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Psychology and Neuroscience. The Distinctness Question

Abstract: In a recent paper, Gualtiero Piccinini and Carl Craver have argued that psychology is not distinct from neuroscience. Many have argued that Piccinini and Craver's argument is unsuccessful. However, none of these authors have questioned the appropriateness of Piccinini and Craver's argument for their key premise - that functional analyses are mechanism sketches. My first and main goal in this paper is to show that Piccinini and Craver offer normative considerations (on what functional analyses should be) in support of what is a descriptive premise and to provide some guidelines on how to argue for this premise. My second goal is to show that the distinctness question should be of great significance for philosophy of cognitive science.

1. Introduction

Traditionally, the debate on the question of the autonomy of psychology from neuroscience has been intertwined with the debate on the question of the reducibility of psychology to neuroscience. The reducibility of psychological theories to neuroscientific theories has been taken to be a necessary condition for the lack of autonomy of psychology from neuroscience. Many philosophers have been of the opinion that psychological theories are actually irreducible to neuroscientific theories and, as a consequence, that psychology is autonomous from neuroscience¹. In the wake of the recent mechanistic turn in philosophy of neuroscience (Craver 2007; Bechtel 2008), however, some

philosophers have initiated a new way of examining the autonomy question, i.e., from a mechanistic perspective (Bechtel 2007; Piccinini and Craver 2011). In particular, Gualtiero Piccinini and Carl Craver have argued that psychology and neuroscience are not distinct and that, as a consequence, psychology is not autonomous from neuroscience. In this paper, the focus is put on the distinctness question, the autonomy question being left aside for later investigation.

Piccinini and Craver's Indistinctness Thesis - (Indistinctness) for short - is the thesis that psychology and neuroscience *as explanatory practices* are not distinct. Thus, their argument is an argument for the thesis that psychological *explanations* are neuroscientific *explanations*. Piccinini and Craver take it that psychological explanations are functional analyses and neuroscientific explanations are mechanistic explanations. The key premise of Piccinini and Craver's argument for (Indistinctness) is the thesis that functional analyses *are* mechanism sketches, that is, abstract mechanistic explanations. I will call this thesis (Sketch) for short. My first and main goal in this paper is to spell out (Sketch) in the best possible way, that is, in a way which makes it both plausible and non-trivial while being faithful to the spirit if not to the letter of Piccinini and Craver's paper, and to determine what the best way of arguing for it is. This paper sits somewhere between Piccinini and Craver and their critics in that I am sympathetic to (Sketch), but I also think that the argument that Piccinini and Craver put forward in its support falls short. However plausible and non-trivial (Sketch) might be, though, it would be of little interest if the distinctness question itself was of little significance for philosophy of cognitive science. So, my second goal in this paper is to show that in fact the distinctness question is of great significance for philosophy of cognitive science.

The first part of the paper will be devoted to (Sketch). One brick at a time, I will build up what I believe to be the best way of spelling out (Sketch) (§2). First, I will introduce the theoretical background to Piccinini and Craver's argument for (Indistinctness) and informally spell out the argument itself, as a way to motivate (Sketch) (§2.1). Second, I will show that the scope of (Sketch) cannot be what

Piccinini and Craver say it is (§2.2.) (more specifically, that it should be restricted to explanations of cognitive capacities which are analyses of the system into components functionally individuated). Third, I will argue that (Sketch) actually needs to be reformulated: (Sketch) should be formulated as the thesis that there are no such things as functional analyses, only mechanism sketches (§2.3.) Spelled out this way (Sketch) may look like a non-starter, but of course it all depends on the interpretation given to the terms of the thesis. I will make this clear and we will see that it isn't a non-starter. In the second part of the paper, I will determine how (Sketch) should be argued for and I will explain why (Indistinctness) is significant (§3). Many have argued that Piccinini and Craver's argument is unsuccessful (Barrett 2014; Stinson 2016; Weiskopf 2016; Roth and Cummins 2017; Shapiro 2017). However, none of these authors have questioned the appropriateness of Piccinini and Craver's argument for (Sketch). I will show that Piccinini and Craver offer *normative* considerations (on how our psychological theories should be supported) in support of what is a *descriptive* thesis and that, as a consequence, the considerations they bring to bear on its truth have no bearing on it (§3.1). Then I will present a way one could argue for it, namely, by showing that psychologists, with time, replaced, in their explanations of cognitive capacities, functional terms by terms referring to neural structures (§3.2). Finally, I will bring out the significance of the distinctness question by making clear that the truth of (Indistinctness) would have important consequences for the debate over the integration of psychology and neuroscience (§3.3).

2. Piccinini and Craver's Argument for the Indistinctness of Psychology and Neuroscience

2.1 The argument and its key premise: (Sketch)

Piccinini and Craver assume both that psychological explanations are functional analyses (Fodor 1968; Dennett 1975; 1978; Cummins 1983; 2000) and that neuroscientific explanations are mechanistic explanations (Craver 2007; Bechtel 2008)². I will embrace these assumptions. As the former view is the

received view in philosophy of psychology and the latter is the most widely endorsed in philosophy of neuroscience, I can do so without worrying about the relevance of my discussion. I will also draw on Piccinini and Craver's presentation of these theses³. I will start by considering psychological explanations and then move to neuroscientific explanations. Three questions will structure my exposition: (1) what are the explananda, (2) of what kinds are the requests for explanation, and (3) of what kinds are the answers to those requests.

The explananda of psychological explanations can be either (i) psychological capacities (for example, the capacity to recognize faces) or (ii) actions (for example, the action of waving at someone).

(i) The request for an explanation of a capacity takes the following form: what makes it the case that one has the capacity to X (with X being, for example, recognize faces)? Two kinds of answers can be given. One can either, (a) if the capacity is complex, analyze the capacity into subcapacities (for example, the capacity to follow a recipe will be analyzed into the capacities to read, to recognize the ingredients mentioned, etc.) (b) if the capacity is simple, analyze the system which has this capacity into its components functionally individuated (for example, a system which has the capacity to recognize faces will be analyzed into, among other components, a component which stores the representations of the faces previously encountered). Piccinini and Craver call an instance of (a) a “task analysis” and an instance of (b) a “boxology”. These two terms presumably come: (a), from the fact that an analysis of a capacity into subcapacities will more often than not mirror the analysis of the task at hand; and (b), from the fact that an analysis of a system in its components can be usefully represented by a box and arrow diagram - the boxes representing the components functionally individuated and the arrows the flow of information from one component to the next.

(ii) The request for an explanation of an action takes the following form: why did she/he/it X (with X being, for example, wave at someone)? An answer is given in terms of the states of

the system functionally individuated. To illustrate: beliefs and desires are standardly taken to be such states and an explanation would go as follows: she waved at Yosra because she desired to greet her and believed that Yosra would see her if she moved her arm in such and such a way. Piccinini and Craver call these explanations “functional analyses by internal states” (referred to hereafter as Internal States Explanations) because the states mentioned are standardly considered to be internal states of the thinker and are functionally individuated.

Piccinini and Craver use the phrase “functional analysis” to cover Task Analysis, Boxology and Internal States Explanation. This use is justified to the extent that Boxologies and Internal States Explanations are both functional analyses (components and states, respectively, are being functionally individuated) and that, when a capacity is complex, a Task Analysis is preliminary to a Boxology.

I now turn to neuroscientific explanations. As in the case of Task Analyses and Boxologies, the explananda of neuroscientific explanations are psychological capacities. The request for an explanation of a capacity takes the same form: what makes it the case that one has the capacity to X (e.g., recognize faces)? However, an answer will be an analysis of the system which has this capacity into its components *both* structurally and functionally individuated in order to determine how their organized activities underlie the system having the explanandum capacity; as Piccinini and Craver aptly note: “Both functional and structural properties of components are aspects of mechanistic explanation.” (Piccinini and Craver 2011, 291). For example, a system which has the capacity to recognize faces could be analyzed into, among other components, the fusiform face area, some part(s) of this area performing the storage of the representations of faces encountered⁴. These explanations have been called “mechanistic explanations” to stress both their structural and their processual aspects.

Functional analyses and mechanistic explanations seem *prima facie* to be distinct. Indeed, whereas functional analyses either do not mention components (neither Task Analyses nor Internal States Explanations do) or individuate them functionally (as Boxologies do), mechanistic explanations do and

individuate them both functionally and structurally. To wit, psychological explanations do not mention parts of the nervous system, but neuroscientific ones do. These appearances, however, do not settle the matter. In his 2007 book, Craver distinguished between complete mechanistic models and incomplete mechanistic models, which he called “mechanism sketches”: “A *mechanism sketch* is an incomplete model of a mechanism. It characterizes some parts, activities, or features of the mechanism’s organization, but it leaves gaps.” (Craver 2007, 113)⁵. In the paper under discussion, Craver and Piccinini leverage this distinction to try closing the gap between functional analyses and mechanistic explanations. For them, we should take functional analyses to be mechanism sketches. And, from this, it follows that psychological explanations are mechanistic explanations:

“The conclusion that functional analyses are mechanism sketches leads to a simple argument that psychological explanation is mechanistic. It is generally assumed that psychological explanation is functional [...]. If psychological explanation is functional and functional analyses are mechanism sketches, then psychological explanations are mechanism sketches. Mechanism sketches are elliptical or incomplete mechanistic explanations. Therefore, psychological explanations are mechanistic.” (Piccinini and Craver 2011, 284)

In this context, in order to determine whether the Indistinctness Thesis - (Indistinctness) for short - is true or false, we need to determine whether (Sketch) is true or false. In the next subsection, I will show that, pace Piccinini and Craver, the scope of (Sketch) should be restricted in two respects.

2.2 On the scope of (Sketch)

In their paper, Piccinini and Craver are only concerned with psychological explanations of capacities:

“When psychologists explain behavior, the explanations typically make reference to causes that precede the behavior and make a difference to whether and how it occurs [...] By contrast, when psychologists explain psychological capacities [...], they typically do so by showing that

these complex capacities are made up of more basic capacities organized together. In this paper, we focus exclusively on the latter sort of explanation [...]” (Piccinini and Craver 2011, 283–84).

Thus, they take Internal States Explanations to be explanations of capacities. My presentation of the different types of functional analyses diverged from Piccinini and Craver's in this respect: I have said that Internal States Explanations are explanations of actions. I therefore don't think that Internal States Explanations are explanations of capacities. Let me explain why.

Piccinini and Craver write that the “functional analysis of the capacities of [a] system is based” on “the functional organization of the system” (Piccinini and Craver 2011, 297). I subscribe to this thesis. However, pace Piccinini and Craver, “the functional organization of the system” is not constituted (i) by *states* of some components of the system only but (ii) by *components* of the system functionally individuated. Capacities are dispositions. Dispositions have manifestation conditions. The manifestation of a disposition rests on the components functioning one way in some environmental conditions and another way in some other environmental conditions. That is to say that the components will be in distinct states in distinct environmental conditions. But, to explain what doesn't vary (our having a disposition) one cannot appeal only to what does vary (the states). Therefore, in order to explain capacities, one needs to invoke the components themselves, not only their states. (And, in any case, even if we want to explain *the manifestation of* capacities, we need to invoke the components the states are states of because these states are individuated by the role they have *in the components themselves*, not in the whole organisms. Indeed, these states are states of the components, by opposition to mental states which are states of the whole organism.)

Though I go against Piccinini and Craver in saying that Internal State Explanations are not explanations of capacities, Piccinini and Craver also argue at length that “internal states postulated by a functional analysis must be states of the system's components” (Piccinini and Craver 2011, 298). Therefore, I am

merely drawing the conclusion that I think they should have drawn from themselves. Given that Internal States Explanations are not explanations of psychological capacities while mechanistic explanations are, there is no hope of showing that they are not distinct. From this fact I infer that the scope of (Sketch) should be restricted to Boxologies and Task Analyses.

That is not all. The scope of (Sketch) should be restricted further still. I wrote in the above that when a capacity is complex, a Task Analysis is preliminary to a Boxology. I should add that it is also preliminary to a mechanistic explanation. This doesn't mean that Boxologies and mechanistic explanations cannot explain complex capacities. Indeed, by using the compositional structure of complex capacities, psychologists and neuroscientists can build, from explanations of basic capacities, (derivative) explanations of all complex capacities of which they are the components. To illustrate, once we have explained why we have the capacity to recognize words, we have one of the building blocks of an explanation of why we have the capacity to read. It is but an instance of a divide and conquer strategy widely used in science: the breaking apart of an explanandum into further (simpler) explananda. A quick glance at the table of content of a handbook of, respectively, cognitive psychology (Reisberg 2013) and cognitive neuroscience (Ochsner and Kosslyn 2014) shows that this strategy is indeed used in psychology and neuroscience alike.

What this means, however, is that Task Analyses on the one hand and Boxologies and mechanistic explanations on the other, don't have the same explananda: while the former ones only explain complex capacities, the latter ones explain both basic and - compositionally - complex capacities. And, assuming that Task Analyses are bona fide explanations (a claim which I won't dispute), this points to the fact that we are here presented not with one but with two distinct types of explanations, regulated by distinct norms (in order to explain why a system has a given capacity the former ones ascribe capacities to the system while the latter ones ascribe capacities to parts of the system). Even if Piccinini and Craver are right in saying that “[...] if a sub-capacity is a genuinely explanatory part of the whole

capacity, as opposed to an arbitrary partition (a mere piece or temporal slice), it must be exhibited by specific components or specific configurations of components.” (Piccinini and Craver 2011, 293), the point remains unaffected.

Because Task Analyses and mechanistic explanations are two distinct types of explanations, there is no hope of showing that they are not distinct. From this fact I infer that the scope of (Sketch) should be further restricted, to Boxologies only. In excluding Task Analyses from the scope of (Sketch), I go against Piccinini and Craver once more. However, I take it that it is, once more, nothing but a mild departure. As Boxologies explain all capacities both basic and complex, showing that Boxologies are mechanism sketches should be more than good enough. As a consequence, in what follows, I will examine their argument for the thesis that *Boxologies* are mechanism sketches. Though, as we will see in the next sub-section, the thesis needs mending.

2.3 On the formulation of (Sketch)

I will get to my proposed formulation of (Sketch) in three steps. First, I will show that Piccinini and Craver are committed to an intentional reading of the phrase “mechanism sketch”: for an explanation to be a mechanism sketch, the explainer has to have specific intentions and beliefs. Second, I will argue that if this is how the phrase “mechanism sketch” has to be understood, then (Sketch) should be given the following formulation: “There are no such things as functional analyses.” Finally, (Sketch) is the key premise of Piccinini and Craver's argument for (Indistinctness) and this argument refers to “psychology” and “neuroscience”. I therefore need to explain, if this interpretation is to be given of (Sketch), how these two words are to be understood in this context.

Mechanism sketches leave gaps. According to Craver, the gaps left are marked by “black boxes or question marks” or “*filler terms*” (e.g. “activate”, “inhibit”, etc.) Now, imagine replacing *all* the neural terms used by black boxes and/or filler terms. Can we find a way of understanding the phrase

“mechanism sketch” such that the product of this process can still legitimately be called a mechanism sketch? It better be the case for Piccinini and Craver. Indeed, psychologists, when offering functional analyses, only use black boxes and filler terms. Therefore, if functional analyses are to be mechanism sketches, then the phrase “mechanism sketch” should be applicable even when no neural term is used. And Piccinini and Craver want to say that functional analyses are mechanism sketches. Fortunately, there is a way to do so.

The phrase “mechanism sketches” can be given two distinct readings: a semantic one, according to which being a mechanism sketch is something that contains some neural terms and some black boxes and/or filler terms (semantically individuated⁶); and, an intentional one, according to which it is a matter of explainers having the intention to refer to neural structures by means of the terms they use. If we favor the former, then, as a matter of logical necessity, there cannot but be neural terms in a mechanism sketch and, as a consequence, the phrase “mechanism sketch” is not applicable when no neural term is used. Indeed, if, by definition, mechanism sketches contain some neural terms and some black boxes and/or filler terms, then something which doesn't contain any neural terms cannot be a mechanism sketch. If we favor the latter, however, the product of the process I described can still legitimately be called a mechanism sketch. Indeed, as long as the explainer who uses black boxes and/or filler terms has the requisite intentions (i.e. to refer to neural structures), then the phrase “mechanism sketch” is applicable. In conclusion, the phrase “mechanism sketch” in (Sketch) should be given an intentional reading: Piccinini and Craver need to be able to apply the phrase “mechanism sketch” even in contexts where no neural term is used; while the semantic reading doesn't make it possible for them to do so, the intentional reading fits the bill.

We are now in a position to see that Piccinini and Craver's way of stating (Sketch) is not optimal. Note first that the phrase “functional analysis” can also be given either a semantic or an intentional reading. Then, consider the following dilemma:

(i) Let us assume that the phrase “functional analyses” should also be given an intentional reading, i.e. what makes it so that something is a functional analysis is the explainer's intention to refer to functional states, not neural structures, by means of it. If so - and assuming that the explainer is coherent - functional analyses clearly are *not* mechanism sketches: the explainer cannot both have the intention to refer to neural structures and the intention to refer to functional states, not neural structures, at the same time.

(ii) Let us therefore assume instead that the phrase “functional analysis” should be given a semantic reading, i.e. what makes it so that something is a functional analysis is that it contains functional terms. If so, saying that functional analyses are mechanism sketches is a category mistake. Indeed something whose individuation conditions are intentional (mechanism sketches) cannot be identical to something whose individuation conditions are semantic (by assumption, functional analyses).

Therefore, (Sketch), i.e. the thesis that functional analyses are mechanism sketches, is false regardless of the way we interpret the noun phrase “functional analyses”. Can (Sketch) be reformulated in such a way that it doesn't turn out to be trivially false? It can. Here is how it goes: (Sketch) is the thesis that there are no such things as functional analyses, only mechanism sketches (“functional analyses” and “mechanism sketches” being *both* used in their intentional sense⁷). Now that we have stated (Sketch) correctly, we can clearly see why (Sketch) has attracted much criticism. It is a radical thesis!⁸⁹

(Sketch) therefore says that when explainers explain cognitive capacities without using neural terms they do so with the intention to refer to neural structures by means of black boxes and fillers terms. (Sketch) is the key premise in Piccinini and Craver's argument for (Indistinctness). The latter is a thesis about psychology and neuroscience, not about functional analyses and mechanism sketches. It thus remains to be seen how to go from functional analyses and mechanism sketches to, respectively, psychology and neuroscience. That is, how should “psychology” and “neuroscience” be interpreted in

this context? Disciplines are social entities. But, from (Sketch), it clearly doesn't follow that psychology as a discipline is not distinct from neuroscience as a discipline. Therefore, “psychology” and “neuroscience” do not refer to disciplines in this context. I take it that they both refer to explanatory practices¹⁰. First, terms like “psychology” and “neuroscience” can legitimately be so understood. Second, it does follow from (Sketch) that there is no such thing as psychology as an explanatory practice. But there is more to disciplines than explanatory practices. Hence the fact that it doesn't follow from (Sketch) that psychology and neuroscience as disciplines aren't distinct.

I should sum up the conclusions I have reached in this section. (Sketch) is the thesis that there are no such things as functional analyses, only mechanism sketches (in the intentional sense). (Sketch) should therefore be evaluated by examining the explanations given using black boxes and filler terms. And saying that psychology is not distinct from neuroscience - (Indistinctness) - amounts to saying that there is no such thing as psychology as an explanatory practice, only neuroscience. The interpretation I have given of the key terms used in these theses should be kept in mind for the remainder of this paper.

3. How to Argue for the Indistinctness of Psychology and Neuroscience and Why Do so?

3.1 A criticism of Piccinini and Craver's argument for (Sketch)

As we have seen, Piccinini and Craver's argument for (Indistinctness) rests on (Sketch), the thesis that there are no such things as functional analyses, only mechanism sketches. I agree with previous commentators (Barrett 2014; Stinson 2016; Roth and Cummins 2017; Shapiro 2017) in thinking that Piccinini and Craver's argument for (Sketch) rests on the thesis that functional analyses are not explanatory, unless the theories they make use of are supported by neuroscientific evidence. Here is, in a nutshell, how Piccinini and Craver support this thesis. The theories psychologists use only answer to behavioral evidence. Now, two theories postulating the existence of distinct internal mechanisms

can agree on all their behavioral predictions, that is, they can be empirically equivalent. But if we have no means of distinguishing a theory which accurately describes the components of the system from one which doesn't, our theories are left unsupported. As a consequence, explanations performed by means of these theories are not explanatory.

And here is textual evidence that this is what Piccinini and Craver have in mind. In their paper, they rehearse the same argument three times, one time for each one of the types of functional analyses they distinguish. For example:

- when discussing Boxology, they write:

“the demand that explanations satisfy mechanistic constraints leads us to produce better [...] descriptions of the system at hand than we would produce if we allowed ourselves to be satisfied with any empirically adequate boxological models.” (Piccinini and Craver 2011, 307); and,

- when discussing Internal States Explanations, they write:

“If functional analysis by internal states is watered down to the point that it no longer makes any commitments to the behavior of components, then it is no longer possible to distinguish explanations from merely predictively adequate models [...] of the system’s behavior.” (Piccinini and Craver 2011, 300); and, finally,

- when discussing Task Analyses, they write:

“In short, to give up on the idea that there is a uniquely correct explanation, and to allow that any predictively adequate and/or intelligible model is explanatory, is essentially to give up on the idea that there is something distinctive about explanatory knowledge [...]” (Piccinini and Craver 2011, 297).

Piccinini and Craver present a dilemma for the defender of the distinctness thesis: either there are only mechanism sketches, or there aren't only mechanism sketches but if that's the case, then the

explanations psychologists give by means of functional terms fail to be explanatory. Or, to borrow Shapiro's both apt and witty title: "Mechanism or Bust". Barrett, Roth and Cummins, Shapiro and Stinson have all attacked (Indistinctness) by arguing that the explanations psychologists give by means of functional terms do not fail to be explanatory. I won't explore Piccinini and Craver's argument for (Sketch) at more length. Nor do I think I need to. I contend that it rests on a mistaken assumption. And, this assumption has gone unnoticed by their opponents: participants on both sides of the debate all assume that the question of the explanatoriness, or lack thereof, of functional analyses has a bearing on whether or not there are such things.

The question of whether functional analyses are mechanism sketches is the question of whether, when using black boxes and filler terms, the explainer of cognitive capacities intends their theories to refer to neural structures or not. But this question is a descriptive question, not a normative one. Piccinini and Craver, by arguing that functional analyses are not explanatory, may have provided support for a positive answer to the normative question, that is, that there shouldn't be anything but mechanism sketches. Or, on the contrary, their objectors, by arguing that functional analyses are explanatory, may have provided support for a negative answer to the normative question, that is, that it is false that there shouldn't be anything but mechanism sketches. However, they haven't provided any support for, respectively, a positive answer and a negative answer to the descriptive question. What should be the case has no bearing on the question of what actually is the case.

One might object to my interpretation of Piccinini and Craver's argument for (Sketch), holding that this argument is an inference to the best explanation of the following form:

(P) Functional analyses are explanatory.

(C) Functional analyses are mechanism sketches.

However, it is clear from the three passages I have just quoted that this cannot be what they have in mind. Here is one more quote: "In short, either task analysis is an elliptical form of mechanistic

explanation or it is no explanation at all.” (Piccinini and Craver 2011, 297). In any case, we would need to be shown first that the conclusion is an explanation of the premise and second that it is the best available. And even if we were to be shown as much, this argumentative strategy would still be facing a dilemma. Either the premise should be read as an existentially quantified statement, that is, as the thesis that *some* functional analyses are explanatory; but in this case the conclusion cannot but be also existentially quantified and Piccinini and Craver's thesis is universally quantified. Or, the premise should be read as a universally quantified statement, that is, as the thesis that *all* functional analyses are explanatory; but in this case, though Piccinini and Craver's thesis might follow; the premise itself is obviously false (surely, not *all* functional analyses are explanatory).

One might object to my interpretation of this argument on other grounds. In his own writings, Craver has been arguing that there is an ontic sense of the word “explanation” according to which explanations are entities in the world and that this sense is in some sense prior to the epistemic sense: “Ontic explanations are not texts: they are full-bodied things. They are not true or false. [...] They just are” (Craver 2014, 40)¹¹. It might be thought that Piccinini and Craver's argument is actually an argument for the conclusion that functional analyses, *as entities*, are mechanism sketches, *as entities*. Here is one way such an argument could go¹²:

- (1) Functional analyses_{EPIST} are explanatory.
- (2) If functional analyses_{EPIST} are explanatory, then functional analyses_{ONTIC} are mechanism sketches_{ONTIC}.
- (3) Functional analyses_{ONTIC} are mechanism sketches_{ONTIC}.

Note, however, that this argument is nothing but the previous argument in ontological clothing. They share their first premise. As for the second premise of the latter argument, it can be seen as a conditional inference to the best explanation: if functional analyses_{EPIST} are explanatory, the best explanation for this is that functional analyses_{ONTIC} are mechanism sketches_{ONTIC}. Therefore, by parity

of reasoning, this cannot be what Piccinini and Craver have in mind – and this is without considering the fact that, in this paper, they don't draw this distinction between the two senses of “explanation”. In any case, we would still need to be shown that the conditional inference to the best explanation holds. Moreover, though this argument doesn't face the dilemma the previous one faced (whether some or all functional analyses_{EPIST} are explanatory, the argument can go through), when we are interested in the question of whether psychology and neuroscience, *as explanatory practices*, are distinct, our interest is in an epistemological question, not an ontological one: the second premise of the argument for (Indistinctness) makes use of the epistemic concept of explanation.

In my discussion of these alternative interpretations of Piccinini and Craver's argument for (Sketch), I ended up pointing out that we were in fact presented with arguments for another thesis: first, for the thesis that *explanatory* functional analyses are mechanism sketches; and, second, for the thesis that *functional analyses*_{ONTIC} are *mechanism sketches*_{ONTIC}. One might suggest that this only shows that a good interpretation of Piccinini and Craver would rest on either one of these two theses instead of (Sketch). This would be a mistake. Again, the question Piccinini and Craver aim at answering, the distinctness question, is a descriptive question about psychology and neuroscience *as explanatory practices*. This is because their target, the autonomy question, is a *descriptive* question about psychology and neuroscience. But, if I were to follow this suggestion, this is not the question they would be answering. If the argument included the first alternative to (Sketch) its conclusion would be that *explanatory* psychology and neuroscience are not distinct; while, if it included the second alternative to (Sketch), its conclusion would be that *true* psychology and neuroscience are not distinct.

3.2 How to argue for (Sketch)?

Before suggesting a way in which (Sketch) can be argued for, a few clarifications are in order. First, I should stress that the question of whether explainers, when using black boxes and filler terms to

explain why we have the capacities we have, actually *intend to refer to* neural structures is to be distinguished from the question of what these functional analyses (semantically individuated) actually *refer to*, if they refer to anything. The standard view on functional terms¹³ is due to Lewis (Lewis 1970; 1972). According to the standard view a term for an internal state occurring in an Internal States Explanation semantically individuated refers to the neural state which occupy in the causal network which connect the input to the action to its input the same location the belief occupies in psychological theory – the presupposition being that there is but one such state¹⁴. A similar story can be told about the reference of a term for a component functionally individuated occurring in a Boxology. Such a term refers to the neural structure which performs the function specified by the theory¹⁵. According to this picture of reference, if there are no neural structures which normally perform the functions specified by the (Boxology), then the theoretical terms of the theory don't refer to anything. Thus, it could be that explainers, when using a specific functional analysis semantically individuated to explain a given capacity, intend to refer to a neural structure while the functional term appearing in the functional analysis they are using either doesn't refer to what they have in mind or doesn't refer to anything. Second, it may be thought that for (Sketch) to be true it has to be the case that the explainer intends to refer to a neural structure they are in a position to refer to by means of neural terms. However, it shouldn't be taken to be necessary. What should be taken to be necessary is that the explainers intend to refer to unique neural structures when using functional terms - *whether they are able to refer to them by means of neural terms or not*. Why choose this way of cashing out (Sketch)? Because this guarantees that the claim that psychology and neuroscience are (or are not) distinct can be truth-apt even in a situation in which the neuroscience of the day is silent on whether unique neural structures perform the functions specified by the boxology and also in cases in which, though the neuroscience of the day does speak to the question, psychologists have no knowledge of the relevant research. And we want this claim to be truth-apt in such cases because it makes sense to ask whether psychology and

neuroscience are distinct even in situations in which either the two sciences or only neuroscience barely got off the ground. Third, it is more interesting to relativize (Sketch) to a time (or a time-interval). Indeed, we can expect the answer to the question of whether (Sketch) is true to differ in function of the time period we concern ourselves with. Psychology and neuroscience could be distinct now, while having been indistinct in the past¹⁶. Given all this I can take a stab at determining how to evaluate (Sketch).

Arguably those who would want to defend the existence of psychology as an explanatory practice think that psychologists, as members of a discipline, are those who engage in this practice, not neuroscientists. That seems right. And, arguably, Piccinini and Craver also have this view as a target: they want to say that psychologists, appearances notwithstanding, do not engage in such a practice. Assuming that this is true, it makes good sense to look at the work of psychologists, as members of a discipline, in order to evaluate the thesis. It is worth noting at this point that psychologists as members of a discipline engage in other explanatory practices whose existence is not in doubt. They do not only aim at explaining cognitive capacities but also character traits and interpersonal or cultural differences, among other things. To take the example of character traits, psychologists who aim at explaining aggressivity in terms of upbringing are trying to establish a causal claim between a certain type of upbringing and the possession of the character trait: there is no doubt that their explanations as explanatory practices are not mechanistic explanations. I do not intend (Indistinctness) to range over these practices. Neither did Piccinini and Craver. By the discipline of psychology, they mean the discipline which aims at explaining cognitive capacities as characterized, or “cognitive psychology” (in one of the meanings of the phrase). I do too¹⁷.

Then the question becomes: what should we look for in the works of cognitive psychologists? We want to determine what are cognitive psychologists' intentions. But intentions are mental states and mental states ascription is a tricky business. Stinson's reply to Piccinini and Craver provides us with a

solution to this problem. Stinson interprets Piccinini and Craver as arguing for the thesis that psychologists should follow the norms of decomposition and localization neuroscientists follow¹⁸. In response, she assumes that we should trust psychologists to follow the norms they ought to follow to produce good explanations, she gives evidence that psychologists do not follow these norms and concludes from this that Piccinini and Craver are wrong¹⁹. In order to show that psychologists do not follow these norms Stinson assumes that if they did, they would have, with time, replaced, in their explanations of cognitive capacities, functional terms by terms referring to neural structures, and she gives several cases (taken from the research on attention and memory) in which this is not what has happened.

Though Stinson's evidence seems to show that psychologists do not follow the norms of decomposition and localization neuroscientists follow, it doesn't show that (Sketch) is false. Indeed, the intention that (Sketch) ascribe cognitive psychologists need not be manifested by a use of terms referring to neural structures. Nor does Stinson take it to do so. She isn't concerned with (Sketch). Stinson's paper, however, gives us a clue on how to evaluate (Sketch). One of the ways to argue *for* (Sketch) is to show that over time cognitive psychologists replaced, in their explanations of cognitive capacities, functional terms by terms referring to neural structures.

One might object to this that the neural terms could just be substituted with functional terms because they help explain how neural structures underly our having the cognitive capacities we have. But note that this answer presupposes an implausible psychological model. On the one hand, psychologists would be explaining cognitive capacities by means of explanations referring to functionally individuated components. On the other hand, and *simultaneously*, psychologists would be explaining cognitive capacities by means of some neural terms. Not only is this multitasking psychologically implausible but given that both the psychological and the neuroscientific explanations would be

missing terms, it is unclear that either one of the explanations would be satisfactory and, therefore, it is implausible to think that anyone could engage in such multitasking.

I would like to close this section by mentioning that those who share Piccinini and Craver's broad orientation have in their hands a way to bypass a discussion of the present distinctness of cognitive psychology from cognitive neuroscience. Let us be agnostic as to whether cognitive psychology and cognitive neuroscience are presently distinct. If we had some knowledge of the kind of factors which could lead cognitive psychologists to stop giving functional analyses but instead give mechanistic explanations, and, if these factors were to obtain, we could predict that functional analyses, though there might still be such things, would be on their way out or soon would be. I will list a few candidates for being such factors. First, cognitive neuroscience is still a young discipline, but it is developing at a very fast pace and it will soon extend its explanatory purview to cover all the capacities cognitive psychologists have investigated. Now, if cognitive neuroscience is to provide explanations of cognitive capacities by means of both functional and structural terms, then it could appear artificial to cognitive psychologists themselves not to think of their functional analyses as mechanism sketches, that is, not to think of the functional terms they use as referring to neural structures.

Second, funding agencies of modern states as well as private funding institutions who provide funding for research are sensitive to the opinion of the general public. They tend to preferentially allocate funds to research areas which are of interest to the general public. And neurodegenerative diseases will no doubt be one of the major challenges of public health in the near future – if they aren't already (World Health Organization 2006). Consequently, funding agencies and private funding institutions will most likely expand the funding of research in cognitive neuroscience in the future. In fact, this is already an ongoing effort, with, for example, the White House Brain Initiative, announced by President Barack Obama on April 2, 2013, and the Human Brain Project, launched in October 2013 by the European Commission, as one of its two Future and Emerging Technologies Flagship Projects. Whether or not

some of the funds allocated to research in cognitive psychology will be redirected to research in cognitive neuroscience, cognitive psychologists will have an incentive to integrate cognitive neuroscience to their research and research projects. If so, then it could also appear artificial to cognitive psychologists themselves not to think of their functional analyses as mechanism sketches²⁰. Now that I have given an idea on how one could argue for (Sketch) and thus for (Indistinctness), I will show why the distinctness question is of great significance²¹.

3.3 The significance of the distinctness question

The fact that the distinctness question has been mostly overlooked in the literature may be taken to suggest that it isn't significant. It shouldn't. By calling a question significant one can either mean that people working in the field have focused on it or that it has important consequences for debates in the field. That the distinctness question is not significant in the former sense doesn't show it isn't significant in the latter. It is the latter sense we should care about. And I claim that the distinctness question is significant in this sense. In this section, I will offer some support for this claim. I will discuss the integration question. The integration question is one of the main issues of present-day philosophy of cognitive science²². As a consequence, if it happens that there is a relation between the distinctness question and the integration question, the distinctness question will show itself to be of great significant. I will show that there is such a relation.

In the literature on integration in cognitive science, integration is usually characterized by being compared/contrasted with other relations of past and/or current interest like autonomy, reduction and elimination. Unfortunately, these words themselves are often used by authors without being defined and are sometimes used in different ways by different authors. As a result, the word “integration” takes subtly distinct meanings in different authors. To illustrate, while David M. Kaplan writes: “Philosophers weighing in on this topic have tended to focus on the prospects of [...] achieving

integration or unification of psychology and neuroscience via theory reduction” (Kaplan 2017a, 4), Piccinini and Craver, in the article I discussed in this paper, write: “[...] we reject autonomy as irreducibility of laws or theories in favor not of reduction but of explanatory integration.” (Piccinini and Craver 2011, 289).

I would like to home in on the two distinct ways in which, Kaplan on the one hand, and Piccinini and Craver on the other, use the word “integration” *in the context of cognitive science*. Let me start by a state of affairs that many philosophers and scientists alike would, it take it, find desirable: a state of affairs in which we would know, in order to change the behavior of individuals, the kind of intervention - whether behavioral, or pharmacological or surgical, or of some other kind - we should perform, and - if it isn't already presupposed by the fact that the intervention is of a specific type - where to perform it. My first characterization of integration is an operational one. It is a relation between disciplines.

Integration as unification: the level of integration of cognitive psychology and the neurosciences (i.e. working on entities at different scales) corresponds to the extent to which the state of affairs just described obtains.

I take it that this characterization is one way to read Kaplan when he equates integration and unification and Piccinini and Craver when they talk of “building a unified science of cognition” (Piccinini and Craver 2011, 284). Hence the name I chose for this concept. This characterization is operational in that I have characterized integration by (one of) the means we have of measuring it: while in a situation in which we are far from the desired state of affairs we can infer that the integration of cognitive psychology and the neurosciences is weak, in a situation in which the desired state of affairs obtains, we can infer that the integration of cognitive psychology and the neurosciences is strong. Under this characterization of integration, we can expect there to be some integration between cognitive

psychology and the neurosciences as disciplines. With this characterization of one type of integration in hand, I can now offer a characterization of a second type of integration, which is, as we will see, related to (Indistinctness). It also applies to disciplines. This time I will only be concerned with the integration of cognitive psychology and cognitive neuroscience.

Integration as means: Cognitive psychology and cognitive neuroscience are integrated if

- (i) both cognitive psychologists and neuroscientists take their functional terms to refer to the same mechanisms; and,
- (ii) their functional analyses semantically individuated are, if not identical, highly similar.

I take it that this characterization is one way to read Piccinini and Craver when they write “explanatory unification will be achieved through the integration of findings from different areas of neuroscience and psychology into a description of multilevel mechanisms.” (Piccinini and Craver 2011, 285). I chose this name for this concept because this type of integration is a mean to reach the goal of integration as unification. Indeed, if conditions (i) and (ii) are true then one can, under the assumption that the explanations given are well-confirmed, expect to know the kind of intervention we should perform and where it should be performed in order to change the behavior of individuals.

Now, in which way is (Indistinctness) related to either integration as unification and/or integration as means? It should be clear that the truth of (i) presupposes the truth of (Indistinctness). Though (Indistinctness) is not a sufficient condition for (i), it is a necessary condition for it. For cognitive psychologists to take their functional terms to refer to the same (neural) mechanisms neuroscientists take their functional terms to refer to, they should at least think that their functional terms do refer to (neural) mechanisms. Consequently, if one desires to integrate cognitive psychology and cognitive neuroscience, one should start by laying down the conditions for (Indistinctness) to be true (for

suggestions see sec. 3.2.). Moreover, as integration as means is a mean toward integration as unification, bringing about the truth of (Indistinctness) would be instrumental in reaching integration as unification. I conclude from this that the distinctness question is of great significance. This discussion of integration is, of course, provisional. But this will suffice for the purpose of this paper, which is now drawing to a close.

4. Conclusion

In this paper I have been concerned with (Indistinctness), the thesis that psychology and neuroscience are not distinct, or, as I analyzed it, that psychology as an explanatory practice is not distinct from neuroscience as an explanatory practice. I have first showed that the key premise of Piccinini and Craver's argument for (Indistinctness) - (Sketch) or the thesis that functional analyses are mechanism sketches - needed to be reformulated as the thesis that there are no such things as functional analyses, only mechanism sketches, and that its scope needed to be restricted to boxologies, that is, explanations of cognitive capacities which are analyses of the system into components functionally individuated. I have argued that Piccinini and Craver's argument for (Indistinctness) fails, because the reasons they give to think that (Sketch) is true, are of the wrong kind: the premise is descriptive; their argument is normative. In order to support (Sketch) one should instead show that psychologists intend to refer to neural structures when using functional terms. I have presented one of the ways the truth of (Sketch) could be assessed, namely, by determining whether (cognitive) psychologists, with time, replaced, in their explanations of cognitive capacities, functional terms by terms referring to neural structures. Moreover, I have pointed out, that, whether or not (Sketch) is true as of now, one can still determine whether the factors which could lead (cognitive) psychologists to think of the functional terms they use as referring to neural structures are likely to obtain and thus whether functional analyses, if there still are such things, are on their way out or soon will be. Finally, I have shown that the distinctness

question is of great significance because (Indistinctness) is a necessary condition for the integration of (cognitive) psychology and (cognitive) neuroscience.

Is (Indistinctness) true or false? An answer to this question is better left for another occasion. In any case, if the editor of *The Oxford Handbook of Cognitive Psychology*, Daniel Reisberg, can go, in the space of a few lines, from the claim that “cognitive neuroscience must be understood as an independent field and will surely become more independent” to the claim that “there is a mutual dependence between these two fields [i.e. cognitive psychology and cognitive neuroscience] and this dependence is likely to grow in the coming years” (Reisberg 2013, 4) we can safely say that these are interesting times to ask the question.

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¹Most prominently, Jerry Fodor (Fodor 1974; 1996). The debate has been revived in recent years, with Ken Aizawa and Carl Gillett as the most prominent anti-reductionists (Aizawa and Gillett 2011).

²For a review, see (Stinson and Sullivan 2018).

³I will depart from them in one respect. On which more in the next subsection.

⁴For a recent review of research on face recognition in neuroscience, see (McGugin and Gauthier 2013).

⁵Unless stipulated otherwise, italics in quotes are the authors'.

⁶Their individuation is semantic in the sense that they are individuated by their inferential role.

⁷Unless stated otherwise, “mechanism sketch” should be read accordingly in what follows.

⁸If one wants to call this thesis an eliminativist thesis, one has to bear in mind that this is not the kind of elimination the Churchlands, for example (in, among other texts, (P. M. Churchland 1981) and (P. S. Churchland 1986)) had in mind. (Sketch) is about functional analyses intentionally individuated. The Churchlands, however, talked about theories (folk psychology as a theory was at the center of their attention) and therefore individuated psychological explanations semantically.

⁹One would expect someone who thinks that (Sketch) doesn't apply to the psychology literature to agree that it applies to the neuroscience literature (assuming that one finds in this literature explanatory contexts in which no neural terms are used). Someone could deny that and could even go further and argue that in explanatory contexts in which no neural terms are used, neuroscientists *always* offer functional analyses as characterized. (In doing so, they would deny the existence of mechanism sketches in contexts in which no neural terms are used, not of mechanism sketches *per se*.) Now, and this is the reason why I mention this possibility in the first place, this might suggest that the argument I have put forward at the beginning of this subsection for the conclusion that “mechanism sketch” should be given an intentional reading in the context of this paper is flawed. The objection would be that this argument begs the question in that it presupposes the existence of mechanism sketches in contexts in which no neural terms are used. Note, however, that this argument presupposes, not that they exist, but that Piccinini and Craver believe that they exist (or, at the very least, should believe that they do). I thank an anonymous reviewer for pressing me on this issue.

¹⁰Here I take a practice to be a regularity in patterns in behaviors that result from shared mental states among the agents

who exhibits these behaviors. This could be further precisified, but this will suffice for our present purposes.

¹¹ Craver's views have developed over time. See (Sheredos 2016, sec. 1) for details, which are irrelevant here.

¹² I use subscripts to distinguish the epistemic sense of “explanation” from the ontic one.

¹³ In what follows I will use the noun phrase “functional terms” to designate the terms for components functionally individuated.

¹⁴ What if the presupposition is false? Then, the functional terms have traditionally been taken to refer to the states of being in one of these states. A debate opposes role-functionalists who are of this opinion (Block and Fodor 1972) and realizer-functionalists, like Lewis, who contend that this presupposition is warranted. On this distinction between role-functionalists and realizer-functionalists, see for example (Mclaughlin 2006).

¹⁵ This function or role depends on the kind of theory which is being put forward (e.g. if the theory is computational, the role is a computational role). It should be noted that though functional terms for components (“adder”, “transducer”...) tautologically indicate the function a neural structure has to perform in order to be its referent (an adder adds, a transducer transduces...), a theory usually goes further than this, specifying also how this function is to be performed, that is, in the case of an adder, the algorithm which it follows.

¹⁶ Piccinini and Craver are silent on this point, for obvious reasons: their argument rests on normative considerations and is therefore not meant to be time-sensitive.

¹⁷ By parity of reasoning, when I talk of neuroscience as a discipline, I also mean *cognitive* neuroscience.

¹⁸ On these norms, see (Bechtel and Richardson 1993).

¹⁹ Here is some textual evidence for my interpretation of Stinson's paper: “It would be uncharitable not to at least consider the possibility that cognitive psychologists might have a good reason for holding on to their models under these circumstances.” (Stinson 2016, 1604), “Furthermore, although the cognitive model turns out to fit quite badly with current neural models, psychologists have not given up their models, contrary to the normative claims made by Piccinini and Craver [...]” (Stinson 2016, 1605), “Instead of throwing out cognitive models whose components do not map neatly onto neural working parts, we could heed cognitive psychologists’ repeated pleas that their field does have a legitimate subject matter, and that their models do track robust regularities in the world.” (Stinson 2016, 1608).

²⁰ There are also, of course, countervailing factors. For example, cognitive psychologists, if they do not want their departments to disappear, will demarcate themselves from cognitive neuroscientists, stressing the distinctness of their explanations (Pylyshyn 1984). A convincing argument for the thesis that functional analyses, if there are such things, are

on their way out or soon will be, will have to measure the relative weights of the factors pushing in the direction of indistinctness and the factors pushing away from it.

²¹ Piccinini and Craver's discussion of the camshaft (Piccinini and Craver 2011, 301–2) and of the general purpose computer (Piccinini and Craver 2011, 294–96) might suggest that Piccinini and Craver run an alternative argumentative line, namely, that functional analyses are mechanism sketches because their proponents have their implementation in mind when offering them (it is therefore likely that they use black boxes and filler terms to refer to neural structures). I thank an anonymous reviewer for suggesting this interpretation. Whether this is a good way to interpret Piccinini and Craver is unclear to me. Indeed, in their conclusion to the section in which they discuss the general purpose computer, Piccinini and Craver rehearse the underdetermination argument I have presented. In any case, this argument is flawed. That engineer who build combustion engines and computers intend to refer to mechanisms when using black boxes and filler terms is one thing, that psychologists do so is another. Psychologists are not engineers. They aren't in the business of conceiving things and, as a consequence, they need not be thinking about neural structures. This alternative argument rests on a mistaken analogy.

²² As the recent publication of an edited book on the topic attests (Kaplan 2017b).

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