

SYMPOSIUM

## Reply to symposiasts\*

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AS A PHYSICIST, FIRST AND FOREMOST, I approach philosophy like all empirical sciences, that is, as proposing models of the domain addressed. This means that each theory abstracts from the complexities of the domain under study and proceeds through idealizations of the “reality” it purports to describe. For this reason, every account has flaws that arise not only from the inevitable occasional errors in analysis, but also from the fact that by design each account, being a model, omits certain aspects of reality that may prove to be important in understanding the issue under examination. The commentaries on my book bring to the fore concerns related to both sorts of problems described above. I am grateful to the commentators for their critical notes and I hope that my replies will promote debate and shed some light on the problem of cognitive penetrability.

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Alberto Voltolini’s commentary consists in two parts. In the first, he discusses my definition of CP as it pertains to early vision and points out that early vision, as I construe it,

may still be CP in Macpherson’s sense of CP-lite. In addition, he states that he prefers Macpherson’s definition of CP, which allows for lite-CP and strong-CP. He also objects that my account of why the CP of late vision does not entail constructivism, which is based on the fact that the effects of cognition could be alleviated since early vision preserves the evidence in a visual scene, does not cover all perceptual situations, most notably multistable perception.

I have discussed elsewhere<sup>1</sup> the problems with Macpherson’s account and I will not repeat them here except to explain why strong CP, the thesis that ordinary perceptual experience is *strongly* cognitively penetrable iff its content is roughly the same as the conceptual content of certain thoughts of the cognitive system, is a non-starter.

First, some of the contents of thoughts are in part constitutively determined by the semantic relations in which they stand with the contents of other thoughts through the discursive inferential interrelations among thoughts. Since no stage of perception involves discursive inferences, perceptual contents do not stand in semantic relations to other perceptual

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\*This paper is part of Book Symposium on A. RAFTOPOULOS, *Cognitive penetrability and the epistemic role of perception*, in: «Rivista internazionale di Filosofia e Psicologia», vol. XI, n. 3, 2020, pp. 355-419.

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contents or to the contents of thoughts. In this sense, the relational role semantics of thoughts is richer than the semantics of perceptual contents, in the sense that the meanings of the terms in thoughts exceed the confines of the thought-state and constitutively depend on other thought-contents.

Second, even the contents of the states of late vision that are CP are constitutively dependent on the visual input, and, also, being hybrid, include nonconceptual content (NCC). This entails that they can represent fineness of grain that is lacking in the conceptual representations of thoughts; they allow, for example, for perceptual discriminations that thoughts do not. Moreover, they constitutively have a visual phenomenal character that thoughts do not have; since this phenomenal character is part of perceptual content, perceptual contents cannot be, even roughly, the same as the conceptual content of certain thoughts.

Do cases of multistable perception threaten my argument that, owing to the CP of early vision, the epistemically harmful cognitive effects on late vision could in principle be alleviated by revisiting the iconic information retrieved from the visual scene by early vision, so that a viewer may be brought to see what another viewer experiences despite the fact that initially the viewer perceived a different percept (Voltolini calls this “perceptual revision”). Voltolini says that in such cases, «[T]he two subjects actually face the very same three-dimensional scene, yet no one is right in grasping that scene one way or another: the opposite protruding-receding movements that the two aspectual perceptions respectively mobilize are merely apparent»<sup>2</sup> and concludes that in the case of aspectual perception the evidence the subjects rely on, induced by their concept-dependent perception, cannot be dispensed with and, hence, one cannot bear to see the percept perceived by the other... perceptual revision is not possible.

Let me first remind that reader that, as both Voltolini and I agree, in such cases, different percepts are formed when viewing the

same scene because viewers organize the scene in different ways. This organization may affect both the interrelations among the parts of the multistable object or the relations of this object with other objects or the background. It is also true that the phenomenal contents of the respective experiences differ. Voltolini’s main point is that in such cases merely refocusing spatial attention cannot achieve perceptual revision because the attention involved is not spatial attention, which allows the viewer to focus on another critical region in the figure and, thereby, see the alternate percept, but rather a sort of holistic attention that affects what is perceived as a whole. In my book I acknowledge that other forms of attention may affect the perception of multistable figures and discuss these effects.<sup>3</sup> Just as refocusing spatial attention may make a viewer revisit some location within the iconic image, reorganize the image, and end up forming another percept, so feature/object attention may make a viewer focus on some feature or object and reorganize the image, by, say, switching to a different reference frame (whether it be viewer-centered or object-centered). The “holistic attention” to which Voltolini refers should be used to describe not a different sort of attention but the effects of the known forms of attention on the perception of the whole figure, because, obviously, the reorganization effected by either sort of attention affects the perception of the whole figure. It seems to me, therefore, that in the case of the multistable figure, nothing precludes the possibility that viewers revisit the iconic image through any sort of attention and reorganize the image differently, switching to the alternate percept.

Finally, Voltolini claims that early vision may be CP in Macpherson’s sense, since early vision may be directly influenced by concepts in so far as the latter enable it to play a certain epistemic role. However, I argued in the book that early vision is not directly affected by concepts both because concepts do not enter the contents of the states of early vi-

sion, and because cognition does not affect the epistemic role of the states of early vision. Since Voltolini does not offer any counterarguments to this diptych, merely noting that according to Macpherson's conception of CP-lite, early vision may be CP is not a refutation of my views, because the two theses put forth show exactly why Macpherson's conception of CP-lite is false.

The second part of Voltolini's commentary concerns a number of problems pertaining to my account of late vision. First, how could hypotheses that have a theoretical nature befitting cognition itself be entertained in late vision *qua* a perceptual stage? It is, indeed, a central thesis of my account that in late vision hypotheses are formed through the confluence of purely visual information extracted from the visual scene during early vision and semantic information contained in the cognitive states that modulate the processes of late vision. These hypotheses concern the identity of the object and are tested against the iconic information stored in perceptual circuits. Let me first stress that in his reconstruction of my views, which is otherwise perceptive, Voltolini says that such hypotheses are matched against the NCC formed (that is, the iconic information extracted) during early vision that is now stored in working memory. However, this is not my view, although this does not affect the rest of Voltolini's comments. For reasons addressed elsewhere, I think that NCC cannot be stored in working memory; it is stored, instead, either in iconic memory or in fragile short-term memory, which are both independent of attention, unlike working memory.<sup>4</sup> Voltolini's concern is how such semantically informed hypotheses, which have a hybrid non-propositional form having both perceptual demonstrative (that is, contextually determined) content and conceptual content, could be entertained in a genuinely perceptual stage, such as late vision; they better befit a cognitive or an imagistic stage. A further concern, which Voltolini intends to relate to the previous one, although, unfor-

tunately I cannot grasp the link, is that since mental events in late vision, such as the hypotheses at issue, may affect behavior, these mental events cannot be purely hypothetical but instead must be doxastic states since do not affect behavior.

With respect to the first concern, Voltolini bases his reservation on the fact that since hypotheses in late vision require semantic information for their construction, they are theoretical constructs that, first, better befit pure reason or imagination and, second, give late vision a constructivist flavor. I disagree with the first point and fully endorse the second. That the formation of hypotheses relies in part on semantic information does not counteract another essential trait of them, namely that they are constitutively dependent on perceptual context in that they still retain the demonstrative component that creates a *de re* link with the world. As such, they cannot be in the realm of cognition. With respect to the second point, I agree that because of its CP, late vision may engender constructivist concerns, which is exactly why the previous chapter in the book addresses this problem.

As far as the second worry is concerned, let us consider Voltolini's own example.

Suppose that at time *t*, in virtue of one's perception in early vision, in late vision one entertains the erroneous idea that that thing over there is a snake. This idea is not a hypothesis. If it were such, one would never run away.<sup>5</sup>

This account is ambivalent as it can have at least two different readings. The "erroneous idea" entertained may be either a tentative hypothesis that needs to be tested (and which if tested, could be proved wrong if sensitivity to the data was not affected by CP, or could be adopted despite being faulty because of CP induced insensitivity to the data), or a faulty recognitional belief, in which case testing would have taken place (and the second possibility mentioned above oc-

curred). In both cases, the relevant state is not doxastic; it is not held by the viewer since it has not been appropriately tested.

If it is a mere hypothesis, it is still possible that, for example, an animal who perceives a threatening figure approach while it is about to drink some water might run away (better safe than sorry) even before the figure is recognized as being such and such? If it is the outcome of some testing and, thus, a recognition-al belief, (suppose the figure was far away and the animal had some time to recognize the figure), the animal would have yet another reason to run away to avoid the figure, even if the recognition-al belief has not been tested against the rest of the animal's beliefs, or whatever this might entail for animals). My point is that hypotheses, and recognition-al beliefs before they are judged against further evidence in the space of reasons, may induce some behavior; thus, causing some behavior does not suffice to make them doxastic states.

Voltolini, second, wonders that if early vision stores all information from the visual scene, two hypotheses that oppose one the other cannot be tested against the NCC of early vision but require testing against two different NCCs of two different episodes of early vision. The reason underlying his concern is that

Appealing to a refocusing attention [...] is not enough. For the revision is not accounted for by claiming that one has missed a detail in the perceived scene that refocusing attention may enable one to capture, as in a sort of *Blow Up*-situation. For once again, attention must play a more active role. Indeed, in order to dispense with the "bad" hypothesis and preserve the "good" one, one must check them not with one and the same NCC of that episode now stored in working memory, but with a new attention-based episode of early vision endowed with a NCC that differs from the NCC of the episode entertained before. So, to come back to the rope-snake case, one may rule

out the "snake" hypothesis once one *notices* that one is facing a ropish Gestalt (as constituting the NCC of a new perceptual episode of early vision) rather than a snakish Gestalt (as constituting the different NCC of a previous episode of late (sis) vision).<sup>6</sup>

Let me repeat that NCC is not stored in working memory and also point out that Voltolini in the last sentence means a previous episode of early vision. The problem with this account is, first, the role of a "more active attention". We know of spatial attention, of object/feature-based attention, of diffuse attention, of transient or sustained attention, but what is this more *active attention* that is needed to explain what goes on when one views a multi-stable figure? Another problem is the reference to an attention-based episode of early vision; since Voltolini agrees with me that early vision is not directly affected by attention, what does this mean?

Finally, why revisiting the evidence contained in the NCC of the perception of the ambiguous figure cannot vindicate the one or the other competing hypotheses? This reexamination, as it were, of the evidential basis can bring to the fore different salient points that support the one or the other hypothesis and all the reexamination requires is spatial or feature-based attention. Early vision outputs an image that can be organized in different ways that correspond to two different interpretations of the ambiguous figure. I hasten to note that this is one of the possible ways the perception of the ambiguous figure could work. In the other cases, however, the same analysis holds. Early vision outputs one gestalt or the other because the attention that acted before the onset of the perceptual act has highlighted the crucial points in the image that support one of the two interpretations. In this case, early vision outputs a ropish or a snakish gestalt and not something neutral between the two as in the previous case, but the point is that revisiting this gestalt and applying attention to different parts

of the image could change the perceived figure. So, testing against two different NCC of two different visual episodes is not required.

Voltolini, thirdly, asks how could hybrid late vision involve a non-propositional and a propositional content; in particular, the recognitional belief that late vision outputs should be better viewed as a post-perceptual content rather than as content of a perceptual stage, as late vision is supposed to be in my account? This time Voltolini unearths a real problem. It is certainly curious that late vision, which has a hybrid state containing both NCC and conceptual content that is not propositionally structured, to output at its final stage a recognitional belief that is propositionally structured (being propositionally structured, I thought, for its entering into discursive inferential relations in order to be tested against further propositionally structured evidence so that it may become a judgment). How does this propositional structure emerge in a perceptual stage? And if it does not, how is the non-propositional recognitional belief that late vision outputs enter into inferential relations in cognition, which it certainly does.

The book does not provide answers to these two questions, neither do I have them to provide them here. I can only say, paving the way to future treatment of these two pressing questions, that despite the fact that the NCC of late vision is cast in an iconic format and the conceptual content of late vision is digitally or symbolically structured, underneath these two different representational formats there are significant similarities.

First, concerning the iconic content of late vision, consider Burge's view that perception is purely iconic in that it has no symbolic conventional elements in it.

Like perception, these types of pre-conceptual cognitive representation have the same structure as noun phrases constituted of contextual-determiner-dominated attributives – the structure of that F or those Fs. When representation occurs, the

representational types are applied in a demonstrative-like manner.<sup>7</sup>

The NCC of the iconic representations of a visual scene in both late and early vision represents a manifold of objects, properties and events and one could render this representational content in the form of a set of subject-predicate structures; let us call the subject-predicate structure the basic syntactic structure R. Thus, perceptual iconic representations have a rich semantic structure that is similar to a nexus of the subject-predicate structures of cognitive representations. In other words, both propositional and purely iconic representations share the same basis syntactic structure R, except that in the case of perception all attributives are contextually-determined-dominated.

Second, concerning the symbolic/conceptual content of late vision, nothing precludes it from being iconic. Modern discussions of iconic emphasize that what makes a representation iconic is not its being analog in the traditional sense, (according to which, a representation is analog if it is dense, homogeneous, continuous, unit-free, comes in information packages), but, rather, it meets the condition that the structure of the representations maps in a natural way onto the structure of representatum so that the semantic relation in the latter are preserved in the former. In this sense, a representation can be iconic even if it contains symbols. So, there is the possibility that all representations in late vision are iconic. This can be extended to apply to some of the representations in cognition. In general, a representation being symbolic/conceptual does not preclude the possibility that it may also be iconic.

Putting together the thesis that the same basic syntactic structure underlies both iconic and propositionally structured representations, and the thesis that even some of the cognitive conceptual/symbolic representations, certainly those contained in visual memories, may have an iconic component, lays the framework on which an attempt to

answer the two important questions should be based. I do not know the answer to them but I think that the scheme

purely iconic representations → symbolic non-propositional iconic representations → (through the basic syntactic structure) propositional iconic representations → propositional non-iconic representations (such as some of the pure, abstract, thoughts)

might be a good starting point.

Finally, Voltolini argues, in order for a belief to be endorsed no further testing against the descriptive content of thoughts is needed, as I claim it does when I discuss the differences between occurrent beliefs and judgments. Here is Voltolini's argument:

[T]he distinction between a dispositional and an occurrent belief [...] makes no difference concerning endorsement. I may now consciously come to entertain an occurrent belief that I have been entertaining dispositionally, say the belief that Pluto is round, and yet, by means of having allowed that belief to enter the fore of my consciousness, my overall behavior does not seem to be modified, as it should be if that change affected my endorsement of that belief. Hence, in order for the belief to be endorsed, no further test with the descriptive content of thoughts seems to be needed.<sup>8</sup>

I am not certain I grasp Voltolini's point correctly. There seem to be two threads in his argument. First, he seems to object to my view that a recognitional belief *qua* a disposition to make judgments needs further testing against other thoughts in order to be endorsed and become a judgment, which is an occurrent state when compared to the recognitional belief that is dispositional only with respect to a judgment, even though the dispositional belief being the output of late vision is an occurrent state, too. In other

words, Voltolini thinks that a recognitional belief needs no more testing in order to become a judgment. This interpretation is the only one that renders comprehensible his last statement that «no further test with the descriptive content of thoughts seems to be needed»<sup>9</sup> since I discuss such tests only to explain the endorsement of a recognitional belief that becomes, thus, a judgment. However, this is obviously false; when one forms the recognitional belief that, in the context of Müller-Lyer's illusion, the two lines are unequal, if one does not know that this is an illusion one may form the judgment that the two lines are unequal, but if one knew that this is an illusion one would not endorse the recognitional belief and would not form the relevant judgment. The difference in these two cases comes from the testing against the thought/belief that this is an illusion, in the one case, and the lack of any counterevidence in the other case, which means that no thought/belief contradicts the recognitional beliefs, which, thus, passes the test and is endorsed. So, an endorsement is essential to the distinction between a recognitional belief and the corresponding judgment.

Voltolini's "Pluto example", however, suggests another interpretation, namely that he considers the relation between a dispositional belief in the sense of an item in the "standing knowledge" and an occurrent state with the same content that I called "thought". If this is correct, Voltolini's point is that when one consciously entertains a thought that was previously held unconsciously in the "standing knowledge" reservoir, one does not endorse that thought and, thus, no further testing is needed, because the behavior does not change, as it would have had any endorsement taken place. There are two problems with this line of thought.

First, I do not claim that any endorsement is needed when a dispositional belief is activated and becomes a thought; it does simply enter, by being activated, into the working memory and becomes an item of consciousness. So, I agree with Voltolini that «the dis-

inction between a dispositional and an occurrent belief [...] makes no difference concerning endorsement»<sup>10</sup> when the terms are interpreted in the way I have explained.

Second, the rest of Voltolini's argument contains a false premise because it is not true that each time an endorsement takes place behavior is modified, unless storing a belief in the data basis as true and not merely probable is a behavioral change, in which case, trivially, a behavioral change does always occur when an endorsement takes place. Behavior modification is not a reliable criterion by which one may judge whether an endorsement has taken place; I may endorse upon reading a paper that the Universe expands according to the provisions of General Relativity, but it seems to me that it is not *necessary* that there will be a concomitant behavioral change.

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Lyons raises three concerns concerning the nature of early vision, the notion and definition of cognitive penetrability, and the nature of the iconic content of the states of early vision. I start with early vision. Lyons remarks that as the notion of early vision is unclear in my book and is hard to tell what Raftopoulos means by early vision. Lyons detects three ways in which early vision may be construed; one by representational content, one by representational format (early vision is iconic rather than symbolic), and one by timing (early vision is formed in less than 120-170 ms.).

Lyons' concern is that with respect to the first two ways my claim that early vision involves iconic representations conflates two sorts of iconicity, one that concerns contents and the other formats (or representational vehicles). While early vision may output structural information in an iconic format, it is unlikely that a distinction based on contents «will draw the line in the same place as a distinction that rests on format».<sup>11</sup> One reason to doubt this is that some of the information represented in early vision, namely affordances, solidity, or persistence in time,

are difficult to be thought of as iconic contents since they require symbolic predicates or something similar. If a representation is iconic because there is a structural isomorphism (I would prefer the term "mapping") such that spatial and semantic relations in the represented domain are preserved in the representation, it is hard to tell how representations that require symbols might be isomorphic to affordances viewed as properties of the environment/agent system. In addition, Lyons says that there is a confusion between "analogicity" and "iconicity"; whether an iconic representation is also analog depends on what one means by analog, and that I do not specify my stance on this problem.<sup>12</sup> Lyons' point is that a representation may be cast in an iconic format even if it contains symbol-like components (since it satisfies the mapping requirement) but its content is not purely iconic owing to the presence of symbols. The main problem is that, as Lyons correctly remarks, I do not specify what I mean by iconicity. It seems that I adopt the structural mapping view, but, then his employment of "iconic content" seems oxymoronic.

Despite the fact that "oxymoronic" is not used correctly, otherwise, Lyons is perfectly right. In the book, I did not explain what I mean by iconic, and how I think of the relation between iconic contents and iconic formats, in particular, whether one could have the one without the other. Therefore, all the concerns he raises are legitimate and well placed. The reason that I did not discuss these important issues is that originally the book was supposed to have one more chapter in which the problem of iconicity would have been addressed, but owing to size restrictions this chapter never made it to the publication process. Upon returning to the rest of the book where iconicity is used to explain what I mean by it, I discovered that the problem is so complicated that I would need another book to address it (which I am doing right now). I decided, thus, to rely on the intuitive sense of the reader on what iconicity means

and on the eventual knowledge of the literature (both old and new) on the reader's part. As a result, the problems earmarked by Lyons emerged. I will return at end of my reply to say a few things about iconicity. Let me return to early vision.

Lyons complains that even though I closely relate early vision with the timing of visual perceptual processes I do not give a cut-off number specifying the time scale of early vision and, also, that I do not explain where this number comes from. Lyons offers a fourth possible way to define early vision based on the work of Lamme and his research team, according to whom visual perceptual processes are penetrated around 150 ms. post-stimulus owing to global recurrent processing (GRP). He notes that this would be a cogent way to understand the early vision but, unfortunately, «Raftopoulos never even explicitly states the numerical threshold, let alone ties it with Lamme».<sup>13</sup> In addition, Lyons remarks that in my previous work I took a very different approach and stipulated *a priori* that early vision is whatever stage of early vision is cognitively encapsulated and, then he searched for evidence that such a stage exists and what its content might be. However, in my book I define early vision by a combination of content, format, and timing and then argue for cognitive encapsulation, but, unfortunately, I did not explain how these three conditions are interrelated to specify early vision.

Instead of pinpointing the problems of Lyons' reconstruction of my work concerning early vision, with one exception, I will succinctly explain what I mean by early vision. The exception mentioned above, which is important, concerns Lyons' remark that I do not even hint at Lyons' work on local and global recurrent processing and its role in defining early vision. In chapter 3, section 2, of my book, I refer to Lyons' work and to his notion of LRP and GRP and the significance of these for the cognitive encapsulation of visual processing 16 times.<sup>14</sup> In section 3.1, there are about 16 references to Lamme's relevant

work.<sup>15</sup> This is important because if Lyons had read (or had not forgotten) these parts of the book, he would have found many of the answers to what he is looking for. Having said that, here is what I mean by early vision.

It is not true that in my previous work I started with the stipulation that early vision (if it exists) is the cognitively impenetrable stage of visual processing and then I searched whether it exists. There were no *a priori* assumptions in the beginning (remember I am foremost a physicist). After studying the relevant literature, I was left with the strong impression that there is a time frame during which visual perception is not affected by semantic (cognitive) information (for example, word-forms are detected and used well before the meanings of these words come into effect, or cognitive signals are registered in visual processing areas with a significant delay). Then I read Lamme's work and his notion of LRP and GRP and his construal of early vision, which, as I explained in my previous book *Cognition and perception*,<sup>16</sup> I adopted. Therefore, it is a plausible empirical finding and not an *a priori* assumption that there exists a stage of visual processing that is not affected by cognition. The time frames were also adopted from Lamme's work (who offers the 150 ms. as the threshold for GRP). Of course, different research protocols, different subjects (many experiments were conducted on non-human apes whose brains are smaller and, thus, signal transmission is faster than in humans), different experimental designs, etc., result in different thresholds for the onset of cognitive effects. In my book, chapter 3, section 4.1, I discuss such a discrepancy between Lamme's work and the work of other researchers who have found earlier timing onsets for recurrent processes. This is why I place the termination of early vision somewhere between 120-170 ms. and not assign a cut-off point. Lyons may find it deplorable, but this is the nature of empirical research.

After having defined early vision in terms of cognitive impenetrability and having established its relative time-frame I looked at the



empirical evidence concerning the kind of information that could be retrieved from a visual scene within this time frame, and I came up with the list of the properties that Lyons mentions in his commentary; in the first book, I had left out the ensemble statistics, but I included them in the present book. Thus, cognitive impenetrability, time-frames, and content are inextricably interrelated in the way I explained, which is why the whole edifice would collapse if one could adduce evidence for GRP at earlier onsets, and which also explains the need to revisit the early vision and discuss recent empirical findings that seemed to threaten the CI of early vision. Up to this point, there is nothing to tie early vision with iconicity although many threads leading to that direction are discernible in the account of early vision offered here. I'll return to this problem at the end of my commentary.

So even though Lyons is right that in the present book I do not explain how I arrived at the timing and the list of contents of the states of early vision, I did not because I asked the reader to consider my earlier account. As I explained in the introduction my main aim was to explore late vision, since my early vision was covered in my previous book and all discussions pertaining to early vision were restricted to addressing some new concerns regarding its cognitive penetrability. Lyons' comments, however, suggest that I should have dedicated a section to reinstating my view of early vision.

Lyons turns next to my attempts to define cognitive penetrability (CP). As I explained in the book, my aim was to pinpoint problems of the earliest accounts of CP to avoid them, take their positive aspects, and synthesize them to arrive at a new improved definition. Lyons' comments address some problems in the definition I proposed.

Lyons examines, first, what I called *CP revisited*, which is a first pass in the attempt to define CP. Lyons remarks that the five conditions for a good definition in *CP revisited* restate what is worth salvaging from older definitions. Although this is generally true, it

does not apply to conditions (c) and (d). Condition (c) states:

C (a cognitive state) affects the perceptual processes that lead to the formation of a perceptual state P in the sense that these processes use information contained in C. The information contained in C is used by the processes that issue P in an online manner, that is, it is used during the course of the processes underwriting P and it does not simply fix the values of some parameters that figure in the state transformations in which the processing in P consists.<sup>17</sup>

That cognitive information should be used by the perceptual processes for CP to occur is explicitly stated by Wu<sup>18</sup> and implicitly by Macpherson,<sup>19</sup> but the elaboration concerning the parameters that figure in the equations of state transformations emanates from an attempt to exclude pre-cueing effects from signifying CP and this is first discussed in the book, so it is a new addition to the literature. The same holds for (d), but these are innocuous problems and I will not discuss them any further.

Lyons offers *CP Re-revisited* as a short head for *CP revisited*; no objection here on my part provided that one keeps in mind the details of the full definition of CP. Then Lyons discusses the relation between *CP Re-revisited* and what I call the Epistemic Condition for CP and wonder whether the two are related to a conjunction or a disjunction. He reconstructs my view of CP as follows:

CP1: a cognitive influence on perception is an instance of CP iff (i) the influence is direct, and (ii) that influence affects the epistemic role or status of the perceptual state.<sup>20</sup>

Lyons worries about two things in this definition. First, he thinks that the revised epistemic condition states necessary and sufficient conditions for CP and, thus, renders the "directness definition" redundant.

In the book, I stated that the directness

definition does not explain why the indirect cognitive effects on perception should be excluded from being cases of CP. To answer that, I proposed that one should augment the definition by including an explanation of why indirect cognitive effects do not constitute cases of CP and this entails that one should include the clause that cognitive effects entail the CP of perception if they affect the epistemic role of perception in grounding perceptual beliefs. Underlying this is the basic assumption about what is at issue with CP, namely the worry that CP renders perceivers insensitive to environmental information since the degree of sensitivity to the data is inextricably linked to the epistemic role of perception, as I explain in the first chapter of the book. The reader should also note that Lyons' *CP1* – which is attributed to me – is, as I point out in the book, Marchi's definition of CP.<sup>21</sup> It is not mine, however, because the relation between the directness condition and the epistemic condition is much more complex. Here is why. (What follows is a synthesis of parts in the second chapter of the book, so I just repeat what I have stated earlier; it is not an attempt to elaborate my views in answering Lyons' comments).

As I said, the epistemic condition is mainly needed to explain why indirect effects do not count as cases of CP. Whether cognition does or does not affect the epistemic role of some perceptual stage hinges on whether cognition does or does not affect directly that stage. This does not render the amendment of the definition by the epistemic condition redundant, because the epistemic criterion explains why indirect cognitive effects on perception do not constitute cases of CP by appealing to the reasons that initially gave rise to the problem and the ensuing discussion. The epistemic condition provides a pragmatic justification for the epistemological decision to exclude some cognitive effects cases of CP. Whether a cognitive effect is direct or indirect has consequences for the way it epistemically affects perception or a stage of it, consequences that have a value that can

be pragmatically cashed out in the given dialectic context. Thus, the relationship between the directness condition, which views CP as a direct cognitive effect on perception, and the epistemic condition, which relates CP with the repercussions of the cognitive effect for the epistemic status of perception is intricate. Let us put this as follows: CDAP (Cognition Directly Affects Perception)  $\wedge$  CP. Thus, the directness condition constitutes a sufficient condition for CP. Does it hold that if a process is CP then it is directly affected by cognition CP  $\wedge$  CD. Could indirect cognitive effects render a perceptual process CP? If they did, the necessary part does not hold, which means that the directness condition is not sufficient and necessary for CP.

This is where the epistemic criterion plays its role. If cognition either downgrades or enhances its role, a stage of perception is CP. As a lemma, cognitive influences on a stage of perception that do not affect in any way its epistemic role are not cases of CP. This excludes indirect cognitive effects on a perceptual stage from entailing CP and allows us to hold that CP  $\wedge$  CDAP (the necessary part of the extended directness condition). It follows that the extended directness condition conjoined with the revised epistemic condition yield a sufficient and necessary condition for CP. Things are intricate because, ultimately, the fact that the indirect cognitive effects are easily alleviated, which is why they should not be deemed to be cases of CP, stems from their being indirect effects that as such do not affect perceptual processing itself. It turns out that the directness condition entails a pragmatic property, namely, that the epistemic consequences of the indirect cognitive effects could easily be alleviated, which when used in the context of the dialectic surrounding CP has an epistemological consequence, that is, that they do not entail the CP of the perceptual stage.

Thus, it is not true that the epistemic condition renders the directness definition of CP redundant. It supplements it by showing why the directness condition is also a nec-

essary condition for CP; this is the first sense in which the epistemic condition is secondary. It is secondary, second, because it can be used to determine whether a cognitive influence on a perceptual stage is CP (recall that it hinges on whether the cognitive effects are tractable) only on account of the directness or indirectness of the cognitive effects because the cognitive effects are tractable only to the extent that they are indirect. Perhaps Lyons is right that things would be easier for me, and would surely better differentiate my views from those of Stokes or Marchi's, had I used the epistemic considerations as motivation for a definition of CP rather than as a constitutive part of it because then the relationship between the two would be easier to grasp. Besides, the fact that I invoke the pragmatic role of the epistemic condition in the dialectic of the discussions about CP seems to reinforce Lyons' suggestion that it would better to talk of an epistemic motivation rather than of an epistemic condition.

The problem with this line of thought is that there seems to be some sort of a bootstrapping relation between the demand that CP occurs when cognition affects perception directly, and the demand that CP occurs when cognition affects the epistemic role of perception. It is true that a cognitive effect on early vision does not threaten the epistemic role of early vision because cognition does not intervene in the process of retrieval of information from the environment and, thus, does not diminish the sensitivity of early vision to the environment because early vision does not use any cognitive information while it retrieves information from a visual scene. It follows that the epistemic role of early vision is unaffected by cognition because early vision is not directly affected by cognition since any cognitive effects on it are indirect, as I argue in the book. But one might wonder why the indirect cognitive effects do not entail that early vision is CP and the answer to this is that by being indirect they do not affect the epistemic role of early vision and the discussion concerning CP is philosophically interesting, as

many philosophers have argued, only if the cognitive effects on perception undermine its epistemic role in grounding or justifying perceptual beliefs.

Second, Lyons worries about the usefulness of adding an epistemic parameter in discussions of CP. As Lyons remarks,<sup>22</sup> *surely* we want to understand neurocognitive phenomena like CP in a purely naturalistic way that, as such, does not involve any epistemology. He does not explain the "surely" and so I cannot discuss his motivation for taking this stance; I can only say that I disagree since I do not think that CP is a purely neurocognitive phenomenon (or a set of such phenomena). Neuroscientists can examine the timing and loci of cognitive effects on visual perception, the role of sustained (endogenous) attention, etc. As I explain in the book, however, discussion on CP started when philosophers attempted to examine the repercussions of such effects for the epistemic role of perception both in scientific practice and in everyday visual interactions with the world. It follows, I think, that any discussion on CP should be sensitive to this motive.

In my view, the more interesting part of Lyons' commentary is his discussion of the directness condition that is the cornerstone of my account of CP. Lyons' comments are insightful and give me the opportunity to develop further my views. Lyons' claims that by "directness" of cognitive effects on perception I have three different things in mind. These are:

- (a) affecting perceptual processes, as opposed to pre-perceptual, or post-perceptual processes;
- (b) affecting how the inputs are processed, rather than which inputs get processed;
- (c) affecting processing "in an online manner" coming into play after that processing has begun and before it is finished.<sup>23</sup>

The first, indeed, was never meant to define or even entail directness; a perceptual process such as late vision is directly affected

by cognition and, thus the distinction between perceptual and pre- or post-perceptual is orthogonal to the direct/indirect cognitive effects on perception.

It is striking that Lyons thinks that I have offered (a) as related to the “directness” condition, which it is not, and leaves out a straightforward account of “directness” which permeates the whole book, namely that a cognitive effect on perception is direct iff the relevant perceptual processes draw on the affecting cognitive state as an information resource. Late vision, for example, is CP because it is directly affected by cognitive states since the perceptual processes use cognitive contents as an informational resource. In this way, the cognitive effects influence perceptual processing by altering the state transformations of which the processes consist. So, this view of “directness” is directly connected to Lyons’ (b).

Lyons’ glossing of the second “meaning” is useful because it captures what I mean by claiming that if a cognitive effect changes the parameters or initial conditions of the state transformation equations this does not entail CP because the equations themselves remain unchanged. This is equivalent to saying that the information included in the affecting cognitive states does not modify the state transformations. Lyons’ reconstruction of my thesis is correct, (with the exception of the fact that he does mention the information resource condition as an indispensable ingredient of my account of “directness”) since the fact that the transformation equations remain unchanged means that the input-output mapping remains the same; the function from inputs to outputs is the same. At the same time, when the initial conditions change the input may change when the parameters are related to early perceptual filters; in this sense, they act as a “filter” that “selects” the information for downstream processing, which may itself be impervious to cognitive influence. These parameters can be construed as the attentional parameters that weight the effect of sensory signals, as they

are postulated in computational models of perceptual attention.

Lyons thinks that (b)-directness is at best a metaphor standing for changes in spatial attention are like changes in inputs. Here is why:

[I]n the perceptual case, this distinction between difference in processing and difference in inputs is supposed to do real work. It’s supposed to rule out standard Necker-cube-type shifts of covert spatial attention from counting as direct (and thus cognitively penetrating) influences on early vision. But we know that unattended stimuli are often still processed to some extent, so it’s not literally true that they no longer serve as inputs. One could insist that “inputs” that don’t win the competition weren’t inputs after all, since they don’t determine the output, but this would obliterate the distinction between output-affecting changes in processing and changes of inputs.<sup>24</sup>

Notice, first, that the “metaphor” charge is not entailed or even hinted to by the example Lyons discusses. Be that as it may, let us examine first this example. It is indeed true that the directness condition is supposed to rule out (among other things) ambiguous-figures cases like the Necker-cube by suggesting that the perceptual shift is the result of covert spatial attention refocusing. This refocusing changes the structure of the image and, thus, amounts to a change in the input that will be processed (which is why is not a direct effect on perception *per* (b)-directness). Note that Lyons does not object to this (he could not even if he wanted to, as the empirical evidence is overwhelming and the only disagreements that exist concern what part of the image or attribute of the image is the focus of spatial attention). Lyons’ objection stems from the fact that attention by itself does not determine fully which inputs will be processed because unattended stimuli are processed too and, thus, they are also inputs. It follows that (b)-directness does not do an ad-

equate job in discriminating between effects that determine which inputs are processed and effects that change the state transformations because it cannot discriminate between which inputs are processed and which are not (remember unattended inputs can be processed too).

Lyons refers to “implicit perception” which is much discussed in perception science and which I also extensively discussed in the second chapter of my previous book *Cognition and perception*. There is, in fact, strong evidence that unattended stimuli are processed at various levels, including semantic processing as well. The problem with Lyons’ account is that the discussion of the processing of these unattended stimuli cannot bear on CP because the implicit perceptual processing seems to be automatic and independent of any cognitive influences (similarly to the on-line processing along the dorsal stream that is not affected by any cognitive factors). Rensink<sup>25</sup> goes as far as calling implicit perception “sensing”, which is CI, and not perception. It follows that even though attention does not exhaust the list of inputs that get processed, the processing of the inputs that are left out is CI. Thus, (b)-directness does the job of distinguishing between CP and CI in terms of directness well.

Another objection to Lyons’ account of my work is that Lyons writes as if I discuss only the repercussions of spatial attention for CP. This is not true, as my account also covers object-feature-centered attention and this applies to the treatment of pre-cueing cases as well. This will prove significant when I address Lyons’ worries concerning pre-cueing that mark his discussion of (c)-directness, to which I turn.

Lyons argues, first, that (c)-directness conjoined with (b)-directness define “directness”. I think he is wrong, at least in the sense that this was not my intention. I think that (b) and (c) draw the same distinction; they are different in that they emphasize different facets of the same phenomenon. If a cognitive effect does not alter the functions per-

formed by perceptual process and, thus, does not affect how inputs are processed (b), then it does not affect perception on-line; an on-line effect means just that, namely that the effect affects the functioning itself of the perceptual system.

He claims, second, that (c)-directness does not mark an epistemically noteworthy distinction because it does not carve nature at some real joints and is used as an *ad hoc* move to preserve CI in the face of counterevidence coming from pre-cueing studies. Lyons’ argument for this claim is two-fold. First, he discusses pre-cueing. It is worth repeating here his argument in full. I render in italics and number those crucial parts that I comment upon.

The biggest challenge to this view comes from pre-cueing effects: subjects on the lookout for some object or feature exhibit enhanced perceptual processing for that object or feature. To take just one example, hearing a word might enhance visual processing of that word, in a way that makes subjects able to see degraded letters that would otherwise remain invisible. *Because this is not a matter of attending to different regions of the distal array, there’s no chance these effects can be equated with moving one’s eyes and thereby a violation of (b)-directness (1)*. Raftopoulos responds to these kinds of cases *by admitting that they involve a change of processing function (not a change in inputs), but this doesn’t make the influence direct in the relevant sense, apparently because the rejiggering of the input/output function happens prior to the beginning of processing (2)*. It’s not “online” and thus not (c)-direct, even though it is (b)-direct. This is odd. It’s obvious – trivial, even – that *pre-perceptual* effects of the sort that concern (a)-directness are not perceptual effects; but what is at issue here is something very different. *The pre-cueing effects of interest are not “pre-perceptual” in the ((a)-directness) sense of occurring before inputs to the perceptual process are deter-*

*mined; they're "pre-perceptual" in the sense of occurring before stimulus onset (or at least before the processing of that stimulus is initiated) (3). (a)-directness carves nature at some real joints, if only because those very joints are presupposed in the distinction between perceptual and pre-perceptual processes. But there's no reason to think that (c)-directness does the same, and thus that it's anything more than an ad hoc move to retain impenetrability claims in the face of counterevidence (4).<sup>26</sup>*

(1) The claim about spatial attention is not part of (b)-directness but can be entailed by it, since a change in function changes the processing of input, whereas, a change in parameters changes the input to be processed. Lyons takes advantage of the fact that this sort of pre-cueing involves the interaction of two perceptual modalities, which I never discuss in any of my books (remember the part of modeling with which I started this text). Be that as it may, we have the following situation; an acoustic pre-cue, the sound of a word, enhances the visual processing of this same world rendering letters that would have remained invisible visible. Lyons hastens to note that there is no spatial attention involvement and, thus, there is no violation of (b)-directness. Here is where Lyons pays the price of completing ignoring the role of object/feature-based attention, which figures predominantly in my account of pre-cueing. This is so because the case is a typical case in which object-based attention plays its role. Upon hearing the word, a form of the word is activated that is transmitted to the visual perceptual circuits and this visual form guides attention and completes the non-visible features. The literature is full of models (both connectionist and standard interactive) of these processes. Thus, it is a matter of attention after all, which Lyons misses because he restricts his account of my work to spatial attention, whereas, I explicitly and in great length discuss the implications of object/feature-based attention for precueing, and I also in-

clude this form of attention in all my discussions of the role of attention for CP.

(2) Lyons claims that I admit that precueing involves a change of processing function and not a change in input. He cites pages (136, 195, and 207) from my book in which I allegedly do claim so. This is a serious misunderstanding of my claims. On page 136, I write:

This is how cognitively driven attention affects the activation values of the neurons in the relevant neuronal assemblies. This boost or sharpening occurs in the course of perceptual processing and is not just an off-line increase in the baseline activation, as is the case in pre-cueing that affects neuronal activations before stimulus onset. Attention, by biasing the competition affects directly the perceptual computations.<sup>27</sup>

But, in my account, pre-cueing typically involves only a change in base-line activation and, therefore, when I write about the way attention affects perceptual processing, obviously, I am not talking about pre-cueing but about how attention renders late vision CP and the rest of the discussion makes this very clear. What about page 195? I conclude that page with the remark that:

[i]n the next section, I argue that the cognitive effects on perception that occur through pre-cueing are not cases of CP because they do not affect directly early vision, and, also, do not also affect its epistemic role in grounding empirical beliefs.<sup>28</sup>

Since I relate directness with non-changing the input but affecting perceptual processes it would be a blatant discrepancy to have claimed that pre-cueing involves a change in the functions performed by the perceptual system and, indeed, nowhere on page 195 do I make such a claim. Finally, let us turn to page 207:

Both effects of pre-cueing (by which I

mean spatial and object-feature based pre-cueing) reflect a change in background neural activity and, thus, rig-up perceptual processing. These effects are called anticipatory effects and are established prior to viewing the stimulus. In this sense, they do not modulate processing during stimulus viewing but they bias the process before it starts; they rig-up, as it were, perceptual processing without affecting it on-line.<sup>29</sup>

I also emphasize that pre-cueing affects the preparatory or anticipatory activity of some neuronal assemblies but does not change the state transformation equations. I do not think that I need to add any further comments.

(3) Lyons claims that the pre-cueing effects that I discuss are not “pre-perceptual” in the ((a)-directness) sense of occurring before inputs to the perceptual process are determined; they are “pre-perceptual” in the sense of occurring before stimulus onset, or at least before the processing of that stimulus is initiated. I am surely missing something, but I do not understand the difference between “occurring before inputs” and “occurring before stimulus onset”. Even if there is one that I cannot see, in the book I take the two to be synonymous.

(4) Lyons argues that there is no reason to think that (c)-directness does the same as (a)-directness, and thus it is just an *ad hoc* move to retain impenetrability claims in the face of counterevidence. First, the distinction between on-line and off-line effects on perceptual processes is a very real distinction that purports to capture significant differences in neuroscience. Discussions about the dorsal system and its interactions with the ventral system are ripe with this distinction. It is the distinction between effects on a process that occur while the process is running to perform a specific function (for example, for the few thousands of ms. that the dorsal system acts automatically and without any intervention from the ventral system to di-

rect action) and effects on this process after it has ended its automatic run and is boosted from outside with information and re-run albeit in a significantly different manner (as where action is delayed in which case the automatic functioning of the dorsal system ends and information from the ventral system pours in to direct action in collaboration with the dorsal system). Moreover, Lyons’s claim in (4) seems to be a result of the three highlighted previous claims, although I do not see a connection, since all three are discredited (or so I argue), (4) does not follow from any true premises even if the argument were valid.

Lyons also argues, second, that the on-line/off-line distinction offered as a distinction between direct and indirect cognitive effects on perception, and, hence, as a springboard for distinguishing between CP and CI, fails also because it does not mark an important epistemic distinction. In fact, Lyons thinks that even if the epistemic condition is used to mark the distinction, as indeed I do when I use this condition to argue why the indirect cognitive effects on perception are not cases of CP, whereas the direct effects are, it fails to do so.<sup>30</sup> Not only does it fail, but it also entails the opposite result, namely, that pre-cueing effects constitute CP, since pre-cueing may enhance our epistemic situation by making us more sensitive to certain environmental cues. Pre-cueing effects, thus, influence the epistemic role of perception and, *per* the epistemic condition, should be deemed cases of CP.

To understand my view on this matter, let us revisit the argument in the book concerning the role of pre-cueing in perceptual processing and its epistemic impact on perception. I argued in chapter 3 that the effects of both spatial and object guided pre-cueing reflect a change in background neural activity and, thus, rig-up perceptual processing. These are anticipatory effects, are established prior to viewing the stimulus, and do not modulate processing during stimulus viewing but they bias the process before it starts; they

rig-up, as it were, perceptual processing without affecting it on-line. I also argued that pre-cueing effects do not select which information is retrieved by early vision from the visual scene once the visual scene has been determined; all information from the visual scene is retrieved in parallel in early vision. In the case of spatial pre-cueing, the anticipatory effects do not determine the percept since pre-cueing enhances the responses of all neurons tuned to the attended location independent of the neurons' preferred stimuli and keeps the differential responses of the neurons' unaltered. In the case of object/feature pre-cueing, although the anticipatory effects enhance the activity of the neurons responding preferentially to the pre-cued object or feature increasing the likelihood that they be selected eventually for further processing, early vision still retrieves in parallel information concerning all the objects and features present in the visual scene and these objects are individuated independently of whether they are targets or non-targets. All information in the visual scene is included in the iconic image. When a hypothesis is tested in late vision, the evidence in the iconic image can either confirm or disconfirm the hypothesis. Thus, by itself, pre-cueing does not introduce any confirmation bias.

If pre-cueing does not affect the information retrieved from the visual scene in early vision, the relevant cognitive states involved do not affect the selection of the "evidence" or the information against which hypotheses concerning object identity will be tested in late vision. It follows that pre-cueing and the various cognitive effects underlying it do not affect the sensitivity of early vision to the environmental data. This means, I argued in the book, that pre-cueing does not affect the epistemic role of early vision and, hence, does not entail the CP of early vision. The effect discussed by Lyons is most notable in cases of object/feature guided pre-cueing. In chapter 4 of the book, I address this problem and start by noting that in cases of feature/object pre-cueing, the in-

formation that matches the cue is highlighted and receives a prior boost in its attempt to win the attentional competition. Then, the hypothesis concerning the identity of the feature/object that matches the cue likely will be the first hypothesis to be formed and tested during late vision; pre-cueing facilitates the formation of a hypothesis concerning feature/object identity in late vision. Early vision still retrieves in parallel all the information in the visual scene. If the facilitated hypothesis passes the test, pre-cueing has increased the efficiency of perception. What is important, however, is that information incongruent with the favored hypothesis is included in the evidential basis provided by early vision so that late vision would have the possibility to reject the hypothesis independent on whether it will finally do so. Thus, my response to Lyons' comment is to agree with him about the potential improvement of a perceiver's epistemic condition owing to pre-cueing but, also and very significantly, to point out that this occurs in late vision. The epistemic condition entails that the perceptual stage that is responsible for the epistemic benefit is indeed CP but this stage is late vision and not early vision, which means that late vision is CP *per* the epistemic condition and not early vision.

Lyons has some more general interesting things to say about the role of the epistemic condition for the discussion of CP that as a matter of course I cannot address them all here. He notes, correctly, that other forms of cognitive influences have epistemic effects,<sup>31</sup> a view that I also discuss in the book and, thus, that the epistemic criterion under or over attributes CP<sup>32</sup> because cognitive effects are all the same irrespective of the locus of influence<sup>33</sup> This a claim concerning CP and its role in philosophy that has been made by Lyons.<sup>34</sup> Lyons argues that the locus of CP does not have any significant epistemic importance and that CP is equally worse when it occurs in early vision as when it occurs in late vision.<sup>35</sup> If the arguments put forth in the book are sound, the locus of CP is crucial in



determining the epistemic impact of CP; if early vision were CP, there would be no way to mitigate the harmful epistemic effects of CP, that is, the insensitivity to the data that CP may inflict on perception. The reason would be that the iconic image, the evidential basis used in late vision for hypotheses testing would be irrevocably subject to confirmation bias since information in the visual scene would be permanently lost and could not be used.

One last problem that Lyons raises<sup>36</sup> concerns the tractability of cognitive effects when they occur indirectly, which I use to argue that indirect cognitive effects are tractable and, as such, do not threaten the epistemic role of perception, from which it follows, *per* the epistemic condition, that they are not cases of CP. Lyons agrees that my reasoning applies to spatial attention (and I add feature/object-based attention) but not to pre-cueing. This is so because one may have voluntary control over where one focuses attention, but

if I have an overweening fear of ducks, what reason is there to think you could simply use pre-cueing to get me to attend to the rabbit in the image, rather than the duck? What reason is there to think that changing what someone is anticipating or cognitively attending to [...] is any easier than changing someone's beliefs?<sup>37</sup>

Consider Newton's prism experiments on white light that led him to infer the non-uniformity of white light. Suppose that when Newton asked his critics who failed to replicate his experiment to use prisms made of Islandic glass and that one of them refused to do so on account of his deep-seated fear of Islandic things.

Or, consider a situation in which two scientists disagree on what they see under the microscope and one of them asks the other to pay attention to what is found on the upper side of the image, and the other retorts that (s)he cannot do that because s(he) is afraid of the upper sides. What are the repercussions

of these for the CP of perception? What do fears that create an irrationality that distorts the sensitivity to environmental data have to do with epistemology?

I will close by returning to the problem of the iconic nature of perceptual states and their contents. I do not have the space to discuss this problem except to note that the issues Lyons raises are important and insightful, especially the relation between the iconic character of contents and states, the relation between iconic and analog contents, etc., even though I do not agree with him on all of them. In my new book, I attempt to address these and much more concerning the iconicity of perception. For the time being, the reader can read a first attempt to deal with some of these problems.<sup>38</sup> I will mention only one thing. Lyons thinks that affordances, for example, require symbols. This is not at all the received view in the relevant literature. Many would even argue that they do not require representations at all in order to function (the reader should consult the literature on dynamic cognitive systems).

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Marchi's commentary impresses both by the depth of his reading of my book, and by the fine grain of the analysis of the themes he chooses to analyze. This means that he offers me the opportunity to explain in more depth some of the views I exposed. Marchi concentrates on my account and critique of Siegel's views concerning the relation between attention and CP. In my rendering of Siegel's selective and responsive mode in which cognition may affect perception, both modes are in essence the selection of information through attention either from the distal scene (Siegel's selective mode), or from the iconic image, that is, the information retrieved from the visual scene during early vision (Siegel's responsive mode). At the same time, Marchi correctly adds, I agree with Siegel that evidence handpicking from the distal scene by attention is not a genuine case of

CP. These two theses raise the following concern

If attention also operates evidence selection from the iconic image in late vision, how does this differ from pre-perceptual evidence selection in such a way that the former constitutes a case of CP while the latter does not?<sup>39</sup>

Marchi will return to this point from another angle later on, but in the meantime, he raises another concern. In my view, the epistemic role of early vision is not undermined by the role of cognitively driven attention that operates before the onset of perceptual processing (as in the case of both spatial or feature/object-based pre-cueing), because early vision retrieves from the distal scene all the information that the scene contains. But, Marchi argues:

[o]ne problem that I see here is that such an epistemic role may also be assigned to the retina, or, more radically, to the external world itself. As long as the epistemic role of *x* is that of providing evidence for further deliberation or processing, distal stimuli in the visual scene and their retinal projections also perform this role, early vision becomes just the next step in this evidence delivery process.<sup>40</sup>

This is a problem because one would not expect the retina or the external world to matter to the epistemic status of visual perception. Marchi is of course correct that the retina could play such a role, but when one discusses the epistemic role of some faculty or capacity it goes almost without saying that the examined faculty or capacity yields representations of the environment (which include internal states); the epistemic credentials of a faculty or capacity are assessed in terms of the epistemic success or failure of the representations the faculty or capacity yields. The rings of the trees may be said in some sense to represent the age of the tree but they play

no direct epistemic role (which means that they can be used for epistemic purposes). I have suggested in the book that the epistemic role of early vision is not undermined by indirect attentional effects because these effects do not undermine its sensitivity to the data. The retina and other mechanisms whose function is not described in terms of representations cannot be said to be sensitive or insensitive to the data in the way “sensitivity” is used in assessments of epistemic roles because sensitivity is related to the possibility of misrepresentation and the retina cannot misrepresent anything, although it may fail in its role due to some organic defect.

Marchi next raises an important question that is worth rendering in full:

in the light of the above discussion of the epistemic role of early vision, and of the way Raftopoulos thinks of selection effects (§3), the CP of late vision endorsed by Raftopoulos may be exposed to analogue objections to those about pre-perceptual input selection failing to qualify as CP, that Raftopoulos also accepts. If in late vision cognition through attention biases perceptual processing toward an outcome consistent with the content of the cognitive states, but this happens by a selective sampling of the “cognitively neutral” iconic image, which is the input to late vision delivered by early vision, how can such a biasing process ultimately be distinguished from pre-perceptual input selection through spatial attention? [...] from an epistemic point of view, it seems that late vision is also merely selecting where to look, i.e. what evidence to retrieve from the iconic image, and if such cases fail to constitute CP in the case of spatial attention and pre-perceptual input selection, Raftopoulos needs to offer a clearer explanation of why this is not the case in late vision.<sup>41</sup>

This, as a matter of course, is the same concern raised by Marchi in his commentary.<sup>42</sup>

Why evidence handpicking from the distal scene is not CP but evidence handpicking from the iconic image during late vision is CP?

I tried to explain this in chapter 4 of the book and here I will attempt to explicate my views on this problem. The effects of cognitively-driven attention on early vision being indirect do not affect the processing of the input during early vision and early vision retrieves from the environment all the information that is there, even in the form of ensemble statistics in peripheral vision. Early vision is, in this sense, sensitive to the environmental data. It does not choose which part of the evidence to allow to get in excluding some other evidence; it allows all evidence to get in and reach late vision. Early vision outputs this information to the “evidence jury” for further use. This jury has all the information at its disposal and this exhausts the epistemic role of early vision; how the jury handles this evidence concerns late vision and not early vision. If it chooses to pick information consistent with a previously held hypothesis and ignore recalcitrant evidence against this hypothesis, this downgrades late vision but not early vision. For reasons discussed in chapter 4, handpicking from the distal scene owing to the indirect cognitive effects on early vision is relatively easily tractable, say, by refocusing spatial or feature/object-based attention. This predicament is tractable, albeit in a more complicated manner than a simple attentional refocusing; it may involve perceptual training and learning, for example. This is why even the CP of late vision does not justify relativistic concerns.

The epistemic status of late vision can be rescued from a previous downgrade by, say, revisiting the information in the iconic image as it stored in iconic memory or fragile short-term memory and reexamining it. This way, recalcitrant information previously ignored can be brought forth and used to reject the hypothesis previously favored. For this to happen, however, the recalcitrant information must be included in the iconic image

for if for some reason this information was not there, late vision’s downgrade would be intractable since there would be no evidential basis to reject the favored hypothesis. This is where the difference between handpicking during early vision and handpicking during late vision makes its presence felt. Handpicking during early vision may favor some interpretation of the distal scene but all the information in the scene gets in, as it were, because early vision processing itself is not affected. Handpicking during late vision is tractable exactly because all information in the visual scene is available to the jury and this allows the jury to revisit the evidence and, perhaps, change its mind. In other words, evidence handpicking in early vision is a curious sort of handpicking. Owing to such handpicking, some information may be favored in such a way that in late vision a certain hypothesis may be chosen but this handpicking allows all the information in the distal scene to get in. If you will, it is a handpicking that will show in late vision but is not felt in early vision. Handpicking during late vision is a real selection of information by late vision of some information at the expense of some other information that is used by late vision.

Marchi’s reading of my work made me realize that I should have been more careful in explaining the handling of the evidence by early and late vision and that I should have avoided talking of handpicking in the case of early vision.

Marchi resourcefully uses the jury metaphor to press his argument home:

One may describe both cases (my note, the selection of information by early and late vision owing to cognitive influences on perceptual processing) as a neutral jury that reaches an optimal verdict while being fed only part of the available evidence due to an independent biasing mechanism. In this situation there is no CP because the epistemic role of the jury itself would not be compromised and the influ-

ence is external to the jury. It does not matter if it was a corrupted policeman (external to the tribunal) or a corrupted prosecutor (internal to the tribunal) who manipulated the evidence. Alternatively, one may interpret the two cases of being cases of a corrupted and biased jury which pays attention only to evidence congenial to its prejudice. Here both instances of selection could potentially be cases of CP as the epistemic role of the jury is compromised in both, regardless of when the neutral evidence has been being gathered and presented. In any case, it seems to me that both cases deserve equal treatment when it comes to selection of inputs being or not being a case of CP. This is why refocusing of attention helps mitigating the effects of cognitive influences in both cases, as Raftopoulos states in the passage mentioned above.<sup>43</sup>

In the first case, both early vision and late vision are a neutral jury that is fed only part of the evidence. There is no CP because the epistemic role of the jury itself would not be compromised and any cognitive influence is external to the jury. This description, however, does not capture the point that I tried to make in the book. Early vision outputs to late vision all the evidence in the visual scene, and, thus, it is not true that late vision has access only to a part of the available evidence, as Marchi contends. Early vision, of course, has access to all the evidence too, since it is directly, in a *de re* manner, related to the visual scene, and any attentional bias does not affect the information processing of early vision. Late vision, owing to cognitive influences, may handpick the evidence and this selection and the relevant cognitive influence are internal to the process or jury and, hence, are cases of CP; in other words, late vision has all the evidence but mishandles it owing to cognitive influences. The indirect cognitive effects on early vision, on the other hand, do not make early vision handpick the evidence; everything gets in (well, almost every-

thing considering the ensemble statistics).

In the second case Marchi describes, both instances of selection (that is, selection by early vision and selection by late vision) are potential cases of CP because the corrupted jury pays attention only to the evidence confirming the hypothesis conforming to its prejudice. In this case, it does not matter when the neutral evidence was gathered. This rendering of my views fails too to render justice to what I tried to convey. Marchi's description captures what transpires in late vision; the evince is indeed mishandled. Not so in early vision. This is so because early vision retrieves all the evidence in the visual scene and delivers it to late vision. As I said, this exhausts its epistemic role. Early vision does not have an active role in the formation of the percept; the identification and categorization of the objects in a visual scene are in the purview of late vision. This is why the cognitive effects on early vision do not affect its epistemic role; being indirect they do not affect the processes of early vision and, thus, do not affect its role in information gathering.

The natural conclusion of Marchi's astute comments is that if both in early vision and in late vision attention handpicks the evidence, whether it be from the distal scene or the iconic image, early vision and late vision are both either CI or CP. This means that epistemically speaking they are on the same boat, which seems to vindicate both Lyons' and Siegel's views.<sup>44</sup> The answer to this problem is inextricably related to the considerations developed in the previous paragraphs. For the reasons developed there, I think that early vision and late vision are not on equal footing when it comes to their epistemic role in perceptual processing. To put it in a nutshell, if early vision were directly affected by cognition and, thus, its information retrieving processes were affected and only information conforming to some belief previously entertained were selected from the visual scene, the neglected information would be irrevocably lost once the visual scene has receded. Even when the visual scene is present, revisiting it would not amelio-

rate the epistemic situation of the perceiver because early vision, affected by cognition, would still handpick the evidence that conforms to the biasing cognitive state and ignore recalcitrant evidence.

It follows that no evidence could disconfirm the hypothesis favored by the biased evidence and, thus, the cognitive downgrade would be intractable, paving the way to all sorts of constructivist or relativistic considerations. The fact that early vision is not directly affected by cognition, which means that early vision is sensitive to the environmental input, ensures that the epistemic downgrade of late vision is tractable blocking the way to perilous relativistic conclusions.

## Notes

<sup>1</sup> Cf. A. RAFTOPOULOS, *Cognitive penetration lite and nonconceptual content*, in: «Erkenntnis», vol. LXXXII, n. 5, 2017, pp. 1-26.

<sup>2</sup> A. VOLTOLINI, *Cognitive penetrability and late vision*, in: «Rivista internazionale di Filosofia e Psicologia», vol. XI, n. 3, 2020, pp. 363-371, here p. 365.

<sup>3</sup> Cf. A. RAFTOPOULOS, *Cognitive penetrability and the epistemic role of perception*, Palgrave Macmillan, Basingstoke 2019, pp. 300-307.

<sup>4</sup> Cf. A. RAFTOPOULOS, *Can nonconceptual content be stored in visual memory?*, in: «Philosophical Psychology», vol. XXIII, n. 5, 2010, pp. 639-668.

<sup>5</sup> A. VOLTOLINI, *Cognitive penetrability and late vision*, cit., p. 368.

<sup>6</sup> *Ibid.*, pp. 368-369.

<sup>7</sup> T. BURGE, *Reply to Block: Adaptation and the upper border of perception*, in: «Philosophy and Phenomenological Research», vol. LXXXIX, n. 1, 2014, pp. 573-583, here p. 574.

<sup>8</sup> A. VOLTOLINI, *Cognitive penetrability and late vision*, cit., p. 369.

<sup>9</sup> *Ibidem.*

<sup>10</sup> *Ibidem.*

<sup>11</sup> J. LYONS, *The cognitive impenetrability of early vision: What's the claim?*, in: «Rivista internazionale di Filosofia e Psicologia», vol. XI, n. 3, 2020, pp. 372-384, here p. 374.

<sup>12</sup> *Ibid.*, p. 374 and 382, note 8.

<sup>13</sup> *Ibid.*, p. 375.

<sup>14</sup> Cf. A. RAFTOPOULOS, *Cognitive penetrability and the epistemic role of perception*, cit., pp. 173-177.

<sup>15</sup> *Ibid.*, pp. 186-191.

<sup>16</sup> Cf. A. RAFTOPOULOS, *Cognition and perception: How do psychology and neural sciences inform philosophy?*, MIT Press, Cambridge (MA) 2009.

<sup>17</sup> A. RAFTOPOULOS, *Cognitive penetration and the epistemic role of perception*, cit., p. 118.

<sup>18</sup> Cf. W. WU, *Visual spatial constancy and modularity: Does intention penetrate vision?*, in: «Philosophical Studies», vol. CLXV, n. 2, 2013, pp. 647-669; W. WU, *Shaking up the mind's ground floor: The cognitive penetration of visual attention*, in: «The Journal of Philosophy», vol. CXIV, n. 1, 2017, pp. 5-32.

<sup>19</sup> Cf. F. MACPHERSON, *Cognitive penetration of colour experience: Rethinking the issue in light of an indirect mechanism*, in: «Philosophy and Phenomenological Research», vol. LXXXIV, n. 1, 2012, pp. 24-62.

<sup>20</sup> J. LYONS, *The cognitive impenetrability of early vision*, cit., pp. 377-378.

<sup>21</sup> Cf. F. MARCHI, *Attention and cognitive penetrability: The epistemic consequences of attention as a form of metacognitive regulation*, In: «Consciousness and Cognition», vol. XLVII, 2017, pp. 48-62.

<sup>22</sup> Cf. J. LYONS, *The cognitive impenetrability of early vision*, cit., p. 378.

<sup>23</sup> *Ibid.*, p. 379.

<sup>24</sup> *Ibid.*, p. 380.

<sup>25</sup> Cf. R.A. RENSINK, *The dynamic representation of scenes*, in: «Visual Cognition», vol. VII, n. 1-3, 2000, pp. 17-42; R.A. RENSINK, *Seeing, sensing, and scrutinizing*, in: «Vision Research», vol. XL, n. 10-12, 2000, pp. 1469-1487.

<sup>26</sup> J. LYONS, *The cognitive impenetrability of early vision*, cit., pp. 380-381.

<sup>27</sup> A. RAFTOPOULOS, *Cognitive penetrability and the epistemic role of perception*, cit., p. 136.

<sup>28</sup> *Ibid.*, p. 195.

<sup>29</sup> *Ibid.*, p. 207.

<sup>30</sup> Cf. J. LYONS, *The cognitive impenetrability of early vision*, cit., pp. 381-282.

<sup>31</sup> *Ibid.*, p. 378.

<sup>32</sup> *Ibid.*, p. 381.

<sup>33</sup> *Ibid.*, p. 378.

<sup>34</sup> Cf. J.C. LYONS, *Circularity, reliability, and the cognitive penetrability of perception*, in: «Philosophical Studies», vol. XXI, n. 1, 2011, pp. 289-311.

<sup>35</sup> Cf. *ibid.*, p. 297 and pp. 303-304.

<sup>36</sup> Cf. J. LYONS, *The cognitive impenetrability of early vision*, cit., p. 381ff.

<sup>37</sup> *Ibid.*, pp. 381-382.

<sup>38</sup> Cf. A. RAFTOPOULOS, *The representational for-*

*mats of cognition and perception and their interface: Part 1*, in: S. DENISON, M. MACK, Y. XU, B.C. ARMSTRONG (eds.), *Proceedings of the 42nd CSS conference*, Cognitive Sciences Society, Toronto 2020, pp. 3219-3225.

<sup>39</sup> F. MARCHI, *The epistemic role of early vision: Cognitive penetration and attentional selection*, in: «Rivista internazionale di Filosofia e Psicologia», vol. XI, n. 3, 2020, pp. 385-396, here p. 391.

<sup>40</sup> *Ibid.*, p. 392.

<sup>41</sup> *Ibid.*, p. 393.

<sup>42</sup> *Ibid.*, p. 391.

<sup>43</sup> *Ibid.*, p. 394.

<sup>44</sup> Cf. J.C. LYONS, *Circularity, reliability, and the cognitive penetrability of perception*, cit.; S. SIEGEL, *Epistemic evaluability and perceptual farce*, in: J. ZEIMBEKIS, A. RAFTOPOULOS (eds.), *The cognitive penetrability of perception: New philosophical perspectives*, Oxford University Press, Oxford 2015, pp. 405-425.

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