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The commentaries: Some are visual illusions.

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In our recent critique (Weisman & Spetch, 2010), we argued for using tests of correspondence (positive transfer and generalization) to determine whether birds saw the same objects in pictures as in nature (i.e., outside the testing apparatus in real life). When we completed our critique (Weisman & Spetch, 2010), I imagined after reading it carefully, researchers would say, “correspondence should now be part of my research program so I can answer questions about nature more directly”. But some of our colleagues used their commentaries to argue against finding the usefulness of direct answers to questions about nature—an argument I hardly thought possible.

The commentaries' authors are esteemed scientists for whom I have the greatest respect. But this should not be taken to mean they are always correct in their opinions. I highly recommend two of the commentaries (see Fagot & Parron, 2010; Jitsumori, 2010): these provide valuable perspectives on cognitive and perceptual acts that are important to categorization (see Spetch, 2010).

Thanks are due to my editors, Sau-Ling Hum, and Lori Vos, who have made the difficult task of criticizing my colleagues less onerous while making the logic of my critique stronger. My coauthor, Maricia Spetch, deserves the credit for seeing the need for our article. Correspond to Ron Weisman, Department of Psychology, Queen's University, Kingston, Ontario, Canada K7L 3N6. Email: ronald.weisman@queensu.ca

Nagging questions

While writing our critique (Weisman & Spetch, 2010), we had a nagging suspicion some of the researchers we reviewed were being confusing in their writing about the relationship between the pictures they showed birds and what the birds actually saw. To help determine whether our interpretation was correct, I sought advice from three colleagues about some of the commentators' articles: an English professor, who had just been awarded an honorary degree for his scholarly writing, a prize winning visual neuroscientist, and an esteemed writing coach. The question I asked this panel was, “based on quotations from the commentators' writings (see Weisman & Spetch, 2010): do these authors claim or imply pigeons saw correspondence between objects in the natural world and the pictures presented in their research?” The panelists agreed the authors had implied correspondence. And the panel thought the authors were less than straightforward about whether correspondence mattered. I felt vindicated by the panelists' collective judgment because, until then, I felt as if I had stepped through Lewis Carroll's looking glass.

All flawed hypotheses are not equal

I have applied two categories of judgments to errors in

commentaries and elsewhere in the commentators' work. We are all familiar with the discovery we, or someone we are reading, got something wrong—a fact misquoted or omitted entirely has rendered a hypothesis or purportedly correct statement incorrect, i.e., simply wrong. Some people can go further, they can sense and sometimes even articulate the discovery that some writing, their own or others, lacks logical coherence. Writing of this sort suffers a more serious handicap than being merely wrong. In the words of the Nobel Prize winning theoretical physicist, Wolfgang Pauli, illogical writing is not wrong, it is “not even wrong.” By “not even wrong”, Pauli meant thinking so logically flawed that no amount of data could render it correct (see Peieris, 1960). Over the years, “not even wrong” has come to mean science that is well-meaning and based on current scientific knowledge but that can neither be used for prediction nor falsified. Two commonly cited examples of well-meaning theories now thought “not even wrong” are string theory in physics (Woit, 2007) and Mead's anthropological hypotheses about adolescence (Orans, 1996); famously, each turned out to lack unique predictions. In this reply, I hope to show Lea (2010), Lazareva (2010), and Soto and Wasserman's (2010b) alternative to correspondence is factually and logically flawed, in Pauli's words, “not even wrong”.

An outline of the reply

This reply is divided into three sections: (i) an example of the benefits of correspondence, (ii) an examination of the logic of reactions against our critique, and (iii) corrections of specific errors in fact and logic found in comments about our critique. An important function of this reply is to uncover at least some of the commentators' errors in logic and fact, and to discuss the relationship between these errors and flaws in their research that our interchange has revealed.

An example of the benefits of correspondence.

Time to collision

Here, I provide an example to help students envision the important role of correspondence in producing powerful and sound explanations of cognition in the real world. In nature, the sight of a rapidly approaching object usually signals danger: e.g., a predator or a falling object. It has long been known the expansion of an object's image (looming) is a critical cue for predicting time to collision with the object. In an extensively cited article published in *Nature*, Wang and Frost (1992) reasoned that if they could simulate a rapidly expanding object on a video screen, they could use the simulation to find and study the neural circuits involved in pigeons' perception of time to collision.

The researchers projected a soccer ball-like object on a

screen and confirmed that a rapid increase in the size of a ball aimed at the pigeon's head elicited activation of the pigeon's flight muscles (attempted escape) and increased heart rate (fear). When the ball was aimed elsewhere, pigeons showed neither escape nor fear. These results provided good evidence of video-to-object correspondence. Notice evidence of correspondence was achieved by inference from a comparison with events in nature. The point here is evidence of correspondence does not need to be obtained any particular way; it just needs to be convincing.

It is important to understand that correspondence is a means to an end. Wang and Frost (1992) went on to demonstrate selective responses of looming detector cells in the nucleus rotundus to a simulated soccer ball aimed at the bird's head but not elsewhere. Furthermore, they were able to provide highly accurate equations (a computational model) for the activity of three distinct classes of neurons based on movement parameters of the simulated soccer ball. It was not by accident that Wang and Frost monitored pigeons' flight muscles and heart rate; they knew correspondence was critical to interpreting their experiments. Without correspondence, the results are merely an intellectual curiosity. With correspondence, Wang and Frost discovered a useful law of nature.

Deconstructing the ersatz correspondence hypothesis

Since the three commentators share a common ersatz correspondence hypothesis, we will cite and discuss only Lea's (2010, p.145) version here.

Researchers with an interest in object representation use discriminations between sets of pictures of (i) natural objects because those sets are likely to have the same structure, in terms of relative similarities, as the sets of views of a particular (ii) natural object that the bird will experience in normal life. Indeed, (iii) they are very likely to offer a better model of the structure of such categories than the kinds of artificial category introduced by Lea and Harrison (1978). It is in this sense, and not because we expect that the birds will recognize the objects they represent, that they are more ecologically valid than abstract patterns. [(i), (ii), and (iii) added here for reference.]

My issue with this statement centers around the repeated and confusing use of the word “natural”. In the language of logic, repeated shifts in meaning are termed equivocation fallacies. Equivocation fallacies blur the meanings of words, ultimately rendering them meaningless. In the classic Abbott and Costello comedy sketch about a baseball team, Costello asks, “Who is on first base?” Abbott answers, “Yes.” Costello asks again and again, each time more insistently. It turns out

that the first baseman's name is Who, the second baseman's name is What, and the third baseman is I Don't Know. These kinds of ambiguities are called equivocations, e.g., who is normally a pronoun and here, it is also a proper noun—the first baseman's name. Equivocation is a standard technique in writing comedy. In science, equivocation is an egregious error: a classic fallacy of ambiguity (see Barker, 1995, p. 163).

Lea's statement blurs at least three meanings of natural, i.e., *found in nature* (the only meaning of interest to me), *lifelike*, and *not artificial*, into an equivocation fallacy. At (i) above, Lea uses the word natural to mean *lifelike* (i.e., a lifelike picture at least to humans), at (ii) he uses natural to mean *found in nature*, then at (iii) the 'they' are natural—as in *not artificial*.

I think it is safe to say the word natural can mean just about anything one wants it to mean. A search of several dictionaries found 24+ distinct definitions for natural. I will consider just the three meanings of natural discussed above. Although equivocation makes it impossible to know exactly what Lea meant, here, I have tried to extract a precisely worded hypothesis. Pictures of natural objects are more *lifelike* (the opposite of artificial) than computer generated illustrations, and because they are more *lifelike*, they are more likely to resemble classes of real objects that are *not artificial*. It is possible to have found convincing evidence for this hypothesis and still have failed to discover anything about how birds classify objects in nature. Pictures can be lifelike without depicting any specific object in nature, e.g., a video of a glider flying in the wind.

Equivocation fallacies are treacherous. A second reading of Lea's hypothesis is that *pictures of objects found in nature* more resemble specific *objects found in nature* than they do artificially generated objects. This is the correspondence hypothesis we presented in our article (Weisman & Spetch, 2010) minus the important requirement that each instance of correspondence must be tested.

Yet a third meaning (perhaps what Lea meant) is a generalization of the second: *pictures of objects found in nature* more resemble *objects found in nature* in some general way more than they do artificially generated objects. This sounds plausible but Lea provided no means of testing his hypothesis.

When, as in Lea's hypothesis, three different meanings of a word are presented in such close proximity and without distinguishing among them, according to logicians (Barker, 1995), the word becomes meaningless. Because of the multiple possible interpretations, one cannot test a hypothesis from which meaning has been drained by equivocation.

The second part of Lea's argument is the claim that sets of pictures of natural objects are more ecologically valid than abstract patterns. In the context of comparative cognition, which is largely a biological science, one might assume that ecologically valid means derived from some ecological principle. Ecology is the branch of biology that studies relationships between animals and their natural environment. Without evidence of correspondence, it is unlikely that any ecologist would see validity in the use of human-defined pictures of objects.

Another idea is that pictures of *natural* objects possess ecological validity because the methods, materials and setting of the study approximate the *real-life* situation that is under investigation (Shadish, Cook, & Campbell, 2002). There is no way to determine whether this idea is correct, because Lea provides no methods, materials, or setting for a convincing experiment. Also, he tells us nothing about the real-life situation to which he intends his hypothesis to apply. 'Natural' and 'ecologically valid' are overworked buzzwords, drained of meaning by indiscriminant use and multiple definitions. The commentators are not the chief offenders in this indiscriminant use. For example, copywriters and advertisers have reduced *natural* to a part of the name of a laxative and a shortened version of the word *ecology* to the name of a dishwashing soap. It is probably too late for these words to be rehabilitated.

Eliminating equivocation from the ersatz correspondence hypothesis

Although Lazareva (2010) was generally critical of our article, I must congratulate her candor and clarity. Near the end of her commentary, she acknowledged that she had used the term artificial to describe a group of eight pictures of cars only because it would have been awkward to refer to a set of pictures, as Category A. Lazareva's acknowledgment is refreshing and helpful.

Labeling the pictures Category A has much to recommend it, but Lazareva should go further. What are needed are carefully crafted words that admit of only one meaning by their definition as scientific terms. Natural is a useful word when defined exactly and used always in the same way; but as already suggested, rehabilitation might not be an option. A better solution would be a newly minted word or symbol defined by one and only one meaning of natural, e.g., N/x for found in nature and A/x for not found in nature. To be more precise, pictures of cars should be labeled as members of Category A/hx , with A denoting pictures of artificial objects, and h denoting a human-defined category and with x changing with human-defined category membership. When referring to pictures constructed by algorithm, one might use A/ax , with x changing with the algorithm.

Obviously, I have not attempted a full nomenclature, but scientists commonly use this kind of renaming to avoid equivocation fallacies. I recommend this approach to students of pigeon visual categorization. And the suggestion is not a stretch: to avoid confusion (equivocation), biologists developed a binominal nomenclature to precisely identify species. Common species names take second place to the nomenclature. The International Union of Biological Science and their national affiliates revise and strictly control the binominal nomenclature. Similarly, one is not allowed latitude in labeling the chemical elements. Comparative cognition may not yet require such strictures but we need to develop procedures to ensure that scientists do not become endlessly creative with our nomenclature, i.e., create multiple meanings and equivocation.

Evidence for the ersatz correspondence hypothesis?

The commentators have sought evidence for their ersatz correspondence hypothesis. Lazareva (2010) depends mainly on evidence that pigeons can categorize photographs into basic-level human-defined categories (cars, chairs, flowers, and people) or superordinate-level human-defined categories (artificial and natural objects) but seem to learn different things about pictures in the artificial and natural categories (see Lazareva, Freiburger, & Wasserman, 2006). A second form of evidence was offered by Lea, Wills, and Ryan (2006), who have shown that various artificial pictures generated by algorithms are difficult for birds to learn and suggested that the reason for this difficulty is the lack of correlation with the features that characterize natural concepts. Be aware that the commentators confuse pictures of man-made objects with pictures constructed by applying abstract rules—yet another equivocation fallacy.

It is probably impossible to test a hypothesis about artificial categories when the definitions of artificial and natural are so fluid and ambivalent. That said, it is possible to suggest tests of some limited form of the ersatz correspondence hypothesis. For example, as experimental tests of the ersatz correspondence hypothesis, the reader might have envisioned a series of comparisons between discriminations of, at least, a few dozen categories of pictures of human-defined natural objects, a similar number of categories of pictures of human-defined man-made objects, and a like number of categories of artificial pictures derived by applying various abstract rules. And one might imagine, given the commentators' claims, that pigeons' discriminations of pictures of natural objects are always different from their discriminations of pictures of artificial objects and again, different from their discriminations of artificial pictures. The experiments just described have never been done—not even close. Except for a few tired human-defined categories of pictures of

natural and artificial objects, the basic premise of the ersatz correspondence hypothesis remains unconfirmed and to my knowledge, direct comparisons between human-defined pictures of natural objects and pictures generated by abstract rules are untried.

If our hypothetical experiments were to uncover one or two exceptions—say, a few categories of artificial pictures easier for pigeons to learn than one or two categories of pictures of real objects—the ersatz correspondence theory disappears in a puff of smoke. Conclusive tests would require many comparisons among pictures of highly disparate categories of natural objects, e.g., mountains, goats, rivers, and fish versus pictures of artificial objects, e.g., windowless office blocks, escalators, reservoirs, and submarines. And, as already suggested, there would need to be a convincing number of these comparisons. These experiments would be extensive but not difficult to perform. I think it is unlikely that the results would conform to the ersatz correspondence hypothesis.

Putting the explanation before evidence that anything needs explaining

No conclusive evidence of the sort proposed here exists to show that in the general case, categorization of pictures of natural objects is different from the categorization of any kind of artificial objects; yet, our commentators have already been busy inventing explanations for why it is so. Lea (2010) suggested that natural concepts are often signaled by only one characteristic. Jitsumori (2010, p.137) speculated, “natural objects . . . contain much richer information than do the man-made objects or the stimuli that vary only on a few physical dimensions” (the opposite of Lea's hypothesis and closer to our thinking).

Soto and Wasserman (2010a) invoked evolution as an explanation of faster learning about human-defined natural categories: “Visual systems may have evolved explicit mechanisms to exploit the statistical structure of natural scenes.” They made similar claims about evolution in their commentary (Soto and Wasserman, 2010b). The quote may sound impressive, but biologists have long rejected such speculative accounts of evolution as meaningless. Many things animals think and do are likely to be adaptive; the trick is to provide the sound scientific evidence. Without credible evidence, one has provided yet another “just so story.” “Just so stories” describe unverifiable and unfalsifiable narrative explanations for the biological traits and behavior of humans and other animals. If spinning out “just so stories” were all that was required, we would all be evolutionary biologists.

Soto and Wasserman (2010b) expect that scene analysis will provide explanatory models for picture perception.

But does scene analysis describe avian visual perception? The science of scene statistics (e.g., Ruderman, Cronin, & Chiao, 1998) is concerned with behavior of an ideal observer viewing photographs of various classes of natural objects. Here, natural scene has a precise definition: typically, a photograph of a human-defined natural landscape. So far, this is a science of human-like observers, with idealized (read computer-robotic) visual characteristics, which can be tightly linked to features of human perception (see Ruderman et al., 1998). Without denying that scene statistics might be useful to the study of human vision, they are at a disadvantage in the study of avian vision. Using human-defined scene statistics to study the categorization of pictures by birds is not really different from using human-defined pictures themselves. It is yet another human-defined categorization of pictures and thus, *ersatz correspondence* based on scene analysis creates another anthropomorphic definition of correspondence and another instance of the equivocation fallacy, i.e., machine-defined pictures equal human-defined pictures equal pigeon-defined pictures. The application of human-based scene analysis to avian species is prone to the same anthropomorphic equivocation errors as the idea that birds represent human-defined pictures of fish as similar to fish in nature (see Herrnstein & DeVilliers, 1980).

Having some experience at modeling avian perceptual processes myself (see Weisman, Hoeschele, M., Bloomfield, Mewhort, & Sturdy, 2010), I have some warnings to pass along. Showing that an idealized observer can solve a categorization problem is not the same as showing that it solves the problem in exactly the same manner as actual humans, and certainly is not the same as showing that it solves the problem in the same way as pigeons. So far, the evidence that scene analysis and similar simulations can solve categorizations problems is scant; the numbers of parameters and their logical relationship to perceptual processes are unclear; the fit between these models and human perception is unknown; and, to my knowledge, there have been no attempts to falsify these models by asking them to make discriminations that might lead them into making errors.

Soto and Wasserman (2010) should first develop an extensive library of successful and unsuccessful attempts to show correspondence between pictures and the objects they represent, then conduct scene analysis and other kinds of simulations on the pictures to determine the kinds of analyses and the parameters of those analyses that successfully separate successful and unsuccessful instances of correspondence. This approach could jump the *ersatz correspondence* hypothesis from a collection of anthropomorphic equivocation fallacies to sure-footed

descriptions of nature.

Countering misleading statements about our critique.

Single-dimension explanations

This section deals with the commentators' errors in summarizing our argument and ideas. I present these so that readers will not be misled by the commentators' missteps. For example, Lazareva (2010) claimed we ruled out any studies that use stimuli that have only simple dimensional features (e.g., Remy & Emmerton's, 1989, fine research on *uv* perception in pigeons). We never said that. What we said was "knowledge about correspondence between pigeons' representations of pictures and real objects is crucial to understanding the results of experimental tests using only pictures" (Weisman & Spetch, 2010).

Another point of view on single dimension explanations

Lea (2010) has claimed that pigeons and other birds mainly use single dimension solutions to recognition problems. The notion that some single feature common to pictures or sound recordings of conspecifics is generally responsible for correspondence with nature is certainly wrong. For example, Galef's (2008) Japanese quail are drab and uniformly colored, yet females can recognize an individual male quail in videos and in life. A male's success in simulated aggressive interactions and sexual encounters: subtle combinations of cues serve to mark him in females' eyes. Female quail can sum visual and behavioral experiences of a male into a judgment about his desirability as a mate.

Research on birdsong provides further evidence against Lea's hypothesis that birds use mainly one or two features or dimensions to make decisions about conspecifics. From extensive field research with dozens of species, it is now well understood that songbirds collect information about several perceptual features (e.g., number of notes, note duration, and the harmonic structure of notes (see Nelson, 1988).

Because they built correspondence between their stimuli and nature into their research designs, neither Galef (2008) nor Nelson (1988) needed to speculate about the polymorphous sources of features their birds used to recognize conspecifics. Their experiments suggest Lea is wrong in this claim. On the positive side, by the end of his commentary, Lea largely agreed with our conclusion about the importance of correspondence.

Ad Hominem Tu Quoque as commentary

Using some convoluted language, Soto and Wasserman (2010) comment on my colleagues and me (see Weisman,

Williams, Cohen, Njegovan, & Sturdy, 2006) using sinewave tones in operant discriminations in zebra finches, because such discriminations do not correspond to anything that would be encountered by these birds in nature. In a similarly motivated comment, Soto and Wasserman (2010) asked what my coauthor, Marcia Spetch's demonstrations (see Spetch & Friedman, 2006) of correspondence between pictures and objects not found in nature could tell us about the evolution of cognition.

I need to point out several flaws in Soto and Wasserman's claims. The first error is the most serious: a logical fallacy called *Ad Hominem Tu Quoque*. Someone captive to this fallacy would question the advice of a physician to quit smoking because the physician herself smokes. Applied here, the fallacy is that our claims about the importance of correspondence must be suspicious or false because they are inconsistent with our own behavior. As many readers will know, the truth of our critique is quite independent of whether Spetch and I have sought or shunned correspondence in our own research.

In an error specific to my research, Soto and Wasserman (2010) assumed birds never perceive sinewave tones in nature. Some birds (e.g., black-capped chickadees and white-throated sparrows) sing virtually pure sinewave song notes. As important, it is a well-known fact of perceptual science that birds' and mammals' ears and brains are so constructed as to resolve out the sinewaves in acoustic stimuli into tonotopic maps that represent pitch. In particular, birds are remarkably able at using the pitches of the loudest frequencies in conspecifics' calls to determine that the calls are in species typical pitch ranges (see Nelson, 1988). Soto and Wasserman (2010b) were mistaken about whether birds resolve the pitches out of sounds in nature and were answerable for fallacious reasoning when they sought to relate our behavior to the truth of our critique.

The most important reason to discount Soto and Wasserman's (2010b) complaint that Spetch and Friedman (2006) studied objects not found in nature is that it is wrong with respect to nature as free-living pigeons and homing pigeons see it. These birds use human-constructed objects and landscapes in conjunction with sun and magnetic compasses to navigate in their world (Keeton, 1969). Millions of pigeons live free in cities, and the sport of pigeon racing involves the release of trained homing pigeons at considerable distance from their home lofts, with a reasonable expectation of their return from nature. Free-living and homing pigeons navigate, in part, using beacons and landmarks of human origin (Bingman, 1998); Spetch and Friedman's (2006) research is an important step toward bringing the study of navigational landmarks into the laboratory. Perhaps, Soto and Wasserman

(2010b) are concerned about the size of the artificial objects in Spetch and Friedman's (2006) study relative to the size of human constructed beacons viewed by pigeons in flight. But surely, they would be quibbling. From high in the air, a beacon (say, a church steeple) appears small, and flight past it would provide multiple views.

Correspondence is pivotal not peripheral to understanding visual cognition

My penultimate criticism of Soto and Wasserman's (2010) commentary is that they sought to depreciate research on picture-object correspondence by claiming that it is of little relevance to research in object recognition. Here, we have supplied a test of relevance: a recent review of picture-object correspondence (see Bovet & Vauclair, 2000) was able to find 65 relevant experimental tests of correspondence and has now collected 41 citations. Soto and Wasserman (2010) much underestimated scientists' interest in the importance of picture-object correspondence to establishing the validity of a researcher's use of pictures to describe how birds perceive and categorize objects in nature. Readers are directed to our critique (Weisman & Spetch, 2010) for a more extensive argument in favor of the routine use of correspondence tests in avian visual category research.

An absurd equivocation fallacy

I conclude my discussion of errors in the commentaries with one last *equivocation fallacy*. Soto and Wasserman (2010) claim correspondence is irrelevant to the study of evolution of cognition because animals are never called upon to demonstrate picture-object correspondence in nature. Notice that the authors have blurred the distinction between (i) correspondence as a test necessary for establishing the validity of the link between the pictures used in a research program and explanations of nature, and (ii) the question of whether correspondence testing itself is a natural process with adaptive value—which is absurd. The error is equivalent to claiming that polymerase chain reaction DNA testing (Cline, Braman, & Hogrefe, 1996) is irrelevant to the study of evolution because animals never test their DNA in nature—which is also absurd. It should be obvious that many protocols, e.g., correspondence testing and familial DNA testing, are useful to understanding evolution without themselves being part of evolution.

Final Thoughts

Under most circumstances, my colleagues, whom I have critiqued here, are sound thinkers and adroit experimenters. The original proposition we presented seems both innocuous and reasonable, i.e., that the explanatory power of visual category research is amplified greatly by ensuring that

birds see objects found in nature in the pictures presented in category discriminations. It is dismaying to me that as I demonstrated here, Lea (2010), Lazareva (2010), and Soto and Wasserman (2010b) responded to our ideas with so many factually and logically flawed objections. I hope readers, particularly reviewers and editors, have gained a fuller understanding and appreciation for our position on the importance of correspondence between pictures and the objects. My impression is that our view has sounder basis in fact and logic and should prevail.

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