Unencapsulated modules and perceptual judgment

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To what extent are cognitive capacities, especially perceptual capacities, informationally encapsulated and to what extent are they cognitively penetrable? And why does this matter? (I will suppose that a penetrable system is just one that isn’t encapsulated, and conversely.) There are a number of reasons we should care about penetrability/encapsulation, but I will focus on two: (a) encapsulation is sometimes held to be definitional of modularity, and (b) penetrability has epistemological implications independent of modularity (in fact, it’s sometimes held that if modularity has epistemological implications, it’s because of encapsulation (Fodor 1985, though see Lyons 2009 for a contrary view)). My main concern is with (b), but I begin with a discussion of (a). I argue that modularity does not require encapsulation; that modularity may have epistemological implications independently of encapsulation; and that the epistemological implications of the cognitive penetrability of perception are messier than is sometimes thought.

**1. Modularity and encapsulation**

Recent discussions of modularity owe a lot to Fodor (1983). Fodor deliberately declines to define ‘module’ and explicitly insists that modularity comes in degrees, but at the same time, he offers a set of nine diagnostic features that, if taken as definitional of modularity, provide a quite demanding theory of modularity. The most important of these features are speed, involuntariness, innateness, domain specificity, introspective opacity, and informational encapsulation. Though few endorse Fodor’s view in its totality, one of the major theses of the book was that cognitive capacities that have some of these properties---“to some interesting extent” (p. 37)---tend to have the others---again, to an interesting extent. This is still an important insight, despite the vagueness of the ‘to an interesting extent’ clause; and it is, as far as I know, widely regarded as true, despite what Fodor’s opponents and his more recent self have done to obscure this contribution by focusing on a sharper but more extreme version of the original proposal. Even if learning informs the development of perceptual systems, for example, they’re still innately constrained “to some interesting extent”; even if they are cognitively penetrable, such cognitive penetration is not entirely rampant; etc. Fodor starts out careful to deny that the nine features are either categorical (e.g., a system need only be innate *to an interesting extent* to count as modular) or definitional of modularity, though he sometimes (here in 1983, but even more so in 1984 and 2000) acts as if they are both.

Fodor’s work on modularity embodies several distinguishable theses; three are worth singling out here. I have already mentioned a claim we might call the Cluster Thesis, which holds that capacities exhibiting some of the aforementioned properties tend to exhibit them all. A second assertion, the Input Thesis, claims that all and only the input systems are modular, “central” systems allegedly lacking these nine properties. These are both distinct from the Plurality Thesis, which claims that the mind is not a single, indivisible Cartesian/Lashleyan whole, but a collection of relatively independent systems. The first and third theses, especially, are fairly uncontroversial, although it is easy to read Fodor as endorsing implausibly radical versions of all three claims, by (a) insisting that the nine diagnostic features constitute necessary and sufficient conditions for modularity, (b) that these features must be present to more than just some interesting extent (e.g., that modularity requires a level of innateness that precludes genuine perceptual learning), and (c) that systems failing to satisfy these very strict criteria are therefore radically Quinean (Fodor 2000) and inseparably intermingled. I don’t claim that this is, in fact, Fodor’s view but only that it’s not hard to read this into him.

Just as a suitably understood version of the Cluster Thesis is pretty uncontroversial, so too there is some version of the Plurality Thesis that should be widely acceptable. This is important, because the Cluster and Input theses presuppose the Plurality Thesis, but not vice versa. A weak doctrine of modularity---or a doctrine of weak modularity---is committed to the Plurality Thesis, and it uses the term ‘module’ to refer to these relatively independent systems. This doctrine of weak modularity is one that I’ve articulated elsewhere (Lyons 2001), but it’s worth reiterating some of the highlights here. This modularity doctrine doesn’t require innateness, even to an interesting extent; it doesn’t require speed, introspective opacity, etc. It does require something like domain specificity and something superficially like informational encapsulation, though it turns out that the differences between these and what the doctrine does require are deeper than the similarities.

When we talk about a “system for face recognition” or the like, we are talking, to a first approximation, about a unified and separable entity that performs the task of face recognition. Although we name tasks by reference to their outputs, it is better, I argue, to think of tasks as input-output functions; and it is convenient for expository purposes to adopt an extensional understanding of functions: as sets of ordered (input-output) pairs. Modularity is a partly implementational concept, and we need to think about the mechanisms, or substrates, that compute these tasks. Suppose a substrate *S* computes a function. I say that *S* **specializes** in *T* iff *T* is an exhaustive specification of the input-output function that *S* computes. This is merely a restriction on the naming of systems (one that is frequently violated without much harm); strictly speaking, it’s not a *face recognition system* if it’s also involved in recognizing individual cows, bird species, etc. *S* is **isolable** with respect to task *T* iff *S* computes *T* and could do so even if no other substrate computed any functions. That is, *S*, if given one of the inputs of *T*, is capable, by itself, of producing the appropriate output, without the assistance of other substrates. Isolability is thus a counterfactual issue about the computational capacities of a substrate. Finally, we need to distinguish parts of tasks from subtasks. A **subtask** is a task that is computed by a mechanism on the way to computing something else; a **part** of *T* is simply a subset of the input-output pairs that constitute *T*. *S* is **unitary** with respect to *T* iff no proper part of *S* specializes in and is isolable with respect to any proper part of *T*. Unitariness ensures that our substrates will be the smallest mechanisms needed for the computation (my left-inferior-temporal-cortex-plus-the-doorknob doesn’t compute anything that left IT doesn’t compute by itself) and that the substrates and tasks are non-gerrymandered (if there’s a system for visual face recognition and one for auditory melody recognition, there will be a disjunctive substrate that computes face-recognition-or-auditory-melody-recognition, but the substrate won’t be unitary with respect to this task).

This gives us a theory about cognitive systems: *S* **realizes a system** for *T* iff *S* is isolable with respect to *T*, is unitary with respect to *T*, and specializes in *T*.

I think these cognitive systems are what people mean by the term ‘module’ these days,[[1]](#footnote-2) so I’ll simply call them ‘modules’ (in fact, it’s what I’ll mean by ‘module’ henceforth), and we can come up with another term, like ‘F-modules’, for the ones that satisfy Fodor’s criteria. Now perhaps many modules, even in this weak, non-Fodorian sense, happen to be domain specific and informationally encapsulated “to some interesting extent.” But notice how different task specificity is from full-blown domain specificity, isolability from informational encapsulation. Anything that computes a function is trivially task-specific, but domain specificity---whatever exactly that is---is surely intended to be harder to come by. A system that specialized in the first order predicate calculus would be task specific but would presumably not count as domain specific. It is hard to know, as the notion of domain specificity has never, to my knowledge, been spelled out in nearly as much clarity as has the notion of computation of functions. One not insignificant advantage of my view over some other views (e.g., Fodor 1983, Coltheart 1999) is that it doesn’t require us to figure out what counts as domain specificity.

More importantly, isolability and encapsulation should not be confused. My brain is isolable from your brain (each can compute even without the aid of the other), but the fact that we’re communicating means they aren’t informationally encapsulated from each other.[[2]](#footnote-3) Isolability is about system boundaries, about what is required to have an intact computational device; encapsulation is a matter of where a given system gets its inputs from. I assume that *S*1 is encapsulated from *S*2 iff *S*1 does not receive any inputs from *S*2. This strikes me as the natural view of encapsulation, although Carruthers (2006) offers a surprising alternative. For some reason---perhaps he is thinking of encapsulation as a monadic property---he starts by defining encapsulation as a mechanism’s being unable to draw on outside information *in addition to its input*. Because the natural way to understand input is just as whatever information a mechanism draws on, there is an obvious threat of trivialization, which Carruthers handles by defining ‘input’ in a more restricted way. These complications vanish on a two-place relational understanding of encapsulation, as just described. If we want a monadic conception of encapsulation (outright) as well, we can say that an encapsulated system is one that doesn’t receive inputs from *any other* system. Obviously, the only systems that might satisfy the monadic conception would be “input” systems that take their inputs from sensory transducers, rather than other cognitive systems. Many “central systems,” however, may be encapsulated from each other and from various input systems.

Fodor has argued (1983, 2000) that encapsulation has a special role to play, that even if the rest of the nine criteria are optional, encapsulation really is a necessary condition for modularity, presumably at least partly on the grounds that “it is a point of definition that distinct functional components cannot interface *everywhere* on pain of their ceasing to be distinct” (1983, p. 87). But this isn’t right. Isolability suffices for distinctness, even though isolability imposes no restrictions at all on information exchange: *a* and *b* could be isolable even if they shared everything, so long as that sharing was unnecessary. If *a* could go on without *b*, then *a* is isolable from *b*. Consider again my brain and yours; if we had perfect telepathy, the brains would “interface everywhere”, but this would not imply that they were not distinct (this is especially obvious if the telepathic communication were voluntary).

It is also important to point out that isolability is different from dissociability, at least if the latter is read as indicating an ability to produce the normal outputs in the absence of other mechanisms. Stokes and Bergeron, in an unpublished paper, point out that my understanding of modularity is superior to Carruthers’s (2006), at least as they understand him.[[3]](#footnote-4) They view Carruthers (see 2006, p. 2) as holding that dissociability is a hallmark of modularity, and although he is not fully explicit about this, as holding that *S*1 is **dissociable** from *S*2 iff *S*1 could operate normally even if *S*2 were removed. This would be the same as my understanding of isolability, *if* normal operation were understood in terms of performing input-output mappings, but it is a very different notion if understood in terms of producing the normal outputs. *S*1 might receive indispensable inputs from *S*2, in which case, the removal of the latter would prevent the former from operating normally, i.e., from producing its normal outputs. *S*1 and *S*2 might, however, still be isolable from each other, in that even though *S*1 needs inputs from *S*2, *S*1 is still capable of performing its input/output mapping without *S*2; i.e., *S*1 could, *if given the inputs it would normally have received from S2*, produce the appropriate outputs without further assistance from *S*2. I’m not sure that this is how Carruthers intends dissociability; perhaps he has something more like my isolability in mind. In any case, this contrast illustrates the proper understanding of isolability and the theory of modularity that incorporates it.

Suppose perception is cognitively penetrated by beliefs, desires, and the like. Then the perceptual systems are receiving input from higher cognitive mechanisms and are therefore not encapsulated. This is not by itself any threat to the existence of perceptual modules, however, for it is no threat to the isolability of the substrates responsible for perception. It may be that the reason beliefs influence perception is that there is really no distinction between the module responsible for those beliefs and the module responsible for perceptual states, i.e., that belief-production and percept-formation are both part of the task of a single, indivisible module. But another potential reason is that, although perceptual modules are distinct from belief-forming systems, the former receive inputs from the latter. The mere fact of top-down influence (i.e., on perception, from beliefs, etc.) is compatible with either possibility. Even if the inputs were indispensable, in the sense that the perceptual systems would be incapable of producing percepts without input from higher cognition, this would not threaten the distinctness of the perceptual systems; their isolability requires only that they be able to compute a certain perceptual function *if given certain inputs*: in this case, inputs from higher cognition; it is the mapping, not the output, that they must be independently capable of.

F-modular systems are, by definition, encapsulated, and this has important consequences for the frame problem, locality of computation, and other issues of tractability, as Fodor (e.g., 1987, 2000) has rightly pointed out. Consequently, we should want to know how many modules in the present, weaker sense are also F-modules. But this weaker understanding of modularity is by itself sufficient to flesh out the Plurality Thesis, and that thesis remains a substantive and insightful claim. It is, as I mentioned earlier, quite plausible, especially in light of the various neuropsychological dissociations, but it is far from trivial. These dissociations are, of course, quite surprising from a commonsense perspective and the architectural thesis they support is highly revisionary of our pretheoretic assumptions.[[4]](#footnote-5)

**2. Penetration and encapsulation**

Before turning to the implications of encapsulation, penetrability, and modularity for the epistemology of perception, I should say a bit more about cognitive penetrability and its relation to encapsulation. When a module (or system, or capacity) is described as being *cognitively* penetrable, something more is being claimed than mere failure of encapsulation. Talk about cognitive penetrability is usually intended to indicate a top-down influence---in particular, an influence by the occurrent (and perhaps fleeting) beliefs, desires, fears, goals, etc. of the cognizer. The McGurk effect (McGurk & McDonald 1976) is a nice illustration of penetrability without cognitive penetrability. Vision influences audition (seeing /ga/ makes you hear /da/ instead of /ba/), so this auditory system is not encapsulated from vision. However, the effect is a classic example of *cognitive* impenetrability; knowing the trick behind the illusion does nothing to dispel it, any more than knowing about the Mueller-Lyer illusion affects the relative apparent lengths of those lines. What we know isn’t influencing what we hear, but what we see is. I’ll call this “lateral penetration” to distinguish it from cognitive penetration. The latter is a species of top-down influence, while the former involves influence from one system to another that is at least approximately at the same “level” as itself.[[5]](#footnote-6)

Lateral penetration and cognitive penetration can interact in interesting ways, making certain modules indirectly cognitively penetrable. If vision were cognitively penetrated in McGurk cases, and cognitively influenced visual states went on to laterally influence auditory experience, these auditory experiences would count as cognitively penetrated, even though the connection was indirect.

Yet ‘receives input from’ is not transitive. It’s not the case that if module *B* receives input from *A* and *C* receives input from *B*, then *C* receives input from *A*, because it might be that, of all *B*’s outputs that were responses to inputs from *A*, none of these are ever fed to *C*, and so *C* never receives any information from *B* that was influenced by *A*. In such a case, *C* might remain encapsulated from *A* (all else being equal). It is tempting to claim that if some of *B*’s outputs that were responses to inputs from A *are* ever fed to *C*, then *C* is receiving inputs from *A* and is thereby penetrated by *A*. Maybe this works, but unless we’re smuggling a lot into the notion of inputs, it will have to be more complicated than this. Consider several cases:

1. Fodor’s heart rate: Fodor once (1988) joked about his heart rate being cognitively “penetrable” on the grounds that an intention to do calisthenics results in his doing calisthenics, which results in increased heart rate. Heart rate obviously isn’t a psychological phenomenon, but the general point is clear enough.
2. Change in fixation: the same moral applies when the “penetrated” capacity really is psychological. I deliberately move my eyes, or turn around, thus altering what I fixate on and thus what I see.
3. Change in attention: without moving my eyes, I change the locations or objects to which I am devoting attention, which affects my visual experience (e.g., the Necker cube shifts, or the subject in the old woman/young woman drawing now looks like the young woman).
4. Oculomotor efference copy: your eyes are paralyzed, but you try to move them to the left, thus causing an apparent shift or relocation of the objects in your field of view (Kornmueller 1931, Whitham et al. 2011). Let’s suppose that they way this works is that the intention causes the motor areas to send not only a signal to the oculomotor muscles but also an efference copy to the visual system so that the visual system can update a post-retinotopic representation accordingly. Thus the desire indirectly influences the post-retinotopic representation and the resulting visual experience.
5. Effort and distance: subjects who intend to throw a heavy weight to a target judge the distance to that target to be greater than do subjects who have no such intention (Witt et al. 2004). Suppose it works as follows: the intention to throw the weight causes an activation of motor readiness routines, where the readiness reflects the expected required effort. The visual system takes the degree of readiness as a cue to distance, with the result that more effort-requiring action plans lead to perception of longer distances. This hypothetical account is similar to another, perhaps more familiar, one:
6. Mindreading and covert mimicry: Suppose a visuomotor system for mimicking facial expressions feeds into a mindreading system, like so: perception of facial features activates motor plans for making the same expressions, thus sending (usually subthreshold) signals to one’s own facial muscles. At the same time, the somatosensory systems are informed to expect the relevant facial movements, and this information feeds into the mindreading system, which then attributes the emotion that corresponds to that expression to the person being perceived (Adolphs et al. 2000, Goldman 2006)

I think that 1-4 are pretty clearly not instances of cognitive penetration, as it is usually understood in the field, and that 5 pretty clearly is. 6 is not, in part because there is no belief, desire, or goal influencing the mindreading system. Consider a variant, however:

1. Mindreading and mimicry II: Suppose instead of (possibly unconscious) *perception* of facial features activating motor plans for mimicry, it’s the perceiver’s beliefs about facial expressions that produce signals to her own facial muscles. The rest of the story is the same.

Now we have a case of cognitive penetration of the mindreading judgment, even though the mindreading system itself is not directly penetrated. 1 and 2 are easy to handle, since it is obvious that the perceptual systems are not receiving inputs from the motor systems. Their inputs change as a result of the outputs of the motor systems, but that’s not the same as taking inputs. I’m not sure how to make this more precise, but I think that the difference between 1-4 and 5 is that in the former case, the causal chain is mediated by overt (1-2) or covert (3-4) *action*, or *behavior*, on the part of the cognizer. Notice that the covert behavior in 3 is something the agent is sometimes aware of engaging in, while in 4 it is completely unconscious. There can be activation of motor systems without the execution of actions, which is what is happening in 5-7, though again, I’m not sure how to make the notion of action more precise.[[6]](#footnote-7)

We can incorporate this into our understanding of penetrability in a few different ways; I’ll do it by including the absence of behavioral mediation as a restriction on inputs. (I think this is more of a terminological than a substantive matter.) *B* receives input from *A* only if *A*’s influence on *B* is not behaviorally mediated in the sense just glossed. Now, I think, we can safely endorse the earlier suggestion that if some of *B*’s outputs that were responses to inputs from A are ever fed to *C*, then *C* is receiving inputs from *A* and is thereby penetrated by *A*.

This gives us a fairly restrictive working understanding of penetration and an even more restrictive understanding of cognitive (as opposed to lateral) penetration. Cognitive penetration occurs when a system takes beliefs, desires, goals, or other similar person-level states as input, where “input” can be direct or indirect, but cannot be behaviorally mediated.[[7]](#footnote-8) I will use ‘influence’ to characterize the broader set of causal relations: 2, 3, 4, 5, and 7 are all instances of cognitive *influence* on perception, even though 2, 3, and 4 are not cases of cognitive penetration.

**3. Epistemological implications of cognitive penetration**

The classical literature on the epistemological implications of cognitive penetrability (e.g., Fodor 1984, 1988; Churchland 1988) and even some of the recent literature on the subject (Raftopoulos 2009, Siegel 2012) lead one to suspect that some or all of the following are common assumptions:

* that cognitive penetration of perception is epistemically worse than lateral penetration,
* that cognitive penetration is epistemically worse than other forms of cognitive influence on perception,
* that cognitive penetration of perception is epistemically worse than cognitive penetration of perceptual *judgment,*
* that replying to the epistemological worries generated by the possibility of cognitive penetration requires defense of encapsulated perceptual systems.

I won’t try to pin these views on anyone; whether or not anyone has explicitly held them, it is worthwhile to show that they are all suspect.

**3.1 Is cognitive penetration worse than lateral penetration?**

In some sense, I agree that cognitive penetration is epistemically worse than lateral penetration, though not for what I take to be the usual reasons. The usual worries about penetration seem to focus on (a) the failure of convergence among perceivers with different theoretical preconceptions, and (b) an illegitimate kind of self-corroboration, akin to epistemic circularity. I have argued elsewhere (Lyons 2011) that (a) doesn’t have any obvious bearing on an individual’s being epistemically justified in believing what she seems to perceive and that (b) fails to capture what is wrong with the bad cases of cognitive penetration. When a belief or desire influences perception, this is not a matter of the perceptual belief’s being epistemically *based on* the penetrating belief or desire, and if it were, all such basing would seem to have to have the same structure and hence the same epistemic status. But not all cognitive penetration is vicious. If my fear of snakes primes certain high-level object recognition templates, facilitating matching and thus making me *better* at detecting whatever snakes are in the woods around me, this would be an epistemically innocuous---perhaps even virtuous---kind of cognitive penetration of perception. If the fear makes me more likely to mistake sticks for snakes, however, or otherwise render me less sensitive to my actual environment, then this is an epistemically pernicious sort of penetration.

In addition, if the problem with cognitive penetration were about circularity, it would be hard to see what’s wrong with penetration from desires and fears, as opposed to beliefs. There is perhaps something vaguely (though, for reasons just mentioned, not strictly) circular about believing that p, which causes you to see that *p*, which causes you to believe that *p*. But desires are quite different. Furthermore, at least in the anecdotal cases, desires can have opposite effects: sometimes the desire that p makes me more likely to see that *p*, while other times a very strong fear that *p* (and thus desire that not-*p*) makes me more likely to see that *p*. It is hard to see how either could be a matter of circularity, given that they’re not beliefs, but even harder to see how they *both* could, given their opposite natures.

There is nothing about the *cognitive* nature of the penetrator that makes any difference here. What matters is whether the penetration---wherever it might originate---makes me a better or a worse perceiver. I argue (Lyons 2011) that ‘better’ and ‘worse’ are best understood in terms of a reliabilist epistemology, though there are other ways one might try to understand them.[[8]](#footnote-9) It might, as a matter of fact, turn out that lateral penetration is more likely to improve perception than cognitive penetration. But even if that is true, it makes the specifically cognitive nature of the penetrator incidental; what matters is whether the penetration makes for improvement or not.

**3.2 Is cognitive penetration worse than other kinds of cognitive influence?**

What about other kinds of influence, besides penetration? Fodor (1988), Pylyshyn (2003), and Raftopoulos (2009) are all insistent that Change of fixation and Change of attention do not involve cognitive penetration. Certainly it is true that different forms of cognitive influence on perception are importantly different and that using ‘cognitive penetration’ in a restrictive way is a good means of registering those important differences. It is not obvious that these important differences are *epistemically* important differences. A couple of paragraphs back, I described a hypothetical means by which fear of snakes might cognitively penetrate snake perception. Suppose it worked pre-perceptually, instead, by priming attentional mechanisms in such a way that attention is more likely to be captured by snake-like items in the environment. It still seems---to me, anyway---that what matters for the epistemology is whether this cognitive influence makes me better or worse at spotting snakes. If the former, then the influence is epistemically good; if the latter, then it’s epistemically bad.

Balcetis and Dunning (2010) describe the kind of “wishful seeing” that recent epistemological discussions have taken to be the paradigm of epistemically pernicious cognitive influence. Thirsty subjects see the water bottle as closer than do non-thirsty subjects; subjects see the gift card with what they are told is a $25 balance as nearer than the one with the $0 balance. Suppose these results are genuine. These distance judgments seem to be less justified than distance judgments that are not thus contaminated by desire. Does it matter, for this verdict, whether the effect in question is genuinely a case of cognitive penetration, in our narrow sense? Here are three possible mechanisms for the effect:

* 1. the direct cognitive penetration of vision by desire: subjects want the item to be closer, and this makes it look closer.
  2. a physiological effect: thinking they’re going to get money makes subjects happy, and this causes biochemical changes that affect perception in systematic ways, making objects look closer, hills less steep, etc.
  3. an attentional effect (Alter & Balcetis 2011): desired objects demand greater attention, and this makes them appear closer.

Some of these mechanisms might make us feel better or worse about the prospects for human perceptual judgment in general---an attentional effect, for example, might be easier to deliberately counteract. But the epistemic status in the present case doesn’t seem to depend on which mechanism is in play. If distance judgments are contaminated by desire, then this is epistemically bad for those judgments, regardless of the means of contamination. We will rightly care about the means for other reasons---curiosity about psychological mechanisms, concern for the contamination of other judgments---but those reasons seem disconnected from the epistemological verdict about the individual case.

Mechanism b), just considered, would be an instance where the mediation isn’t even entirely psychological. Consider a similar possibility that is another variant of Mindreading and covert mimicry. It is known that manipulating facial expressions affects subjects’ assessments of their own moods (Strack et al. 1988), and if simulation plays any role in mindreading, something like the following seems possible:

1. Perverse mimicry: I’m holding a pencil in my mouth sideways, which makes my facial muscles approximate a smile, and this makes me more likely to attribute positive emotions to others than I would have been without the pencil.

My intention to put the pencil in my mouth clearly influences my third person mental state attributions, and this influence is patently nothing like cognitive penetration, given the highly indirect and extramental nature of the causal link. Nevertheless, does this decision affect the justification of these attributions? Suppose, for further specification of the case, that I’m holding the pencil in a deliberate attempt to positively influence my own mood, while oblivious to the possibility that this might affect my judgments of others.[[9]](#footnote-10) Epistemologically speaking, this case seems to me to be relevantly similar to cases of cognitive penetration. I’m not in any sense blameworthy for the effect my action has on my judgments (nor am I in the standard cases of pernicious cognitive penetration), but still, these judgments are being influenced in such a way that I have temporarily become a worse judge concerning the emotional states of others.

One interpretation of the situation is that, although the people around me look happy, they shouldn’t look happy; that is, this perceptual or quasi-perceptual state I’m in---of others looking happy to me---is a state that I should not, epistemically, be in. Another interpretation foregoes evaluation of the ‘looks’ state and claims merely that the perceptual or quasi-perceptual judgment is to some extent ill-formed because it was influenced by factors it should not have been influenced by. In any case, the kinds of accounts that would be given for the standard cases of pernicious cognitive penetration seem equally appropriate here, even though the case is far from being an instance of cognitive penetration.[[10]](#footnote-11)

**3.3 Is cognitive penetration of perception worse than cognitive penetration of perceptual belief?**

In discussions of cognitive penetration, the term ‘perception’ tends to have a narrower meaning than usual. This makes room for the possibility that perceptual belief---in some more or less ordinary sense of the term---is cognitively influenced even if perception---in this narrow sense---is not.

What is this narrow sense? There is almost certainly not just one, but let’s start by looking at a much discussed line of research by Proffitt and colleagues (e.g., Proffitt 2006), who claim to find that wearing a heavy backpack makes hills look steeper and that holding a heavy weight makes the target to which that weight is to be thrown look farther away. Suppose this is a cognitive, rather than a lateral effect: it’s not the mere haptic sense of weight that affects the visual appearance, but the expectation that the subject may have to climb the hill or throw the weight, or an imaginative rehearsal of doing so. One line of criticism of this research (Durgin et al. 2009; Durgin et al. 2012) argues that the effect results from a response bias due to experimental demand characteristics; the idea is that subjects can guess that the experimenter wants to show that the presence of the backpack will make hills look steeper, and they---unconsciously and unintentionally---comply by judging the hills to be steeper.[[11]](#footnote-12) This would certainly be a *cognitive* effect, but it’s not cognitive penetration of *perception*, because the effect is entirely post-perceptual.

The ensuing debate has been rather inexplicit about what counts as perceptual vs. post-perceptual in this context (see also Witt 2011, Woods et al. 2009). Elsewhere, one encounters two main views about the perceptual/post-perceptual distinction. The first (confined for ease of discussion to the visual modality) maps the perceptual/post-perceptual distinction onto the distinction between early vision and late vision (Pylyshyn 2003, Raftopoulos 2009). Early vision comprises the processing up to about the level of Marr’s (1982) 2 1/2-D sketch; it “extends to the construction of the sort of percept that we might have of a totally unfamiliar scene . . . where nothing is recognized as familiar” (2003, 123); it includes information about “spatiotemporal [properties], spatial relations, surface shading, orientation, color, binocular stereopsis, size, shape and movement” (Raftopoulos & Müller 2006, 210). One reason for drawing the line here is that perceptual processing up to this point seems to be encapsulated, while further processing seems not to be.

A second way of drawing the distinction locates the perceptual/post-perceptual divide at the point where visual (perceptual) experience and its associated sensory phenomenology gives way to something else (Siegel 2006). Sometimes this something else is cognition without phenomenology, sometimes it’s cognitive phenomenology. Though it’s hard to make this very clear, we seem to have a rough intuitive grasp on the notion of perceptual experience, as something distinct from belief and which has a rich sensory phenomenology.

The first way of drawing the distinction embodies an architectural approach, while the second embodies a phenomenological approach. It is unlikely that these two approaches converge. Proponents of the latter approach often endorse a liberal, or rich-content, view about the content of perceptual experience, allowing it to represent high-level contents, not just things like *dog* and *cat* (which are already richer than early vision) but perhaps even semantic and causal relations (Siegel 2005, 2006), highly subordinate category membership (e.g., *being a 1968 Cadillac Eldorado*), etc.

I will suppose that our perceptual judgments (or perceptual beliefs---I will use these terms interchangeably) include concept-level attributions of properties to objects and identification of particulars and of category members at the basic or entry level, judgments like (depending on the perceiver and circumstances) ‘the dog is on the sofa’, ‘that’s a giraffe’, ‘it’s raining’, ‘this pumpkin pie is chewy’, and plausibly ‘there’s Martha Jones in a 1968 Eldorado’. There is quite a gap between such judgments and the outputs of early vision. The phenomenological approach sees a much tighter relationship between perception (i.e., perceptual experience) and perceptual judgment, especially if it takes on a liberal view about the contents of perceptual experience.

My own view (Lyons 2005a, 2005b, 2009) is something of a compromise between these two approaches. I endorse a different kind of architectural account, though my target is perceptual belief, rather than perception in one of the narrower senses. The account starts with a view about what a perceptual module is---it is one that takes inputs from sensory transducers and none of the other inputs to which are under the direct voluntary control of the larger organism---and defines perceptual beliefs as the doxastic outputs of perceptual modules. This gives us a fairly liberal view about the contents of perceptual belief (on the plausible assumption that ‘the dog is on the sofa’ and ‘there’s Martha Jones in a 1968 Eldorado’ and the like will satisfy the definition), while remaining agnostic about---and thus compatible with a very conservative view regarding---the contents of perceptual *experience*.

I take the agnosticism, if not the conservatism, to be a good thing, in part because it is very difficult to tell where sensory phenomenology leaves off and cognitive phenomenology begins. When the expert and I both look at a pileated woodpecker, it looks like a bird to both of us, but it also looks to her (and not me) like a pileated woodpecker. Our overall experiences are different, but is there any reason to think that there must be a difference in *visual* experience, in sensory phenomenology? Obviously, the expert has a firm sense that it’s a pileated woodpecker, but why should we not think that this is merely a matter of cognitive phenomenology? You and I look at the same painting, and I am struck with the conviction that I saw this painting yesterday. There’s an introspectable, phenomenological difference between us, but it doesn’t seem to be a *visual* phenomenological difference. Why think the woodpecker case is any different?

Consider a case of spectral inversion without error: you and I have inverted color qualia, but we associate these qualia with different color concepts, in such a way that our color judgments all come out to be the same.[[12]](#footnote-13) You and I both look at a green thing in standard lighting conditions. In some sense, we want to say that it looks the same (green) to both of us---whether or not we actually believe it to be green (we might currently be worried about skeptical scenarios). In another sense, however, and *ex hypothesi*, it looks very different to us: our visual experiences are different. Now, you look at a green thing and I look at a red thing. What you’re looking at looks green to you, and what I’m looking at looks red to me, *even though our visual experiences are identical*. I don’t know if this is the *right* way to describe what’s going on---I’m staying agnostic about the contents of experience, after all---but it strikes me as sensible and plausible. One who holds a certain type of liberal view about the contents of perceptual experience will, of course, deny that our perceptual experiences are really identical. Perhaps only some component of the experiences is identical. But it’s hard to see what’s motivating this denial other than the liberal theory about the contents of experience.

My view is not, however, agnostic about the contents of perceptual *judgments*. I hold that you and I make the same perceptual judgments about colors, whatever the nature of our visual experiences. I hold that the expert’s belief that it’s a pileated woodpecker is a perceptual belief, even if her experiential state is the same as mine.

It is at least as likely that perceptual judgment is cognitively penetrated as that perception (i.e., early vision, or visual experience, or whatever perception in the narrow sense amounts to) is, regardless of how we understand the latter, since if penetration of perception is possible, then penetration of perceptual judgment is, but not vice versa. Suppose the expert’s pileated woodpecker identification is cognitively penetrated in such a way that her expectation that she might see some in these woods has made her better at spotting them. This seems have all the hallmarks of an epistemically good case of cognitive penetration, and it doesn’t seem to matter whether this penetration occurs early (it penetrates perception narrowly construed) or late (i.e., “post-perceptually”), on either approach to the perception/perceptual judgment distinction.

Return to the debate about the perception of distances and steepness of hills. Proffitt and colleagues think these are cases of cognitive penetration of perception; Durgin and colleagues think the effects are post-perceptual. On my theory of perceptual belief, it is plausible that the beliefs are perceptual beliefs in either case. Then, on either view, these are cases of the cognitive penetration of perceptual belief; on Proffitt’s but not Durgin’s view, they are also cases of the cognitive penetration of perception. (I will follow convention and proceed in terms of “post-perceptual” penetration, though it is post-perceptual penetration *of perceptual belief* that I am concerned with, here and in what follows.)

The epistemic situation seems to be the same regardless of where the penetration occurs. In neither case is the subject aware of what’s happening; in neither case is the subject in a good position to stop it from happening, but in both cases the resulting belief is influenced by factors that should not influence perceptual beliefs. This is not, of course, to claim that the beliefs have no degree of justification, just that they have less than they would had they not been penetrated in these ways. Whichever way the influence works, it makes the subjects worse at judging distances and slopes. If distance judgments really result from the subjects’ desire to help the experimenter, the epistemic status of these judgments is threatened in just the same way as if they result from the subjects’ anticipation of effort. Surely the overall epistemic consequences might be different. Response bias might introduce more serious distortions (threaten reliability in a worse way) than effort anticipation does; on the other hand, response bias relegates the bad epistemic news to those rare cases where there is an actual experimenter around to please, while leaving our judgments intact in most real world applications. Nevertheless, the epistemic worries that arise from cognitive penetration at the perceptual level arise from cognitive penetration at the post-perceptual level as well. And in any case, if there is a difference in epistemic implications here, it doesn’t seem to generalize past the particular details of this case and the candidate mechanisms (i.e., response bias vs. anticipation of effort). There is no lesson here regarding early vs. late cognitive penetration in general; which is worse will have to be decided on a case-by-case basis.

The cognitive impenetrability of the strictly perceptual is sometimes defended with vehemence that suggests that things would be worse, epistemically speaking, if cognitive penetration happened at the perceptual level than if it happened at the post-perceptual level---that is, if, for some given case of cognitive penetration, it turned out to be perceptual, rather than post-perceptual penetration (see, e.g., Fodor 1988, Raftopoulos 2009). Perhaps it is thought that post-perceptual penetration would be somehow more corrigible than perceptual penetration, that if cognition penetrated *early* vision, or perceptual *experience*, we’d be stuck with that influence; we would have no way, aside from learning a lot of perceptual psychology, to detect and counteract these effects. And insofar as cognitive penetration makes us worse at judging perceptual matters, that would be a bad thing. However, a salient feature of the present cases is that even if the subject were to somehow know that her perceptual belief were cognitively penetrated, she would be in little or no position to tell whether the effect were perceptual or post-perceptual. This is an instance of a general truth with pervasive epistemological implications: we are bad judges of where perceptual *experience* leaves off and perceptual *judgment* takes over. More generally, introspection and other nonempirical means are poor guides to drawing a line between the perceptual and the post-perceptual. (Obviously, I favor the architectural over the phenomenal method for drawing the perceptual/post-perceptual distinction.) So even if the penetration is a post-perceptual effect on perceptual belief, we seem to be just as stuck with it as if it had occurred at the perceptual level.

In fact, to the extent that there’s an epistemological problem here, it is one that generalizes beyond even perceptual belief. Many of our belief forming processes are “intuitive” in the psychologist’s sense of the term: they are perception-like, System-1 (Kahneman & Frederick 2002) processes that operate quickly, automatically, and without the aid of conscious deliberation. Some of these are clearly cognitively penetrable. The belief bias (Evans et al 1983), for example, occurs when our antecedent belief in the conclusion of an argument affects our intuitive judgment concerning the validity of that argument. This is obviously not cognitive penetration of *perception*, but it is obviously a species of the same genus. And it is problematic in just the same way. What makes this case worrisome, and what makes the perceptual case worrisome, is not just the absence of encapsulation, but the presence of another of Fodor’s diagnostic features: introspective opacity. We don’t have introspective access to the inner workings of our perceptual or intuitive processes, so when cognitive penetration does occur, we have no way of knowing that or counteracting it. This is true, however, not just of perception in one of the narrow senses, but of perceptual belief and of intuitive judgment more generally.

**3.4 Do perceptual systems need to be encapsulated?**

I mentioned just now that the vigor with which the encapsulation of perception is sometimes defended suggests that responding to the epistemological worries of cognitive penetration requires that *perception* be encapsulated. But does it also require that perception be *encapsulated*? I won’t take a lot of time with this, as it isn’t very surprising, but the answer is no. Being encapsulated “to an interesting extent” is good enough.

Again, if we think that cognitive penetration has epistemological implications we need to know what it is about it that makes it have epistemological implications. If the problem of penetration is one of epistemic circularity, then perhaps complete encapsulation would be necessary to guard against it. But as I’ve argued above and in more detail elsewhere (Lyons 2011), this isn’t what’s epistemologically threatening about cognitive penetration.

Rather, what is most epistemically worrisome about the very idea of cognitive penetration of perception is that it would threaten to cut us off from the world, to render us less sensitive to the world around us. In this respect, cognitive penetration is more of a danger, at least in general, than lateral penetration. In part, this is because cognition is already less directly linked to the world than perception. If I were simply to fall in with the wrong social crowd, they might, without even trying, convince me to believe and desire all kinds of crazy things. It would be relatively hard for them, even if they were doing it on purpose, to get me to see and hear crazy things.

Whether cognitive penetration involves the influence of doxastic states like beliefs, or conative states like desires and fears, whether believing that *p* makes us more likely to perceive that *p* or less likely, we don’t want our perceptual beliefs to be endogenously determined. The whole point of perception is to put us in touch with the world as it actually is, and there’s little room here for endogenous determination. Or at least, putting us in touch with the world is the point of perception *epistemologically* speaking. If the point of perception evolutionarily speaking was to enable us to interact with our environments, and if the pursuit of this goal was sometimes better served by certain kinds of systematic falsehoods than by objectivity and truth, then the evolutionary and the epistemic aims of perception are sometimes at odds with each other.

This is why Proffitt’s studies raise a credible threat. It is not absurd to think that our distance and slope judgments and the like might be influenced by considerations of effort; if I judge the distance to be longer when I’m tired or encumbered, then I don’t have to explicitly take encumbrance or tiredness into account in planning routes or predicting timetables, and the reasoning task is thereby simplified. Insofar as the penetration here is cognitive, rather than lateral, we have an endogenous influence on our belief, one that reduces our contact with the world. Although this is a credible threat, it’s not a very threatening threat, for it doesn’t diminish our contact very much. We certainly don’t want our perceptual beliefs to be entirely endogenously *determined*, but degree of endogenous causation can vary, and the degree of epistemic degradation brought about by cognitive penetration can vary accordingly.

It is a commonplace that perceptual beliefs are the result of the brain’s forming a best guess about the distal environment on the basis of a number (sometimes a large number) of different cues, none of which entails the presence of the inferred environmental features, and some of which have individually fairly low predictive value. Cognitive penetration might influence some of these cues without influencing others. If my intention to throw a given weight penetrates my motor anticipations, and these anticipations influence my visual estimates of distance, then these visual judgments are cognitively penetrated. But they are far from being entirely endogenously caused. The intention influences the value of one cue. But there are several other entirely visual cues to distance (e.g., texture gradients, aerial perspective, binocular disparity, eye convergence, angular projection of familiar items, etc.), and we have no reason to think that my intention to throw has any influence on any of these cues. Consequently, we might expect that the cognitive penetration doesn’t have a very dramatic influence on the distance estimate; it might influence it without influencing it much. Thus, penetration might make us worse at judging distances, without making us *much* worse. And in fact, our epistemological intuitions about the cases seems to be that, though the fact of penetration results in a lower degree of justification for the relevant beliefs, it may not be a very much lower degree of justification.

Once again, we need to take things on a case-by-case basis. The belief bias strikes me as a far more pervasive and insidious problem of penetration than any of the perceptual cases I’m aware of. And the problems it poses are practical ones that are visible to the naked eye, as anyone who has paid attention to public discourse about politics or other matters of policy can easily attest. Yet there is room for optimism, at least in the perceptual realm, even though perception---perceptual *belief*, anyhow---is clearly cognitively penetrable.

**4. Conclusion**

Perceptual systems might well be modular without being F-modular, i.e., without satisfying Fodor’s strict criteria for modularity. In particular, there is nothing incoherent about unencapsulated modules. The cognitive penetrability of perceptual modules has little direct epistemological consequence, as the penetration can be either epistemically advantageous or pernicious, depending on the details of the case. To the extent that perceptual modules have or approximate F-modularity, it is introspective opacity that threatens to render perceptual errors intractable, not penetrability.[[13]](#footnote-14)

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1. In Lyons (2001), I presented the view as an alternative to a modularity view, although this choice was entirely terminological. I had assumed, probably incorrectly, that ‘module’ would retain enough ties with the Fodorian view that we would need some other term for the components posited by the Plurality Thesis. I think the term is now used in a less restrictive way, although I don’t care much if I am mistaken about usage. Ordinary language philosophy is bad enough when it’s about ordinary language; how much worse when it’s about terms of art! [↑](#footnote-ref-2)
2. I have defined isolability (with respect to a given task) as a monadic property of a substrate, but a two-place relation of isolability-from is easily defined: *S*1 is **isolable from** *S*2 with respect to *T* iff *S*1 computes *T* and could do so even if *S*2 didn’t compute any functions. [↑](#footnote-ref-3)
3. Stokes and Bergeron ms. This isn’t how they put it, and it may not be quite their intention, but credit for recognizing this belongs to them. [↑](#footnote-ref-4)
4. Prinz (2006) holds that the Plurality Thesis is uncontroversial. This hardly makes detailed articulation of that thesis a worthless endeavor, however. Nor is this thesis trivial, simply because it’s uncontroversial among cognitive scientists. Although we’re all used to it by now, it is really a shocking discovery, and one that turns commonsense on its head. [↑](#footnote-ref-5)
5. For the present purposes (but only for these), I will suppose that any system lower than the level of beliefs and desires etc. counts as approximately at the same level. Thus, if late vision feeds back into early vision, this will count as lateral penetration for the present purposes. For other purposes, of course, we would want more than just two kinds of penetration: lateral and cognitive. [↑](#footnote-ref-6)
6. I intend 6 and 7 to be understood in such a way that the somatosensory systems receive their inputs *whether or not* a motor command is ever sent to activate the facial muscles; otherwise the signal is behaviorally mediated. [↑](#footnote-ref-7)
7. Two caveats: first, this isn’t meant as a piece of conceptual analysis; cognitive penetration is a highly theoretical concept. Still I think this articulates the understanding of cognitive penetration most standardly employed in the field, and whatever terminology we use, it is important to distinguish relevantly different phenomena. Second, I’m leaving somewhat open the question of which states count as person-level and cognitive in the relevant senses. I don’t think they need to be conscious to count as cognitive penetrators, but they do need to be the sort of thing one could change one’s mind about, fairly quickly, as the result of practical or theoretical reasoning. [↑](#footnote-ref-8)
8. Siegel (2013) claims that the difference between the innocuous and the pernicious cases of penetration is a matter of the rationality or irrationality of the etiology, which is a matter of being sufficiently analogous to a rational or irrational doxastic etiology. Whether this will work out, and whether it will offer different verdicts than my view, will depend on whether the details concerning analogy and rationality can be worked out. It’s not obvious that the reliabilist can automatically get the cases right; whether she can depends in part on how we are to individuate processes, which is obviously a vexed issue for reliabilism. I make some initial stabs at this problem in Lyons (in prep). See McGrath (2013) for another internalist approach to distinguishing good and bad cases of penetration. [↑](#footnote-ref-9)
9. I don’t think it matters whether this is my motivation or whether I just like chewing on pencils. Obviously, if I do it knowing or suspecting that it might affect my mindreading judgments, then this adversely affects the status of those judgments. That’s not an interesting case, however, so we should assume I’m unaware of this possibility. [↑](#footnote-ref-10)
10. Here it is important that, as mentioned above and argued in Lyons 2011, the problem with cognitive penetration is *not* one of circularity. There’s clearly nothing circular about the current case. [↑](#footnote-ref-11)
11. It is important for the present purposes that this be a matter of judgment, rather than insincerely accommodating report, on the part of the subjects, i.e., that they really believe the hills to be steeper and aren’t just saying so to please the experimenter. This seems to be how Durgin et al. (2009) interpret things, and it will be how I understand the case. [↑](#footnote-ref-12)
12. I recognize that not all philosophers will allow that this is possible; I won’t try to argue that it is. [↑](#footnote-ref-13)
13. A version of the paper was presented at the Southern Society for Philosophy and Psychology in Austin, TX in 2013. Thanks to the audience there, especially James Genone and Fred Dretske. Special thanks to Philippe Chuard for comments on a written draft. [↑](#footnote-ref-14)