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1 Running Head: REASONING VERSUS ASSOCIATION

2 Reasoning Versus Association in Animal Cognition:

3 Current Controversies and Possible Ways Forward

4

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6 Jan De Houwer: Ghent University

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8

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25

Abstract

26 The study of animal cognition is rife with controversy, and among the most long-standing and
27 most intensely debated controversies in the field is the question to what extent the behaviour
28 of non-human animals can be fully understood on the basis of purely associative principles, or
29 whether some behaviours exhibited by animals necessitate the assumption of inferential
30 capacities in animals that defy an associative explanation. Remarkably, the continuing debate
31 on the topic seems to be spawning little genuine progress in terms of substantial accumulation
32 of new, generally accepted, insights. As an introduction to a special section of the *Journal of*
33 *Comparative Psychology* on the topic, the present paper outlines a number of reasons for the
34 stalemate and suggests ways to re-fertilise the debate. In particular, we claim that progress
35 will not come from the adoption of general principles like Morgan's Canon or the primacy of
36 prediction over postdiction. Instead, emphasis should be placed on a careful analysis of what
37 it is that different sides in the debate do and do not agree on and an increased willingness to
38 engage in adversarial collaboration, in the spirit of a shared interest in furthering our
39 understanding of animal behaviour.

40 Key-words: Animal cognition, Reasoning versus association, Morgan's Canon, Psychological
41 continuity, Adversarial collaboration

42

43 Reasoning Versus Association in Animal Cognition: Current Controversies and Possible
44 Ways Forward

45 The study of animal cognition is rife with controversy, and among the most long-
46 standing and most intensely debated controversies in the field is the question to what extent
47 the behaviour of non-human animals can be fully understood on the basis of purely
48 associative principles, or whether some behaviours exhibited by animals necessitate the
49 assumption of inferential capacities in animals that defy an associative explanation (Heyes,
50 2012; Penn & Povinelli, 2007). One of the most remarkable features of this controversy
51 regarding the nature of animal cognition is its tenacity. Indeed, the origins of the debate can
52 be traced back to Romanes (1882), Morgan (1894), and beyond (see Greenwood, this issue,
53 for a historical overview). Yet, despite its long standing, it does not seem like we are making
54 much progress towards consensus. Papers are continually being published suggesting
55 cognitive capacities in non-human animals that defy an associative explanation, which keep
56 being met with scepticism by people in the associative camp (indeed, the very fact that
57 “camp” is a natural term to use in describing the situation is evocative of the degree to which
58 this appears at times to be an ideological debate). It is true that some degree of controversy
59 and disagreement is beneficial, as it provides fuel for scientific inquiry. However, for the
60 present debate the continuing controversy seems to be spawning little genuine progress in
61 terms of substantial accumulation of new, generally accepted, insights. As a result, the
62 enduring controversy often seems more sterile than fruitful. Against this background, in late
63 2013 a special meeting was arranged by Tom Beckers and Jan De Houwer in Ghent to bring
64 together people of different views in an effort to re-fertilise the debate and move beyond the
65 current stalemate. The collection of papers assembled in the present special section represent
66 (in part) the content and subsequent effects of that meeting. Here we outline a number of

67 critical issues in furthering the debate, discuss how the papers in the special section contribute
68 to that aim, and point to other developments that are likely to improve on the status quo.

69

70 **Conceptual issues: History and the (in)adequacy of Morgan's Canon**

71 The debate regarding the associative or more complex (that is, more cognitively rich;
72 see Haselgrove, this issue) nature of animal cognition is strongly connected with the debate
73 on the continuity between human and non-human animal cognition (see Shettleworth, 2012)
74 and the status of Darwin's famous claim that "the difference in mind between man and the
75 higher animals ... is certainly one of degree and not of kind" (Darwin, 1871, p. 105).
76 However, as Greenwood (this issue) points out, the connected debates over
77 continuity/discontinuity of cognitive processes between humans and other animals and over
78 the continuity/discontinuity of associative and inferential processes come with a great deal of
79 (typically unacknowledged) historical baggage. In making this history more explicitly
80 available Greenwood provides a timely caution against expecting entirely systematic and
81 general answers in this field.

82 On one side of the debate, people have rightfully argued that while animals exhibit
83 remarkably complex behaviour, such complexity does in itself not rule out that behaviour is
84 determined by associative principles (Dickinson, 2012). Elementary associative principles,
85 particularly when operating in combination, can yield surprisingly complex and rich
86 behaviour (see Haselgrove, this issue). On the other side, people have rejected the idea that an
87 associative explanation of behaviour should by default trump a more cognitive account for the
88 same behaviour (see Hanus, this issue). While Morgan's Canon (Morgan, 1894; see
89 Greenwood, this issue) indeed suggests that "lower" explanations for a given animal
90 behaviour should be preferred over "higher" explanations, proper epistemological justification
91 for Morgan's canon has been found wanting (Heyes, 2012). Moreover, the Canon is open to

92 multiple interpretations and may support vastly different conclusions depending for instance
93 on the scope of findings that is considered or the level of taxonomy at which it is applied –
94 e.g., at the level of an entire class (e.g., the cognitive functioning of mammals), at the level of
95 a specific order (e.g., the cognitive functioning of rodents), or at the level of a selected subset
96 of a class (e.g., the cognitive functioning of all mammals other than humans). For example, it
97 is now widely accepted that human cognition cannot fully be accounted for on the basis of
98 purely associative principles. In the human causal learning field, discussion is now between
99 those that support a single-process propositional view of human learning (e.g., Boddez, De
100 Houwer, & Beckers, in press; De Houwer, 2009) and those that argue for a dual-process
101 model (e.g., McLaren et al., 2014). Given that state of affairs, any comprehensive theory of
102 learning that also aims to encompass human learning will need to include non-associative
103 processes. But what is most parsimonious then: to assume that learning in non-human animals
104 reflects simpler associative processes only, thus creating different theories for learning in
105 different mammalian species, or assuming that supposedly more complex theories that explain
106 human learning are also applicable to non-human mammals (thereby increasing the
107 complexity of explanation for most mammalian species, but maintaining a single overall
108 theoretical framework across mammalian species)? Greenwood (this issue) further argues that
109 simplicity at one level of explanation (i.e., the biological level) need not be isomorphic with
110 simplicity at another level of explanation (i.e., the psychological level). We would add that,
111 considering arguments for a careful distinction between functional and cognitive levels of
112 explanation within psychology (De Houwer, Hughes, & Barnes-Holmes, this issue; Hughes,
113 De Houwer, & Perugini, in press), simplicity at the functional or behavioural level need not
114 be isomorphic to simplicity at the cognitive or mechanistic level of explanation.

115 In summary, despite being so commonly invoked, Morgan's Canon does not appear
116 to provide much help in choosing between different accounts of animal behaviour. Instead, it

117 could be considered as an example of the sort of systematic general principle that Greenwood
118 cautions us not to expect to work. But if we cannot rely on general principles to arbitrate
119 between positions, then what should we do instead? The answer would appear to be
120 deceptively simple: start without prior assumptions for or against particular classes of theories
121 and look to developing decisive experiments (an idea echoed more or less explicitly in all the
122 contributions to this special section). This is in no way a novel view (see Heyes, 2012), but its
123 widespread acceptance should help breaking the stalemate created by incompatible default
124 assumptions of principle. While it is important not to trivialise the difficulty in moving
125 beyond established assumptions (see Hanus, this issue, for a discussion of some of these), the
126 overview provided by Greenwood (this issue) may help in making it explicit that some
127 entrenched positions owe more to history than to scientific principle.

128

129 **Clarifying the terms of the debate.**

130 To begin the process of developing decisive experiments without a-priori assumptions
131 about general classes of explanation, it would be helpful to set out as clearly as possible what
132 it is, and what it is not, that both sides of the debate disagree on. De Houwer and colleagues
133 (this issue) begin their contribution by making a clear distinction between the behavioural or
134 functional level of analysis (what it is that humans and non-human animals do and do not do)
135 and the cognitive level of analysis (what difference in underlying cognitive capacities we
136 infer from those behavioural differences). Clearly distinguishing between those levels should
137 serve to illuminate the exact nature of the controversy. Sometimes, less than optimal research
138 designs will cause disagreement about what it is that animals actually do, in functional terms,
139 such as adapting their inspections of a food-containing hide in response to the potential
140 presence or absence of a human observer (Taylor, Miller, & Gray, 2012, versus Dymond,
141 Haselgrove, & McGregor, 2013; see Haselgrove, this issue) . More often, however, there is

142 reasonable agreement about similarities and differences between human and non-human
143 animals at the functional level, but strong disagreement about what those imply for the
144 cognitive level – in particular when there are multiple candidate mechanisms in associative
145 and inferential terms which are consistent with the same functional behaviours.

146 A second issue for clarification is what exactly is meant by “associative” or
147 “rational” in the context of this debate. Implicit within the contributions by Hanus (this
148 issue), as well as by Dwyer and Waldmann (this issue), is the idea that “associative” is often
149 shorthand for a whole class of models, with various degrees of complexity. Even when each
150 individual model is clearly specified on its own terms, considering the multiplicity of models
151 and principles as a whole does not necessarily allow a simple unambiguous account for a
152 given set of behaviours. Moreover, the diversification of contemporary associative theory can
153 make it hard for those outside the field to know what principles are captured by this very
154 general term (Hanus, this issue) and lead to the perception that associative theory can explain
155 almost any possible pattern of behaviour (especially if applied in a post-hoc manner). Hanus
156 rightfully argues that an associative explanation is to be preferred only if it is possible to come
157 up with a clear and precise prediction of an experimental observation, not if it merely
158 manages to posthoc explain anything. In such a case, a theory that provides precise prediction
159 should be considered more parsimonious empirically. That principle ought to be applied with
160 caution, however. While prediction is important in science (just like simplicity), the fact that
161 one account predicted an effect and another only explained it post-hoc is not a logical proof
162 that the first account is correct. Moreover, a similar argument about the excessive power of
163 post-hoc explanation and opacity to outsiders can be made regarding cognitive or rational
164 accounts of behaviour. Still, by placing the emphasis on the predictive value of different
165 theoretical accounts, the challenge laid out in Hanus’ contribution is for associative theory to
166 be specified in a way that allows the expert and non-expert to know what it predicts.

167 In this light, the contribution by Haselgrove to this special section is particularly
168 valuable in reviewing the current state of associative theory and providing an integrated
169 description of a number of otherwise separate models and principles (as well as highlighting
170 freely available resources which would facilitate simulating different models). This material is
171 presented through a discussion of what needs to be done to overcome associative accounts,
172 thereby setting a bar for anyone who wants to claim that a certain instance of non-human
173 animal behaviour (or human behaviour, for that matter) defies an associative explanation.

174 One additional aspect of Haselgrove's contribution is to highlight something that
175 associative accounts explicitly do not do – namely say anything about *how* two events are
176 connected (see also Dwyer & Waldmann, this issue). The importance of the relationships
177 between events is central to the contribution by De Houwer et al (this issue). De Houwer et
178 al. discuss a functional-cognitive approach, and propose a new candidate distinction between
179 human and non-human cognition that at the same time appears to allow for both some
180 continuity and for some qualitative distinction between species, in terms of the arbitrariness of
181 the types of relations that subjects can learn to respond to. To illustrate, humans can learn to
182 relate almost any arbitrary stimuli in a multitude of ways, and derive novel relations as a
183 result (e.g., to relate the word GLASS with the object glass and to relate the word VERRE to
184 the object GLASS after it has been related to the word GLASS). We can learn and derive
185 those types of relations like sameness, oppositeness, and others, for seemingly any arbitrary
186 pair of stimuli. Non-humans, it appears, can also learn to respond to the relation between
187 objects (e.g., learning to respond to the sameness of two stimuli presented in succession; e.g.,
188 Peña, Pitts, & Galizio, 2006) but only for stimuli that are non-arbitrarily related (e.g.,
189 physically identical).

190 Having considered (and rejected) the idea that broad general principles like Morgan's
191 Canon could provide a means of arbitrating between competing accounts, and having looked

192 at ways to clarify the exact issues in dispute, the next step is to consider the sort of empirical
193 work which might help answer the questions that have been proposed.

194

195 **Developing empirical tests**

196 As we implied above, focusing on developing decisive experimental tests is simple
197 only on the surface – as an answer to how to overcome the lack of progress in the field it begs
198 the critical question by assuming we know what genuinely diagnostic tests would be. But do
199 we? As we noted above, a multitude of research papers are being presented as demonstrating
200 animal behaviour that cannot be explained associatively (and many arguing the opposite). We
201 must assume that the authors of these papers believed that their work was diagnostic (at least
202 at the time it was prepared for publication), and yet the field has remained in an effective
203 stalemate. This brings us to the final question raised in the special section: what can be done
204 to raise the probability that empirical tests will actually resolve critical issues? Or to put it
205 another way, how do we ensure that one camp will take work performed by the other one
206 seriously? We have already considered the ways in which clearly specifying the nature of
207 different classes of theoretical accounts and resetting entrenched default positions could
208 improve the quality of the debate. The same things should also enhance the impact of
209 experimental work: For example, a better working knowledge of associative theory should
210 assist in designing experiments aimed at disconfirming its predictions. However, there are
211 many steps between theory and experimental design, and so improving the quality of
212 theoretical knowledge will only go so far.

213 One possible way to make progress is to embrace the fact that researchers from
214 different perspectives also bring different empirical and analytical expertise. Instead of
215 performing research entirely “in house” and relegating the input from other perspectives to an
216 after-the-fact analysis of the experimental work performed, the complementary expertise from

217 different perspectives can be used in the development and conduct of the experiments
218 themselves. That is, people from across the aisle can collaborate to develop a design that both
219 parties agree could unambiguously decide between an associative and a rational/inferential
220 account, and then perform that work. The contribution by Dwyer and Waldmann (this issue)
221 represents an (incomplete) example of this process.

222 Given the divergence in perspectives, this is expecting a great deal from adversarial
223 collaboration, as the exercise has proven to be far from trivial in execution, and it is unlikely
224 to settle all debates (after all, no one researcher is a perfect representative of their “camp”).
225 Notwithstanding these caveats, where such collaborations are feasible, they should prove to
226 be a useful tool. This is partially because they instantiate the general ideas that we have
227 already considered: The focus on empirical collaboration assumes that it is data rather than
228 general principle that will decide the issue; the involvement of researchers from different
229 perspectives mitigates against the unexamined influence of biased default assumptions; and
230 joint experimental design requires explicit pre-experimental specification of the relevant
231 predictions. Moreover, the very fact that the process as a whole is based on researchers from
232 different perspectives working together means that they are taking each other seriously. We
233 would also note that the times are clearly receptive to such adversarial collaborations, as they
234 are on the rise in other fields of psychology (e.g., Matzke et al., 2015). Other emerging trends
235 in experimental practice, in particular pre-registration and pre-experiment review (Chambers,
236 Dienes, McIntosh, Rotshtein, & Willmes, 2014), also reflect key aspects of this approach
237 through recognising the benefit of making the basis for theoretical claims explicit and open to
238 external scrutiny prior to the conduct of experimental work¹.

239 While we have high hopes for progress based on collaboration and cooperation
240 between different theoretical camps, it is also instructive to compare the broad general
241 analyses of principle (from Greenwood, Hanus, Haselgrove, De Houwer et al.) against the

242 more humble aims of the experimental work proposed by Dwyer and Waldmann. These
243 authors are careful to point out that their proposed studies are not aimed at arbitrating between
244 associative and inferential accounts in general, but instead are aimed at comparing one
245 particular aspect of one inferential account (namely the influence of representing uncertainty
246 within a causal model) against specified associative alternatives (and even this comparison is
247 contingent on a number of simplifying assumptions). This is not a lack of ambition in Dwyer
248 and Waldmann's proposal, but instead reflects the fact that empirical work is typically highly
249 incremental, addressing focused comparisons between specified theoretical alternatives one at
250 a time.

251

252 **Cautious optimism about the ways forward**

253 The six papers in this special section, and the meeting which inspired them, represent
254 the efforts of people from a range of usually competing theoretical perspectives to explore
255 together possible ways to reinvigorate the somewhat stalled progress in investigating the
256 cognitive capacities of non-human animals. This is not an easy process – enculturation in a
257 particular tradition imposes biases, some of which are explicit but many of which are implicit.
258 Recognising these biases, and looking to move beyond the heuristics which reflect them to
259 focusing on potentially discriminating empirical studies is a key step in this process. There
260 needs to be a willingness to approach issues with an open mind and to try and find common
261 ground, however limited it may be. Mutual recognition that many of our disagreements are
262 situated at a cognitive level of analysis, which implies both sides of the debate rely on fallible
263 inferences from data rather than on facts that are directly given, might be helpful in this
264 regard. The rich set of principles represented by associative processes and the encompassing
265 nature of inferential processes are both too broad to be amenable to direct falsification
266 through a limited number of empirical studies. And nor should we expect them to be. Both a

267 historical reflection, and the example of dual-process accounts in human cognitive
268 psychology, caution against the expectation of simple and sweeping explanations applying
269 generally across species and situations. We may have described this special section as
270 “reasoning versus associations” but instead of seeking the (probably mythical) one true
271 explanation of all animals behaviour, the route to genuine and lasting progress may well lie in
272 looking incrementally for the best account of a multitude of specific behaviours. Overall, we
273 feel there should be an emphasis on being collaborative rather than being adversarial – in the
274 end, we all share the common goal of wanting to find out more about animal behaviour and its
275 underlying mechanisms.
276

277 Footnote

278 ¹We would also acknowledge other recent developments in psychological research, including
279 the emphasis on replication and meta analysis, detailed considerations of experimental power,
280 and the limitations of classical statistics (along with the potential of Bayesian alternatives; see
281 Lindsay, 2015). For new empirical work to have a lasting impact, it must also be reliable, and
282 to the extent that these trends drive more reliable experimental work they will be as important
283 for comparative psychology as they will be elsewhere.

284 Author Note

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301

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