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S.I. : EXPLANATIONS IN COGNITIVE SCIENCE: UNIFICATION VS PLURALISM

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Representational unification in cognitive science: Is embodied cognition a unifying perspective?

Marcin Miłkowski[1] · **Przemysław Nowakowski**[1]

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Abstract

In this paper, we defend a novel, multidimensional account of representational unification, which we distinguish from integration. The dimensions of unity are simplicity, generality and scope, non-monstrosity, and systematization. In our account, unification is a graded property. The account is used to investigate the issue of how research traditions contribute to representational unification, focusing on embodied cognition in cognitive science. Embodied cognition contributes to unification even if it fails to offer a grand unification of cognitive science. The study of this failure shows that unification, contrary to what defenders of mechanistic explanation claim, is an important mechanistic virtue of research traditions.

Keywords Explanatory unification · Representational unification · Explanatory integration · Mechanistic explanation · Embodied cognition · Research tradition

In cognitive science, to classify pieces of research one appeals to general approaches, paradigms, frameworks or research perspectives—as these approaches are variously called. Just how these contribute to cognitive explanations requires a comprehensive analysis. We claim that one of their roles is representational unification. In the past, the proponents of mechanistic explanation have mostly focused on integration, as exemplified in the rich study of interfield integration in life sciences (Craver and Darden 2013; Darden and Maull 1977). But there is further insight to be gained by looking at why explanations are considered unified or not.

✉ Marcin Miłkowski
marcin.milkowski@gmail.com

Przemysław Nowakowski
prrono@wp.pl

[1] Institute of Philosophy and Sociology, Polish Academy of Sciences, ul. Nowy Świat 72, 00-330 Warsaw, Poland

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Unification is a recurring theme in the debate over theories of cognition. Many fear that interdisciplinary cognitive research may fall prey to extreme fragmentation (e.g., Newell 1973). This fragmentation may, in turn, cause duplication of effort and a lack of understanding. While various approaches have been proposed to achieve unification in cognitive science, from the study of cognitive architectures (e.g., Newell 1990), through unifying modelling methodologies (e.g., Eliasmith and Anderson 2003), to unifying frameworks such as predictive coding (Friston 2010), there has been less focus on how these researchers understand unification and on how to assess the unificatory success of a given approach to the study of cognition. Some ascribe such success to embodied cognition (e.g., Glenberg 2010; Schubert and Semin 2009). In this paper, we hope to address both questions from the mechanistic perspective by sketching an account of how these approaches may contribute to the representational unity of cognitive science. In effect, we defend a novel, multidimensional account of representational unification.

The paper unfolds as follows: first, we argue that the variegated, and sometimes contrasting approaches that are grouped under the label 'embodied cognition' (EC) are best viewed as a research tradition in the sense defined by Laudan (1977). In contrast to Larry Shapiro (2007), we argue that embodied cognition is not a nebulous research program but a diversified research tradition. In Sect. 2, we argue that the research agenda of EC should not be understood in terms of a single abstract claim. Instead of proposing yet another definition of EC, we argue that it is best understood as exemplified and constituted by a number of similar approaches that retain only a few common metaphysical and methodological commitments. In the subsequent section, we focus on the unificatory role of these commitments in cognitive science and argue for a dimensioned view of representational unification. Then we turn to the issue of how a research tradition can be evaluated. We point out that the failure to unify phenomena is at the core of recent harsh criticism offered by Goldinger et al. (2016). In our view, however, this criticism is not entirely justified because it confuses a grand research tradition with detailed models by requiring EC to feature the degree of detail typical of models. In conclusion, we argue that our approach enriches the mechanistic perspective on the dynamics of scientific progress by stressing the importance of unification of explanatory projects, even when unification progresses in a piecemeal fashion, distributed among a number of publications in a research tradition.

1 From a research program to a research tradition

EC has been characterized by Shapiro as a research program. To justify his terminological choice, he contends that "the point of labelling EC a research programme, rather than a theory, is to indicate that the commitments and subject matters of EC remain fairly nebulous" (Shapiro 2007, p. 338). This strikes us as odd.

First of all, the notion of the research program is by no means nebulous in philosophical usage. It was coined by Lakatos (1970) as an alternative to the Kuhnian notion of a paradigm (Kuhn 1962), which, some claim, is extremely ambiguous in Kuhn's work (Masterman 1970). Shapiro neither cites Lakatos in his paper (perhaps because of a strict word limit), nor does he introduce his own understanding of research programs. Thus, we may plausibly suppose that he tacitly assumes the analysis offered by Lakatos.

Most likely, Shapiro supposes that research programs play different roles from theories and should therefore not be evaluated by exactly the same standards as theories. Indeed, the notion of a research program has its advantages when compared to the notion of a theory, especially where historical development is concerned. As Lakatos pointed out, it can be used to compare different programs and to choose the most progressive ones. While component theories in a given program may fail, the grand assumptions may retain their explanatory and heuristic power. This may be of utmost importance in the debate over EC: most of its proponents repeatedly claim that their assumptions are in some sense more appropriate than the mainstream approaches to the study of cognition, although they do not pretend to have a complete theory of cognition (e.g., Barsalou 2008; Goldman 2012; Rizzolatti and Sinigaglia 2008). If this is the right way to interpret Shapiro's claim, it would follow that we might compare research programs without begging methodological questions in cognitive science; we hope to settle at least some of the current hot debates.

For example, Livins and Doumas (2012) criticize EC by claiming that it is not really falsifiable because it can adapt to different outcomes of experimental interventions. But falsification of research programmes, according to Lakatos, is not a question of a single experiment that decides that a specific theory does not hold; instead, one should see whether a change of assumptions of the theory make it possible to predict and explain new facts. Research programmes are less easily falsifiable than specific theories but their progress (or lack thereof) is crucial for determining whether they are degenerate or not.

Unfortunately, there are reasons to believe that impartial evaluation of research programs becomes markedly difficult if we adopt Lakatos's framework. Notwithstanding its lasting influence on scientific practice, including cognitive science (cf. Cooper 2006), it imposes ill-justified burdens. Lakatos is indebted to Popper in his reliance on empirical novelty as the ultimate proof of scientific progress. But progress need not come in the form of new scientific evidence; for example, Copernicus had no better empirical evidence than his competitors. His account of astronomy was not simply more mathematically parsimonious than the Ptolemean model (Kuhn 1957). Copernicus's advantage was largely theoretical, as Laudan (1977) has argued. Laudan also stresses that not all science focuses exclusively on attaining new empirical predictions. According to Lakatos, however, some branches of science—such as mathematics or theoretical modelling—could never be evaluated as progressive, even if they had their own research programs and successes. They would be degenerate by definition as long they could not contribute to new empirical predictions.[1] However, the intention behind the idea of developing the account of progressive programs was to claim that degeneration arises from adding auxiliary hypotheses ad libitum just to protect the essential claims of a theory (cf. Lakatos 1970, p. 117), instead of developing it further.

[1] Of course, Lakatos, with his stress on quasi-empirical nature of mathematics (1976), could reply that the ultimate proof of progress, even in mathematics, is empirical. This reply, plausible as it may sound, implies that formalist approaches in mathematics are degenerate by definition. Even if one is highly critical of Bourbaki, for example, one should not terminologically decide that their approach can never be progressive.

Laudan proposed a fruitful alternative that we adopt here. Instead of presupposing that science is merely concerned with generating novel empirical predictions, he argues that science should solve problems, theoretical or empirical. In other words, if EC is progressive, it should solve its empirical or theoretical problems. As we stress below in Sect. 4, the evaluation depends on what one takes to be *the* problem of EC. In particular, if it is a grand unifying theory of cognition, it should help solve all problems of cognitive science, and the failure to do so is detrimental. If one adopts a view that EC complements traditional cognitive science, then it should be progressive in solving more and more complementary problems such as mapping information flow in sensorimotor networks, critical for adaptivity, robustness, and learning (Lungarella and Sporns 2006).

While proposing that new approach to evaluating research programs, Laudan also reinterprets the very notion of the research program; to avoid any confusion, he refers to his version as a *research tradition.* Research traditions are characterized by three features: (a) "every research tradition has a number of specific theories which exemplify and partially constitute it" (Laudan 1977, p. 78); (b) they exhibit "certain metaphysical and methodological commitments which, as an ensemble, individuate the research tradition and distinguish it from others" (ibid.); (c) lastly, traditions go through a number of formulations and usually have a long history (ibid., p. 79).

Thus, research traditions in Laudan's sense are more "nebulous", at least in the sense that their identity is no longer the matter of adhering to their essential claims. All that is required for a theory or model to belong to a certain tradition is for it to bear a certain resemblance to the prototype model, theory or theories that have initiated the tradition and, arguably, some historical continuity in terms of problems assumed to be crucial for the tradition to solve.[2] Thus, research traditions are not treated in an essentialist manner, but rather like growing and varied populations of constituent theories or frameworks—or an evolving, dynamic network of theories or approaches. Moreover, traditions are generally stated in a more abstract fashion than specific theories. In this sense, one could see evolutionary theory (which is, according to Laudan, actually a research tradition and not a theory per se) as more "nebulous": it is less easily testable and much more abstract than any specific explanatory model or text in the life sciences.

In the next section, we will analyse EC as a research tradition.

2 EC as a research tradition in cognitive science

There is no shortage of attempts to characterize EC in an essentialist manner as a series of its essential claims (e.g., Goldinger et al. 2016; M. Wilson 2002). For example, Chiel and Beer summarize EC in a single sentence: "body structure creates constraints and

[2] We understand research traditions to be similar, in this respect, to fields of science, characterized by Darden and Maull (1977) as consisting of four elements: "a central problem, a domain consisting of items taken to be facts related to that problem, general explanatory factors and goals providing expectations as to how the problem is to be solved, techniques and methods, and, sometimes, but not always, concepts, laws and theories which are related to the problem and which attempt to realize the explanatory goals" (p. 43). The difference is that in a given field, multiple traditions may appear with different metaphysical and methodological commitments, which need not be shared by all researchers in that field.

opportunities for neural control" (Chiel and Beer 1997, p. 553). A similar but slightly more complex account is offered by Wilson and Foglia (2017): "The body can function as a *constraint* on cognition, as a *distributor* for cognitive processing, or as a *regulator* of cognitive activity."

While such attempts to *define* EC may serve a didactic purpose, they simplify or even distort the historical dynamics of the tradition. Three distinct functions listed by Wilson and Foglia were not considered by early theorists of embodiment, such as Lakoff and Johnson (1980), and with time, more functions can appear—their list is not logically exhaustive. Therefore, we do not treat these characteristics as defining EC. They only hint at possible theories that are part of EC as a genuinely grand research tradition.

As we indicated above, instead of defining traditions in terms of a single claim or even a single theory, Laudan's approach is to demonstrate that the research tradition is exemplified and constituted by a number of constituent specific theories. In other words, research traditions need not be simple or intrinsically unified; they may be complex and intrinsically varied. A single tradition could, we submit, subsume a number of smaller traditions, which we call *subtraditions* for brevity. Constituent subtraditions are, in turn, exemplified and constituted by detailed models and theories. In the case of EC, these subtraditions include neo-empiricism (Barsalou 1999, 2008), ecological psychology (Gibson 1986; Wilson and Golonka 2013), embodied cognitive linguistics (Lakoff and Johnson 1980), research on emotion and interoception (Herbert and Pollatos 2012), and sensorimotor approaches to cognition (O'Regan and Noë 2001; Thelen et al. 2001; Chemero 2009), including the vast and varied research on mirror neurons (Arbib 2012; Gallese et al. 2004), morphological computation in robotics (Pfeifer and Bongard 2007), and the skin–brain thesis (Keijzer et al. 2013). One may also include enactivism with its various flavors (Stewart et al. 2010).[3]

An overview of how we see these different approaches as related is offered in Fig. 1.

The identity of research traditions is not decided *only* by their methodological and metaphysical commitments, though these remain the focus of most attempts to define research approaches such as EC. According to Laudan, these commitments are, however, also important for singling out a research tradition among other traditions. Indeed, the stress on the role of bodily structures in the control of behavior distinguishes EC from classical symbolic cognitive science.

In short, we claim that EC should be understood as composed of multiple, and sometimes quite extensive, component research subtraditions.[4] No wonder, then, EC is not as uniform as a single theory. Some of the constituent EC subtraditions are in direct competition and assume mutually contradictory theoretical commitments. For example, neo-empiricism is usually committed to (so-called *grounded*) symbolic representations, while radical sensorimotor approaches tend to eschew symbolic representationalism. Some reduce the bodily aspect of cognition to body representations

[3] The term *enactivism* has already become ambiguous (Ward et al. 2017).

[4] Here, we abstract away from the question of how EC is related to previous approaches to cognition such as cybernetics or phenomenology. We suspect that a deeper inquiry could reveal overlap (perhaps even substantial overlap) between embodied and non-embodied approaches to cognition. For our purposes, however, we need not assume that EC is wholly distinct from other approaches in cognitive science.

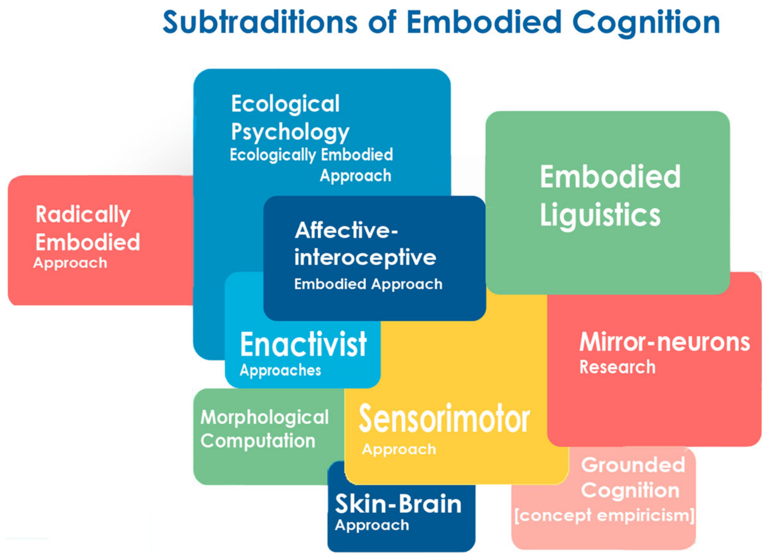

Fig. 1 Subtraditions of embodied cognition. Spatial overlap represents the partial overlap of traditions; however, the size of each subtradition is arbitrarily adjusted to make overlaps possible. Drawing © authors of the paper

(Goldman 2012), and others focus on the physical body (Pfeifer and Bongard 2007). Moreover, boundaries between subtraditions of EC are difficult to draw, as they are intrinsically varied. For example, computational methodology and embodied representations do not fall within the strict boundaries of single subtraditions: Lungarella and Sporns, along with Pfeiffer and Bongard, belong to a different subtradition than Pezzulo et al. (2011). The latter embrace theoretical commitments of so-called grounded cognition (Barsalou 2008) in contrast to the former. Any attempt to define EC in terms of metaphysical and methodological commitments shared by all its proponents will necessarily abstract away from all this complexity, which will in turn make such a definition uninformative about the real richness and diversity of EC subtraditions.

The core metaphysical and methodological commitments of EC are exemplified by its particular subtraditions. A particularly influential one, owing to the large number of empirical results in experimental psychology it has inspired, is embodied cognitive linguistics (henceforth ECL), founded mostly by George Lakoff and his collaborators (Lakoff and Johnson 1980).

Lakoff (2012) points out problems that prompted him, as a generative linguist, in the early 1960s (long before any embodied approach was proposed), to adopt a more embodied way of thinking. He discovered that he could not solve relevant problems using methods and concepts of the then-dominant generative linguistics. Two example sentences of import are: "Yastrzemski doubled to left" and "If I were you, I'd hate me." Both, for different reasons, challenge the tradition in which Lakoff was trained. Understanding one requires bodily understanding of the movement of the ball (e.g., the meaning of the term "double" and "left" in the first case, taken from baseball), and understanding the other requires attributing the location of consciousness (to realize who is actually hated by whom). As Lakoff claims, such examples were quite common and formative for future development of embodied research in cognitive linguistics. Moreover, they were difficult to analyze in the traditional terms of Chomsky's linguistics, where the primary stress lay on syntax, while meaning was usually held to be arbitrarily attributed to symbols, or syntactic entities. However, problems or anomalies alone are not enough for a tradition to thrive. An alternative framework must be proposed.

In Lakoff's opinion, the alternative framework was already present in four studies, offered as lectures in Berkeley in the mid-1970s, which made possible the further development of EC. The first is Kay and McDaniel's (1978) work on the role of physiology and morphology of the visual system in color vision. The second is Eleanor Rosch's work on prototypes—basic-level categories dependent on embodiment, mainly on the gestalt, perception, mental imagery and motor programs (Rosch et al. 1976). The third is cognitive primitives (cogs, or "image schemas") influenced by the body, proposed independently but in parallel by Talmy (1983) and Langacker (1987). Finally, the fourth is Fillmore's (1977) account of semantic roles as dependent on embodied notions such as agents, sources, goals, etc. Taken together, these studies supplied new metaphysical and methodological commitments for the emergence of ECL and research on embodied conceptual metaphors.

ECL rejects the assumption of amodal symbols with arbitrarily assigned meaning. Instead, it relies on cogs and their relationship to the body, combined with embodied experience and conceptual metaphors. Thus, meaning is mediated by the bodily structures of a human being. The focus is on embodied experiences in the formation, development, and cross-cultural character of primary metaphors, emotion metaphors, and metonymies, as well as on relations between conceptual metaphors and gestural metaphors, etc. (Cienki 2005; Duncan et al. 2007; Kövecses 1986; Lakoff and Johnson 1999; McNeill 1992). Lakoff (2012, pp. 776–778) enumerated more than twenty important results of ECL.

While ECL remains heavily opposed to disembodied work in linguistics and artificial intelligence, over the years, it has also developed an original approach toward the computational modeling of cognition. This is evident, for example, in Lakoff's general framework, Neural Theory of Thought and Language (NTTL), which moves beyond linguistics (Lakoff 2012, pp. 778–781). This line of ECL research is still very much under construction. Although it is closely related to other prominent domains of EC, such as mirror neurons or sensorimotor integration, it has not developed beyond the narrow circle of Lakoff's collaborators. Still, such historical development could be difficult to predict, given the mostly conceptual arguments and small case studies in his early work with Johnson (Lakoff and Johnson 1980). This, again, illustrates that research traditions usually change over time, as Laudan stresses in his account.

ECL is not the only subtradition of EC of interest to us. One can also easily point to work on affordances, mirror neurons, or interoception and emotion (see again Fig. 1). Let us briefly focus on the subtradition spawned by affordance research. The initial formulation of the concept was proposed by Gibson (1986) in his work to define the field of ecological psychology. Subsequently, reformulations were proposed, some forced by ontological considerations (Turvey 1992), and some inspired by a change in the domain of application (Gaver 1991; Norman 1999). But at that time, this concept and related studies were only recognized rather locally, by ecological psychologists, and by few cognitive researchers.

The idea re-emerged in recent studies on (radically) embodied cognition (Chemero 2009) and seems to flourish. This led to studies of complex affordances (Rietveld and Kiverstein 2014), social and canonical affordances (Costall 1995, 2012), and normative affordances (Heras-Escribano and de Pinedo 2016). The concept was also applied to research in human–computer interaction (Kaptelinin and Nardi 2012), robotics (Chemero and Turvey 2007), and neuroscience (Cisek 2007).

A closer analysis shows that only a few researchers developed their own approaches to affordances. The focus is usually on the relationship between body and environment, possibilities for action in such environments, and the role of this particular relationship in perception and cognition in general. The methodological and metaphysical commitments of this subtradition usually take inspiration from Gibson's direct realism, which was heavily criticized by defenders of classical cognitive psychology (Fodor and Pylyshyn 1981). Thus, again, this subtradition is, on the one hand, based on a specific theoretical framework, and, on the other hand, heavily opposed to the view that cognition is the processing of amodal symbols.

Members of the affordance subtradition often share the same or significantly similar metaphysical commitments regarding the role of the body and its action and morphology in cognition or perception but assume relatively independent ways of conceptualizing and solving problems, which means that their commitments diverge. Some follow in the steps of Gibson by developing ecological psychology with its rejection of mental representation, which can be also extended by dynamical explanations (Wilson and Golonka 2013). Others, especially neuroscientists investigating brain mechanisms involved in controlling the interactions between an organism and its environment, focus on "representations of affordances" as realized by a specific activity of certain brain systems (Cisek 2007). A similarly representational approach is assumed in design research (Gaver 1991; Norman 1999), which examines the properties of objects and how these properties are used, perceived, and represented.

As we stress the differences between various subtraditions of EC, one could object that EC is not really a coherent approach to cognition. Thus, before EC could unify cognitive science, shouldn't it be unified itself? Our reply to this line of argument is relatively simple: as we demonstrate below, there could be some unificatory power in views that are not fully unified. In other words, unification is a graded property.

3 A new perspective on unification in EC

In spite of its heterogeneity, some consider EC a unifying perspective for virtually all cognitive science (Glenberg 2010; Schubert and Semin 2009). Psychology and cognitive science are now obviously disunified. Still, defenders of the idea of unity argue that these fields should become, eventually, somehow unified.

How, then, should EC unify research? Glenberg claims that EC can unify research on social and cognitive development; memory; social psychology and emotions; motor resonance, theory of mind, autism; psychopathology and clinical psychology; and applied clinical psychology. It is an impressive list, even if some fields of psychology are missing (the obvious being perception and decision-making). Of course, he does not deny that EC has not unified these research fields *yet*. Thus, we need a way to evaluate the plausibility of his claim.

To do this, we assume the new mechanistic approach to explanation, which will make our task at the same time easy and rather novel, as unity is rarely seen as a mechanistic virtue (Craver 2007; but see Miłkowski 2016a). According to the new mechanistic approach, to explain phenomenon *P* constitutively is to describe a mechanism responsible for *P* (Craver 2007; Glennan 2017; Machamer et al. 2000). If appropriately orchestrated, the constituents of the mechanism, or its parts or entities, perform certain operations or activities and interact causally, leading to the fact that *P* is produced, maintained, or otherwise constituted.

Mechanistic explanatory texts may be produced on a case-by-case basis, but one of the reasons research traditions endure is that they offer researchers exemplar explanations upon which to base their own proposals. By *exemplar explanations* we mean explanations that are considered particularly successful and which serve as patterns to be followed (for instance, in computational cognitive science, Marr's (1982) explanations of edge detection are exemplars). If these exemplar explanations are appropriately constrained by theoretical and/or methodological commitments, then the derived explanations will resemble the original explanation. In this way, a potentially huge part of the space of possible explanations will be left unexplored. But at the same time, researchers may save substantial effort by extrapolating the same principle to new phenomena. Intuitively, adherence to some theoretical and/or methodological commitments of a tradition will contribute to a greater theoretical unity of the explanations produced in this tradition.

This intuitive claim requires further elucidation. However, it is far from clear whether one can offer a general analysis of the notion of unity in science (Hacking 1996). Margaret Morrison (2011) even claims that a unified theory of unity is unattainable. To make matters worse, unification, in particular in the context of mechanistic explanations, is frequently conflated with integration: for example, Craver (2007) talks of "mosaic unity", when he means patchwork integration of multiple models. This conflation is quite common (e.g., Grantham 2004; Fagan 2017).

Let us first define and distinguish integration from unification, and then characterize the latter. Integration has been understood by Craver (2007) in terms of constraints on the space of possible mechanisms: the more mechanisms constrain one other in different respects, the smaller the space of possible mechanisms we must explore to find the real causes of phenomena. A version of this approach, expressed in terms of constraints on plausible models of mechanisms, and not just mechanisms, is defended by Miłkowski (2016b). In his account, to integrate a set of models of mechanisms, one should find the most coherent model that preserves all constraints from all constituent models (Thagard 2000). We could, for example, integrate results from ECL with results from sensorimotor research (Pezzulo 2011); this is the usual approach in EC.

The integrated mechanism model may also be extremely complex, just like a Rube Goldberg machine. There is no warranty it will generalize to other similar phenomena or that it will be simple. In other words, even maximally integrated scientific representations can be—intuitively speaking—disunified.

What is unity, then? We submit that unification is a multifaceted achievement, with multiple dimensions that depend not only on its target (reality, method, set of laws, language, theory) but also on the ways such unity may be achieved. Here, we are interested in the unity of an explanatory text, understood as one that explains its phenomena mechanistically, and we follow Boone and Piccinini (2016) in claiming that this kind of explanation is suitable for cognitive (neuro)science. We call this kind of unity *representational*, as its target is an explanatory representation of a mechanism. For representational unity, however, the unity of language is not required (mechanisms may be explained verbally, using diagrams, equations, etc.).[5]

Arguably, at least four dimensions of representational unity can be singled out: (1) simplicity and non-redundancy; (b) generality and scope; (c) organic fertility, or non-monstrosity; and (d) systematization. Our account draws on Miłkowski (2016a), while adding one important dimension, which was missing in our previous account, viz. systematization. These dimensions can be seen as virtues of explanatory texts in general.

Dimensions of representational unity cannot be straightforwardly applied to research traditions. It is scientific representations, not traditions, that are supposed to be unified. In cognitive science, these representations are usually stated as particular computational models in terms of flowcharts, boxes-and-arrows, or mathematical descriptions (Cooper and Guest 2014), but they may be also specified verbally, or as complex diagrams of the mechanisms involved. These representations tend to be of limited scope; some of them explain a single phenomenon, which, in extreme cases, is the behavior of a single subject in a single task over a single session. Such representations are sometimes called *microtheories* (Newell and Simon 1972). The problem is that when a field uses microtheories to explain phenomena, there is a danger of appealing to divergent theoretical principles in explaining even related classes of phenomena, which may lead to eclectic ad hoc models (Newell 1973). The framework of mechanistic explanation does not, per se, view such eclecticism as detrimental to the quality of the explanations at hand. Nonetheless, such fragmentation may be a symptom of the lack of a systematic body of explanatory principles governing a class of phenomena.

To avoid the charge that similar phenomena are explained ad hoc using divergent theoretical principles, a cognitive scientist may either increase the scope of the explanatory representation in question, or produce individual representations using the same or a similar set of theoretical principles, following exemplars from a research tradition. The first solution was defended forcefully by Newell (1990), who argued that a single model—depicting a cognitive architecture—should explain a vast range of cognitive phenomena. The alternative solution, which is widespread in the case of EC, is distributed unification, which follows basic metaphysical and methodological commitments of EC as a research tradition.

In practical terms, particular models are usually produced under an umbrella of a specific subtradition of EC. It is hoped that a number of explanations, distributed over a

[5] Use of the same vocabulary may not imply representational unity; a theory expressed in the same language could be disunified if it is not organically fertile (e.g., when it contains a mere logical conjunction of two confirmationally independent propositions). See later in the text for a more detailed elucidation of organic fertility.

series of publications, will offer, nonetheless, a unified picture of phenomena because they are constrained by the same theoretical principles of EC. In the case of distributed unification, the properties of unified representations should be therefore applied to a whole distributed corpus of explanations offered currently in a given tradition for a number of related phenomena. This corpus is kept theoretically unified in particular by following influential exemplar explanations of a given subtradition.

Thus, our notion of representational unification may be applied both to a monolithic specification of a mechanism in a single publication (usually a book) and to a mosaic model of a mechanism, whose description is distributed over a number of publications. This notion of distributed unification is indebted to an observation made by Hochstein (2016), who noted that mechanistic explanations usually span a number of separate publications. In our view, this idea of distribution is equally applicable to representational unification.

Let us now turn to unification. First, a representation may be said to be unified when it is in some sense simple and non-redundant. Explanatory texts and models with needlessly complex and repeated parts are disunified, while they could be perfectly integrated in the mechanistic sense. For example, one could integrate two explanations of how the meaning of a complex expression depends on meanings of its constituents. One could rely on the standard notion of compositionality, and another on Lakoff's (1987) alternative, the notion of *motivation*, which was supposed to replace compositionality altogether, by claiming that one applies to, say, abstract concepts, and another to concrete ones. Needless to say, such an integrated explanation is not unified, as it mechanically connects two disconnected positions.

Technically, one could define simplicity in statistical or information-theoretic terms [e.g., Akaike information criterion (Forster and Sober 1994) or Salomonoff–Kolmogorov complexity (Li and Vitanyi 1993)]. EC explanatory texts, if they repeatedly appeal to the same explanatory mechanisms—such as mirror neurons—in multiple explanations, are in this sense unifying: they offer general explanatory patterns, which, if true, render the explanatory text simpler, as far as these mechanisms do not involve significant additional overhead. For example, one could claim that a needlessly complicated representation, even if applied to multiple phenomena, is still not really unified if it requires introducing additional linking or intervening mechanisms.

Second, the generality of an explanatory text, or its explanatory scope, is often taken to be *the* feature of a unified explanation (Kitcher 1989). Alternatively, one could analyze this feature of explanatory texts as the level of invariance of regularities or causal structures in mechanisms. Again, take mirror neurons: the more phenomena that could be explained by recourse to the purported embodied mechanisms of action simulation, the bigger the explanatory scope of EC. As we will see in Sect. 4, this is the feature of unification that critics think EC lacks: the charges mounted against EC by Goldinger et al. (2016) can be summarized as pointing to a possible lack of invariant generalizations true of *all* cognitive phenomena. However, at the same time, as long as sensorimotor mechanisms (whatever we take them to be) explain a large range of action-perception phenomena, they are unificatory in this regard.

Third, unified representations are "organic wholes": they are characterized by *organic fertility*, as Watkins (1984) called it. More recently, this feature of scientific representations has been analyzed in terms of non-monstrosity by Votsis (2015). To simplify, a representation is monstrous only when it is a merely superficial collection of disjoined parts, for example, a conjunction of logically and statistically independent claims. Intuitively, if there are parts of a representation that cannot be disconfirmed by the same proposition, the representation is monstrous.[6] Now, as long EC is merely a diverse collection of hypotheses about the role of the body that remain disconnected, it could be monstrous (as our Fig. 1 could suggest). However, in spite of methodological and theoretical differences, there may be a series of unifying principles common to at least some subtraditions of EC, which would be falsified if, for example, it turns out that mirror neurons have an extremely limited (if any) influence on reasoning or language—as Hickok (2014) suggests.

Fourth, unified representations are not just simple, organically fertile representations of grand ambition and scope. The inference patterns in explanations, which are, according to Kitcher (1989), the necessary tools for unification, cannot constitute a mere collection of arbitrary axioms, even if they are confirmationally connected, or non-monstrous, and non-redundant. They should systematically elucidate reality (Bartelborth 2002). The idea that systematicity is the defining feature of science has been recently proposed by Hoyningen-Huene (2013) but it was already one of the requirements in the account of reduction defended by Kemeny and Oppenheim (1956). Kemeny and Oppenheim insisted that the reducing theory should be as well (or better) systematized as the theory reduced. As they say, “Without the requirement of systematization we cannot even understand the need for theories” (ibid., p. 11).

We consider systematization a necessary feature of any scientific discourse. Hoyningnen-Huene stresses that manifestations of systematicity depend on what kind of scientific discourse is meant.[7] In our case it is mechanistic explanations in cognitive (neuro)science (including microtheories). The claim we defend, therefore, is that unified representations should offer systematic mechanistic explanations and descriptions of their phenomena, striving at completeness in terms of particular explanations and of the whole range of phenomena. Moreover, they should aim at systematization just because they are theoretical representations but also because, as unified, they aim to explain the whole range of phenomena in a uniform way. Even if there are several kinds of phenomena to be explained, there could be underlying principles of taxonomy or periodization of the phenomena at hand that show that they are systematically related. Thus, because they look at connections between phenomena, unified

[6] To spell this idea out in a satisfactory manner, Votsis had to use much more logical artillery, whose introduction would require too much space. For full details of his account, see Votsis (2015). The disconfirmatory proposition need not be empirical, so this account should also be applicable to the non-monstrosity of formal sciences.

[7] His account of systematicity is also multi-dimensional; instead of offering a cluster of properties that are jointly paradigmatic for systematic theories, he is concerned with different dimensions of science itself. These dimensions are descriptions, explanations, predictions, the defense of knowledge claims, critical discourse, epistemic connectedness, an ideal of completeness, knowledge generation, and the representation of knowledge. As Hoyningnen-Huene argues, systematicity of descriptions is not the same property as the systematicity of the defense of knowledge claims. The first is related to axiomatization, creation of taxonomies, periodization, quantification, and empirical generalizations, and the latter with systematic attempts to reduce error in science, which is related to how knowledge is defended, including non-evidential considerations, empirical generalizations, causal influence, etc.

theoretical representations are systematic. Otherwise, they become fragmented and explanatory pluralism is forced.

One intuitive way researchers in cognitive science try to achieve systematic explanations is to appeal to a multilevel, or stratified structure of cognitive processes. In this regard, the three-level account of computational explanation in cognitive science defended by Marr (1982) is a prime example of stratifying cognitive reality with the goal of providing complete computational explanations (completeness is one of the features of systematicity cited by Hoyningen-Huene). EC is usually presented, as is most cognitive research, as offering multilevel explanations in a Marrian fashion. Interestingly, Hoyningen-Huene subsumes axiomatization, periodization, creation of taxonomies, and the like, under the dimension of description. Nonetheless, researchers devise methodological precepts aimed at achieving completeness of explanations, and not just descriptions, by systematically covering explanatorily relevant factors.

The purpose of Marrian levels is exactly to provide complete explanations by covering essential factors. Marr's levels are defined by general research questions rather than by a strict organizational hierarchy with a clearly defined ordering relationship. For example, at the computational level, one is preoccupied with the following questions: "what is the goal of computation?", "why is it appropriate?", and "what is the logic of the strategy by which it can be carried out?". The subsequent algorithmic level deals with the questions "how can this computational theory be implemented?", "what is the representation for the input and output?", and "what is the algorithm for the transformation?". Without further elucidating Marr's framework, we want to stress that it holds the promise of systematic inquiry into complex information-processing systems. By answering the series of questions, one should arrive at a complete explanation of how a complex information-processing system works. One of the enduring attractions of Marrian methodology in cognitive science is that it specifies clearly what research questions should be asked, in what sequence, and what the end result should look like. However, *contra* Hoyningen-Huene, these systematic questions are not just cases of systematic description but of systematic explanation. In other words, Marr's stratification of complex systems is aimed at producing unified explanations in computational cognitive science.

Thus, as long EC produces mechanistic explanatory representations that are simple, general, non-monstrous, and systematic, it can contribute to representational unification in cognitive science, in spite of the diversity of methodological and theoretical commitments of EC. Essentially, it can still contribute to representational unification by elucidating a range of cognitive phenomena by recourse to the same mechanisms (or at least the same kinds of mechanism), which results in simpler and more general explanations. Researchers aim to provide non-monstrous and systematic explanations, which tend to be more consistent if constrained by particular subtraditions of EC. Thus, even though EC is diversified and does not offer a single unified view of embodiment, its explanatory texts exhibit a certain pattern, which is evidence that they are not entirely ad hoc and eclectic, at least not in the way described and criticized by Newell (1973) in his complaints against fragmentation of cognitive psychology.

Notably, only some of defenders of EC, like Glenberg, seem to suggest that EC may offer complete explanations at least for some domains in psychology and that the body serves as their unifying principle. But the body is conceptualized in different ways depending on the subtradition and does not always play the same role. Hence, the only thing that the reference to bodily factors in mechanistic explanations can do, in principle, is to constrain the models of mechanisms by requiring that they cite these factors. This is, however, insufficient for a systematic unity of explanatory models over all subtraditions of EC because these factors are themselves poorly understood, and different subtraditions cite their favorite bodily factors, which may remain unrelated to those favored by other EC subtraditions. For instance, Pfeifer and Bongard (2007) consider body morphology *the* crucial factor shaping our cognitive processes, while for Goldman (2012), body morphology is irrelevant. Both approaches also have opposing attitudes toward the role of interception in cognition. Hence, while EC works against extreme disunity and fragmentation, its diversity still contributes to divergent explanatory practices and, consequently, divergent explanatory models of cognitive phenomena. This diversity and intellectual rivalry inside research traditions need not be considered detrimental for unity (van Strien 1987).

This leads to another question. Even if EC traditions may contribute to some representational unity in cognitive science, it is still questionable whether it may eventually unify all of cognitive science. How, then, should one evaluate the success of EC in the context of representational unification?

4 Evaluating evolving research traditions in light of unification: lessons from embodied cognition

EC is a research tradition. While citing evidential support is sufficient for endorsing one theory over another or for rejecting it as flawed, empirical evidence may not be enough to justify endorsing or rejecting a research tradition. In their criticism of EC, Goldinger et al. (2016) discuss nine phenomena for which EC offers no predictions nor explanations, such as the word frequency effect. The word frequency effect comprises the fact that the more frequently used the word, the faster and more robust its lexical access becomes. Goldinger et al. deny that this phenomenon could ever be explained by EC. However, other research traditions in cognitive science fare no better. Computationalism as such offers no predictions, for it merely affords computational explanations in cognitive science but has nothing to say about the word frequency effect. This is because computationalism is not a single theory but a varied research tradition that is methodologically committed to computational modelling and ontologically committed to claims about physical computation in cognitive systems (cf. Miłkowski 2018). But empirically valid computational models of the word frequency effect could eventually be built. Similarly, one could argue that it is possible to build an embodied explanation of the phenomenon (we will return to this issue shortly).

Moreover, no matter if a research tradition is unificatory or not, it may be very difficult to disconfirm it empirically, in particular when their metaphysical commitments are extremely abstract.[8] Indeed, Laudan stressed that research traditions, in

[8] Of course, it can turn out that one of the metaphysical commitments of a research tradition is wrong: for example, a huge research tradition in physics assumed the existence of luminiferous aether, and the Michelson-Morley experiment provided evidence that the concept could be discarded from physics.

contrast to specific theories, are a "much more general, much less easily testable, set of doctrines or assumptions" (Laudan 1977, p. 71). For example, it is certainly not a trivial task to experimentally disprove the metaphysical commitments of computationalism or Bayesian cognitive science. So how should one evaluate EC as a research tradition?

The increase or decrease of the problem-solving effectiveness is the measure of the progressiveness of a research tradition.[9] On Laudan's account, one can focus on the general progress "by comparing the adequacy of the sets of theories which constitute the oldest and those which constitute the most recent versions of the research tradition" (Laudan 1977, p. 107), and on the momentary rate of progress during any specified time span. The momentary progress may be stalled owing to a lack of appropriate measurement apparatus, for example.

Let us then consider how this could be applied to the problem at hand: the explanation of the word frequency effect. The momentary rate of progress of EC for this problem is simply zero. Now, the question is whether EC could make progress on this problem in the future—and whether it should. A proponent of EC explanations for a limited scope of problems, for example, those related to sensorimotor processing, may reject the diagnosis offered by Goldinger et al. (2016), simply by saying that EC is not committed to solving all problems in cognitive science (e.g., Wołoszyn and Hohol 2017 offer such a reply). However, Glenberg or Schubert and Semin have no such option because they claim that EC does unify (almost) all cognitive science.

Thus, what Goldinger et al. (2016) suggest is simply this: EC has no methodological or metaphysical commitments that could help in offering an alternative explanation of the word frequency effect. Just because the word frequency effect is a classical explanandum in cognitive science, any respectable attempt at unifying all cognitive science should include it. The standard models of the effect usually refer to learning dynamics (often simulated using artificial neural networks implementing learning rules that are considered explanatory for the phenomenon). For example, Goldinger et al. refer to Hebb's learning rule "neurons that fire together, wire together" (2016, p. 965). Still, learning is not on the list of phenomena listed by Glenberg (2010). However, he lists memory, and there are several phenomena cited by Goldinger et al. (2016) that are directly related to memory (serial recall, implicit memory, working memory capacity) that seem to have no embodied explanation—and it is unclear how one could be built. In other words, it is unlikely that EC can scale up to cover the phenomena listed by Goldinger et al., including memory, listed as already covered by Glenberg [for his approach to memory, see (Glenberg 1997)].

In other words, the assessment of EC as progressive with respect to such problems depends on how one understands the proper problem domain of EC. As Goldinger et al. say, "EC proponents selectively focus on a subset of domains that work, while

[9] In his recent defense of an alternative account, Bird (2007) criticized Laudan's approach for eschewing the issue of truth or adequacy in scientific progress [for a short overview of current accounts of scientific progress, see (Dellsén 2018)]. Indeed, Laudan (1981, p. 145) claimed that "we evidently have no way of ascertaining whether our theories are more truthlike or more nearly certain than they formerly were." According to Bird, this leads Laudan to conflate an actual solution of a problem with something that is merely assessed to be a solution. However, Laudan's account need not be understood this way: a scientific realist may, in spite of Laudan's comments, simply assume that only actual solutions to scientific problems may contribute to progress.

ignoring nearly all the bedrock findings that define cognitive science" (2016, p. 961). However, only some proponents of EC, such as Glenberg, would readily accept the challenge to explain all the bedrock findings that define cognitive science. Note that one can apply, again, Laudan's criteria of progress to underline that there actually are "domains that work"; ECL has inspired a flurry of research in cognitive linguistics and experimental psychology as well (Lakoff 2012): Experimental studies show that being leaned forward can underwrite desires (Harmon-Jones et al. 2011, 2012), holding a hot cup makes the person we talk to more warm and trustworthy (Williams and Bargh 2008), experienced temperature influences the emotional tone of memories (Zhong and Leonardelli 2008), etc.

ECL, even if diversified, remains committed to a set of research heuristics that point out sensorimotor factors as possibly explanatory for phenomena such as metaphors (Miłkowski 2019). Understood as offering a narrower set of explanations, EC remains unified to a degree: while it does not explain all the findings of cognitive science, it offers a specific perspective on some of its phenomena. Usually, this is achieved by extrapolation of the same or similar mechanisms to explain multiple phenomena, as in the case of sensorimotor factors used to explain abstract cognition. In general, subtraditions of EC are quite constrained in their theoretical commitments, and thus they would eschew Newell's (1973) charge of increasing fragmentation of cognitive psychology. Representational unification can be achieved locally, for a limited number of problems that a research tradition tackles, without offering a grand theory of everything.

But for Goldinger et al. (2016), the scope and generality of EC thus conceived is insufficient, and there is no systematic way to increase it in the future. EC remains essentially incomplete and impoverished according to this criticism. In other words, the charge against new proposals in cognitive science that they will not "scale up" (e.g., Edelman 2003) is simply meant to say this: the research tradition in question has no resources to offer a unified view on cognition; it has a limited scope and is insufficiently systematic to be developed further. Interestingly, as this is a frequent charge, it strongly suggests that researchers in cognitive science assume that general frameworks for explaining cognition should be at least to some degree unified. Otherwise, proponents of yet another approach to cognition could simply bite the bullet and ignore the criticism.

In our analysis, representations of smaller scope still count as unified as long as they do not form a disunified body of research, i.e., as long as they are relatively simple, appropriately general, non-monstrous, and systematic. Surely, different subtraditions impose more or less strict constraints on what they deem satisfactory explanations. For example, ecological psychology as a subtradition in EC appeals to environmental constraints and to the notion of affordance in most explanations, whereas ECL appeals to bodily underpinnings of metaphors. While these subtraditions are distinct, their explanatory texts remain theoretically uniform, and can be hardly confused with one another. This way, they systematically build a relatively uniform body of explanatory representations in a distributed fashion. In other words, unified explanations of cognition need not rely on a monolithic model of cognitive mechanisms. They can be constrained by a distributed set of methodological and theoretical commitments of a particular research (sub)tradition.

5 Conclusions

In this paper, we pointed out that at least some research traditions in cognitive science aim at offering unified representations of cognitive phenomena. Even if actual theorizing in cognitive science is very fragmented, and cognitive science is itself a grand research tradition that encompasses many competitive and sometimes even mutually exclusive subtraditions, methodological and theoretical debates clearly show that researchers care about unification. We proposed a new multidimensional account of representational unification, which includes factors such as simplicity, generality and scope, non-monstrosity, and systematicity.

At the same time, we stressed that representational unification should not be conflated with representational integration, as they impose different commitments, nor with scope, which is but one of its four dimensions. Whereas more general representations could be more unified, the scope of problems solved by a research tradition need not be arbitrarily high. To be unified, an explanatory mechanistic representation need not be a grand theory of everything. It simply has to be a good explanatory text. An explanatory text can and usually should be selective. There is no principle that requires EC to be a unified theory of *all* cognition.

In the past, the defenders of the mechanistic approach almost exclusively studied representational integration; unification was mentioned mostly in passing. This could be because of the suspicion that unification implies that nature is itself unified; as Newton stated, “Nature is wont to be simple and consonant unto itself.” But new mechanists, predominantly scientific realists, have doubts about whether such a claim could be true. Thus, they could think of unification as a purely epistemic virtue related to our theoretical interests and pragmatic considerations. Hence, even a dappled world could be explained, at least to some extent, in a unified fashion. A successful mechanistic theory of a class of phenomena should cover them all systematically, while remaining simple and non-monstrous. Otherwise, if it fails to be systematic, is overly complex or fairly incomplete, or has totally disconnected parts, it does not constitute a satisfactory explanatory text, but a mere collection of unrelated bits and pieces. Unification is thus not an optional feature of mechanistic *theories* in contrast to mere mechanistic models. If one strives to build a theory that can systematically explain a given domain of phenomena, one must follow the normative guidelines that effectively require striving for representational unification.[10]

We stressed that Laudan’s focus on research traditions has allowed us to better understand the dynamics of cognitive explanations. In the past, new mechanists mostly studied fields and inter-field integration. We consider our perspective complementary and suited for understanding the scientific practice and programmatic debates. In other words, if we focused on fields, we could not really clarify why Goldinger et al. criticize EC: cognitive science is not a well-defined field but a conglomerate of multiple overlapping and partially integrated fields. There is no single problem of cognition, which can be evidenced by the fact that there is no consensus, among different fields, on how to conceptualize cognition (Akagi 2018). But cognitive science is a grand research tradition that has a historically growing number of problems to be solved,

[10] However, the issue of what constitutes a mechanistic theory goes beyond the scope of this paper.

including explaining classical cognitive phenomena, which remain exemplars for current cognitive scientists. This is why talk of research traditions is particularly fruitful for this case. There are also other advantages of our approach related to mechanistic explanation, but they will be developed in a different study.

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