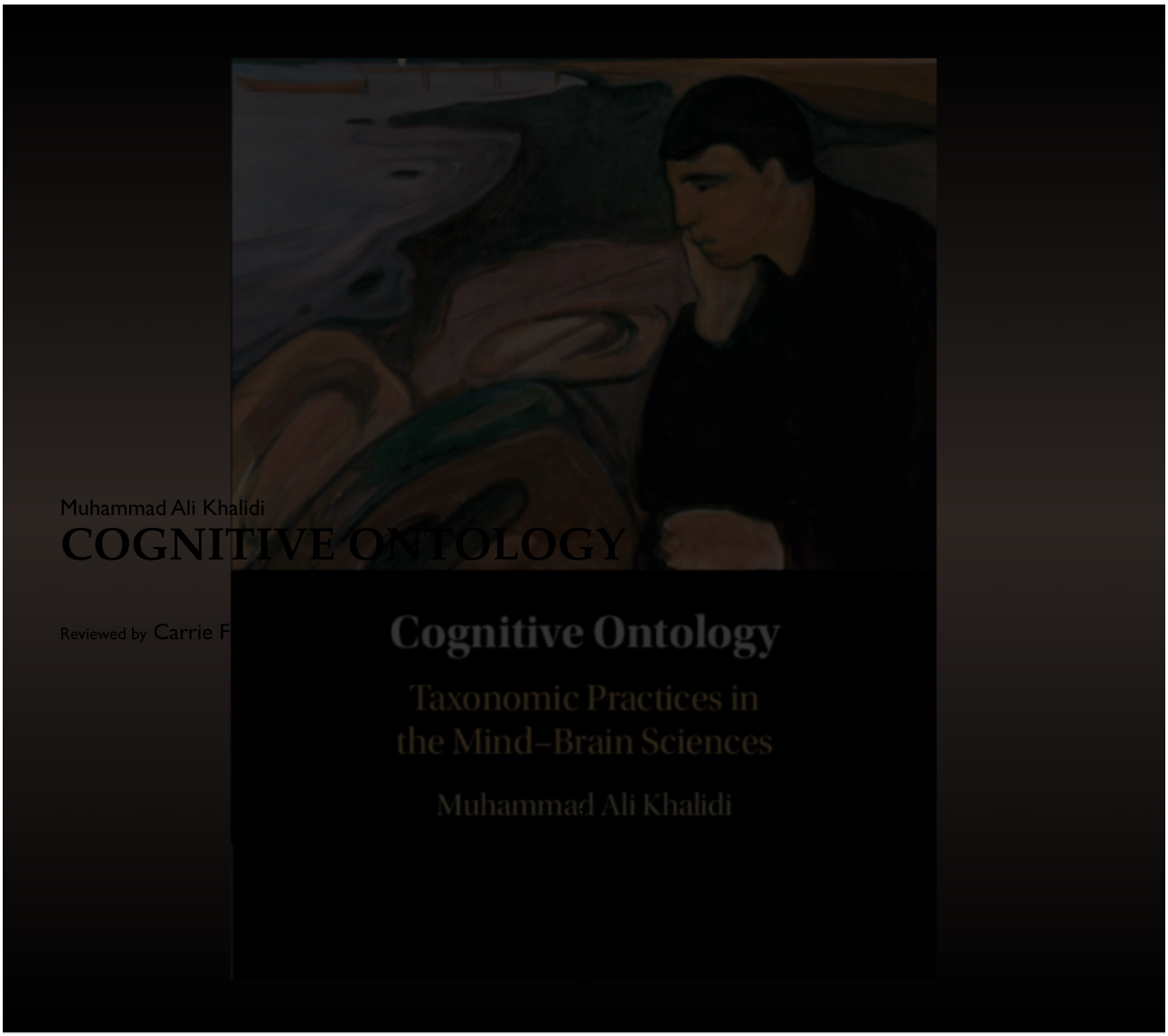

COGNITIVE ONTOLOGY

Muhammad Ali Khalidi

Reviewed by
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Cognitive Ontology: Taxonomic Practices in the Mind–Brain Sciences
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'Cognitive ontology' currently refers to theorizing about psychological conceptual revision in the light of a general failure to obtain one-to-one structure–function mappings between neural activation patterns

observed (mainly) via fMRI of human brains while their owners perform psychological tasks. Muhammad Ali Khalidi's *Cognitive Ontology: Taxonomic Practices in the Mind–Brain Sciences* shares this concern with how our best human psychology should individuate its cognitive concepts and the categories to which they refer. Khalidi favours a mildly revisionist outlook within a non-essentialist yet realist framework—we won't wholly abandon many concepts inherited from folk psychology, for example. He also affirms a form of non-reductionism about human cognitive kinds in which a distinction in individuation practices explains and justifies many-to-many structure–function mappings: cognitive kinds are externalistically individuated, while neural kinds are not (or 'not usually'; p. 22, note 13). But the main purpose of the book is to put Khalidi's causal-nexus account of natural kinds to work in cognitive science as it revises its ontology, in response to neuroscience or whatever else. In what follows I hope to convey my overall assessment that Khalidi's book is informative, challenging, and flawed in philosophically interesting ways.

On Khalidi's view, real kinds can be identified with clusters or nexuses of properties linked by causal connections. Unlike Boyd's homeostatic property cluster account, these causal networks or nexuses need not be unified by a single underlying causal mechanism to explain their existence. Khalidi also holds that there is no deep metaphysical difference between kinds and properties—he's a realist about both. The core issue here is thus whether certain superordinate cognitive properties should be in our cognitive ontology. What should our ontological commitments be when it comes to the broad, undifferentiated cognitive categories that scientific psychology has inherited from folk psychology? The causal-nexus account is used to support an affirmative answer or a negative one across a variety of cases.

The causal-nexus view and suite of background metaphysical commitments are introduced in Chapter 1. The chapters between that and the epilogue (chap. 9) use the causal-nexus account (for the most part) to defend some cognitive categories as real and deserving of a place in cognitive ontology, and others as not real although some subordinate category might be. Concepts (chap. 2), innateness (chap. 3), domain-specificity (chap. 4), episodic memory (chap. 5), and body dysmorphic disorder (chap. 8, co-authored with Amy MacKinnon) are among the real kinds. 'Language-thought processes', a more precisely articulated version of the Safir–Whorf hypothesis, instead subdivides into two distinct subordinate kinds (chap. 6); and while heuristics is not a kind, the myside heuristic subcategory is (chap. 7, co-authored with Joshua Mugg). Diagrams of causal nexuses of properties (listed on p. ix) illustrate many of these applications. These are discussed in each chapter in the context of expert mini-literature reviews that provide excellent, if intentionally non-exhaustive, summaries of the debates over each concept (or category). These will be helpful for veterans in these debates as well as those who are new to them.

The book's main goal of showing us why we should (or should not) think these are real cognitive kinds is conditional on accepting at least for the sake of argument Khalidi's causal-nexus account of cognitive kinds. He does not defend this account in detail; for example, we are not given principled constraints on which causal nexuses (or nodes in causal networks) within the cognitive sciences we should consider kinds and which not, assuming not every causal nexus should count as a kind. (His earlier book, *Natural Categories and Human Kinds* ([2013]), discusses this account at more length, albeit in the context of distinguishing special-science kinds from physical or chemical kinds, not that of determining which special-science kinds there are.) In contrast, the main goal is independent of his defence of non-reductionism and many-to-many structure–function mappings, which is just as well. For Khalidi, externalistic individuation of cognitive kinds includes aetiological (ontogenetic and phylogenetic) and environmental factors. But phylogeny plays an ineliminable role in the individuation of human neural kinds: human brains are primate brains, whether 'human' refers to hominins or *H. sapiens*. And any (*ceteris paribus*) categorical differences between brains in different human

populations are likely to be individuated in part by ontogenetic and environmental factors, just as population-level cognitive ability differences would be. We can also expect integrated neurocognitive kinds that eschew the dualist tradition of medium-independent functionalism, given that cognitive science 'straddles the biological and psychological sciences' (p. 7) and reality is not easily divided into a 'layer cake' (p. 14). Many-to-many mappings may be expected, but a coarse distinction in taxonomic practices in psychological and neural sciences won't be the reason why.

General observations aside, then, does the causal-nexus view illuminate the kindhood (or not) of the selected cases? I think it's a mixed bag. The causal-nexus account may work for justifying cognitive kinds that have (or had) been accepted (or mild revisions of them), but not for identifying new cognitive kinds. For example, Khalidi makes *prima facie* reasonable cases for affirming kindhood for the superordinate category of concept (or the concept of concept—for brevity, I'll focus on categories), or for both the superordinate category memory and a subordinate category episodic memory (although he argues for the kindhood of the latter capacity, and the states it generates, independently of whether memory is also a kind). Both superordinate categories (concept and memory) have been subject to eliminativist attacks in recent years. Episodic memory, meanwhile, has been characterized in terms of phenomenal (autonoetic consciousness) and aetiological (personal history) features. Are we lumping together distinct kinds? Khalidi argues that these features do converge in a single real kind on the basis of a possible evolutionary scenario in which past personal information and a distinctive phenomenology are functionally co-adaptive (p. 149).

I leave readers to investigate most of the other chapters. In the space remaining, I'll discuss two of Khalidi's marquee cases, innateness and domain specificity, where I don't think he succeeds in showing why we should accept these superordinate categories as kinds. They are still properties—saying they are not kinds does not entail nominalism or eliminativism. It does mean they should continue their slide towards dispensability in serious cognitive scientific theorizing, hypothesis formation, and experimental test.

The innateness chapter (an updated version of his [2016] article) begins with a list and discussion of the competing definitions that have been offered of innateness (p. 77) and the various properties associated with these definitions (triggerability, lack of learning, invariance across a broad range of environments, early onset, canalization, and so on; p. 80). The causal cluster they allegedly form is illustrated by a diagram (fig. 3.1, p. 88): triggerability (acquirable in conditions of informational impoverishment) strongly causes not learned (acquirable without inference, experimentation, repeated observation, or other forms of learning) and weakly causes invariant (acquired across a range of environments); not learned weakly causes early onset (acquired relatively early in ontogeny); invariant strongly causes pan-cultural (present in all cultures) and weakly causes canalized (buffered against environmental variation); and so on.

While he asserts that these properties are conceptually distinct and concedes that the causal connections may not be obvious (p. 79), to me the conceptual connections are obvious and we agree the causal connections aren't. For example, what is acquirable on the basis of relatively impoverished input (triggerable) is acquirable without relatively rich input (not learned): these concepts exist on a continuum of richness of input (assuming an empirically measurable unit of richness, which we don't have). Khalidi agrees: triggerability and learning are two ends of a continuum (p. 81). But different values of a continuous variable are not independent, and so cannot be linked causally. Similarly, it is not clear that not learned (or lack of learning) causes early onset (or early acquisition), even if one agrees it is a cogent inference to the best explanation from early onset to not learned. Certainly no empiricist would agree. I don't doubt the concepts (or properties) in the flowchart are associated in a nativist's conceptual cluster (specifically, the core cognition

research programme; p. 80). And if a nativist account is given of one cognitive capacity, it's more likely than not that a nativist assessment of similar observable features will be made for other capacities. But without the mind-set of a nativist, there is no cluster, hence no causal cluster. Real kinds are not supposed to be mind-dependent in this way.

Fundamentally, I'm puzzled about the motivation for 'rehabilitating' innateness (p. 77). Innateness must be externalistically individuated, even though intuitively what's innate is not supposed to depend on external influences. Of course, no thoroughly modern nativist thinks genes determine phenotypes; innateness is a matter of degree, as Khalidi notes. But this is why once-raging debates over innateness have been marginalized in recognition of the complex interplay of genetic, epigenetic, bodily, and extra-somatic environmental factors in phylogeny and ontogeny, plus differences in researchers' explanatory emphases and interests. The proposal doesn't even cohere all that well with Khalidi's naturalism: the cognitive neuroscientific practices that help motivate his affirmation of many-to-many structure–function mappings don't treat innateness as a kind. In short, I wonder why Khalidi doesn't reach the same conclusion regarding innateness that he does with heuristics: that it is a superordinate property that is not a real kind but which may encompass one or more real kinds.

My response to his defence of domain specificity as a real kind is similar. The intuitive idea of domain specificity involves a restriction in a capacity's utility or application assessed relative to some intuitive idea of usefulness or success (p. 111). Khalidi doesn't provide a causal flowchart for this concept, although he does argue that all domain-specific cognitive kinds are innate, while not all innate cognitive kinds are domain-specific. (This would explain the presence of domain specificity—more precisely, the concept of domain specificity—in many nativists' conceptual clusters.) Instead, he offers a 'suitably described' version of the concept with a two-criterion definition; this mildly revised concept allegedly picks out a cognitive kind. The first criterion holds that a construct is domain specific if it is generalizable in principle. This rules out vacuous domain specificity, such as for bodies of knowledge (so here Khalidi departs from those who affirm domain-specific core knowledge). Non-vacuous domain-specificity is thus restricted to capacities (or abilities) or rules (or principles). The second criterion invokes the notion of adaptive (evolved, proper) function to define a domain: a domain-specific capacity or rule is one that systematically fails to yield a correct output (or any output) given inputs that it did not evolve to deal with. Evolution provides the objective criterion of usefulness or success. All domain-specific capacities or rules are adaptive (p. 100; Khalidi uses 'adaptive' as equivalent to being an adaptation—that is, being selected for).

This chapter too left me disappointed. Couldn't a restricted cognitive ability be a spandrel or an exaptation? More broadly, psychology has been agonizingly slow to move beyond 'evolutionary psychology's' attempt to explain the evolution of human cognition in terms of adaptation alone. Do we need to feed this albatross? Everything biological is restricted in some way in its form and/or function; the question is how (or by what). These details will tell us its actual phenotypic variability in a population, how restricted it is compared to other traits given that variability, what potential variation (or 'reuse') it may manifest in response to which relevant changes in its internal or external contexts at different timescales, and so on. And even if we could identify a human cognitive capacity's adaptive function (or functions) without just-so stories, that effort will almost inevitably lead to cognitive kinds that are not human-specific.

Consider how easy it is to neutralize new evidence that an allegedly domain-specific human capacity generalizes. Using the example of face recognition (p. 113), the dialogue might go as follows: It's a (domain-specific) human-face-recognition capacity—No, it's any-face recognition, so dogs are already included—No, it's

a particular-configuration-of-salient-parts capacity, so houses are already included. Khalidi suggests it's likely the capacity evolved to detect human faces given their social salience. Just so. But it would be even more adaptive to recognize the faces of prey and predators (including humans): how else would we know in what direction to run or where best to aim the rock? A few false positives are worth the cost. And since empirical tests show we share face-recognition abilities with at least some other species, we need to know more about its phylogenetic extent before we can even begin to isolate the relevant ancestral environment and adaptive function. If this capacity emerged in phylogeny prior to hominins or *H. sapiens*, but we only use it for face recognition, is it domain-specific in us but not domain-specific *tout court*? Would a potential for utility wider than any adaptive function we might identify make it domain general even if that potential is not yet actualized? Any reasonable position on these questions will rest on investigating the many factors in phylogeny, ontogeny, the environment, and their interactions (for example, gene– environment co-evolution) to discover the constraints on the actual and potential functioning of any cognitive capacity, however those constraints compare to those of other cognitive capacities. At the very least, we should be far more cautious about judging a cognitive capacity's comparative restrictiveness based on the current absence of information about cognitive evolution and development, particularly in the case of non-humans.

In short, the more cognitive science (and psychology) takes on board what the biological sciences have to tell us about evolution and development, the less innateness and domain-specificity will have any serious explanatory role to play. Domain-specificity, unlike innateness, did not even have a folk history we might want to preserve. These concepts may retain residual utility for gesturing towards some subset of explanatory factors rather than others, but interesting and testable hypotheses in scientific psychology can do without them.

So—to return to the main theme of the book—what should the ontological commitments of cognitive science be when it comes to broad cognitive categories, particularly those inherited from folk psychology? Khalidi's responses to this question offer the reader plenty to think about, whether or not you agree with his basic metaphysical commitments or with his conclusions about the particular categories he discusses.

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