RICH CONTEXT- MULTIPLE REPRESENTATIONS OR RICH CONTEXT – GOOD PROTOTYPE?

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Introduction

We are cognitively functioning in a rich context of stimuli. Whether supplied by our mere life experiences or by education, the variety and complexity of the surrounding world is a fact. The capacity of manipulating, producing and communicating upon these stimuli is based on the general assumption of the existence of mental representations. Are these mental representations specific to humans through their organization, form and content? And, since we can communicate with each other about these stimuli, does this mean we all necessarily share the same representations?

The existence of the verbal communication system (c.q. language) in humans may make us believe the answer to the first question is rather positive (since language is only present in humans). I will not doubt on it here. However, sharing the same communication system and, within the system, the same language, may not necessarily implicate we all have the same representations. This is the main issue I will try to discuss in the following.

To address this question, we shall take a look at theories of representation. Theories of representation would refer in general to language development, concept formation, categorization and symbol use, all putting concept acquisition at its heart (Fodor, 1998). The way they address these correlative issues depends on a basic distinction of the way information is processed: although all theories assume context as departure point, concepts would be acquired through an abstract/amodal way OR a modality-specific way.

Abstract theories of representation assume that in the process of the acquisition of concepts, non-essential elements are eliminated, and the essential elements are selected, memorised and consolidated. Concepts, as a form of generalized knowledge, will be found at the basis of higher cognition, being also linguistically labelled. The operations through which concepts are achieved would be abstractisation /generalisation, analysis/synthesis and the similarity/analogy mechanism (Goswami, 1998), so far. Categories will differ from concepts in the sense that concepts have content, i.e. "lexical definition", while categories do not have a content in themselves, but represent rather a theoretical space as "an extension of a concept". Categories would have an index function, by relating similar exemplars experienced, and would use the linguistic label of the concept subscribed (my assumption). Further, traditional logic shows that the more restricted the content of a concept is, the larger the extension of the category, and vice-versa, the more essential features put together, the lower the applicability of the concept as a category.

For example, "dog" vs. "all dogs one can find" and "dogs with red eyes" vs. "all dogs with red eyes one can find".
In contrast, modality-specific theories of representation assume representations are eidetic, concrete, reenacting experiences, and not necessarily, individually linguistically labeled**.** The brain would capture states across the modalities and integrate them into multi-modal representations. Categories and concepts alike would result through a memory inference process, with simulation as its central mechanism (Barsalou, 1999).

As we can see, the two types of theories make a radical distinction not only between the way information is processed, but also between the nature of representations, letting open the question whether the former being a consequence of the latter. In other words, are representations processed in a necessary way or are they a result of a processing mechanism that could be incidentally and/or accidentally at work? Generally speaking, radical distinctions make it easier to choose for an answer on basis of the empirical evidence found. However, the huge experimental work done until now did not provide yet the conclusive evidence. Recently, neuroscience has been added to this mass of research, with a big promise to fulfill these expectations. The potential role of neuroscience in the service of cognition (i.e., cognitive neuroscience) is exactly in providing evidence on the nature of representations, as well as helping answering other subsequent questions, like organization, storage and processing.

How could neuroscience make a distinction visible between amodal and modality-specific ways of representation? What would justify the search for one or the other type of evidence at the neural level? And in which form may we find it? An initial answer would be that amodal representations are justified to expect due to the fact that mental representations are used to infer knowledge and/or to generalize to a certain category of objects. So, representations would need to be "formatted" in a more general way, suitable to different situations. The problem with amodal representations comes from the observation that we update, modify and transform continuously our knowledge, and implicitly our representation of that knowledge. This means partial representations, as a temporary representation (vs. lexical, frozen definitions) would seem correct to assume as well (Mareschal et al., 2007). Also, it would be correct to assume (on basis of evidence from child studies cf. Goswami, 1998) that the processes of abstractisation and generalisation do not occur simultaneously, especially during development. Therefore, categorisation would be dynamic and graded, presumably beginning with a prototype as a central concept.
In its turn, the justification of modality-specific representations comes from the fact that we indeed reenact experiences, although with a certain loss of details. Also by the fact that we do keep in memory non-essential information, like, for example, our personal experiences with the implicated referents (see "dog" vs. "my own dog"). I personally term this type of information ***idiosyncratic connotations*** (not to be confused with connotations as in lexical definitions one could find in dictionaries). However, according to Estes (1994), memory cannot be just reenactment (storage of experiences as such) because this would not be sufficient for problem-solving activities (we cannot apply individual experiences as such to future experiences). Memory is essential to adaptive behavior, thus needs to be functional, i.e**.** to organize information c.q. to classify (idem). Memory is storage, Estes says, but in an organized way.

From these two perspectives, amodal vs. modality-based representations, it seems we may need to expect different types of representations. Therefore, the problem to be solved by neurocognitive studies, to my opinion, is to explain **how concept formation can rely on non-generalized knowledge (but) stored (i.e., fixed in memory) information? And how language, in its idiosyncratic expressive form (as meant above), could be used as an index of the organization of knowledge in both its categorical and continuous form?** For example, taking the following concepts, "dog" (concrete concept), "freedom" (abstract concept) and "animal" (summum genus category, i.e. highest level of categorisation with no genus proximus, concept that cannot be defined), how would each be represented in the brain during ontogenesis, by different individuals? And would the representation of "animal" resemble more over that of "freedom" (abstract) or "dog" (concrete)?

In my attempt to demonstrate all this, I would like first to recall here by Fodor. According to Fodor (1998), the traditional cognitive science program[1] **has been carried out in a wrong way. This is because the theory of concepts assumed by cognitive science was seriously mistaken**. Thus, paying attention to Fodor's theory when the rest of the cognitive science (according to his own statement) opposes him, offers us the possibility of clearly **observing the difference between mind and brain issues,** between the cognitive level of description and the neural level, to observe whether the latter may or may not support the former. This is because Fodor is referring to a *computational* theory of mind, and computational theories take artificial intelligence (A.I.) as its paradigm, i.e. "mind as [*informational*, my remark] machine" (Boden, 2006). However, A.I. is not equal to the human brain, and principles of A.I. subsequently may or may not be valid for the human brain and vice-versa. **Therefore, any cognitive model trying to incorporate neural evidence would have to answer a fundamental question: is atomistic semantics (as postulated by Fodor) a necessary condition (Fodor, p. 37) or is inferential (contextual) semantics a real possibility in explaining the functioning of the brain? And could the two conditions eventually co-exist and be complementary? Thís is the question.**
Otherwise formulated, could we, humans, start any act of cognition from a complete *tabula rasa* or something of some kind must be prespecified? It is thus in the light of this contrast (**mind as machine vs. mind as brain**) that I would like to present some of my findings as a result of a secondary analysis and reinterpretation of neurocognitive data from recent studies on categorization (Vellinga-Firimiță, 2008).

But, before proceeding to the empirical evidence, I propose to carefully observe Fodor's critique: For a (computational) theory of representation [2] to be valid, according to Fodor (1998), thus having a certain degree of generality in the explanations it proposes, it is dependent on the extent to which mental contents are supposed to be shared by everybody[3]. Mental contents are made out of concepts, concepts

[1] which has as scientific goal to understand what mental representations are and to make explicit the causal laws and processes that subsume them;
[2] RTM as it is called by Fodor;
[3] Conceptual relativism, like distinguishing between my concept of DOG and yours, for example, would be misleading, according to the same author, because it would deny the very concept of 'concept', i.e. mental content shared by everybody. However, diachronic (i.e., ontogenetic studies cf. Goswami, 1998; Mareschal, Quinn & Lea, 2010) as well as synchronic (across individuals from a similar group; cf. conceptual development in children with the same mental age, normal children vs. children suffering of the Williams syndrome, Thomas et al., 2010) show evidence of a difference between the content of the same concepts involved.

viewed by Fodor (idem) as equivalent to categories[4] and as equivalent to symbols[5]. Words, in their turn, would merely express concepts (idem, pp. 23-28). Consequently, a valid theory of concepts should answer all of the following principles:

1. Concepts are mental particulars; specifically they satisfy whatever ontological conditions have to be met by things that function as mental causes and effect;
2. Concepts are categories and are routinely employed as such;
3. Concepts are constituents of thoughts and, in indefinitely many cases, of one another. Mental representations inherit their contents from the contents of their constituents (compositionality issue).
4. Quite a lot of concepts must turn out to be learned.
5. Concepts are public; they're sorts of things that lots of people can, and do, share.

In addition, Fodor focuses his main critique on concept possession, and denies the value of the following two statements observed in the history of cognitive science:

1. Concept possession is parasitic on the explanation of concept individuation (p. 2). If concept X = a + b + c (identity conditions), having X is having whatever X turns to be (this is the older explanation cognitive science gave us);
2. Having X = having a + b + c, and being X is derivative (current explanation).

Fodor himself moves freely between the above mentioned statements, between concepts and word meanings and proposes that having a concept is having a mental particular, but not necessarily having mental traits nor capacities (idem, p. 3). In turn, he proposes an informational semantics to be true. Informational semantics is atomistic and denies that the grasp of any interconceptual relations is constitutive of concept possession (he denies thus the role of insight as *causative* [my emphasis] in concept formation). Thus, inferences may exist, but they are not necessarily constitutive of concept possession. Neither, if a conceptual connection is necessary, it does not mean it constraints concept possession (idem, p. 72-74). Concepts may be individuated by the properties they denote, and properties may be individuated by their necessary relations to one another, but knowing about the necessary relations among the properties is NOT a condition of having a concept.
Informational semantics denies thus that DOG means 'dog" because of the way it is related to other expressions like 'animal', 'barks' etc. However, informational semantics as a theory of content does not deny the existence nor necessity of connections between concepts. These are viewed as metaphysical rather than semantic (idem, p. 74), thus not constitutive. Definitions may exist but they are not

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Related to this issue, Fodor questions whether content similarity and not content identity could ever be the clue in solving these observed differences. If this is the case, he says, the differences implicated cannot be essential, but rather *relative* and *idiosyncratic* (my remark and emphasis). I need to mention here that to me, it is exactly this difference (the relative and idiosyncratic elements in the content of a concept) that might offer a substantial clue in understanding how we represent the world internally. But, this difference can be made meaningful only if it is analysed in conjunction with the here by reformulated 5th Fodorian principle, that publicity (i.e. shared content) imposes a substantial constraint on concept formation (i.e. concept similarity is necessarily dependent on concept identity). To understand this idea, you may think of translation activities and the subsequent aspect of 'compensation' when loss of meaning occurs, for example.

[4] See 2nd principle of RTM on this page;

[5] See 5th principle of RTM on this page;

constitutive, in the same way correlations are not the cause of an event. Informational semantics denies also meaning through conceptual hierarchy or network. DOG means “dog” because of a nomic connection between two properties of dogs: being a dog AND being causes of actual and possible DOG tokenings in us.

The key Fodor offers is that of conceptual necessity (vs. mere necessity) to constraint concept possession. From this point of view, he says, it is important to understand that neural structures would resonate to ‘doghood’ for example, and not how. In addition, he mentions, perceptual mechanisms would be important mediators to the semantic access (of doghood, for example), but are not the only mediators. Other mechanisms may rely on having and exercising perceptual capacities, but not in having any particular perceptual capacity. This means the relationship perception – concept possession is empirical indeed, but not constitutive either .

In other words, Fodor says that the experiences we may have are a fact, but are not constraining on their own our concept formation. **To have concepts we must have the capacity of having those concepts, whatever the modality to achieve it may be.** Think for example of the concept of ‘infinite” (my remark). We cannot really know about it (can we?), although we may be confronted with it and think somehow of it. Experience is necessary but not sufficient. In order to have epistemic access (know about) we must have first semantic access (think about). This means, concept acquisition requires “something to have gone on in your head in consequence of the interactions”, c.q. experiences one had (idem, pp. 75-80, 133).

So, what could have gone on in our head as a result of experience and how could we see all this in our brain? In Fodor’s opinion, concept acquisition is learning HOW rather than learning THAT (idem, p. 125). This means the following:

1. To be nomologically locked to the property a concept expresses (idem, p. 125);
2. Acquiring a concept does not require acquiring beliefs, and consequently, hypothesis testing does not play a role in concept acquisition (idem, p. 126);
3. Experience (the right kind of) is essential to all theories (idem, p. 127). The difference is whether concepts are abstracted from experience or occasioned by. Concepts may not be learned from experience (this is the case when hypothesis testing is evidential) but caught (in this case good, bad and irrelevant experiences all would count).

Reformulating his ideas, this would mean that **to have a concept is to be locked to the corresponding property (X to X-hood, i.e. to the category of Xs). The property X expresses is constituted by the way X-hood would strike us if we had appropriate experiences with stereotypic X-hood concepts (idem, pp. 141-2). This means further that a concept X can be predicted through its stereotype and vice-versa, because concept X and concept ‘stereotype of X’ are independent of each other.** And this would greatly avoid the circularity problem implicated and somehow too easily accepted by previous studies (idem, pp. 3, 124, 129, 132, 138, 140).

But, if experience is not the cause of having a concept and just a necessary correlation, what could then be its cause? Fodor suggests an a priori mechanism, a built-in capacity, an intentional property for resonating, and proposes the essence of concepts to be revealed by a special science, as molecular chemistry revealed for example the essence of water: He illustrates this idea with Homer's knowledge on water compared to ours (idem, p. 134): Homer had a concept that was locked to water via its familiar phenomenological properties, i.e. in a pre-theoretic way, whether our chemical concept of water is post-theoretical. We are nowadays locked to 'water' via a chemical-cum-metaphysical theory, that specifies its essence, and this is a different mechanism of semantic access from the one that Homer relied on (idem, p. 157).

This view implicates the possibility of having different concepts, with different corresponding words and different mental representations (MRs), although their content may be identical (cf. Evening Star vs. Morning Star). What remains unmodified is the content, which must be understood as an absolute truth value (my remark). What gets modified is the appearance of this value (i.e., content in our heads, c.q. brains). We may get to know this truth value through scientific advance, but in the history of humankind it was not always like this (see for this purpose definitions across dictionaries and across ages). Thus, the mechanism that effects the locking does not depend on the superficial signs of the kind, but onto its essence, and hence to hold even in possible worlds where members of the kind may lack those signs (idem, p.157).

If neuroscience would be the promised science to reveal all this, then it should specify in neurological vocabulary the initial state from which concept X acquisition proceeds (idem). Fodor suggests further that we should ask if this neurological state is intentional (i.e. does it have conditions of semantic evaluation and which ones? (idem, p. 143), stating that not innateness is the key but the intentionality of the innate stuff. So far, Fodor.

Let's move now briefly at the other end of the spectrum. Here we would find developmental empiricism, which rejects thus representational nativism. However, the reasoning offered is incomplete, because they admit the existence of an innate bias of some kind[6] (Elman et al., 1996, p. 365), but, compared to Fodor, would lack a truly" incisive explanatory bite" (Plotkin, 2007, p. 155) in showing what is present at conception, where it comes from and what accrues through development (idem).

The apparent contradiction mentioned above may even increase when developmental empiricism embraces connectionism modeling as a way of demonstration. Although connectionism modeling would resemble biological systems in essential ways, however, the resemblance is not yet justified. Their modeling is constrained by how the modeler himself decides to represent the input (Goswami, 2010) and this is a subjective and man-made decision (since we do not know yet how learning actually occurs at synaptic, cellular or network level (Plotkin, p.157).

This lack of a secure foundation of knowledge on which connectionism in this case builds its models (idem) brackets finally all findings and claims based upon these models. However, this state of affairs

[6] to be modeled, for example, as attentional or differential weightings in the network;

does not categorically deny the possible truth value of computational models in several cases. For example, computational models were able to reproduce the so-called priming effect or even to anticipate the number-of-features effect (for examples, see: Vellinga-Firimiță, 2008, p. 64). In conclusion, computational modeling and, in particular, connectionism become themselves one hypothesis more rather than an explanation of any theory, and become dependent on factual observations independent of the model itself. And here is where connectionism may also need neuroscience.

Making the balance, if Fodor is right and the rest is wrong, or the other way around, what is the kind of evidence we may search for through brain studies? Here below, a subsuming of some important implications and predictions resulting from both theoretical directions presented above (for simplicity, marked with *F* (Fodor-like) and *anti-F* (contradicting Fodorian theory):

Implications at the cognitive level:

- a concept is a kind of mental particular, and not constructs out of possible experiences (F);
- thought without language is possible (anti-F);
- mental representations are possible without thought (anti-F);
- there is conceptual content without constituent structure (F);
- MRs are not structurally complex, in fact MRs have no structure, they are atoms; some innate kernel is thus needed (F);
- concepts are atoms as well: satisfying the metaphysically necessary conditions for having one concept never requires satisfying the metaphysically necessary conditions for having another concept (F);
- there should be a mechanism whereby satisfying the antecedent of an intentional law necessitates the satisfaction of its consequent (F);
- primitive concepts do not exist uncompiled in ontogeny (F);

Predictions at the cognitive level:

- *to think "brown cow' does not mean to think "brown*" nor "cow" ➔ "brown cow' has a different MR than 'brown' and 'cow'; also 'bachelour" and "unmarried man" would have different MRs;
- different MRs might correspond to the same content, since the analogy MR – modes of presentations (F);
- if concepts are definitions, then learning the definition (parts) should lead to learning the concept (anti F);
- if concepts are learnt, concepts are not innate (anti F);
- if not all concepts can be defined, we should find the primitives (F?);
- if definitions play a role in acquiring and operating on concepts, then understanding sentences made of words with complex definitions should be harder than sentences made of words with simple definitions (anti-F);
-if definitions by genus proximus/species, then genus concepts are easier to think than species concepts (anti-F);

From the perspective of brain studies, at the **neural level** we would have a domain-specific hypothesis and a domain-general hypothesis.

The **domain-specific hypothesis assumes:**

- the brain is a sum of modules (F);
- one type of information only is processed per module (F);
- *category specificity:* the brain represents different categories in different ways and/or in different regions (F);
- knowledge is stored in the regions were/through which it was acquired (anti-F?);

The **domain-specific hypothesis** predicts:

- a damage to a certain brain region will result in an impairment of the category subserved by the respective region (F).

The domain-general hypothesis assumes:

- the brain processes different stimuli in a similar way (anti-F);
- the difference in processing is of degree rather than of nature (anti-F);
- concepts are organised in an associative network. The semantic system is undifferentiated by category, domain, nor feature at the neural level (anti-F);

The domain-general hypothesis predicts:

- a damage to a certain brain region will not result in absolute impairments per semantic category, nor per feature (anti-F);

(NB: The list here above is not comprehensive, but should be sufficient for the intention of this paper.)

In sum, what would be the consequences of Fodor's theory (in contrast to the rest) for the evidence (to be) collected from neurocognitive studies? If we admit together with Fodor that content identity is not equivalent to concept identity (e.g. "bachelor" and "unmarried man", or "water and "H2O" have a plus/min identical content), we will have not only two different words, according to the analogy of systems, conceptual and linguistic (Fodor, 1998, p. 41), but also two separate mental representations. Therefore two different concepts. Thus, we should be able to find different mental representations for related, and even identical content. The same for "brown cow" (I just borrow Fodor's own examples in order to avoid confusions): it does not suffice to have a representation of "cow" and one of "brown" in order to infer "brown cow" by conjunction, but we may need to have a representation of "brown cow" by itself (note however that "brown" and "cow" are two words and not just one)[7].

Switching to the empirical evidence found, **the point is to see if this could account for or against atomistic semantics**. In the personal search for evidence on the nature of representation (Vellinga-Firimiță, 2008), our initial view on concepts was based on the previously mentioned difference between an agreed content (shared, public, target content) at a certain moment in the history of humanity (to be found in lexical definitions in different dictionaries across time, for e.g.), and the personal experiences that load and shape the meaning (content) of a concept for a given period of time in an individual. The

[7] This is because (Fodor says) concepts are not definitions, i.e. they are not decomposable in lower meaning units. The consequences for linguistics (which claim that definition is a central notion in lexical semantics), is that polysemy does not exist, but it is artificially created when admitting the possibility of definitions (Fodor ,1998, p. 53); so we may have synonymous words, but not polysemic ones.

question we had was how could we study concept formation with this similarity/dissimilarity relationship at stance? Another related question was what could language tell us about the internal organization of this type of content? According to Fodor (idem, p. 25), words just express concepts, and subsequently, the structure of the linguistic system is parallel to that of the conceptual system. From this point of view, thought is language and vice-versa. However, we suspected even at a common sense level that thought is more than language alone, and that even thought without language may occur. The second question could be reformulated here by as follows: ***taking into account the dissimilarity of concept content that we may expect as a result of a variability in life experiences, what would words be? If inferences, c.q. experiences/context only determine concept acquisition, what do words exactly refer to?***[8]

A graphical representation of our general hypothesis at the cognitive and neural level of description could be synthesised in the following (Vellinga-Firimiță, 2008):

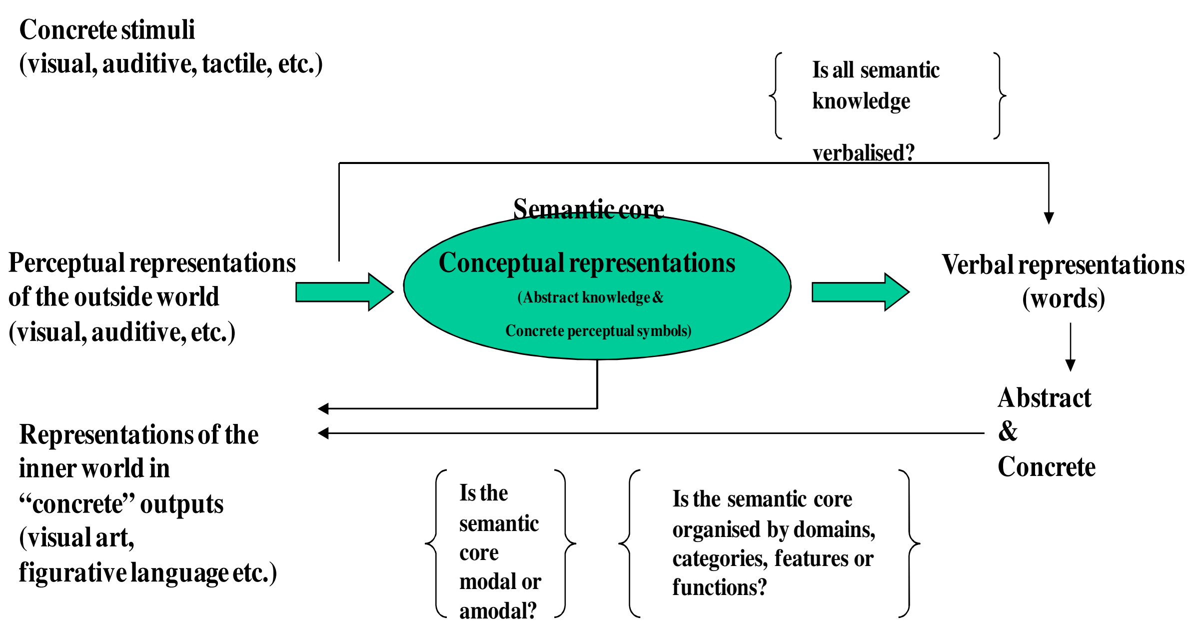

Figure 1. The conceptual system. An hypothesis about the representation of the world in abstract and concrete concepts.

[8] "If categories were innate, then verbal language should be just an expression of the same interpretation of the surrounding reality, and words should have equivalent meanings across cultures. If, on the other hand, categories were learned, then language may play the central role in shaping the meaning of our world. Therefore, a large linguistic variability should be observed, as expression of a large variability of semantic cognition (i.e., interpretation of the reality), up to individual differences." (Vellinga- Firimiță, 2008: p. 8). Linguistic evidence shows a combination of both. What does this mean from a developmental point of view? Is there a neural specialisation according to category, domain or feature-type, or is conceptual knowledge represented within a distributed and non-differentiated neural system?

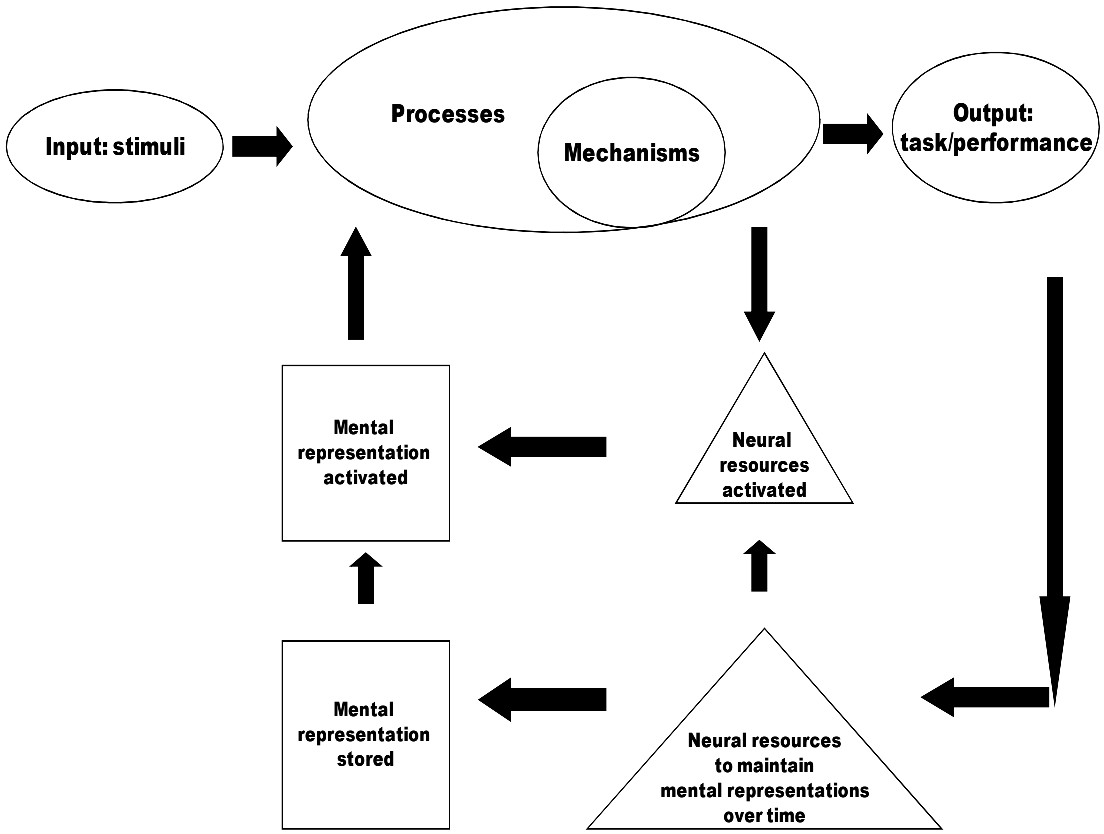

Figure 2. The emergence and/or activation of (partial) representations in on-line tasks.An hypothesis.

At the conjunction of the previous levels of analysis, a more specific hypothesis could be: whether (the mental representation of) a category should be predicted through its prototype (to be defined as belonging to a core semantic idea with a denotative (generally applicable) value, a concept may be more difficult to predict, because it contains in addition to general information, also exceptions and personal experiences attached to it (idiosyncratic connotations), which may be represented in different ways at the individual level:

Category ←——→ concept
↓ ↓
Prototype core idea + additional information
[semantic: generally applicable
core idea/denotative] exceptions
personal experiences
(associated?)

Figure 3. Category vs. concept

The Fodorian view would apply rather to the relationship category-prototype, while our personal view on concepts would include also idiosyncratic elements.

The evidence reviewed came from neurocognitive studies on categorisation, especially functional Magnetic Resonance Imaging (fMRI) - based, but included also other techniques, like event – related potential (ERP), Magnetoencephalography (MEG) and Positron Emission Tomography (PET). Subsequently, we observed the neural response in two types of tasks: elicited categorisation and memory (recognition) tasks (Vellinga-Firimiță, 2008, p. 30) for visually presented stimuli. Also, the difference in the processing of concrete vs. abstract words in lexical decision – and semantic categorisation tasks has been scrutinised.

As one may expect, the answer to the above mentioned questions could not possibly have emerged from just one review. Therefore, our answers are partial and open-ended. The general view obtained was that of a large variability of data, making it difficult to generalise it. However, the evidence offered in change very interesting qualitative details. For example, every study showed a different localization in the brain for the processing of the same category. Also, that different categories could activate the same neural regions. If any specific-effects measured, they were very weak and task-dependent. And both type of evidence made it difficult to conclude on an anatomical specialisation. However, data converged in showing that knowledge that comes via the senses is organized at the basis per features.

In conclusion, the brain does not seem to be organized per category, but certain categories may depend critically on certain types of information, and this information may be localized (in the form of associated features). How the organization goes on further, we do not know yet. In addition, the difference between the information processing among normal and impaired subjects, especially when differentiating among the strategies used, showed very often a quantitative difference of the time course of the process. This suggests impairments may be at the level of the process, eventually of the mechanism affected, rather than a damage to a storage of a specific type of information.

At a more specific level, the evidence on the processing of concrete vs. abstract words showed that ***semantic richness qualifies as a constraint in conceptual processing***[9]**.** However, it was not possible to derive if this constraint would be causal in the acquisition of the concepts implicated. The semantic richness of concrete words demonstrated just a facilitatory effect i.e., a faster lexical decision, faster categorisation and word naming (Pexman, Hargreaves, Edwards, Henry & Goodyea,2007; Pexman, Holyk & Monfils, 2003). The semantic richness of concrete stimuli appeared also indirectly proportionally correlated to less cortical activation and this is similar to priming effects (i.e., repetition suppression): there is a neural efficiency for targets that follow related primes. In contrast, concrete words with low semantic richness required more language-based processing. Also, impairments in access to perceptual features relative to other aspects of word meaning resulted in a poor performance on concrete words vs. abstract, and within the concrete domain, in a poorer performance on living vs. nonliving things.

Further, the evidence showed that concrete words are processed more intensely in lower left parietal and left prefrontal regions, outside of the primary language processing areas, which might reflect an extended

[9] This is because the evidence from neuroimaging studies (e.g., Pexman et al., 2003, 2007) on semantic richness effects converges with that from several behavioural and computational modelling studies. However, this conclusion requires further verification.

associative process induced by concrete words. Or that synonyms vs. associated concrete words (assuming that synonyms have a greater semantic overlap) showed priming effects for both, but more rapid for synonyms.

Common areas involved in the processing of concrete words are the left inferior frontal gyrus and the left inferior temporal gyrus. During encoding, respectively during the learning process, concrete words induced stronger activation in the lower left parietal area, in the left inferior frontal gyrus anterior of Broca's region, in the lower right parietal lobe and the precuneus. In addition, ERP studies showed that concrete words vs. abstract are associated with more negative-going potentials, beginning in the time window of the N400 component (250– 450 ms post-stimulus onset) and continuing to up to 800 ms. This concreteness-based difference was most pronounced over frontal scalp sites, while, generally, N400 effects have a centro-posterior focus. However, when the semantic ambiguity was reduced (predictive sentence contexts or repetition), such differences were strongly attenuated for both concrete and abstract words. Also, irrespective of word class ambiguity, nouns tended to elicit larger N400s than verbs. It seems that semantic ambiguity affects concrete words, and in particular nouns. According to Lee and Federmeier (2008, p. 156), concreteness has dissociable effects at the electrophysiological level and this may impact multiple aspects of the neurocognitive processing.

In recognition tasks, abstract words vs. concrete words showed stronger activation in the left inferior frontal gyrus (Broca's region) and in the right lateral occipital gyrus. According to Jessen et al. (2000), the activation within the Broca's area reflects a greater pronunciation effort of the abstract words, while the occipital activation difference might be related to a more complex and unfamiliar visual pattern of abstract words. In respect to the category of numbers (a particular combination of concrete and abstract representations) the evidence showed a left angular gyrus activation when the task required verbal processing. Interestingly, deficits would arise at an abstract, notation-independent level of processing (Dehaene et al., 2003, p. 493).

The general interpretation of the data led to the conclusion that context affects the processing of both abstract and concrete words. But, although concrete and abstract words would share some effects, evidence also showed that they might have a qualitatively different structure. For example, when comparing two conditions with the same visual stimuli, but involving two different semantic domains, two different patterns were observed (Mahon & Caramazza, 2003), or concrete sentences were comprehended more quickly and accurately than abstract sentences, and were responded to faster in meaningfulness judgement and truthfulness judgement tasks (Lee & Federmeier, 2008).

In sum, there seems to be an apparent contradiction about abstract concepts: On one side they would describe a concept that cannot be perceived by the senses, are general and synthetic, on the other side they are very much dependent on a context to become functional. The question remains to examine if an abstract concept can operate label-independently. Our opinion at that moment was that abstract words did not denote an underlying representation, as concrete words would do, but they were rather acting as an index, a link among particular states. In this sense, the representation of "animal" would resemble more over an abstract concept than its concretization through the concept of "dog".

The empirical evidence presented also many other interesting aspects. Unfortunately, there is not enough space to present it here all. However, there is one general remark I would like to add, because it applies to all evidence and this invites us to a special reflection: all studies were on-line studies, dealing in fact with partial representations (which are variable, per definition, see graphic representation of hypothesis 2), while giving account of "full representations". This is **a fundamental bottleneck** that impairs the integration of apparently contradictory empirical evidence. Even if one may believe in an a-priori mechanism, the question is if we could possibly find it at the neural level. If nature-nurture are interwoven determinants of both human thought and behaviour (Plotkin, 2007, pp. 12-17) would it be reasonable to find just dissociated evidence for one or the other factor? We see the adult brain as a product of ontogenetic development (Karmiloff-Smith, 1998), where learning processes and their constraints play a major role, and not only of a genetic determinism to some extent. If we may wish to make sense of neural data, the process of categorisation needs, ideally, to be studied in its evolution, from the child brain to the adult brain in the same individual. We may need to observe the self-organisation process and the progressive elaboration of cortical structures. And we would definitely need to specify the initial state.

In conclusion, no common factor could have been possibly found (yet) at the interface between language (verbal representations) and semantic cognition (e.g., categorisation).

Putting this general and particular evidence into the balance of atomistic semantics (see previous explanation why should we do this), the most robust conclusion we could draw from adult studies of categorisation is that the processes underlying elicited categorisation and/or acting on semantic information that could qualify as "categories" are distributed in space and time. This distribution is particularly visible in the activation of modality-specific areas especially in the early stages of processing, moving towards prefrontal activity in the later stages. Apparently, prefrontal cortex activity shapes further the neural representations in a top-down manner, solving the competition among input features, presumably by a specific mechanism.

The difference in processing was thus much more evident than a difference in representation structures (Vellinga-Firimiță, 2008, pp. 61-63). However, there is also evidence that apparently shows that all concepts that can be verbalised are represented modality-based. But, a more elaborated analysis would show that this aspect is a direct consequence of the way the issue was tested: each time "words" were tested as an access to semantic representations, they were presented via one or the other senses (visually, auditory).[10] The evidence did not support thus without doubt Barsalou's theory. As his theory would compromise Fodor's, his proposal sounds revolutionary to many, but this revolution must still be done by future effort (see Dennett & Viger's open peer commentary in Barsalou, 1999). The question whether the brain would decode what the senses perceive in a way that was learned, or according to some principles that might be innate remained unanswered. Consequently, we may need to take a look at the way we operate with these concepts, when modality as a variable is kept constant. The key issue to answer thus is if there is any semantic component that is not modality-based (modality is here intended as sensory-based), and also if there is any semantic component that is not verbalised.

[10] It is no surprise that modality-specific areas (like visual areas) would become active.

We have still many doubts. Nevertheless, it is time to advocate now for or against atomistic semantics. As Barrett and Kurzban (2006) previously observed, we may assume our interpretation of Fodor was wrong, and subsequently compromise in recognizing emergent modularity as a liaison-theory between Fodor and the rest[11]. But, emergent modularity as a *compromis* theory, despite appealing, has an open end and does not tell us **how structure would (come to) reflect function**. This implicates that **defining 'functionality" at the neural level becomes essential in order to make really sense of any neural data**. However, cognitive neuroscience is not that far yet in answering this question (and Fodor, by the way, doubts she will ever manage). Although beyond the scope of this note, I will try to briefly explain this aspect by taking a look at fMRI as technique and see why (the comment however would apply to all techniques discussed previously): Usually, in fMRI, the nature of functional specialisation is attributed to the sensorimotor or cognitive processes manipulated experimentally, and the visualisation (functional image) of this specialisation is defined by the temporal correlations between spatially remote neurophysiological events, i.e. functional connectivity (Friston, 1994). This definition, as one may well observe, is just a construct, thus it contains a statement about observed correlations between haemodynamic variables, with no explanation about how these correlations are mediated. This implicates that fMRI is just an isomorphic transformation between two unknowns: cognition and neural systems, where the isomorphic transformation itself is neither justified (Friston, 1996; Price & Friston, 2002).[12] fMRI lacks thus a basic criterion to determine the cognitive functionality of the neural structure. This means the challenge becomes to explain how the brain constructs cognition without reducing one level to another nor re-describing them through the terms of one other (Mareschal et. al., 2007). The key issue before invoking any evidence from neurocognitive techniques as an argument for/against any cognitive theory is to establish the causal significance of a functional neuroimaging effect (cf. Rugg, 1999, p. 30). As this fundamental aspect has not yet been elucidated, and until we can definitely prove the contrary, Fodor may well say cognitive science went wrong.

In conclusion, the question whether we may have multiple representations in ontogeny and/or among individuals, or just share an a priori mechanism that would lead us to a similar content is not possible yet to answer on the basis of the above mentioned evidence. Yet, despite the lack of a criterion for

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[11] see Karmillof-Smith (1992) and Clark (1993) as cited by Plotkin (2007, p. 167) on the issue of *minimal nativism*;

[12] A general, but not superficial, examination of this critical state of affairs suggests that the root of the problem may lie in the paradigms of reasoning that have been applied: the inference and reversed inference paradigms. The *Inference reasoning paradigm* is explorative and inquires the role of a brain region in a certain cognitive function, and makes use of a comparative analysis between activations. The guiding principle is Function IS Activation and the result of the analysis is a set of neural correlates. A common example is the activation of Broca- and/or Wernicke areas in a language experiment. Schematically, we can view it as: [cognitive process A at work] →brain region(s) active: [Z+ Z1 +...Zn], where [Z+ Z1+...Zn] are its neural correlates. The *Reverse Inference* reasoning paradigm looks like a kind of brain reading, or attempts explaining unpredicted brain region activation during a task. The guiding principle is Activation IS Function by Task Comparison. For example, inferring which cognitive processes are engaged by task X, when region Z is active. Based on previous data, one would say that the language function is engaged if Broca region is observed active in task X. This reasoning type implicates the preservation of the same isomorphism as previously (it is just reversed), and as discussed earlier in this section, this isomorphism is not justified because it simply contradicts the brain dynamics and the complex psychological significance of the behaviour observed.

functionality, the neural evidence maintains its value by its intriguing richness. This obliges us in readjusting our focus and modifying accordingly our research designs for further attempts on elucidating this amazing yet elusive issue.

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