Levels of Organization: A Deflationary Account

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Abstract

The idea of levels of organization plays a central role in the philosophy of the life sciences. In this article, I first examine the explanatory goals that have motivated accounts of levels of organization. I then show that the most state-of-the-art and scientifically plausible account of levels of organization, the account of levels of mechanism proposed by Bechtel and Craver, is fundamentally problematic. Finally, I argue that the explanatory goals can be reached by adopting a deflationary approach, where levels of organization give way to more well-defined and fundamental notions, such as scale and composition.

Keywords: levels; levels of organization; mechanistic explanation; scale; composition; Bechtel; Craver

1. Introduction

The term 'level' is notoriously ambiguous, and even 'level of organization' is used in different ways in different contexts. This paper concerns 'levels of organization' understood as compositional levels, such that things at a higher level are composed of things at the next lower level. In other words, there are wholes at higher levels and their parts at lower levels. This idea goes back at least to British Emergentism (see, e.g., Alexander 1920, 53-55, Broad 1925, ch. 2,), but more influential accounts are those of Oppenheim & Putnam (1958), Simon (1962), and Wimsatt (1994/2007). In their seminal article, Oppenheim and Putnam (1958) proposed the following list of levels: social groups, (multicellular) living things, cells, molecules, atoms, and elementary particles. In this hierarchy, entities at each level are composed of things of the next lower level.

Many philosophers have raised doubts about the usefulness or coherence of the idea of levels of organization (e.g., Bechtel 2008, ch. 4; Craver 2007, ch. 5; Kim 2002; Ladyman & Ross 2007, 53-57; Love 2012; McCauley 2009; Potochnik & McGill 2012; Rueger & McGivern 2010).[1] The common theme of these authors is that global levels of organization in the style of Oppenheim & Putnam (1958) fail to characterize the organization of science and nature. The world is too complex to be described with such a monolithic hierarchy. For example, things such as a microchip, a human brain, a neutron star, and a glacier do not easily fit into an Oppenheim & Putnam -style hierarchy, and moreover, comparing the level of, e.g., a human brain and a glacier makes little sense. It is much more plausible that levels are more local and do not extend horizontally across nature. For example, there is a certain hierarchy of levels in a human brain, and a different one in a glacier.

Recently William Bechtel and Carl Craver have developed such a localized approach to levels of organization in the life sciences, arguing that there are whole mechanisms at higher levels and their components at lower levels (Bechtel 1994, 2008, Craver 2002, 2007, Craver & Bechtel 2007).[2] These levels are identified relative to a mechanism, and do not extend horizontally across the nature or the organism.

This account of "levels of mechanisms" is arguably the most coherent and scientifically plausible account of levels of organization to date, and will be the main focus of this paper. I will show that even this local and minimalistic account of levels is fundamentally problematic. I will also consider the explanatory goals that an account of levels is supposed to serve, and show that all these goals can be reached with more fundamental and well-defined notions such as scale and composition.

[1] John Heil (2003) has forcefully argued against ontological levels of being, but these are different from levels of organization, as they are not defined in terms of composition, and involve stronger metaphysical assumptions.

[2] Bechtel and Craver have developed their accounts of levels independently, but they are very similar. I will point out the differences where they are relevant. Bechtel and craver also discuss levels in one joint article (Craver & Bechtel 2007).

This leads to a deflationary account of levels, where the term 'level´ can be taken to refer to either scale or composition.

I will first analyze and make explicit the incentives or explanatory goals that have motivated accounts of levels of organization (Section 2). Next, I will briefly present the most state-of-the-art account of levels of organization (Section 3) and show that it is fundamentally problematic (Sections 4-5). Finally, I will defend a deflationary approach to levels and argue that all the explanatory goals for theories of levels can be reached with this approach (Section 6).

2. The Explanatory Goals of Theories of Levels

The idea of levels has been criticized from many different angles, but one crucial question has been largely neglected: why do we need a theory or account of levels? In this section, I will argue that the main motivations for giving an account of levels of organization have been (G1) to provide a framework for understanding reduction and reductive (or mechanistic) explanation; (G2) to capture and understand significant features of the organization of nature; (G3) to clarify and understand the talk of levels in the relevant sciences, and (G4) to provide a framework for analyzing top-down or downward causation. I will start by discussing Oppenheim and Putnam's (1958) classic article "Unity of Science as a Working Hypothesis", which contains probably the most widely cited and influential account of levels of organization, and then contrast it to the views of Bechtel and Craver, which are the main focus of this article.

The main explanatory goal that motivates Oppenheim and Putnam (1958) to introduce levels is clearly goal G1. Their aim is to defend what they consider to be a credible working hypothesis: as science proceeds, different branches and disciplines will be unified through reductions, resulting in unitary science (ibid., 8). Oppenheim and Putnam consider the possibility of ordering the branches of sciences in such a way as to indicate the major potential reductions standing between the current situation and unified science, and offer a system of "reductive levels" for this purpose (ibid., 9). The branch of science with the things of a given level as its universe of discourse is a potential reducer of a branch with the things of the next higher level as its universe of discourse (ibid.).

The general hypothesis that Oppenheim and Putnam are defending is that all special sciences will be eventually reduced to the fundamental physical science. However, it would be implausible to claim that, for example, psychology can be directly reduced to physics. It is far more likely that the reduction happens stepwise: first, psychology is reduced to biology, then biology is reduced to chemistry, and finally chemistry is reduced to physics. Levels are introduced in order to make sense of such stepwise reduction and the right order of the steps.

Although the main reason why Oppenheim and Putnam introduce levels is to work out their view on reduction, at least implicitly they are also attempting to characterize features of the organization of nature (goal G2). The levels of Oppenheim and Putnam are not just an ordering of branches of science or theories; they involve "things" (ibid.). Oppenheim and Putnam assume that at each level there are certain types of things: elementary particles at the lowest level, atoms at the next higher level, and so on. In this sense, the levels of organization are not just reflecting features of theories and sciences, but also features of the world.[3] In general, one of the main reasons that have led philosophers to discuss levels is the assumption that nature is in some sense organized into levels. For example, Wimsatt makes this very explicit: he claims that levels of organization are "*a deep, non-arbitrary, and extremely important feature of the ontological architecture of our natural world*" (Wimsatt 1994/2007, 203).

Let us then move on to the more recent account of Bechtel and Craver and consider their reasons for introducing levels. The context where Bechtel and Craver discuss levels is their account of mechanistic explanation. The idea of mechanistic explanation is roughly the following: in order to explain a phenomenon (such as a mental function), scientists typically describe the (biological) mechanism that results in or accounts for the phenomenon to be explained. Mechanistic explanation is presented as an alternative framework to earlier accounts of scientific reduction and unity of science, such as Oppenheim & Putnam (1958), Nagel (1961) or Schaffner (1993). Whether mechanistic explanations are reductive or non-reductive has been a matter of debate (see section 6), but both Bechtel and Craver agree that a key feature of mechanistic explanations is that they span multiple levels. In order to give a more precise meaning to the idea that mechanistic explanations are multilevel, Bechtel and Craver introduce levels of mechanisms (goal G1).

In the approach of Bechtel and Craver, levels are also clearly intended to capture features of the organization of nature (goal G2). This is particularly evident in the way Craver classifies levels of mechanisms as "levels of nature" (Craver 2007, ch. 5) and defends the causal and explanatory relevance of higher-level things (ibid., ch. 6). Also Bechtel uses ontological terms when describing levels of mechanisms: "a mechanism as a whole and its working parts are at different levels. When dealing with a mechanism, investigators are dealing with two different levels of entities (the mechanism itself and its parts). Entities at each level engage in causal interactions with other entities" (Bechtel 2008, 148).

[3] This does not imply any non-reductive view of levels or higher-level properties. The fact that levels capture features of the organization of nature does not mean that those features are irreducible. The account of Oppenheim and Putnam is ontologically reductive: "Any whole which possesses a decomposition into parts all of which are on a given level, will be counted as belonging to that level. Thus each level includes all higher levels. However, the highest level to which a thing belongs will be considered the "proper" level of that thing" (Oppenheim & Putnam 1958, 9-10).

Thus, the two goals that were evident in Oppenheim & Putnam (1958) – understanding reduction (G1) and describing the organization of nature (G2) – are also driving Bechtel and Craver to introduce their levels. However, in addition to these two goals, Craver and Bechtel have two further goals motivating them to define levels of mechanisms. Importantly, they want to capture or clarify the talk of levels that is common in the life sciences (goal G3) (Craver 2007, 163; Bechtel 2008, 130; Craver & Bechtel 2007, 549; Wright & Bechtel 2007, section 6.1). Craver (2007, ch. 5) takes as a starting point the levels involved in the neuroscientific explanation of spatial memory, and attempts to provide an account of levels that captures them. Bechtel states at the outset of his analysis: "to understand the mechanistic framework in which scientists work, I first develop an appropriate notion of levels" (Bechtel 2008, 130). In their joint article, Craver and Bechtel write that levels of mechanisms are "ubiquitous" in the biological sciences (Craver & Bechtel 2007, 549).

The fourth motivation for Bechtel and Craver to analyze levels is to make sense of top-down causation (G4) (Craver and Bechtel 2007; see also Bechtel 2008, 153-155). This is in line with the long tradition of discussing downward causation in the framework of levels: the question of downward causation has traditionally been whether higher-level causes can have lower-level effects (e.g., Campbell 1974; Emmeche et al. 2000; Glennan 2010; Kim 1992, 2003). Craver and Bechtel (2007) argue that what appears to be top-down causation should be understood as normal intralevel causation – the interlevel effects are then mediated downwards or upwards in the mechanism due to the constitutive relations in the mechanism. (I return to this in Section 6.)

In sum, Bechtel and Craver present their account of levels of mechanisms in order: (G1) to provide a framework for understanding reduction and reductive (or mechanistic) explanation; (G2) to capture and understand significant features of the organization of nature; (G3) to clarify and understand the talk of levels in the (life) sciences, and (G4) to provide a framework for analyzing top-down or downward causation. These four goals cover also the motivations most other philosophers have had for introducing levels of organization. For example, as we saw above, the goals of Oppenheim and Putnam are (G1) and (G2). The main goal of Wimsatt's (1994/2007) account is explicitly stated to be (G2). The goals of Churchland and Sejnowski (1992) are apparently (G2) and (G3). There may be other reasons for providing an account of levels of organization, but I take these to be the central ones.[4]

[4] Levels also often appear in the context of theories of emergence, but I take this to fall under goals G1, G2 and G4. Alternatively, one could add to the list "understanding emergence" as a further goal – the list is not intended to be exhaustive. In any case, emergence is not a topic of this article.

3. Levels of Mechanisms

Now that we know the goals motivating the account of Bechtel and Craver, let us turn to the account that is put forward to reach those goals. The account of "levels of mechanisms" introduced by Bechtel and Craver is arguably the most state-of-the-art and scientifically plausible account of levels of organization. As noted before, the core idea of levels of organization is that they are compositionally related – wholes are at higher levels and their parts at lower levels. In levels of mechanisms, this compositional relation is made more specific: the relata are not simply wholes and parts; there are behaving mechanisms at higher levels and their active components at lower levels. This means that the higher-level entity is an active mechanism performing some function, and the lower-level entities are components that contribute to the mechanism for this function.

Craver gives the following characterization: "In levels of mechanisms, the relata are behaving mechanisms at higher levels and their components at lower levels. These relata are properly conceived neither as entities nor as activities; rather, they should be understood as acting entities. The interlevel relationship is as follows: X's Φ-ing is at a lower mechanistic level than S's Ψ-ing if and only if X's Φ-ing is a component in the mechanism for S's Ψ-ing. Lower-level components are *organized together* to form higher-level components." (Craver 2007, 189)

Bechtel writes: "Within a mechanism, the relevant parts are … working parts—the parts that perform the operations that enable the mechanism to realize the phenomenon of interest. These may be of different sizes, but they are distinguished by the fact that they figure in the functioning of the mechanism. It is the set of working parts that are organized and whose operations are coordinated to realize the phenomenon of interest that constitute a level." (Bechtel 2008, 146)

This kind of talk of mechanisms and their components might seem to imply a two-level hierarchy: there is the behaving mechanism at the higher level, and its components at the lower level. However, this two-level picture is easily expanded into a multilevel hierarchy, when we include the possibility that a working part of a mechanism can be a mechanism itself (Bechtel 2008, 147). Thus, the components of that "nested" mechanism form another level, which is two levels lower than the mechanism as a whole. This process can be iterated as many times as necessary.

Levels of mechanisms are not general levels of organization that extend horizontally across the world or the organism (as in Oppenheim & Putnam (1958), Churchland & Sejnowski (1992), and partly also Wimsatt (1994)). Levels of mechanisms are identified with respect to a given mechanism and the function it performs, and other mechanisms may have completely different hierarchies of levels. One consequence of this is that it does not make sense to ask whether things that belong to different mechanisms are at the same or different levels. We cannot, for example, say that a Na^+ ion in the hippocampal LTP mechanism is at the same level as a Na^+ ion in the retinal mechanism of phototransduction, since they are involved in distinct mechanisms (cf. Craver 2007, 191).

Prima facie, the account of levels of mechanism serves well the explanatory goals stated in section 2. It appears to provide a well-defined framework for analyzing issues such as reduction and mechanistic explanation: for example, the higher-level properties that are targets of reduction can be understood as occupying higher levels of mechanisms, and the reducing properties lower levels of mechanisms. Similarly, it seems to lead to a clear understanding of what it is to be a higher-level cause and a lower-level effect. Levels of mechanisms arguably capture the talk of levels in science as well, since they are supported by detailed examples from science. However, as I will show in the next two sections, a closer analysis reveals that levels of mechanisms lead to fundamental problems.

4. Problem 1: Levels in Science

Bechtel and Craver argue that their account of levels of mechanisms captures a notion of levels that is central in the life sciences (Craver 2007, 163; Bechtel 2008, 130; Craver & Bechtel 2007, 549). However, Bechtel and Craver may have overestimated the extent to which their account reflects the talk of levels in the relevant sciences.

Let us start with Craver's (2007, 165-170) main scientific example, which is the case of spatial memory and LTP (Long Term Potentiation). He identifies four levels in the spatial memory mechanism: the level of *spatial memory*, the level of *spatial map formation,* the *cellular-electrophysiological* level, and finally the *molecular* level (Figure 1). For example, the *cellular-electrophysiological* level includes neurons that depolarize and fire, synapses that undergo LTP, action potentials that propagate, and so on. At the *molecular* level we find NMDA and AMPA receptors, Ca^{2+} and Mg^{2+} ions, and so on. In this hierarchy, entities at each lower level are active components in the higher-level mechanism. For example, the NMDA receptor is a component of the synaptic mechanism of LTP, and the LTP mechanism is in turn a component of the hippocampal mechanism of memory consolidation (at the level of spatial map formation). Craver first argues that extant accounts of levels (such as Oppenheim & Putnam 1958 or Wimsatt 1994/2007) are inherently problematic or do not successfully capture these levels, and then puts forward levels of mechanisms as the solution.

**** INSERT FIGURE 1 AROUND HERE ****

However, it is doubtful whether Craver's levels of mechanisms do any better than the other accounts. In order to get the four-level hierarchy (the molecular level, the cellular-electrophysiological level, and so on), we need to locate same kinds of things in the spatial memory mechanism at the same level: various NMDA receptors should all be at the molecular level of the memory mechanism, all neurons in the mechanism should be at the cellular-electrophysiological level, and so on. The problem is that, as we will see in the next section, the account of Bechtel and Craver does not have this outcome: NMDA receptors in different kinds of neurons *cannot* be at the same level, since they are components in different submechanisms. Thus, Craver's own account of levels does not result in the levels of spatial memory it is intended to capture. It results in a much more complex and branching hierarchy, where there is no single molecular level, cellular-electrophysical level, etc., but rather numerous molecular levels, numerous cellular-electrophysical levels, etc., and there is no sense in which different NDMA receptors in different kinds of neurons are at the same level.

More fundamentally, why should we even accept the four levels of spatial memory as a starting point? It is clear that scientists are often appealing to compositional relations in their theories and explanations, but it is far less clear that they are committed to the kinds of levels indicated by Craver. While no scientist would deny that NMDA receptors are components of neurons, it does not follow from this that scientists are assuming that there is a cellular-electrophysiological *level* where all neurons in the mechanism are located, and a molecular *level* where all NDMA receptors in the mechanism are located. Interestingly, also the figure presented by Craver (Figure 1) shows only compositional relations, and no levels over and above them (I return to this point in Section 6).

In general, Bechtel and Craver may have exaggerated the importance of the notion of levels of mechanisms in science. Let us consider for example neuroscience, which is the branch of science where Bechtel's and Craver's main examples are drawn from. In most neuroscience articles, the term 'level' (in the sense discussed in this paper) does not come up at all. For example, a search for the phrase "cellular level" in *The Journal of Neuroscience* revealed that the term appears in less than 3.5% of the articles published in the last ten years.[5] This applies also to the case of memory consolidation and LTP, which is Craver's paradigmatic example of a multilevel mechanism – see, e.g., McGaugh (2000) and Malenka & Bear (2004) for two representative review articles, where there is no mention of levels.[6]

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[5] A full-text search for the phrase "cellular level" for the period January 2004 – January 2014 found the phrase in 572 articles. The total number of papers involved in the search was around 16 500 (the journal publishes roughly 50 issues per year, and there are usually at least 30 papers per issue). This means that the phrase appeared in less than 3.5 % of the papers. The results for "molecular level" and "behavioral level" were even lower (269 and 212 respectively).

[6] In some fields of the life sciences, such as the study of protein structure, ecology, or systems biology, talk of levels (of organization) may be more common. However, also in these fields the talk of levels can mostly be understood in terms of more

One might object that even though talk of levels is rare in research articles, the idea of levels plays an important role in textbooks, discussions, and introductory texts. However, even in standard textbooks such as Kandel et al. (2000) or Purves et al. (2008), levels-terminology is rare. When the notion does appear, it is typically not used in the sense of levels of mechanisms. Let us consider a representative example. In a section titled "Nerve Cells Differ Most at the Molecular Level", Kandel writes: "because the nervous system has so many cell types and variations at the *molecular level*, it is susceptible to more diseases (psychiatric as well as neurological) than any other organ of the body" (Kandel et al. 33, my italics). Kandel is describing the general idea that the nervous system is susceptible to a broader range of diseases than other organs because nerve cells vary greatly at the molecular level. Here the term 'level' is used in a sense that is *not* restricted to one mechanism: the issue here is molecular variation in nerve cells *throughout* the nervous system. The term 'molecular level' seems to be referring to a general level of organization, in the style of Oppenheim & Putnam (1958). However, as I will argue later (Section 6), a more coherent and plausible way of making sense of levels of this kind is to think of them in terms of *scale*: what is at issue here is the molecular scale in cells throughout the brain. In any case, the main point is that levels-terminology is not common in neuroscience, and when the term is used, it is typically *not* used in the sense of levels of mechanisms.

5. Problem 2: The Same Level Criterion

Above I have shown that it is doubtful whether 'levels of mechanisms' captures a notion of levels that is important in science. This is, of course, not a knock-down argument against levels of mechanisms. Capturing the levels talk in science is just one way in which the theory of levels can be justifiable or relevant from the point of view of science. Alternatively, the notion of levels may help to structure and understand actual scientific explanations, or the general explanatory or ontological framework, even if scientists themselves don't use the notion. However, in this section, I show that this is not the case, due to a deeper problem related to the way in which levels of mechanisms, and particularly the same-level relation, are defined.[7]

In contrast to some other philosophers (e.g., Wimsatt 1994/2007), Bechtel and Craver make the criterion for being at the same level explicit, which is laudable. Bechtel argues that it is the working parts of a mechanism that constitute a level (Bechtel 2008, 147). Craver discusses the issue in some more detail, and suggests the following criterion: "X and S are at the same level of mechanisms only if X and S

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fundamental notions, such as scale and composition. See Love (2012) for a discussion of levels of organization in proteins; Potochnik & McGill (2012) for a critical account of levels in ecology; and Richardson & Stephan (2007) for an interesting discussion of levels in systems biology.)

[7] This problem is briefly introduced in Eronen (2013).

are components in the same mechanism, X's Φ-ing is not a component in S's Ψ-ing, and S's Ψ-ing is not a component in X's Φ-ing" (Craver 2007, 192).[8] This of course gives us only a necessary condition for being at the same level ("only if"). However, what Craver later writes suggests that this condition is also intended to be a sufficient condition: "what places two items at the same mechanistic level is that they are in the same mechanism, and neither is a component of the other" (Craver 2007, 195). This makes more sense, since a mere necessary condition would not be helpful in locating same-level things – it would only allow us to determine when things are *not* at the same level. For these reasons, I will assume here that Craver's criterion is also intended as a sufficient condition.

For illustration, let us consider the mechanism for phototransduction (the conversion of light signals into electrophysiological information) in the retina. The most important components in the phototransduction mechanism are the photoreceptor cells, rods and cones, which reduce their rate of firing in response to light stimuli. It follows from the criteria proposed by Bechtel and Craver that rod cells and cone cells are at the same level: they are working parts in the same mechanism (Bechtel), and they are components in the same mechanism but neither is a component of the other (Craver).[9]

So far this seems unproblematic, but unfortunately, this approach leads to a dilemma. The same-level criterion essentially depends on the notion of a component or working part (in this context, the two mean essentially the same), and the accounts of Bechtel and Craver seem to involve two different notions of component, a strong and a weak one. If we adopt the stronger notion, levels become so limited that they practically disappear. If we adopt the weaker one, almost everything in a mechanism turns out to be on the same level.

Let us start with the weaker notion. Craver introduces the following mutual manipulability criterion: "a part is a component in a mechanism if one can change the behavior of the mechanism as a whole by intervening to change the component and one can change the behavior of the component by intervening to change the behavior of the mechanism as a whole" (2007, 141).

For example, rod cells in the phototransduction mechanism clearly satisfy the mutual manipulability condition: one can change the behavior of the phototransduction mechanism by depolarizing the rod cell, and one can change

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[8] Craver (personal communication) has pointed out that he no longer believes we need a criterion for being at the same level – what is important is determining when things are at *different* levels.

[9] To be exact, in Craver's (2007) framework we should not simply talk of components, but always include the activity the component is engaged in (X's Φ-ing, S's Ψ-ing, etc.). However, for the sake of readability I have left out the variables denoting the activities, and mostly just talk of components. Nothing crucial turns on this, and it should be noted that Craver himself switches back and forth between talk of components and acting entities.

behavior of the rod cell by exposing the retina to a light stimulus. However, an interesting outcome of the mutual manipulability criterion is that also subcomponents turn out to be components of the overall mechanism. For example, a Na^+ channel of the rod cell also satisfies the mutual manipulability condition: by intervening on the Na^+ channel (e.g., by blocking it) we can change the behavior of the phototransduction mechanism as a whole, and by intervening on the mechanism as a whole (e.g, by exposing the retina to a light stimulus) we can change the behavior of the channel. Thus, one important consequence of the mutual manipulability condition is that also subcomponents (and in turn their subcomponents, and so on) count as components of the overall mechanism.

This leads to a problem for the same-level criterion proposed by Craver. As we saw, Craver suggests that any two components in the mechanism that are not in a component-mechanism relation with each other are at the same level.[10] Let us consider, for example, components C_1 and C_2 in mechanism M (Figure 2). They are at the same level, since C_1 is not a component of C_2 and C_2 is not a component of C_1. Consider then a subcomponent S_1 of C_1. Assuming that it satisfies the mutual manipulability criterion, it is also a component of M. Furthermore, S_1 is not a component of C_2, and C_2 is not a component of S_1, so following Craver's criterion, C_2 and S_1 are also at the same level. However, if C_2 and C_1 are the same level and C_2 and S_1 are at the same level, it follows that C_1 and S_1 are at the same level, under the very plausible assumption that the same-level relation is transitive. This leads to a contradiction, since S_1 is a component of C_1 and thus at a lower level than C_1. Even if we don't assume transitivity, the outcome is that C_1 is at the same level as C_2, while also S_1 is at the same level as C_2, and any further subcomponent of S_1 is at the same level as C_2. This is clearly an absurd and unacceptable outcome.

**** INSERT FIGURE 2 AROUND HERE ****

It should be noted that this problem is not just due to Craver's criterion of mutual manipulability, which has been recently criticized. In general, a criterion for what is constitutively relevant for the mechanism does not need to make any distinction between components and subcomponents. For example, the INUS approach proposed by Couch (2011) has exactly the same outcome for the same-level issue as the criterion of mutual manipulability.

Fortunately, this outcome can be avoided if we adopt the stronger notion of component. When discussing levels, Bechtel and Craver apparently use 'component' or 'working part' to refer to not just any component of the mechanism, but those first-order components that are directly involved in the mechanism and not parts of

[10] Bechtel's criterion for being at the same level leads to the same conclusion when combined with the mutual manipulability criterion.

any other components. In order to make sense of this, we need to single out a subclass of components: *direct* components.[11] Components are those parts of the mechanism that satisfy the mutual manipulability condition, or whatever the preferred condition for componenthood is, while direct components are those components that are not components of any other component of the mechanism. For example, a Na^+-channel of a rod cell is a component of the retinal mechanism for phototransduction, but it is not a direct component of that mechanism. In contrast, it *is* a direct component of *cellular* mechanism of phototransduction (in the rod cell), since it is not a component of any other component in that mechanism. In Figure 2, components C_1 and C_2 are direct components of M, and S_1 and S_2 are direct components of C_1, but S_1 and S_2 are *not* direct components of M.

It is clear that in order to avoid the problem of (nearly) everything in a mechanism being at the same level, the suggested same-level criteria should only apply to direct components. Craver's criterion would then state that two items are at the same level if and only if they are *direct* components of the same mechanism and neither is a component of the other. However, the latter part of the condition is now superfluous, since two components cannot both be direct components of the same mechanism if one of them is a component of the other. Thus, Craver's criterion in the revised form states simply that two things are at the same level if and only if they are direct components of the same mechanism. Bechtel's criterion in the revised form states that it is the direct components of a mechanism that constitute a level, so the two criteria practically collapse into one.

This same-level criterion based on the stronger reading of 'component' seems to be closer to the intentions of Bechtel and Craver. However, it also leads to a problem. If we adopt this approach, *only* direct components of the same mechanism can be at the same level. Direct components of two *different* components can never be at the same level. For example, in Figure 2, S2 and S3 cannot be at the same level, no matter what sorts of things they are. This is actually something Bechtel (2008, 147) and Craver (2007, 193) seem to be willing to accept, but it has very undesirable consequences. There are many cases where the subcomponents of the different components are exactly the same types of things – consider for example the fact that there are exactly the same kinds of cGMP-gated Na^+-channels in both rod cells and cone cells. Furthermore, subcomponents of different components often causally interact (Fazekas & Kertesz 2011). Thus, one outcome of the stronger form of the same-level criterion is that the same types of things that are playing the same role in the same overall mechanism, and that potentially causally interact with each other, are often not at the same level (see Eronen 2013 for more). A further outcome is that levels of mechanisms do not capture the kinds of hierarchies in science that they are intended to capture – for example, in the spatial memory mechanism, instead of a neat four-level hierarchy, we get a very complex and branching hierarchy of levels (see Section 4).

[11] The distinction between components and direct components is briefly introduced in Eronen (2013).

Thus, with the stronger reading of 'component', the same-level criterion becomes extremely restrictive: it implies that in many cases same types of things or things that are causally interacting are *not* at the same level. Furthermore, this approach effectively amounts to reducing levels to (direct) compositional hierarchies, meaning that there are no levels over and above compositional hierarchies. This is of course not a problem as such, but since Bechtel and Craver extensively discuss levels and emphasize their importance, it is obvious that they are aiming at something more substantial than just giving another label for compositional hierarchies. Furthermore, this reductive outcome is in conflict with the idea, defended in Craver and Bechtel (2007), that intralevel causation is somehow less problematic than interlevel causation (see Section 6 and Eronen 2013).

Perhaps one could argue that giving a criterion for being at the same level is not necessary for an account of levels of organization – it is enough if we can say when things are at *different* levels.[12] The account of levels of mechanism certainly gives a sufficient and necessary criterion for being at a different level: things that are in a component-mechanism relation are at different levels. In other words, A and B are at different levels iff A is a mechanism and B is a component of that mechanism, or the other way around. However, the problem with this approach is that it hardly makes sense to give an account of levels where things can only be at different levels but never at the same level. At least, such an account would not resemble any notion of levels used in science or philosophy. If we get rid of the idea of being at the same level, it rather seems that we are talking about something else than levels.

The conclusions of this section have implications not only for Bechtel and Craver, but for the idea of levels of organization in general. There does not seem to be a plausible way of defining the same-level relation based on just composition. With a weaker criterion that is based on being a component in the same system or mechanism, almost everything in the system or mechanism turns out to be at the same level. With a stronger criterion that is based on direct components being at the same level, the levels seem to reduce to compositional hierarchies. In order to make the same-level relation plausible, something more than just compositional relations seems to be required, such as considerations of similarity of scale or causal interactions. It remains to be seen whether such an account can ever be coherently formulated. In any case, there is no reason why such levels should be restricted to the borders of the system or the mechanism, so they would not resemble much the levels of mechanisms. Furthermore, as I will argue in the next section, it is not at all clear whether there is any room or need for such levels.

6. A Deflationary Account of Levels of Organization

[12] As pointed out in footnote 4, this appears to be the approach Craver himself now favors.

I have argued above that the account of levels of mechanisms is unsatisfactory: it results in a notion of levels that is either very restricted or very broad, depending on how we understand 'component'. The restricted notion collapses into (mechanistic) composition, while the broad notion is incoherent. One possible reaction to this would be to try to revise the account or to develop an alternative account, but I leave this (daunting and possibly futile) task to other authors. Instead, I will consider in this last section how the explanatory goals outlined in section 2 can be reached *without* any distinct theory of levels, or to put it differently, with an entirely deflationary account of levels (see also Eronen 2013).

Both Bechtel and Craver start their analysis of levels by discussing the problems of more general theories of levels of organization, and the account of levels of mechanism is then presented as a minimalistic and local account of levels that supposedly avoids the problems of more ambitious accounts. However, Bechtel and Craver still hold on to some basic intuitions about levels, such that there is a substantial sense in which things are at the same level, and that causal interactions generally take place within a level. In my view, it is better to take the next natural step and to deflate the notion of mechanistic levels into simply mechanistic composition. In other words, we should just accept the outcome based on the stronger reading of "component", and concede that there are no mechanistic levels over and above mechanistic compositional hierarchies. The attempts at supplementing compositional relations with further requirements (such as the same-level criterion) only lead to problems and unintuitive results. We may still refer to the set of direct components in a mechanism with the term 'level', but no further meaning or intuitions should be attached to the term.

However, it is clear that compositional relations alone are not sufficient for satisfying the explanatory goals outlined in Section 2. For analyzing reduction, downward causation, or levels-talk in science, we need something more than just compositional relations. For example, there is a difference in kind between the activity of a rod cell causing a bipolar cell to fire, and the activity of a rod cell causing glutamate ions to be released. This difference between bipolar cells and the glutamate ions cannot be captured in terms of composition, since glutamate ions are not components of the bipolar cells. A typical way of conceptualizing this difference in philosophy is to say that glutamate ions are at a lower level than bipolar cells. However, we do not need levels for capturing these kinds of differences. Instead of asking whether things are at the same or different level, we can locate things on a *scale* and see how they relate to each other (see also Eronen 2013, Rueger & McGivern 2010, Potochnik & McGill 2012). Glutamate ions are clearly found at a smaller size (and temporal) scale than bipolar cells.

Thus, we should supplement considerations of mechanistic composition with considerations of scale. All that is needed for arranging things on a scale is some property (such as size) that can be quantitatively measured in those things. How to exactly measure that property may be problematic, but once it is measured, arranging things at different points in the scale is straightforward. The scale can be divided into segments (e.g., scale of atoms, scale of molecules, scale of cells, etc.), but this is merely heuristic – the scale as such is entirely continuous, in contrast to levels or compositional hierarchies.

Although the most commonly discussed scale, at least in the context of levels, is size scale, other scales should not be neglected. Consider for example the time scale: there is great variety in the rate at which processes in biological organisms can take place (see, e.g., Simon 1962; Wimsatt 1994/2007). For example, molecular interactions happen at a much faster rate than interactions between neurons, which are again faster than interactions between brain areas. One can also apply energy, momentum, force, or spatial scales, among many others (Rueger & McGivern 2010), but I will focus on the size and time scales here.

It is worth noting that our widely shared ideas of hierarchical organization seem to combine composition and (size) scale. This is also reflected in the account of Oppenheim & Putnam (1958). Their levels are in the first place compositional, but at each level there are also entities of similar size. In this picture, composition and scale work together in harmony: there are wholes at higher levels and their parts at lower levels, and the things at any given level are of a similar size. A more recent account of levels where considerations of scale and composition are combined is that of William Wimsatt. Wimsatt (1994/2007, 206-221) argues that levels of organization are compositional levels that are organized by part-whole relations, and presents a long list of features that levels of organization typically have, for example that things at a level typically causally interact with other things at that level. He also suggests that if we draw a curve of regularity and predictability against a (logarithmic) size scale, levels appear as peaks (or local maxima) in the curve.

However, the world is generally not organized in such a neat way that considerations of scale and composition mesh with each other.[13] Usually size and composition come apart: the (direct) components of a mechanism can be of radically different sizes, and two same-sized things can be (direct) components of radically different mechanisms. For example, the (direct) components of the rod phototransduction mechanism include things as different as outer segments and Na^+ ions, and O_2 molecules can be (direct) components of steelmaking furnaces, the atmosphere of the earth, or the cellular respiration mechanism. For these reasons, it is a better to keep the dimensions of scale and composition apart and not try to combine them into levels.[14]

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[13] Wimsatt (1994/2007, 221-240) also explicitly states that levels of organization do not apply in many domains of the life sciences, where neat compositional relations break down and it is more appropriate to talk of "perspectives" or "causal thickets". It should also be noted that Wimsatt's idea that at some scales we find peaks of regularity and predictability is entirely compatible with my deflationary approach.

[14] Potochnik and McGill (2012) have proposed an approach to levels that comes close my deflationary view. Focusing on ecology, they argue that the idea of

In section 2, I argued that the following explanatory goals have motivated accounts of levels of organization: (G1) to provide a framework for understanding reduction and reductive (or mechanistic) explanation; (G2) to capture and understand significant features of the organization of nature; (G3) to clarify and understand the talk of levels in the relevant sciences, and (G4) to provide a framework for analyzing top-down or downward causation. To illustrate the deflationary approach I have outlined above, I will now briefly show how these goals can be reached without any distinct notion of levels.

As we saw in section 2, the original context where levels were introduced by Oppenheim and Putnam was reduction, and also one of the main reasons why Craver and Bechtel discuss levels is to provide a framework for their analysis of reduction and reductive explanation (goal G1). Craver (2007, ch. 7) argues that contemporary neuroscience is antireductive in the sense that it results in multilevel explanations where there is no fundamental level. Bechtel (2008, 148-152) highlights more the reductive aspects of "downward-looking" mechanistic explanations, where the higher-level behavior of a mechanism is explained in terms of the components of the mechanism and their organization, but agrees that explanations in neuroscience are inherently multilevel.

The main ideas of both authors can be reformulated without loss in the framework introduced above. Craver's antireductionism can be expressed in terms of composition and scale: in mechanistic explanations, scientists often need to refer to properties and generalizations throughout the compositional hierarchies and at different scales. The notion of levels does not add anything to this basic idea. For example, in the explanation for phototransduction we need to consider the eye as a whole converting light to electrophysiological signals, but also components of the eye, such as rod photoreceptor cell hyperpolarizing, and their components, such as retinal molecules changing shape. The various rod cells in the retina are found at the same size and time scale relative to each other. Similarly, we can understand the reductive aspect of mechanistic explanation simply in terms of composition: mechanistic explanations are reductive in the sense that the behavior of the whole mechanism is explained in terms of the organization and behavior of its (direct) components. No reference to levels (over and above scale and composition) is needed.

The notion of levels also often comes up in discussions of theory reduction (e.g., Nagel 1961, Bickle 1998, Hooker 1981) or functional reduction (Kim 1998, 2003, 2005). The question that is often posed there is whether a higher-level theory or property can be reduced to a lower-level theory or lower-level properties. However,

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(universal) hierarchical levels of organization is deeply problematic, and suggest replacing levels with scales or "quasi-levels" that are derived from scales. The idea is that in a particular context or domain under study we may find that causal processes cluster at a given scale, and therefore it makes sense to talk of a quasi-level at that scale.

a closer look at the actual models of reduction shows that the notion of levels does not play any substantial role in them. In the specific conditions or definitions of theory reduction, there is no mention of levels (Bickle 1998, Hooker 1981, Nagel 1961). Sure enough, there needs to be an asymmetry between the theory to be reduced and the reducing theory, since reduction goes only in one direction, but this asymmetry need not be (and generally is not) spelled out in terms of levels. The requirement is rather that the reducing theory should be in some sense more powerful, general, or accurate than the theory to be reduced.

Similar considerations apply to functional reduction. Kim himself argues that the properties to be reduced, such as mental properties, are *not* higher-level properties but rather second-order properties – that is, properties that some first-order properties have. Alternatively, if one does not want to accept the problematic idea of higher-order properties, the asymmetry can be spelled out in terms of composition: the property to be reduced should be a property of the system as a whole, and the reducing properties should be properties of the (direct) components of the system (this is analogous to the sense in which Bechtel considers mechanistic explanation to be reductive). A third possibility is to appeal to the same strategy as in the case of theory reduction above, and point out that the reducing properties should be embedded in a theory that is more powerful, general, or accurate than the theory that involves the property to be reduced. In any case, no notion of levels is needed for understanding functional reduction.

One could argue that even if we accept all of this, there remains a clear sense in which sciences such as psychology (and the properties they deal with) seem to be higher-level than sciences such as neurobiology or physics. However, these kinds of intuitions can be cashed out more precisely and unambiguously in terms scale and composition. For example, psychology (understood as traditional cognitive psychology) is concerned with properties of whole human beings, or at least whole brains or brain areas. It studies phenomena that occur at relatively slow time scales. In contrast, neurobiology is mainly concerned with the components and subcomponents of brains and brain areas. It studies phenomena that occur at much faster time scales, such as synaptic transmission between neurons, or the binding of neurotransmitters to receptors. Physics, being the most general science, applies in principle to phenomena at all scales.[15]

This naturally brings us to goals (G2) and (G3). In the light of the discussion above, the idea that levels are needed for capturing important features of the organization of nature (G2) becomes questionable. Other authors have convincingly showed that the idea that there are global levels of organization in nature is doubtful and problematic (Bechtel 2008, ch. 4; Craver 2007, ch. 5; Kim 2002; Ladyman & Ross 2007, 53-57; Love 2012; McCauley 2009; Potochnik & McGill 2012; Rueger &

[15] I am indebted to McCauley (2009) for this idea. He develops a similar approach, but his account is not deflationary, since he aims at defending (something like) the traditional idea of levels of organization.

McGivern 2010). In this paper I have argued that the only coherent sense in which there are mechanistic levels in nature is that there are simply compositional hierarchies in nature. Furthermore, as I have argued (and will argue) in this section, in all of the central contexts, the features that levels were supposed to capture are more accurately and coherently captured in terms of various other notions such as composition and scale. There does not seem to remain much room for a substantial account of levels in this respect.

Regarding goal (G3), i.e. clarifying the talk of levels in the relevant sciences, I have argued in Section 4 that terminology referring to levels of organization is not as ubiquitous in science as has often been assumed. In fact, talk of levels of organization is relatively rare in neuroscience, and typically appears in discussions or introductions, not in actual theories or explanations. This makes the goal of clarifying the notion far less important than authors such as Bechtel and Craver assume. Furthermore, as I argue in this section, in the debates where levels typically appear, clarification is best achieved by replacing the framework of levels with the framework of scale and composition. These notions are relatively well-defined, and adequate for conceptualizing issues such as reduction and causation. Thus, if the aim is to clarify talk of levels of organization in the sciences, the best way of achieving this is by adopting the deflationary approach.

However, this does not imply a full-scale elimination of the notion of levels (of organization). One way of characterizing the situation is this: An explication of the term 'levels of organization' reveals it to be ambiguous and to contain two conflicting components: scale and composition. If we try to combine both of these elements in the same notion, this leads to contradictions and problems. However, this does not mean that we cannot refer to *either* scales *or* compositional hierarchies in terms of levels. In a sense, the notion of level (of organization) can be reduced to more fundamental notions, such as scale and composition, but this is different from elimination, since much of the existing talk of levels can be preserved; it is simply made more coherent and unambiguous in the new framework.

Finally, let us turn to the issue of downward or top-down causation (goal G4). Also in this context, the idea of levels has only muddled up the discussion, and makes the solution proposed by Craver and Bechtel (2007) unnecessarily confusing. Craver and Bechtel (2007) claim that causation does not cross levels: what appears to be downward causation or upward causation should be understood as intralevel causation that has mechanistically mediated effects downwards or upwards in the mechanism. What is meant by "mechanistically mediated effects" is roughly that the effects downwards and upwards in the mechanism are due to the constitutional relations in the mechanism (Craver & Bechtel 2007, 554-555).

However, it is clear from the examples that Craver and Bechtel (2007) present that what they are actually arguing against is causation between things that are compositionally related, that is, against causation from mechanisms to the components of that same mechanism or the other way around. Understood in this way, the notion of levels does not play any substantial role in their argument. It only makes the argument more ambiguous and potentially confusing, since it suggests that something more than just compositional relations is at issue. On the other hand, if we do interpret the position of Craver and Bechtel strictly in terms of levels of mechanisms, the idea being that intralevel causation is less problematic than cross-level causation, it makes little sense. As we have seen above (Section 5), the same-level criterion proposed by Bechtel and Craver has the consequence that things of radically different scales can be at the same level and that causally interacting things can fail to be at the same level (see also Eronen 2013).

Thus, the account of downward causation proposed by Craver and Bechtel would only become clearer and more coherent if it was formulated simply in terms of (mechanistic) composition instead of the framework of levels. In general, there seems to be no reason to discuss causal issues in terms of levels as opposed to discussing them in terms of more well-defined notions, such as scale and composition.

We have now seen that we need levels of organization for none of the goals (G1-G4) outlined in section 2. Debates on reduction and mechanistic explanation do not require levels as a framework. Levels do not seem to be needed for capturing significant features of the organization of nature. Talk of levels is not ubiquitous in science, and when it needs to be clarified, this is best done in terms of more fundamental notions, such as compositional relations and different scales. Issues of downward causation become only clearer when we abandon the framework of levels. These considerations suggest that developing a further account of levels of organization is likely to be pointless, and that we can wholeheartedly embrace the deflationary approach.

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Figure Captions

Fig. 1 Craver's presentation of the multilevel mechanism of spatial memory, modified from Figure 10.2 in Craver & Darden (2013) (copyright of the original: University of Chicago Press)

Fig. 2 Mechanism, components, and subcomponents: M stands for the overall mechanism, C_n are components of the mechanism, and S_n are subcomponents