which stimuli are recalled is interesting. It may show **clustering**, which Galizio, Stewart, & Pilgrim, (2001), claimed indicated that some degree of ECF had already occurred when recall took place prior to MTS testing, but with a greater degree after testing. It is not clear how many of the links in their cluster analysis were simply concatenating trained relations, or whether some at least of the consecutively recalled items were linked by derived relations. Some specific recall sequences were presented in *dwd: 16*: Table 18: from the recall order there both trained and derived relations between adjacent stimuli can be identified, and runs of three stimuli from the same SE class are also apparent in some sequences. Participants attaining equivalence (with delayed emergence) on the MTS tests recalled more stimuli, showed more frequent occurrences of derived or grouped transitions, and mostly showed superior sorting scores compared with those who showed no signs of ECF on the MTS tests. Some of the latter ‘failing’ participants did however succeed in separating out all or most of the SE classes, in the (final) sorting test, again suggesting that seeing them all on the screen provided a kind of bird’s eye-view

of the stimulus array, finally enabling the equivalence relations to be distinguished.

A more subtle indirect measure of ECF makes use of the phenomenon of **semantic priming** (Meyer & Schvaneveldt, (1971); Barnes-Holmes, D., Staunton, Whelan, Barnes-Holmes, Y., Commins, Walsh, et al., (2005). Two words are presented, a “prime” followed by a “target”. If they are semantically related, such as “tiger-lion”, the RT to recognize the target is shorter than if the two words are not semantically related, such as “tiger-house”. In Barnes-Holmes et al, 2005, Experiment 2, AB, BC, and CD training was given to provide two prospective ABCD equivalence classes. Before MTS testing, within-class and between-class pairs of stimuli were presented in a priming task in which participants made a ‘lexical’ response to the second stimulus to indicate whether it had been used in prior training or not. After this, MTS testing for ECF was conducted. Correct decisions in the priming task, for participants subsequently achieving ECF, were made more quickly to targets from within-class pairs than to those from between-class pairs. For those who did not go on to achieve ECF these effects were confined to trained relations, and were not found in within-class pairs related by symmetry or equivalence. In a third experiment Event-Related Potentials (ERPs) were also extracted from EEG recordings of brain activity during responding in the priming task, looking in particular for the presence or absence of the late negative N400 waveform. This had been shown by Weisbrod, Kiefer, Winkler, Maier, Hill, Spitzer, et al, (1999) to be more pronounced for semantically unrelated word pairs than for directly or indirectly related pairs. Similar results were found by Barnes-Holmes et al for between-class versus within-class stimulus pairs in participants who subsequently attained ECF. They did not however obtain these results in participants who were first MTS tested and then given the priming tests.

The reverse was the case in an otherwise similar study by Haimson, Wilkinson, Rosenquist, Ouimet, & McIlvane, (2009). Only participants given ERP tests after MTS testing for equivalence showed a robust differentiation of the N400 measure i.e. greater for between-class than for within-class pairs. Haimson et al, (2009) however used a one-to-many training structure (training AB, AC, AD, AE, and AF relations) to give three such 6-member SE classes. Tests for

symmetry, such as BA, FA etc. and tests for equivalence, such as BC, ED etc were organized into two distinctive groups so that different individual pairs were used in MTS and priming trials.

It seems in some cases there may be some endogenous derivation of novel interrelations from trained associative relations before these are induced to emerge on an MTS test. In Section K we will consider whether neuropsychology can help in other ways to track the generation of novel relations as inferences from previously presented associative pairs.

F: HOW ARE SE RELATIONS FORGED?

If processes occurring during testing are instrumental in forging the untaught derived or inferred links, how can they be investigated? This is a tricky question, but other than at a neuropsychological level (some possible leads from which are discussed in Section K) perhaps a combination of the **chronometric analysis** of behavioural data (*dwd: 12*), and **protocol analysis** (*dwd: 16*), and to some extent **post-experimental debriefing** (especially *dwd: 16 & 17*), may provide the beginnings of an answer.

The term **chronometric analysis** (Posner, (1978) refers to the measurement of the effects of independent variables, such as the timings of the onset and offset of the sample and comparison stimuli, and the relation between these, upon the dependent measures of behaviour, particularly reaction time (RT) and/or the accuracy of responding. As we shall see, if sample offset precedes comparison onset, the duration of any delay between these may be influential.

An example of such a study was *dwd: 12* (see also *dwd: 11*, Fig.2). Here the idea was, after participants had reached a criterion of MTS training, to manipulate the **delay** between sample offset and comparison onset to enable subtraction of the time taken for an anticipatory process triggered by the sample from the total RT to the comparison stimulus. If the exposure time for the sample was kept as brief as possible, just long enough for it to be “registered”, any subsequent process preparing for the imminent arrival of a particular stimulus could take place with the screen blank. If there were sufficient time for this

process to go to completion, the participant would be completely ready to swiftly locate, identify, and respond to an anticipated stimulus, so that the RT (measured from the onset of the comparison(s) until the choice response was made) would be minimized. If on the other hand the preparatory processes occurred while the comparisons were already on the screen, the time they took would be incorporated within the RT thus defined. As a control, the same participants had been previously trained and tested on identity MTS, where it was hypothesised no such anticipatory processes would be needed. Measurements were taken under both conditions when the participants' behaviour had stabilized. The finding was, as predicted, that interpolating a delay made no difference to the RT in identity matching: 'registration' of the sample seemingly entailed being prepared for the exact same stimulus as the comparison, without the need for extra time. By contrast, in most participants, in arbitrary MTS, there was the predicted inverse relation between delay and RT .

There were several problems with this study however, making it little more than a pilot study⁵ which it would be good to see modified and replicated.

- There was evidence for individual differences in participants' strategies (some probably tended not to anticipate but rather to wait until the comparison was present to select their response)
- Though earlier (unpublished) evidence had been obtained for the same effect with standard multi-comparison MTS, *dwd:12* employed the same/different procedure. RTs were longer for different-correct than for same-correct trials and the delay effect was statistically discernible mainly for the former
- The inherent variance and non-Gaussian distribution of RT measures made statistical analysis difficult: box-and-whisker diagrams got over this to some extent
- The order in which different delays should best be applied presented problems because of shifting baselines. One solution to this would be to arrange repetitive cycling of a rising and falling delay to see if the running average of RT tracks this, or by using a titration procedure in which the delay would be steadily increased until a running average of RT ceases to fall, at

⁵ hopefully in a more positive sense than the definition of a pilot study offered by Sidman in his pre-SE days (1960): "a pilot experiment is an experiment which didn't work"

which point the delay is decreased until RT ceases to rise. Logically the latter point should be at zero delay.

Arithmetic tests were used in *dwd:12* to simulate the effects of anticipation on the SE task. If a single number (x) served as the sample and two numbers ($y + z$) served as the comparison a participant could be asked to indicate as rapidly as possible whether the equation $x = y + z$ was correct or incorrect. Here the participant would have to remember x but wait until after a delay before being able to do the arithmetic. With the opposite arrangement, $x + y = z$, the addition could be done while the sample was on the screen, so that whether or not it tallied with the sum on the comparison could be more swiftly determined. Manipulation of the delay permitted calibration of the calculation times (which could also be related to manipulations of the difficulty of the sums).

In a somewhat similar context Arntzen (2006) found that a delay during initial training of 4s or even 9s between sample offset and comparison onset with MTO or OTM training structures enhanced subsequent ECF, presumably due to the facilitation of some process of 'mediation', such as "rehearsal", which strengthened the initial associative learning. It should perhaps be pointed out that most studies of the effects of interpolating an inter-stimulus delay have been concerned with deteriorating performance as a function of the delay duration, perhaps contra-preparing investigators for such chronometric explorations.

A more explicit set of **component processes**, each requiring a finite time, was tentatively postulated in *dwd: 12 & 14* to fit the narrative described above:

1. **registration** (perception and recognition) of the sample stimulus
2. **retrieval** via an established sample-comparison link of an "**anticipatory representation**" of the appropriate comparison (held thereafter in some kind of working memory)
3. When they appear, **scanning** and **registration** of the comparison stimuli
4. **Recognition** of the comparison stimulus which matches the "representation" from stage 2.
5. **Motor organization** of corresponding response.

How these correspond to a similar analysis of tests of multi-nodal SE classes is discussed below, in Section G.

Protocol analysis, (Ericsson & Simon, (1993); Ericsson, (2003) strictly speaking, is conducted by inducing participants to say aloud whatever thoughts “come into their head” whilst performing on a particular trial, as distinct from introspectively speculating about the entirety of their thinking in the experiment generally (which would be encouraged in a post-experimental debriefing). It should be able to be shown that the verbal report has not detracted from the performance of the task, though the opposite effect, of performance enhancement – which may have been present but was not assessed by means of a participant-costly control in *dwd: 16*- would also compromise the objective of such a measure. This technique was diligently used in an early study of ECS by Wulfert⁶, Dougher & Greenway, (1991), who found, for example, that participants combining the separate (visual) stimuli into one was associated with failure to form equivalence classes.

There were similar reports (of portmanteau words) from some participants in *dwd: 16*, but these were made in post-experimental debriefings. The distinctive feature of this study (and later in *dwd: 17*) was the use as stimuli of **phonologically correct non-words (PCNWs)**- readily pronounceable but not to be found in any (English) dictionary - and the idea was to encourage participants to say these aloud when they came to mind and throw light first of all on anticipatory recall prompted by the sample stimuli in AB and BC training. Then, in the SE tests of equivalence, this might reveal the possible symmetric recall of B nodal stimuli when C stimuli served as sample, with perhaps the further report of the appropriate A stimulus, prior to the presentation of the comparisons on the screen. There was evidence that where these were correctly vocalized subsequent choices compatible with SE were more likely to be made.

Post-experimental debriefing. Participants may be able to give a coherent and plausible account of their behaviour during the various stages of the experiment in retrospect. This is usually both promoted and systematised by means of a restricted set of carefully constructed questions which need to be

⁶ In their study the large investment of time required to comb through the voice recordings limited the proportion of participants whose protocol data was actually analysed (Wulfert, personal communication). In *dwd:16* this was a relatively simple task as participants' recorded utterances were simply the 'names' of the stimuli.

open-ended and neither constrain what participants might reveal nor supply notions which may bias or distort what they say. In some instances specific reports can be cross-checked from details of the behaviour records.

Sometimes an algorithm may be described which tallies with the participant's objective behaviour but conflicts with the experimenter's planned elicitation of an SE algorithm. It may describe an alternative way of making correct choices without necessarily construing SE relations. For example, in *dwd*: 17, attending to shapes of individual stimuli and any resemblances between them rather than simply learning individually the arbitrary links between particular shape stimuli and responses on particular keys led some participants to adventitiously form correct and some to form systematically incorrect SE classes.

Stabilization of incorrect SE classes is sometimes found, e.g. Holth & Arntzen, (1998), perhaps due to systematic allocation of supposed links so that they are mutually exclusive but unrelated or only partially related to the baseline trained relations, almost as if an SE algorithm was being applied, but in which the 'remembered' links did not correspond to the actual links established by the end of training.

Of course it should never be simplistically assumed that a participant's report necessarily reflects how the behaviour was in fact organized. Cases described by Nisbett & Wilson, (1977), led them to conclude that any match between participants' reports may be in accordance with actual cognitive processes relating to their objective behaviour without reflecting any true awareness or insight into these processes, but merely based on their expectations separately formed from noticing salient stimuli and speculating on what might have been their deliberate strategy.

G: MORE ON NODALITY

Delayed emergence of SE relations after linear training not only indicates that ECF may be fostered in some way in the context of testing: it may provide some insight into how this comes about. As we have seen, in *dwd*: 5,

response accuracy increased and RT decreased over four batches of MTS tests, but remained a function of the number of nodes in each test, irrespective of the direction of the test (i.e. to the same degree for both transitivity and equivalence). If these findings are reliable and typical they may be combined with the list of putative component processes (from *dwd*: 12 & 14: see Section F) to sketch out a **hypothetical model** of ECF from linear multimodal training structures.

We may postulate that when fully trained the presentation of the sample elicits the memory retrieval of the stimulus with which it has a trained relation. An internal representation⁷ of this correct comparison will trigger recognition and selection of that actual comparison stimulus on tests (whether reinforced or not) of that trained relation. Now in a test of transitivity (where one or more nodes are involved) one can hypothesise that the initial representation elicited by a sample may itself in turn elicit recall of (the representations of) a further comparison stimulus for which it was the sample in training, and so on where further nodes are involved.

To account for tests of symmetry and equivalence (transitivity cum symmetry) similar processes of “**spreading retrieval**”, but in reverse, must also be postulated. These may also be triggered by the comparison stimuli when they have been presented during training, perhaps in parallel with an anticipation process. Whilst all the experiments in the current collection, other than Experiment 1 in *dwd*:1, used a zero delay or finite delay procedure (in which the sample terminates at or some seconds before the onset of the comparison/s) it is quite likely that a participant will deploy some process of double check. With multiple comparisons any stimulus that could be associated by some such reverse process with a sample different from the sample just registered on the current trial could be excluded from being chosen, and the one that was associated with the sample would have its link confirmed by such a ‘retrospective’ process. In the case of a single comparison in a same-different procedure, as in *dwd*:13, there is evidence that confirmation of a “same” comparison is faster than disconfirmation of a “different” comparison, on go and

⁷ using this term in a similar way to McLaren, Green, & Mackintosh, 1994: “a representation can be as simple as the activation of a specific unit or neurone (*sic*) though often it will correspond to a pattern of activity over units”

no-go trials respectively. This was not measured in that study, but in *dwd:18* RTs for same-correct (SC) and different-correct (DC) were compared for 24 participants on 48 equivalence tests and 24 tests of trained relations over eight repeats of this large block of trials. These showed a similar difference that was marked and sustained despite a general speeding up over repeated trials (see Figure 3). Such results resemble the case of different patterns of brain activation in reaction to correct versus incorrect arithmetic equations analysed by Menon et al, 2002.

Now on the chronometric principle each hypothetical step in such retrieval processes would take a finite time which, apart from any parallel processes, would be additive, hence the relation between nodal number and RT. .

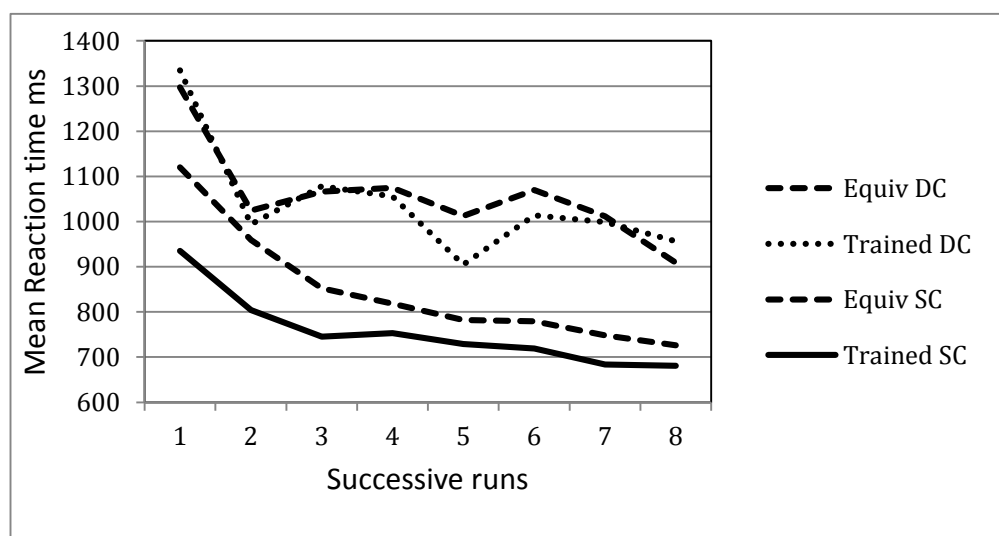


Figure 3: mean RTs for correct responses on same/different test trials of trained relations ("Trained") and equivalence relations ("Equiv") over eight repetitions of the 72 tests. (n=24 participants) (from *dwd:18*)

Similarly, in terms of accuracy, each step would incur a risk of error, reducible with practice, so that the probability of an error would be cumulative, as increasing numbers of nodes were involved.

Sidman, Kirk, & Willson-Morris, (1985) reported that the formation of links with many nodes followed the formation of links with fewer nodes, suggesting a gradual assemblage of longer and longer links:

“..... n-stage relations sometimes failed to emerge until lower-stage relations had been tested. Furthermore, once testing had established classes of n-1 members, previously nonexistent n-member classes often were manifested immediately. In this way, tests that demonstrated n-member classes were separated at least from lower-stage tests that were required to bring the n-member classes into existence, thereby providing almost a direct proof that the tests had generated the classes.” (Sidman et al, (1985), p.39)

It is not clear what implications this progressive construction hypothesis might have for RT. Sidman et al (1985) did not measure RT. In *dwd:5*, Experiment 1, different nodal distances were compared between SE classes but not within them (see above, Section B), enabling only 0, 1, or 2 nodes to be tested despite the overall ABCDE 3-nodal structure, but over this range the same relations between nodal number and both accuracy and RT were found.

Where RT was measured (*dwd: 5*, Spencer & Chase, (1996) it was only the correct responses whose RT showed a relation with nodal number.

Further testing than was carried out in *dwd: 5* might have produced completely flat curves in which nodal effects were no longer discernible, though Spencer & Chase indicate they never reached such a point in their study. How best to represent the RT data in view of its variance and nonGaussian distribution (Whelan, (2008) so that appropriate statistical inferences can be drawn is problematic however, and the absence of statistically significant results is not necessarily strong evidence for the absence of a nodality effect.

In the interesting situation in which entirely within-class comparisons are conducted, such as those of Fields and his colleagues, reviewed in Fields & Moss, 2007, preferences for the nearest comparison to the sample in terms of the intervening number of nodes between it and the sample have been demonstrated, even if RT for different nodal distances in between-class tests are no longer discernible. Fields et al ran through various theoretical accounts of ECF and found only Relational Frame Theory (Hayes, Barnes-Holmes, & Roche, (2002) compatible with their account. The notion of spreading retrieval with respect to the training structure (rather than to the physical array of comparison stimuli on a particular trial) is not mooted in relation to these results, despite the fact that the concept of spreading activation in a semantic memory network (e.g. Collins & Quillian, 1969) was compared by Fields, Adams, Verhave, & Newman, (1990) to nodal effects in SE.

An empirical way to test some of the predictions of models allowing for successive and parallel processes of retrieval of nodal links as sketched above⁸ might be by means of the tracking of participants' eye-movements while they are choosing between comparisons. This is technically easy to achieve with contemporary technology but the data might be hard to analyze, and differences between individuals' strategies would probably need to be taken into consideration. An early example of this arose in *dwd:5*, (Experiment 1). Here in informal debriefing two participants revealed that during training they had stage-by-stage incorporated the associated stimuli such as A1, B1, C1, D1, and E1 into a single mnemonic image. Recall of this composite image when any one of the component stimuli was presented as a sample in testing enabled the same speedy and accurate responding with little influence of nodal distance.

H: SERIAL LEARNING: ANOTHER KIND OF INFERENCE FROM ASSOCIATIVE LEARNING

Serial learning (SL) is another kind of inference⁹ which may be demonstrated following training on interlocking series of paired stimuli (often referred to as the premise pairs). Here, in each pair, one stimulus has to be chosen in preference to the other. For example in the pair A+B- (the signs denote to us that the correct choice is A), the participant has to learn that selecting A is followed by informational feedback and often some other form of reinforcement: choosing B is not reinforced. However, when B is paired with a different stimulus, C, it becomes the stimulus to choose, so that this pair would be designated B+C-. In this way a series, preferably of at least five such pairs, e.g:

A+B-, B+C-, C+D-, & D+E-

can be devised and repetitively presented to the participant in random order, until a criterion of reliable performance is attained. Set out on the page like this

⁸ other than running particularly prolix participants on a protocol analysis with PCNW stimuli (this was tried recently using three ABCDE classes – *dwd*: unpublished - but none of 11 participants attained criterion on multi-nodal relations).

⁹ Relational frame theorists, e.g. Hayes, Barnes-Holmes, & Roche, 2002, see SE and SL as only two among many such relational frames, mostly associated with the variety of ways in which human language provides a rich variety of ways of construing the world, with interesting implications and applications, but beyond the scope of the present review.

the reader will perceive that an ordered series is being presented in a covert way, accounting for the label serial learning (SL)¹⁰.

Tests can now be given in unreinforced test trials consisting of novel pairings of the stimuli, for example BD. Here, if the participant reliably selects B in preference to D, this is described as **transitive inference (TI)**. Preferring A over C, or C over E, or A over E would not strictly qualify as TI because for the end or “anchor” stimuli (A and E) A is always reinforced in training and B is never reinforced, giving an alternative and probably more parsimonious account of such preferences. With longer series however a greater number of authentic tests of TI may be given with varying numbers of “nodal” stimuli in between.

Referring back to Section A it can be seen that unlike SE, SL only exhibits transitivity. Symmetry and reflexivity do not apply. The relation between the stimuli in SL can be characterised by the mathematical symbol “>”, so that A+B is tantamount to A>B. (Obviously the relation B>A cannot also apply, unless “<”, the logical opposite of “>”, is substituted, giving B<A, and A>A is nonsense).

Comparisons between these two types of inference are further described in *dwd: 11* and explored empirically in *dwd: 14*. Two fascinating differences can be mentioned here. One concerns nodality. In SL the greater the number of nodes intervening between the two test stimuli the faster the choice: this is the **symbolic distance effect (SDE)**, exactly opposite to the **nodal distance effect** in SE (see Section G). This is illustrated in *dwd:11* by an experiment reported by Anderson & Dickinson, (2003), in which another independent variable was whether or not participants had been informed at the outset that the stimuli formed a series they were required to learn or were just exposed to the contingencies. As can be seen in Figure 4, while the SDE was found in both groups RTs were actually longer for the informed group.

Perhaps the results summarized in Figure 4 imply that in humans SL can be achieved in 2 different ways. An explicit overview of the series as a whole would make clear the relative positions of 2 test stimuli, with bigger separations perhaps being more salient. The existence of such a “map” was tested by Acuna, Sanes, & Donoghue, (2002) in which an 11-item series of abstract coloured

¹⁰ This is obviously different from learning a series when a particular series as a whole is presented and has to be memorised, as pioneered by Ebbinghaus, 1885.

shapes had to be learned from randomly presented premise pairs, as described above. One criterion for mastery was the requirement to accurately draw the series. Acuna et al argued that the SDE indicated that participants referred to some such overall representation.

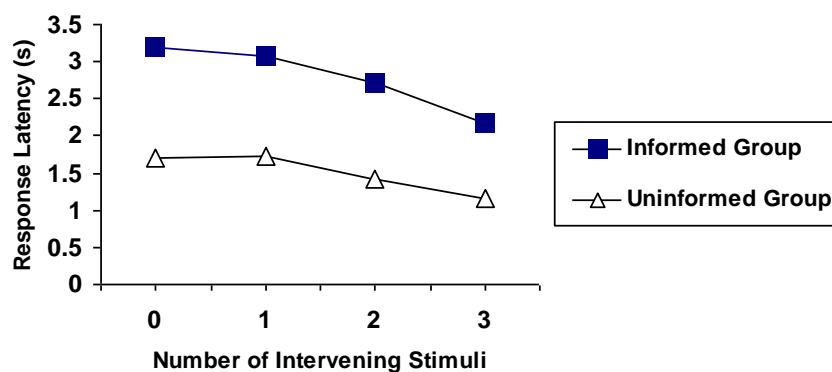


Figure 4: The Symbolic Distance Effect (decreasing RT with increasing numbers of intervening stimuli in the series) for Informed and Uninformed participants

However the other fascinating difference between SE and SL is that SL has been clearly demonstrated in many non-human species, e.g. Cebus monkeys (D'Amato & Colombo, (1988); pigeons (Fersen, Wynne, Delius, & Staddon, (1991); and some Corvids (crows) (e.g. Bond, Kamil, & Balda, (2003), where the suggested adaptive function of this capacity in a highly social species was the monitoring of social status). Alternative explanations of SL in terms of general learning theory have been put forward, e.g. by Wynne, (1995, 1997). Lazareva, Smirnova, Bagozkaja, Zorina, Rayevsky, & Wasserman, (2004) cleverly juxtaposed “representational” and reinforcement history explanations of SL in corvids. One group of four crows received ordered feedback after each SL training trial by being given sight of coloured discs graded in size along the A+B-, B+C-, C+D-, & D+E- series. These individuals achieved TI on BD tests (significantly preferring B). This was despite their having been exposed in training to unequal frequencies of the premise pairs so that the relative frequency of B+ and B- and D+ and D- trials was tilted in favour of a greater +/- ratio for D. Choices by four control birds given discs of the same diameter on

every trial, who could only rely on the reinforcement history which would predict a preference for D, did not in fact differ significantly from chance.

Are SL and SE entirely different psychologically as well as in their logical formulation? Does a group of stimuli forming a series have any other shared properties? In *dwd:14* an attempt was made by training two 7-member SL series in parallel to see if subsequent between-class MTS tests would exhibit SE-type emergent relations, and what were the effects of nodality in this context. Two 7-member SE series were trained in a second group of participants. Tests appropriate for each group's initial training were given first, followed by the opposite kind of test, but overall results were poor in terms of accuracy, especially for those trained on the demanding linear multimodal SE procedure. RTs were similar for all nodal numbers for SE testing in the SE group, but declined with nodal number in the subsequent SL tests. RTs declined with nodal number in both kinds of test in the SL group.

At least one neuropsychological study (Wendelken & Bunge, (2010) has made within-participant comparisons between SL and SE (see Section J). Further research will be needed to make more effective comparisons between these two procedures and their associated behavioural phenomena.

I: NAMING AND LANGUAGE

Horne & Lowe, (1996, 1997) produced a target article which elicited a range of commentaries (including *dwd: 6*) in which they postulated that ECF depended upon the overt or covert **naming** of stimuli. Their account of naming is set forth in the context of a rich synthesis between a behaviour analytic account of the ontogeny of language expanding on the ideas of Skinner, (1957) and mainstream developmental psychology, with a broadly Vygotskian perspective (e.g. Vygotsky, (1987).

As described in Section B, in (*dwd: 1*) we attempted to manipulate the availability of naming, by showing that ECF was faster with nameable iconic stimuli than with hard-to-name "abstract" stimuli, and that by teaching supplied names to the latter ECF was facilitated, and strongly, when all three stimuli in a

potential SE class were given the same group name, but not at all when an individual name was given to each abstract stimulus. In *dwd: 4*, with six potential ABC linear SE classes, the names individual participants themselves gave to iconic stimuli were used, by means of oral paired associate (PA) training, to make AB pairings which conflicted with MTS-trained relations between the icons themselves. In subsequent MTS testing for SE relations some of the name-name links displaced the MTS-trained components in all types of derived relation. If some testing was given after MTS training but before similar PA training such incorporations did not occur. However if some of the visual icons themselves were associated in conflicting pairs using PA-type training, disruption in subsequent MTS tests was similar, whether the conflicting training preceded or followed initial MTS tests of SE. In *dwd: 2* discordant PA training of AC pairings between names displaced transitivity and equivalence relations on subsequent SE testing, but not trained or symmetric relations.

This led us to the claim in the *dwd: 6* commentary on Horne & Lowe, (1996), that these results provided evidence for an effective facultative role for the names of individual stimuli in ECF, but not for their obligate involvement.

Upon reflection all this is too simple. Naming is a key behaviour which a child develops from listening, echoing, and discrimination learning in a rich succession of social, scaffolded contexts so that it comes to encompass in practice the same kind of integrated sets of mutually interconnected stimuli as the artificial phenomena of laboratory SE classes. If naming in this naturalistic sense can somehow be extended in the laboratory situation to construct common names for all of the stimuli in a nascent SE class this would work. But the experimental stimuli, typically in themselves often abstract and meaningless, are also selected so that they have no pre-experimental connections with one another. Only their conditioned associations in the context of the baseline training procedure seem to provide any reason why a common name should be applied to that pair, or spread to interlocking pairs. If distinctive individual names were applied to the stimuli separately this would simply mean that the participant would have the option of memorizing the links between the names: this might mediate choices in lieu of the links between the actual stimuli. In effect a mnemonic device, this would itself have to be learned, in addition to recalling

the correspondence between its separate components and the stimuli themselves. Such name-name linkages Horne and Lowe would identify as “intraverbals” in Skinner’s (1957) terminology, and they postulate how they would develop and might support, for example a symmetric relation, by means of repeated alternating iterations. Participants can sometimes be heard behaving in exactly this way whilst attempting to master trained relations, as was observed for instance (*dwd*: 16) where the stimuli were self-named by being phonologically-correct non-words (see Section F).

An intriguing device for providing ready-made name-name linkages, encompassing all the pictorial stimuli in a putative SE class, was adopted by Randell & Remington, (1999), who chose familiar stimuli which were semantically unrelated but of which the natural names (in English) rhymed with one another. When the rhymings were within the putative SE class this greatly facilitated ECF, in comparison with controls in which the rhyming relations were orthogonal to the would-be SE relations. In fact ECF in the within-class rhyming group was so rapid that it resembled the groups given semantically pre-associated icons, or common names, in *dwd*: 1, . This indicates that names can be handles, without engulfing the experimental stimuli with added meaning. Further evidence for this was provided by Mandell & Sheen, (1994), whose participants reached ECF more rapidly and with fewer errors with pronounceable phonologically correct non-words (PCNWs) as stimuli than with harder-to-pronounce phonologically incorrect items, or simply punctuation marks as stimuli. Also participants required to (somehow) pronounce non-phonological items did better than those who were required to silently transcribe them.

However the intrinsic **meaningfulness** of a stimulus can independently contribute to its likelihood of being incorporated into an SE class, and this property can then somehow extend to the other stimuli in the same putative class, even if these are selected for meaninglessness (Arntzen, (2004). But what is “meaning”? This is not the place to refer to philosophical or even behaviour analytic treatises, but several different meanings of the term meaning have been distinguished, as pointed out by Fields, Arntzen, Nartey, & Eilifsen, (2012), for example the distinction between an item’s denotative properties (its dictionary

definable features), and its connotative properties (its associated attributes and feelings). Examples of the latter are studies of the influence that anxiety, gender issues, or political orientation may exert on the formation of SE relations between emotive stimuli (Leslie, Tierney, Robinson, & Keenan, (1993); Moxon, Keenan, & Hine, (1993); Watt, Keenan, Barnes, & Cairns, (1991). Shared features of this type can override relations derived from formal trained relations.

Fields, Arntzen, Nartey, & Eilifsen, (2012), were also able to show that if one stimulus in a prospective SE class of otherwise abstract stimuli was first established as a discriminative stimulus for an (irrelevant) response it could increase the likelihood of ECF against a background of low yield in linear structures. This effect was similar to but less than that of a odd meaningful stimulus similarly incorporated.

Canovas, Debert, & Pilgrim, (2014), seemingly fostered ECF by first establishing functional equivalence classes between the stimuli, by a so-called simple discrimination: requiring the same key-press to each of three shape stimuli and pressing another key to each of a different three shapes. When this had been trained new key presses were taught to one stimulus from each of the groups after which spontaneous unreinforced transfer of these responses to the other stimuli in each group (and thus functional equivalence) was demonstrated. ECF was then tested by presenting compounds of 2 stimuli which were either both from the same class or one from each. Participants with no further instruction reliably made a response indicating “correct” to the former and withheld this response to the latter.

In *dwd:17* a replication & extension of the Canovas et al, (2014) study confirmed this compound test of ECF with a subsequent conventional MTS test . However, terminologically Canovas et al’s technique for setting up the functional classes could also be seen as an MTO training structure for SE. The label on the key could be seen as the comparison stimulus which the participant had to select following each of the three shape stimuli. This also applied if vocalizing a PCNW was substituted for pressing a particular key in further experiments described in *dwd:17*, where the participant learned to associate a PCNW with two or three triads of other printed PCNWs by vocalizing it in advance, reinforcement (or confirmatory feedback) being provided by showing the correct printed word

after the participant's vocal response. Without the need to demonstrate transfer of this training to new PCNW stimuli ECF was readily achieved in these experiments, for three as well as just two SE classes in parallel.

There is a profound sense in which the possession of language makes available a vast array of behaviours (both in communication and thought), and the early acquisition, plausibly outlined by Horne & Lowe, (1996, 1997), by hearing, speaking, and interacting with categories of objects and actions, of names for these phenomena (words), is fundamental to the flowering of these language skills, a life-long process. There are few human activities which are not bracketed and suffused with linguistic activity. Thus our perception and interpretation of much of our experience has been held by some to become **propositional**, and many of these processes we can make explicit to ourselves and others.

The above view is put forward in another interesting peer-reviewed paper by Mitchell, De Houwer, & Lovibond, (2009). These authors maintain that all human learning is propositional, whereas some of the commentators, and in particular McClaren, (2009), and see also McClaren, Forrest, McLaren, Jones, Aitken, & Mackintosh, (2014), find room for associative processes that are presumably based on neurophysiological processes demonstrably homologous across the animal kingdom¹¹ with no reason to exclude our own species.

So can SE also be derived from associative processes, or does it depend upon propositional construal? Horne & Lowe in effect regarded naming as the ontogenetic start of propositional learning and that SE is an instance of how this works, in the formulation of categorical perception associated with listening and speaking. The alternative is to regard SE as firmly anchored in associative processes, as does Sidman (e.g. Sidman, 1994), and his prediction is that SE should be demonstrable in non-human species. Until recently this had arguably not been achieved, except perhaps in a sea lion (*Zalophus californianus*) by Schusterman & Kastak, (1993) (analyzed critically in Horne & Lowe, 1996). Of late, symmetry at least is beginning to be demonstrated in the pigeon (*Columba livia*, var. *domestica*) (Urcuioli, (2015) and imminently in crows (*Corvus corone*)

¹¹ They cite the neurophysiological studies by Kandel and his colleagues, (Hawkins et al, 1983, 1986, 2006) in which all the main conditioning phenomena were demonstrated in isolated ganglia of the mollusc *Aplysia*.

(Wasserman: pers. com. See Smirnova, Zorina, Obozova, & Wasserman, (2015)¹² but it may be that seemingly similar phenomena in birds and humans are analogous but not homologous.

We have already seen in relation to SL in Section H some interesting attempts to disentangle (and not only in humans) propositional processes (if for example map-like spatial representations can be seen in this way), from purely associative explanations.

J: THE EVOLUTION OF LANGUAGE

Having presented papers on SE and language to the first four of the (still ongoing) biennial conferences (1996-2002) on “The Evolution of Human Language” (<http://evolang.org/>) one felt in a minority group and dubbed as “Skinnerian” by some amongst the rich variety of scientists – linguists, anthropologists, sociologists, neuroscientists, primatologists, archaeologists....- all with something interesting to contribute, but mostly concerned with problems of grammar and syntax. Such a variety of specialists are attracted because language evolution is a very complex topic and any part played by mechanisms of associative learning can only be small. In retrospect perhaps the contributions of Savage-Rumbaugh (e.g. Savage-Rumbaugh, (1986) and Bickerton, (Bickerton, (1990, 1996, 2000), were among the nearest to a consideration of the possible role of SE in the origin of symbols/words.

Dwd:9 highlights the work of Place, (1995), who stressed the resemblance between an SE class and the relation between a word and its referents and near synonyms. Savage-Rumbaugh had diligently taught both species of chimpanzee, being successful especially with the bonobo (*Pan paniscus*), to use keyboard symbols seemingly to represent objects which they sought in their captive environment. This could however be the misperception by a language animal of the behaviour of a non-language animal: there is some doubt as to whether these “symbols” were more than discriminative stimuli, or conditional discriminative stimuli, guiding the actions of the chimps to other discriminative stimuli in a particular context. SE, defined in the strict Sidmanian sense introduced in

¹² See Delius et al, 2001 for a discussion of cognitive abilities of different bird species and pigeon varieties.

Section A, i.e. manifesting the triple phenomena of reflexivity, symmetry, and transitivity, has yet to be clearly demonstrated in the chimpanzee. Indeed with Savage-Rumbaugh's early "signing" common chimps (*P.troglodytes*) Dugdale & Lowe, (2000), failed even to demonstrate symmetry.

Bickerton's contribution as a language evolution theorist is most relevant to SE with his proposed evolutionary stage of "proto-language". This would have worked as a useful means of communication at a putative 'halfway' stage prior to the development of syntax, simply by giving names to objects or phenomena, and to actions: nouns and verbs. This was thought to correspond to the age of about two years in the development of a child today in normal society, largely consisting of at most two-word utterances. Proto-language may have been very similar also to **pidgins** which arise in communication between present day adults who share no true language in a polyglot social environment (Pidgins have a tendency to develop into fully-fledged syntactical **creoles** if this kind of environment persists, particularly thanks to the linguistic activities of children growing up in this situation.) Perhaps a unique human capacity for SE preceded proto-language in phylogeny, or the two co-evolved, establishing the proto-language level. Alternatively these language abilities emerged for some other reason, and, along the lines of Horne & Lowe, (1996) (discussed in Section I) conferred the capacity for ECF (waiting for behaviour analysis to be formally demonstrated) simply as a by-product)¹³.

Just as the development in ontogeny, even just of naming, as well as syntactical language in all its complexity, requires an optimal mix of experiences and interactions in a richly socially communicative context, so too the phylogeny of language must have required multiple abilities, not all necessarily confined to humans¹⁴, but almost certainly uniquely combined on this planet in just this one species¹⁵.

¹³ This question of "which came first ..." in our first paper delivered to EVOLANG we characterized, with etymological flair, as a *gallinovular* problem (Dickins & Bentall, (1996).

¹⁴ Vocal imitation for example, which is of course found in several bird taxa, is absent in non-human primates and only found to any developed degree among the two main groups of marine mammals: Cetaceans (whales & dolphins) and Phocids (walruses, seals - & yes- sea lions)- see Fitch, (2010, 2011).

¹⁵ For a broad evolutionary view see for example Hauser, Chomsky & Fitch, (2002). (The linguistically well known second author of this article was seen to be won over to this perspective at the Harvard meeting of EVOLANG 2002.)

K: INSIGHTS FROM NEUROPSYCHOLOGY

Most neuropsychological studies of inference from associative learning have concerned TI in SL, and *dwd: 8* was the first of the small number of brain imaging studies of SE so far published. It was primarily a trawl for any pattern of activations which could be associated with the emergence of derived relations , and therefore much depended upon the **control** conditions with which the tests could be contrasted. This key issue and other methodological matters are discussed in *dwd: 11*.

MTS training (in a conventional laboratory) and testing (next day, in the scanner) of six 3-member groups of icons resembled that in *dwd: 1,2,4,5*. The control task during testing was similar in all respects to the SE tests, except that the sample stimulus was always an asterisk symbol, and the single comparison was also this asterisk, the other stimulus boxes being empty. This was therefore procedurally an identity matching task, which on repeated trials would become essentially a cued simultaneous spatial discrimination.

A **block design** (see below) was used: control and experimental blocks of trials were alternated every 15 seconds, with ten blocks of each in a functional run. There were four such runs corresponding to the four types of test in the order trained relations, symmetry, transitivity, and finally equivalence.

Despite not having refresher training of their training from a previous day all 12 subjects attained criterion on the first (unreinforced) test of trained relations, but within the limited number of trials they were given (about 40 at most for each test) two failed symmetry, five failed transitivity, and three failed equivalence.

There was a characteristic pattern of fMRI activations when the control results were subtracted from the experimental, including principally in the dorsolateral prefrontal cortex (DLPFC), one of the regions generally considered to be involved in executive function (Duncan & Owen, (2000), and in the posterior parietal cortex. Conspicuously absent from the list of regions was Broca's area, famous for being associated, inter alia, with the production of speech. If this was a true silence, and not an artefact, because for some reason the control task also involved some kind of inner speech, this would seem

evidence against the role of vocal or sub-vocal articulation of names during SE testing in this experiment, despite the fact that the stimuli were nameable icons.

Serendipitously the successful participants demonstrated a higher degree of left lateralization of activity in the DLPFC compared with those who failed. Across participants there were significant correlations between the degree of left lateralization of activity in the frontal cortex and both response accuracy (on all tests except symmetry), and response latency (RTs) in the tests of transitivity and equivalence (with the more left-lateralized subjects responding most rapidly).

In a second fMRI study (*dwd*: 10) participants were given refresher training immediately prior to the scan, and 9/10 participants tested positively for transitivity. Participants who were informed in advance about the nature of the tests made more correct transitive responses than participants who had not been so instructed but who also attained SE. The uninstructed participants showed more extensive and more left lateralized patterns of activation cf. baseline in parietal cortex, and in the prefrontal gyri, suggesting that extra effort was required in a search for the appropriate algorithm.

These early investigations were somewhat “spread-shot” preliminaries, since though quite powerful, the comparisons suffered, owing to the block design, from including all the activities within and between trials, and combining different numbers of trials in different participants and conditions, in the sum total of activation calculated. A later study (*dwd*:18), not yet published¹⁶, used two interlocking experiments, one of which deployed an **event-related design** (confining the data collection to the within-trial events), which indicated that when words were used as the B nodes with photos of faces and pictures of objects serving as the A and C stimuli, Broca’s area was active on CA equivalence tests compared with control tests of directly trained X (object)-Y (face) relations where no nodal word stimuli were involved. This was unsurprising since unlike *dwd*: 8 actual words served as nodes, and in the companion experiment it had been shown that recall of such words activated Broca’s area significantly more than recognition of the other two types of stimuli.

¹⁶ These results were reported at a conference presentation (Dickins, 2013) but are only now being written up for submission to a neuro-journal

As shown in Section G, Figure 3, the longer latencies for same-correct equivalence choices diminished more over successive trials than those for same-correct trained relations, so that over eight repeats of the 72 trials they had virtually converged, while at the same time the differential fMRI activations in Broca and elsewhere also progressively diminished and disappeared. This would accord with nodal activation becoming redundant with repetition. We had also predicted that the hippocampus, on early equivalence trials, would be involved in the retrieval of roughly twice as much material to confirm or disconfirm the equivalence relation than was required for the control trained relations. We also hypothesized that this extra hippocampal activity might become less necessary, if the retrieved equivalence relations themselves became learned over a number of repetitions of the same-correct test pairs. In parametric analyses over the eight trials, there was a linear decrease in activity for equivalence trials with a massive reduction of activity in the hippocampus and parahippocampal gyrus, particularly on the right, and a similar reduction in the inferior frontal gyrus, as well as in the locus coeruleus. Similar but smaller decreases were also found for the control trials. When these linear decreases were compared for the two types of trial, the anterior medial prefrontal cortex and Broca's area stood out as having reduced in activity much more for equivalence than for control trials, with a much smaller difference in the putamen and right hippocampus.

Schlund and his colleagues have performed fMRI tests on several types of behaviour emanating from the behaviour analytic laboratory including two studies of SE. Schlund, Hoehn-Saric, & Cataldo, (2007), used event-related procedures with two ABC classes and they compared trained relations with derived relations, which highlighted greater activation in several prefrontal regions, the caudate, thalamus, and putamen. Schlund, Cataldo, & Hoehn-Saric, (2008), first demonstrated ECF behaviourally in 10/12 participants by testing all derived relations. FMRI activations were then recorded while participants decided whether pairs of stimuli were or were not related, essentially a same/different procedure. Contrasts were made between same-correct and different-correct trials of each type of relation, despite the fact that RTs were substantially longer on different-correct trials. On tests of 'nodal dependent'

trials (transitivity and equivalence) the same-correct > different-correct contrast revealed bilateral activation in the anterior hippocampus, whereas on symmetry trials activation was in the right parahippocampus. For the opposite contrast (different-correct > same-correct) there was generally bilateral activation in the parahippocampus, as well as in frontal and parietal lobe regions.

In an fMRI study using five AB, BC trained relations with dauntingly abstract stimuli Ogawa, Yamazaki, Ueno, Cheng, & Iriki, (2010) compared activations on tests of symmetry, transitivity, and equivalence. “The prefrontal, medial frontal, and intraparietal cortices were activated during all modes of inference. Additional activation in the precuneus and posterior parietal cortex was observed during transitivity and equivalence, which may reflect the need to retrieve the intermediate stimulus (B) from memory.” (op.cit. Abstract)

The work of Zeithamova and her colleagues represents a powerful blow-by-blow fMRI analysis of some ways in which transitive inference may occur in an SE rather than an SL context. They used the “associative inference task” in which AB and BC pairs are learned and then transfer to AC pairings is tested. In Zeithamova & Preston, (2010), the ‘trained relations’ (between grey scale pictures of objects) were formed in a single respondent trial. Participants were able to learn these at well above chance. Non-overlapping XY pairs were also learned as a control, as well as exposures to single X stimuli. AC transfer tests were then given, followed by tests of the various types of trained relation. It was hypothesised that successful performance on AC transfer tests could depend either upon (i) the successful encoding and accessibility from partial cues at retrieval of both the AB and the BC associations individually; or (ii) integrative encoding so that when a BC pair is initially presented the overlapping (nodal) B element serves as a cue for reactivation of the previously experienced AB, leading to the formation of a new AC association that was not directly experienced. This would then support novel AC decisions on test without the need for retrieval of the individual AB and BC associations.

Detailed analysis of fMRI activations in various brain regions, and of interconnectivity between specific regions, both during encoding and testing could be compared with the detailed outcomes of individual trials. An overview

of these analyses cannot be more succinctly summarized here than by quoting the following lines from Zeithamova & Preston's (2010) abstract:

“Within regions predicting subsequent associative memory for directly learned associations, encoding activation in MTL (medial temporal lobe), including hippocampus and parahippocampal cortex, uniquely predicted success on novel transfer trials both within and across participants, consistent with an integrative encoding mechanism where overlapping experiences are linked into a combined representation during learning. In contrast, during retrieval, PFC (pre-frontal cortex) activation predicted trial-by-trial transfer success while MTL predicted transfer performance across participants. Moreover, increased MTL-PFC coupling was observed during novel transfer trials compared with retrieval of directly learned associations.”

These virtually ‘molecular’ analyses may not be relevant, or more likely only partially relevant to the relatively ‘molar’ happenings in SE studies. They do suggest however that the building of ‘novel’ relations may depend upon normally unobservable nascent associations during training and indeed testing in a conventional SE experiment. Zeithamova & Preston themselves point out one caveat. Participants were explicitly instructed about the nature of the task and given pre-experimental practice, and the authors themselves stress the importance of studying comparable processes under incidental learning conditions.

Wendelken & Bunge, (2010), presented an elegant fMRI study in which, in behaviour analytic terminology, SE and SL were studied side-by-side (see Figure 5). They were testing specifically the hypothesis that the rostralateral prefrontal cortex (RLPFC), as well as the more commonly implicated hippocampus, plays a key role in relational integration.

As Figure 5 shows, participants were presented on each trial with four relations between coloured balls together with a target relation. They were asked to decide whether the target relation (in the circle) was correct, given the other indicated relations between balls. For the ‘general relation’ (=SE) trials (top row) the basket icons represent equality relationships. For the ‘specific relation’ (=SL) trials (bottom row) the icons represent balance scales indicating (in most cases) inequality relationships. In the Figure all of the encircled figures represent valid relations which do follow from the information above. Invalid trials could have been created by having an orange instead of a blue ball in the SE trials, or switching over the position of the two balls in the SL examples.

Wendelken & Bunge found that RLPFC, but not the hippocampus, showed

stronger activation on trials that involved “relational integration as compared with trials that involved relational encoding without integration”¹⁷ (that is derived relations in the “Inferential” column versus what are in effect trained

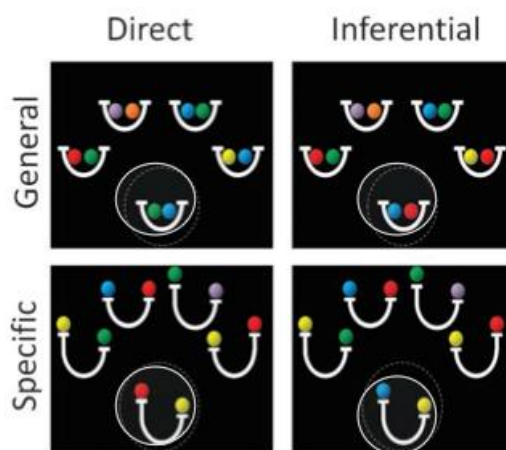


Figure 5: Combined tests of SL and of SE from Wendelken & Bunge, (2010).

In each of the four cells the top four figures represent given relations between balls of different colour. Do the encircled figures show a relation which is, or is not, commensurate with these?

Top row (General) indicates balls that “go together”, as in SE.

Bottom row (Specific) indicates relative weights of balls, equal, or one heavier than the other, a kind of SL. **Cells on the left** (Direct) show a direct correspondence between the encircled figure and one of the four figures above it. **Cells on the right** (Inferential) require an inference to be drawn from more than one of them.

relations in the “Direct” column). Hippocampus on the other hand showed stronger activation on trials “requiring encoding of relational predicate-argument structure as compared with trials requiring encoding of item-item associations”. In our terms this means SL versus SE. Using functional connectivity analyses Wendelken & Bunge, (2010) hypothesized that the RLPFC “... draws on hippocampal representations of mental relations during the process of relational integration.”

¹⁷ all the quotes in this paragraph are from the abstract of Wendelken & Bunge, 2010

Again we have a situation in which participants, unlike in most SE experiments, were explicitly pre-instructed and rehearsed for the task in hand.

The last two brain imaging studies of inference from associative learning describe above are almost alone in deploying what can be seen as a SE format, but are not addressed to or cognizant of the SE literature. A good deal of conceptual diplomacy between these two fields will be needed to design some critical studies.

L: SUMMARY AND CONCLUSIONS

In the earlier empirical studies in this collection (dwd: 1, 2, 4, 5, 8, 10, 15) the baseline relations were trained by MTS with multiple comparisons, and with reinforcement and informational feedback for correct selections. The later studies (dwd: 12, 13, 14, 16) mostly deployed a go/no-go (or single comparison, alternate response) MTS, usually without reinforcement or informational feedback, but coupled with alternating respondent presentation of the correct pairings from which the participant was evidently able to determine what responses to make in the MTS trials. In all except the first experiment in dwd:1 the sample stimulus was always cleared away before the onset of the comparison stimulus or stimuli. This was originally instituted (in Experiment 2 in dwd:1) to prevent the possibility of the participant, confronted by a prospective comparison, from glancing back at the sample stimulus, still on the screen, which may have provided external support for the generation of a symmetric association. This led to an exploration of the influence of a delay between sample offset and the onset of the comparisons, opposite in sign to the usual study of the decay of working memory as the delay is lengthened.

Some of the studies explored variables influencing the progress of forming the trained relations. Techniques of chronometric analysis and protocol analysis are described in Section F. The orientation of these enquiries, though deploying the laboratory procedures and terminology of operant (and respondent) behaviour analytic approaches, was towards a more cognitive analysis if by this is meant hypothesising about the minimal component operations needed to be performed to commit the trained relations to memory,

and to draw on these memories to make inferences, once the kind of derived relations required by the experimental format, in the unreinforced tests, was correctly construed.

The argument here was whether such a construal was brought to the experiment in the light of prior experience by language-endowed human participants (i.e. familiar with the 'relational frame' of equivalencing), perhaps indicating an entirely propositional interpretation of the experimental situation, or whether an underlying mechanism of associative learning was sufficient to support a re-jigging of relations underpinning performances compatible with stimulus equivalence relations, operationally defined.

This propositional/associative dichotomy may map approximately on to the implicit/explicit alternatives discussed in dwd: 3. It was later pointed out (dwd: 11) that a kind of inference from associative pairs in many ways resembling SE, that of transitive inference (TI) in serial learning (SL), could be shown to involve awareness in human participants, but that this was not always necessary since all the phenomena (TI, the symbolic distance effect, and the serial position effect) could also be demonstrated in human participants not seemingly aware they were learning a series. Also what parsimoniously may be seen as the same behavioural phenomena in SL have many times been demonstrated in other species. Perhaps SE can also occur without awareness, and perhaps the trio of elements of SE, which have been demonstrated separately in other species as identity matching, transitivity, and – more recently – symmetry, will sometime soon be shown in conjunction in a species devoid of language, and/or in human infants.

Probably the main thrust of these experiments is to offer methods which merit further development and parametric application to a range of procedures known to lead reliably, under appropriate conditions, to ECF. For example, in dwd 12 a titration schedule could be designed so that the delay tracks up and down around the minimum required to give the shortest RT. Having been thus honed in the conventional laboratory, it would then be fruitful to apply these methods to a co-ordinated suite of neuropsychological

investigations, combining multiple measures, including fMRI, TMS (transcranial magnetic stimulation), and MEG (magneto-encephalography).

Some colleagues would not agree. They might say they were not concerned about how relations of equivalence are implemented in humans and possibly other organisms, or how these capabilities evolved. They would be more interested in the uses to which equivalencing is put in people's lives. There should be room in behaviour science for both laboratory experimenters and practitioners concerned with analyzing the contingencies influencing a client's patterns of behaviour in order, as often as not, to seek to change them.

Here, as implicit in the title of *dwd*: 11 ("On aims and methods...") which it may not have been noticed is an allusion to the celebrated paper by Niko Tinbergen, (1963), I have tried not to stray too far from my contention (I would rather call it an insight) that psychology cannot but be a branch of biological science. Recognition of the extent to which our astonishing and essentially species-specific language behaviour may determine how we respond to the laboratory puzzles presented here may be a check on oversimplification. There is the danger though that fairly neat results from fairly neat experiments may constitute a largely manufactured scenario. The comments of Rehfeldt & Hayes, (1998), are illuminating in this respect.

Besides cataloguing the ethograms, the total behavioural repertoire of animal species in the wild, and trying to understand their adaptive significance, Tinbergen of course was concerned with the phylogeny of behaviour. Allusions here to the field of Comparative Cognition, (see e.g. Wasserman & Zentall, (2006), have been as valuable so far in emphasising the potentially illuminating differences between species, e.g. between humans and pigeons, as in the quest for underlying homologies in the organisation of behaviour. Tinbergen too was addressed to the mechanisms of behaviour, and how behaviour developed in individual ontogeny. We have tried to sketch some gross components of the former, deducible from simple observations, and surveyed one or two efforts, naïve excursions like our own, and much more sophisticated and elegant forays, into details of neuropsychology, to understand how the brain "does" equivalence (and allied phenomena). No actual studies on children were cited, and studies of very young normal children at the start of their language development are

ironically still very much in their infancy¹⁸. I hope however that all these doors have been left open and that we will get more insights, both theoretical and practical, out of the continued sceptical consideration of the 'four causes' of stimulus equivalence.

13226 words

¹⁸ I am planning a collaborative experiment on one- year olds this summer

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19. [In preparation: Dickins, D. W., Montaldi, D. and Mayes, A.R. "How the neural mechanisms supporting stimulus equivalence change as a function of repeated testing" (to be submitted to *Neuropsychologia*)]

DEDICATION

This opus is dedicated to my patient wife Anne, who fondly imagined she and I, upon retirement, would travel blissfully around the planet. Instead my academic enthusiasm is a chronic condition, and the best part for her has probably been to accompany me on trips to many cities in Europe and the U.S.A. , plus two magical trips to Africa, with free time, while I was attending and contributing to seminars etc, to explore, often solo, and with her geographer's eye, some interesting places.

These have not been unmixed blessings for her either, but she has also had the pleasure of getting to know a bunch of astute and charming buddies on the international circuit (they know who they are).

I think she is glad I have had a chance to do this ruminative collation, but knows it will not grant "closure", but stimulate me to do some new stuff. I must get the balance right from now on however: more time gardening and maybe some foreign trips just (a) for the geography, a brilliant compendium discipline, not unlike psychology, and (b) for fun!

To our sons too, Thomas and Ben, who appear in the collection, and have always been encouraging. Tom's PhD was on "The Origin of Symbols" which shows in *dwd:09*, and he is now a professor of evolutionary psychology with many interests we share. Ben's is in molecular genetics, but he lent his help primarily in the data analysis in *dwd: 12*, both by writing programs to gobble up the raw data, and by introducing me to the statistical package "R", enabling *inter alia* the construction of box-and-whisker plots that are a useful alternative way of looking at RT data. If I perhaps had some influence on their selection of academic careers in behavioural and evolutionary biology I now find myself very belatedly trying to follow my sons' lead into "broad, sunlit (post-doc) uplands" and marvel at their grasp (not always unanimous) of contemporary evolutionary biology.

ACKNOWLEDGEMENTS

First and foremost I must applaud Richard Bentall for in about 1991 or 1992 coming over to my office, from Clinical Psychology at the other side of the University of Liverpool campus, to sketch on my whiteboard the elements of what was to become *dwd: 01*. This followed several conversations we had had, more at conferences than in Liverpool, on the topic of stimulus equivalence. We applied successfully for a small grant, hired Research Assistant Steve Fox, and began a happy and productive collaboration. Gradually Richard's contribution diminished – he is much better known for outstanding contributions to clinical psychology – but we remain firm friends and in touch over SE issues, and now plan to collaborate again, with Colin Bannard, on an attempt to demonstrate symmetry in 1 year old infants using eye-fixations as an operant, thanks to legitimate spin-off from a large ESRC grant enjoyed by Professor Caroline Rowlands and other members of the Language Learning Group at Liverpool (and Manchester and Lancaster universities) which gives us access to spare participants and technology.

Too many people to mention have been excellent colleagues in these studies, from undergraduate students to professors of neuroimaging and of cognitive neuropsychology.

Of particular importance was the constant and cheerful availability of Phil Jimmieson of the University of Liverpool Department of Computer Science who wrote and constantly updated the background programs for MTS and go/no-go schedules over this entire time period.

The rigorous running of his undergraduates by my friend and colleague Andrejs Ozolins of Linnaeus University, Sweden (*dwd: 13*) deserves special mention.

One eminent worker in the field of SE, Lanny Fields, until recently of City University New York, has been both a mentor and a rival (as I told him recently) throughout this period, there at our early presentations at the EABG in London, and inviting me to contribute to the 1992 SQAB (Society for the Quantitative Analysis of Behavior) Symposium, the last in its annual series at Harvard, of

which the special number of *The Psychological Record* (which includes *dwd:02*) is the published outcome. Also for suggesting me as an International Speaker at the Washington ABAI conference in 2000 (on the fMRI work). This support, and the constant challenge of his numerous papers, closely akin in approach but not identical to mine, is as stimulating and thought-provoking right now as it was when I began work on SE.

AUTOBIOGRAPHICAL REMARKS

My first B.Sc. was in Zoology, at Queen Mary, London, and my personal tutor and would-be PhD supervisor was the behavioural entomologist John Carthy. He wanted me to work on ant behaviour, but I failed to get funding and did a postgraduate teaching course at the Institute of Education, London, and then four years of school teaching whilst studying for another B.Sc in Psychology at Birkbeck college, London, in the evenings. This went well and I was taken on as a Research Assistant at University College, London in the Psychopharmacology Research Unit where my main problem was to try to demonstrate the attenuation of forgetting by general anaesthetics in the rat, my supervisors (Arthur Summerfield and Hannah Steinberg) having shown this with subanaesthetic concentrations of nitrous oxide in humans. I had a trio of objectives: to substitute operant technology for the multiple alley water maze they suggested (which meant constructing from scratch a somewhat Heath Robinson but effective in-house programming set-up); to similarly explore and organise ways of anaesthetising rats in the Skinner box; and to devise suitable behavioural tests superimposed on an operant baseline which were in some sense the obverse of the procedures in the literature which were all concerned with the negative effects of anaesthesia (in addition to electro-convulsive shock and other agencies) on the consolidation of memory for brief negative events such as a single footshock.

All these techniques I perfected, though the work ran over after three years at UCL into my new job as a lecturer in Liverpool, but somehow I never put them all together in a thesis or published account. My teaching ranged over all behavioural biology, (including extra work in the Department of Zoology for

many years, in exchange for rat laboratory facilities that Psychology only acquired later) and I initiated and for many years ran a field course in behavioural ecology for psychology undergraduates and my work with rats also diverted into tests of optimal foraging theory. I became very interested in the observational analysis as well as the experimental analysis of behaviour, and spent a lot of time teaching this and developing video films and dvd media to facilitate this.

Apart from undergraduate practical classes in pharmacology and psychology I had done no work with human subjects until Richard Bentall and I started our collaboration, after which I said goodbye to rats and all the rigmarole of Home Office licences, though I continued observational work on animals, especially on the kittiwake gull. The rat work though was very enjoyable and illuminating and I am glad I partly “came up” through the operant route as a way of trying to clarify the elements of behaviour, and a powerful way to control these, though I was always interested too in the neurophysiological mechanisms of behaviour, as well as its adaptive function.

I once, at a conference, was about to shake Murray Sidman’s hand and thank him for opening up the intriguing problem of stimulus equivalence, but he was pounced on by press photographers and I missed my chance. I think I would still on balance want to express such thanks, though I doubt if my wife would.

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