A Physicist’s Road to Emergence

A Revisited Story of “More Is Different”

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Abstract The aim of this paper is, in a nutshell, to rethink the common narrative around one of the most influential papers of the second half of the 20th century: “More is different” (1972) by Philip Anderson. In particular, my aim is to open a new, more realistic way of reading the article, avoiding the two usual simplistic readings which involve, on the one hand, an over-historicisation of the paper which mainly focuses on the socio-ideological battle between solid state and particle physicists starting in the 1970s, and, on the other hand, an under-historicisation focusing mainly in extracting a philosophically coherent view of Anderson’s emergence from the paper. Avoiding these two caricatures of “More is different” as the “political manifesto” or the “philosophical manifesto”, another reading is possible which places Anderson’s research at the confluence of crucial developments in condensed matter physics in the 1960s and offers a possible explanation to the puzzle of why did emergence in physics appeared only in the 1970s, and seemingly by the hand of a single actor?

I will back up my hypothesis by offering an additional context alongside the socio-ideological and philosophical ones: the bibliographical context of Anderson’s research between 1949 and 1972. I will highlight his most important contributions to the field of condensed matter physics connecting them one by one to the examples chosen by Anderson to exemplify his ideas in “More is different’ and how they shaped his understanding of what he will later call emergence.

1 Introduction

The entrance of physicists in the debates over emergence and reduction in the sciences entailed for the first time a true universalization of the concept which had traditionally let physics aside. Originally conceived as a middle ground between the mechanist and the vitalist view on the origin of life from inanimate matter, the British emergentists of the end of the 19th century, beginning of the 20th, rarely contradicted the reductionist character of physics, where the notion of emergence didn’t propagate as it did in biology or philosophy of mind, for instance. It was not until the 1980’s that a physicist used the term “emergence” for the first time in a paper. It was Philip Anderson with his “Broken symmetry, emergent properties, dissipative structures, life: are they related?” published in 1981. However, Anderson’s story with emergence began almost 10 years earlier, more precisely with the publication of his iconic “More is different” in 1972. Although there is no explicit mention to the concept of emergence in there, the essential idea that will later constitute Anderson’s emergence is already present. It is not the aim of this paper to go through the fascinating story of Anderson’s first contact with the notion of emergence and the gradual steps for its appropriation in the early 80s. Rather, I want to focus here on the period before the publication of “More is different” (MID) to provide a realistic

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1In reality, Michael Polanyi was probably the first physico-chemist to use the word in his *Personal Knowledge* (1958), but his book was more philosophically oriented, which didn’t allow his ideas to permeate the physics community as Anderson’s did [Pol58]. See section 2.1.2
account of what were the necessary ingredients for a physicist to write (even if unintentionally) the most iconic manifesto for emergence not only in physics, but in the whole of science and philosophy of science.

First, I will comment on the ingredients traditionally emphasized by the commentators, such as the sociological and philosophical factors, as well as raising doubts on their sufficiency for explaining why emergence didn’t enter into physics until Anderson’s “More is different” (section 2). Secondly, I will put the focus on an aspect usually left aside by the commentators: Anderson’s choice of examples in the paper (section 3). The intention is to individually connect the examples chosen with a facet of his career in condensed matter physics. This exercise will help us better understand the reason behind an apparently chaotic election of phenomena, as well as to provide a tentative answer to the question: why did emergence appear in physics only in the 70s and seemingly by the hand of a single actor?

2 Limitations of the common readings

The appearance of emergence into the field of physics is usually portrayed as: in 1972, the condensed matter physicist Philip Anderson wrote a manifesto for emergence under the title “More is different”. This depiction is incomplete for many reasons, one merely being the fact that Anderson was not aware of that philosophical concept until possibly 1978. If one considers nonetheless that the main characteristic features of Anderson’s later emergence –the one he embraces from the early 80s on– are already present in MID, this anachrony can be comfortably neglected. Of course, there is not a single definition of emergence where Anderson’s views can unequivocally fit into, but we can certainly consider it to fit somewhere into the vast taxonomy of emergence, which explains the rapidity with which readers associated Anderson’s views with the pre-existing discussions on emergence.

Then we are left with the question: if Anderson was really portraying a self-created concept of emergence in his 1972 paper, what factors drove him to that point? In other words, what are the necessary ingredients for a physicist to write the first manifesto for emergence in physics, ruling out the trivial factor of an external influence? With external influence I mean here the direct interaction with the notion of emergence before writing MID. There are many indices that this was in fact not the case, whose discussion is not my main goal here.

Commentators of MID have commonly tend to focus mainly on two ingredients: 1) the sociological ingredient, i.e. the ideological battle between condensed matter and particle physicists for funding and recognition from the 70s to the 90s [Cat98, Ste03], and 2) the philosophical ingredient, i.e. the failure of constructionism and the existence of emergent properties [Mai06, Hum15]. Although these two ingredients are important, they are not sufficient to uniquely determine the reason why emergence didn’t enter into physics until Anderson’s MID, since it could have come by the hand of any other condensed matter physicist (based on 1)) or at any other time (based on 2)), for example in the discussions about the reduction of thermodynamics to statistical mechanics in the 19th century.

In what follows, I will present these two readings and comment on their limitations, not with-
out alerting the reader of an important aspect before. My intention will not be the one of caricaturing these two readings as if the author has a one-dimensional viewpoint of Anderson’s MID. It is important to stress out that each author mentioned hereinafter has indeed a specific intention which motivates their depiction of MID in a certain manner. My goal, rather than to invalidate those depictions, is to point out their limitations to offer a multi-dimensional story of how “More is different” came about, which I believe is lacking in the literature. I will therefore go through the two usual readings—the sociological and the philosophical—superficially to concentrate on the new aspect I want to put forward: the specificity of Anderson’s trajectory as a condensed matter physicist to clarify obscure points about MID such as the choice of examples or the surprising originality of its message.

2.1 The Political Manifesto

A crucial point in the story of the coming about of “More is different” is the tension between solid-state physicists and particle physicists during the 60s to the 90s in the United States: an ideological battle for recognition and funding that culminated in the cancellation of the Superconducting Supercollider in 1993\(^6\). This battle opposed “pure science”, represented by particle physics, to “applied science”, represented in this case by solid-state physics—or condensed matter as it would later be known. The common narrative from the first side was that the only truly fundamental field in physics was the one dealing with unravelling the ultimate components of our universe and their interactions, and that the rest was application of their discoveries, very creative, but application in the end. Naturally, this narrative was not limited to the ideological plane and affected the spreading of government funds, the distribution and number of positions in universities and the practicability of research in general. The prestige granted to nuclear and particle physics was of course a heritage from the Second World-War and persisted through the Cold-War. However some difficulties—such as the anti-war movement of the 60s or the decreasing applicability to social needs—began shifting the perspective on the particle physics utility and relevance for society [Ste03]. It is precisely in this socio-political context where we must situate Anderson’s “More is different”.

Anderson graduated from Harvard in 1949 with a thesis in molecular spectroscopy which opened the doors of Bell Laboratories for him. Bell Labs was an industrial research company with a remarkable investment in theoretical scientific development and all the good solid-state physicists at the time were there\(^7\). They hired Anderson as part of their solid-state research group, where he would develop one of his most characteristic skills that would always drive his research: the importance of creative thinking vs. brute force calculation. That’s why he couldn’t be in more disagreement with the narrative according to which his field of research was, very complex but, mere application of the Schrödinger equation.

A particularly painful instance of disappointment for him were a series of three publications by his particle physics friend Victor Weisskopf in 1965-67 [Wei65, Wei66, Wei68]. The message was clear: it is only as we go to smaller and smaller units that we discover “new laws, new phenomena in the real sense of the word” and the rest (solid-state, biology and so on) is application of those fundamental laws [Wei66, p.468]. This immediately triggered Anderson’s response on the occasion of a public lecture he gave during a one-month position as Regent’s Lecturer in UCSD La Jolla in 1967. This lecture (from which no records seem to have survived) evolved five years later into the famous 1972 article, in which Anderson fiercely replied: “The main fallacy of this kind of thinking is that the reductionist hypothesis does not by any means imply a “constructionist” one: The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe” [And72, p.393], in a

\(^6\)For further reading on this topic, see [RHK15] and [Mar18].

\(^7\)This situation started to be challenged when universities began to realise the importance of solid-state and low-energy physics research after the success of the invention of the transistor precisely at Bell Labs by Bardeen, Shockley and Brattain [Ber84, p.134].
nutshell: *more is different.*

Anderson’s attack to the particle physics establishment at La Jolla in 1967 and the later publication of the talk in 1972 have been taken as a symbol of the socio-political battle against high-energy physics in the 60s and 70s (as well as his opposition to the construction of the SSC before congress, against Steven Weinberg’s testimony, in 1987\(^8\)). It is not unusual then to find citations of “More is different” in historical works that deal with this American scientific episode or in historico-epistemological publications dealing with such concepts as fundamentality or unity in physics. From this vast literature, I have selected two papers to comment in more detail: “Fundamental physics and its justifications, 1945-1993” (2003) by Hallam Stevens [Ste03], and “The physicists’ debates on unification in physics at the end of the 20th century” (1998) by Jordi Cat [Cat98].

**2.1.1 “Fundamental physics and its justifications, 1945-1993”**

Stevens’ paper deals with the story of the success and decline of American high-energy physics in the second half of the 20th century. He underlines the importance of symmetry as the cohesive element that glued together the high-energy physics community after the second World-War, and how it served as justification for considerable public expenditure on particle accelerators. He points at condensed matter physicists as one of the factors responsible for the ending of that status quo [Ste03, p.152-3]:

> condensed matter physicists asserted the importance of broken symmetry in understanding real-word problems; this counter-narrative not only suggested an alternative approach to fundamental physics, but also implied that no single, “grand”, approach could capture the complexity of nature. Under these circumstances the physics community fractured and the cohesive narrative of the 1960s and 1970s began to dissolve.

As it is commonly associated with this story, Anderson and his anti-fundamentalist message are portrayed as the representatives of the whole solid-state physics community, and so the moment comes to cite “More is different” [Ste03, p.167]:

> Solid-state physics had grown even faster than particle physics after World War II. [...] Yet during the 1960s it too experienced financial cutbacks and many solid-state physicists began to resent the large amounts spent on HEP\(^9\). In a famous article that appeared in *Science* in 1972, Philip Anderson challenged the notion that particle physics was more fundamental than other branches of physics.

What a would like to point out is that there is no direct evidence that this was a commonly shared opinion all across the solid-state community in the 60s and 70s. Surely, the communal resentment against the privileges of particle-physics was there, but certainly not the connection of this resentment with the concept of broken symmetry as a tool to justify the narrative that condensed matter physics is autonomous from the developments of the more “fundamental” physics and that a unified theory of everything is an absurdity. This connection, as I will argue in section 3, is Anderson’s original contribution to the story of the political and ideological battle between low-energy and high-energy American physics. What I believe is unfortunate about this simplistic depiction of Anderson as just “expressing what everyone was thinking” is that it doesn’t explain why no other solid-stater raised similar opinions at that time, and it puts under the carpet the interesting epistemological issue of how did broken symmetry become a synonym for dis-unity, complexity and emergence in general. There doesn’t seem to be a trivial connection between those concepts and the uses of broken symmetry by solid-staters in

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\(^8\)https://www.the-scientist.com/opinion-old/the-case-against-the-ssc-63734
\(^9\)High-energy physics.
the 60s and 70s (if they were using it at all!) and I believe it is fair to investigate whether this connection corresponds solely to Anderson rather than to a whole community.

Another controversial point about the article—which I believe derives precisely from the problem I just pointed out—is that Stevens associates symmetry to particle physics and broken symmetry to condensed matter physics, as if it was the discovery of broken symmetries what illuminated them to march against the alleged unity of particle-physics (if one wants to argue this, it should refer to Anderson only, as I already expressed). In a later paragraph he makes clear, after all, that broken symmetry pertains also to the domain of high-energy physics. However, he persists on the idea that the true symmetry disconnect, the spontaneous symmetry breaking (SSB), corresponds only to the condensed matter side [Ste03, p.168]:

Of particular importance to Anderson’s argument was the concept of spontaneous symmetry breaking (SSB), first described in superconductivity research. [...] Unlike the broken or approximate symmetries of high-energy physics, SSB implied a fundamental disconnect between the symmetric and disordered states and gave a concrete example of how the reductive chain of explanation could be broken.

Stevens is right to say that the concept SSB appeared thanks to the study of superconductivity, but he fails to mention that it arose as an analogy to superconductivity gauge symmetry-breaking for particle physics through the work of Nambu and Jona-Lasinio and that it was coined for the first time by particle physicists Baker and Glashow in their article “Spontaneous breakdown of elementary particle symmetries” of 1962. Even Anderson didn’t use the term spontaneous symmetry breaking until the 80s and, even if he says the contrary in “More Is Different”, he is aware that it was thanks to particle physicists that the concept acquired full maturity [And94]: “It is important to realize that particle physics’ borrowing of broken symmetry was repaid by causing condensed matter people to refine and conceptualize their vague notions of broken symmetry, which had been floating around previously and which had been used several times before, as in my antiferromagnetic ground state paper of 1952.” Actually, there were not so many people using this concept in his field before MID a part from him.

The bottom-line of the discussion of this paper is that, although this narrative about Anderson being the false landmark of a whole community might be sufficient for the purposes of Stevens’ argument, it becomes too simplistic if one wants to understand how did Anderson truly arrive to the synthetic message he delivers in “More is different”. The usage of broken symmetry in the solid-state community was not homogenous in the 50s and 60s and it was only after MID that its usage became standardised in a certain manner. It is only then that Anderson became the true landmark of the community.

2.1.2 “The physicists’ debates on unification in physics at the end of the 20th century”

Cat’s article is an historico-epistemological analysis on the concept of unification and its plurality of meanings taken during the physicists’ debates at the second half of the 20th century, mostly in American physics. For this, he goes through the major actors involved in both sides of the debate: the “reductionist camp” represented by nuclear and particle physicists such as Victor Weisskopf, Leon Lederman, Robert Wilson, and theorists Abdus Salam, Sheldon Glashow and Steven Weinberg; and the “emergentist camp” with condensed matter physicists James Krumhansl, Leon Kadanoff, Kenneth Wilson and, of course, Philip Anderson. He emphasizes the role taken by Weinberg and Anderson as leaders of each camp respectively.

Of great importance is Cat’s appreciation of Michael Polanyi’s role as an “early spokesman...”

10 find article
against reductionism” [Cat98, p.257]. The Hungarian solid-state physicist and chemist began writing about emergence and the hierarchical structure of science (with a special emphasis in biology, chemistry and physics) as early as 1958 in his book Personal Knowledge [Pol58], by the end of his career as a scientist. The shift towards philosophy and sociology of science in the book might be a reason for the scientific community to have missed Polanyi’s anti-reductionist discourse, and might explain why Anderson, and not him (who was explicitly talking about emergence), was labeled as the first emergentist physicist.

When it comes to Anderson, Cat offers some historisation of the coming to be of his anti-reductionist thinking: his thesis with solid-state physicist Van Vleck in 1949, the courses he took with Schwinger in Harvard and how they shaped his search for generalisation rather than specialisation, joining Bell Labs and his work on disordered systems, his admiration for Fritz London’s phenomenology and the links he established with John Bardeen, Leon Cooper, Robert Schrieffer and Brian Josephson, specially between 1956 and 1961. Cat situates here the naissance of Anderson’s “opposition to particle physics reductionism”. Next in the story is the already mentioned famous reply to Weisskopf’s fundamentalism by Anderson at La Jolla in 1967 and how it “became a manifesto when published [five] years later under the title “More is different”.”[Cat98, p.263]

After an attempt to bring philosophical clarity to Anderson’s position in the 1972 article –an arduous task for everyone who has come across it (see section 2.2)– Cat concludes that Anderson’s ontological and explanatory reductionism is not clear and that “all that can be inferred safely is that new forms of behaviour cannot be and could not have been specified, suggested, or predicted solely on the basis of fundamental laws and concepts, in the absence of corresponding new concepts and laws.”[Cat98, p.265] After this brief philosophical analysis of “More is different”, we finally arrive to, in my opinion, its pivotal concept: broken symmetry. It is essential to understand (as I will further stress in section 3.3) that Anderson’s central argument to claim the autonomy of his field with respect to particle physics is precisely the concept of broken symmetry [And72, p.393]:

In my own field of many-body physics [...] we have begun to formulate a general theory of just how this shift from quantitative to qualitative differentiation takes place. This formulation, called the theory of “broken symmetry”, may be of help in making more generally clear the breakdown of the constructionist converse of reductionism.

Both concepts, anti-constructionism (or emergence as he will later call it when he adopts this terminology in the 80s) and broken symmetry go hand in hand in Anderson’s thinking, and “More is different” is the clearest exposition of that interconnection since it’s the first time when both concepts appear in full maturity and appear together in the chronology of Anderson’s work12. However, this interdependency of both concepts is rarely stressed out in the literature. Just as in Steven’s case, broken symmetry comes only at a second stage in Cat’s article, as a way to support the allegedly pre-existent anti-constructionist13 philosophy of Anderson [Cat98, p.265]: “Anderson supports and illustrates the argument against constructionism by appeal to the notion of broken symmetry. Asymmetrical states cannot be derived solely from symmetric fundamental laws. They represent “emergent” properties.”

12 A more detailed paper with historical evidence supporting this thesis will be presented as the continuation of the present one.

13 Anti-constructionism is actually the word Anderson uses to express his position, rather than anti-reductionism, often used as a replacement of “against the rationale that accompanies reductionism” (i.e. everything can be reduced to fundamental laws, so everything could in principle be derived from those laws as well) but not against reductionism per se, which Anderson endorsed.
As for the examples used in the paper, Cat focuses only on the first one, the ammonia molecule, before moving onto Anderson’s campaign against particle accelerators. The idea is, in a nutshell, that the asymmetric pyramidal structure of the molecule would violate the no-net-dipole-moment principle, so the stationary state results in a quantum superposition of the two asymmetric states with inverse dipole moment. In Anderson’s opinion, this is an emergent property since one could not have predicted the asymmetric momentary states of the molecule just based on the fundamental principles of quantum mechanics (which assert that the stationary state should be symmetrical) without the hint given by the observable properties of the ammonia inversion from one to the other asymmetrical state. From Cat’s way of paraphrasing Anderson’s reasoning, it is clear that his intention is not to analyse, much less to challenge, Anderson’s claim that the inversion of the ammonia is a physical emergent property, which is far from straightforward (as we will see in section 3.1). Furthermore, Anderson then argues that a bigger molecule is needed in order to truly break the symmetry of the laws, such as in the case of a sugar molecule, which is so big that an inversion between the left and right-handed chirality is impossible through quantum mechanical tunneling. Would that leave the ammonia out of the examples of true symmetry breaking? Does a “temporary” breaking of the symmetry not suffice? Does broken symmetry come in degrees? We will see in section 3.1 how does Anderson solves this puzzle by appealing to other physical (and non-physical) examples, which will hopefully show the importance of presenting all the examples in MID as a whole, rather than just focusing on one, as it is usually done. This practice, as in the case of Cat’s paper, gives the impression that the author grants credibility to the solidity of Anderson’s argument in “More is different”, such that a deeper analysis of the actual examples he uses to back up his argument would be superfluous. Some of the rest of examples brought by Anderson to build his argument are briefly mentioned by Cat, for the sake of completeness, without adding much value to the discussion [Cat98, p.266]: “The same argument can be made, for different kinds of emerging asymmetries, in the cases of macroscopic crystalline structures, superconductivity, and phase transitions.” These brevity in surveying MID’s examples is not uncommon in other commentaries of the famous 1972 article, in which examples are mentioned superficially, already assuming their consistency and validity as rightful indicators of emergent properties in physics. This attitude of considering MID’s examples, or the general idea of broken symmetry, as mere “illustrations” of an alleged pre-existing notion of emergence (or anti-constructionism, to avoid anachronisms) in Anderson’s thinking camouflages the real role of those examples as the grounding pillars of the message defended in the paper. First, it is very unlikely that, even if tremendously resentful against his particle physics colleagues, he would have presented such a pervasive message without a convincing general physical mechanism to justify his claims about the autonomy of condensed matter physics with respect to the more fundamental laws. Second, it is unlikely that he would have thought of broken symmetry as a sufficiently general principle if it weren’t for the multitude of examples, all pertaining to different areas of expertise of a condensed matter or many-body physicist (and beyond), that he explores in the paper.

In summary, Cat’s paper offers a good historization of some of important episodes for the construction of Anderson’s anti-constructionism, but two deficiencies can be pointed out: first, not giving broken symmetry the importance it deserves in this story and appealing to it only as a “back-up argument” for Anderson’s claims in MID, and second, not presenting all the examples in MID as a coherent whole and rather putting the emphasis only in one, which fails on its own to capture the essence of Anderson’s message.

2.1.3 The time for emergence in physics

What is enriching about the historization of Anderson’s “More is different”, such as in Stevens and Cat’s paper, is that it gives a tentative answer to the puzzle of why the emergence vs. reduction debates didn’t appear earlier in physics, such as in the connection between thermo-
dynamics and statistical mechanics at the second half of the 19th century through the works of Maxwell and Boltzmann. A tentative answer is that such a connection was seen as a successful reduction of the former to the latter, for example in the reduction of the concept of temperature to the mean kinetic energy of the particles in a gas. However, one of the main arguments for Anderson to claim that there exist emergent concepts and laws in physics is that they cannot be derived solely from first principles but need the phenomenological approach in order to even understand what one is trying to derive in the first place. In this sense, Anderson certainly considers temperature as an emergent property. Why then no physicist before him challenged that “successful” reduction of thermodynamics to statistical mechanics? To be fair, it is unlikely that temperature would have been catalogued as an emergent property right away by physicists since the debates on emergence were just starting concomitantly. But, once emergence became a buzzword in philosophy and biology, why no physicist connected the dots? At first glance, one might answer that it was simply because physicists were not aware of those debates and that Anderson was the first one to be immersed in them due to his participation in several conferences with biologists, etc. But Anderson was not aware of those debates in 1972 either! It was precisely because of his first move to connect physics phenomena with a broader philosophical and political message that he attracted the interest of the community who was already discussing about emergence and was eager to find a physicist expressing such ideas, to finally upgrade emergence to a global scientific enterprise.

For this reason, a historical analysis of MID seems to offer a stronger reason for these debates to enter precisely in American physics during the 60s and 70s: the ideological and political need of differentiation of condensed matter with respect to particle physics in the battle for recognition and funding.

However, such an analysis might appear too one-dimensional. As Mainwood says in his thesis about “The New Emergentists” (referring precisely to this new school of physicists, usually from condensed matter, that adopted Anderson’s views) [Mai06, p.113]:

I feel that Cat’s interpretation focusses on a single point raised by Anderson as an argument for spreading government resources amongst areas in physics, rather than concentrating them to build a single expensive particle accelerator. But these are issues specifically to do with funding and status in the physics community, and are separate from his position on emergence.

Are they really so separate? Does that mean that any other less politically concerned solid stater could have had the revelation about emergent properties in physics just by looking at the phenomena, as Mainwood seems to point out? Why does it seem then that only Anderson was capable of reaching that revelation?

In the next section we will consider a different approach, taken precisely by Mainwood (2006) and also Humphreys (2015), which lacks the historical context and focusses instead on the philosophical coherence –or incoherence– of “More is different”.

\[\text{14} \text{ Again, with the exception of Polanyi, who we are excluding from the discussion since he failed to become the first emergentist physicist.}\]

\[\text{15} \text{ Further details on Anderson’s immersion to the philosophical debates on emergence and reduction in biology will be given in an upcoming paper.}\]
2.2 The Philosophical Manifesto

As in the previous case, this section will focus on a specific point of view in the story of interpreting “More is different”. In this case, we will focus on the philosophical point of view, which connects the paper with the ongoing debates on emergence and reduction in the sciences. Before going deeper in this connection, a brief introduction to those debates should be helpful.

The standard notion of scientific reduction (i.e. based on scientific evidence) can be put as follows: an entity/property/law \( y \) is said to reduce to another entity/property/law \( x \) if it fully depends on, can be fully understood in terms of \( x \), also implying that \( x \) is more fundamental, more basic than \( y \). The distinction between the reduction of entities and laws, for instance, usually draws a line between ontological reductionism, which deals with the relation between constituents of different systems, and epistemological reductionism, dealing with the relation between concepts, laws or explanations pertaining to different scientific theories or models. On the contrary, when a system’s constituents or properties, or the laws of a given model, cannot be understood fully by appeal to entities, properties or laws of an underlying more basic system or model, the relation between the higher level is said to be emergent with respect to the lower level (and again, this emergence can be ontological or epistemological depending on the case).

This definition is extremely simplistic, but it can already give us hints about some of the problems that arise from such notions: one concerns the status of the relation between \( x \) and \( y \) and what counts as a successful instance of “understanding or explaining fully \( y \) in terms of \( x \)? Should \( y \) only be counted as reducible to \( x \) if the reduction can be achieved in practice, and not only assumed to be possible in principle? Another concerns the status of \( x \) or \( y \) themselves, for instance, what counts as a part or a whole? Should systems or properties which highly depend on the interaction between their parts be considered non-reducible (i.e. emergent)?

The concept of emergence materialized through a group of philosophers dubbed by Brian McLaughlin as the “British Emergentists” [McL92]. It began with John Stuart Mill’s System of Logic in 1843 and ended with C. D. Broad’s The Mind and Its Place in Nature in 1925. The movement started as a way to set a middle ground between the mechanistic and the vitalist view on the question of the origin of life at the end of the 19th century. They proposed a way to understand the appearance of living systems out of inanimate matter which denied the appeal to mysterious forces such as an élan vital, refusing at the same time the belief that living systems are simply machines whose behaviour can be understood as a mere aggregation of simple mechanisms. The idea is that, when a system is complex enough, new laws, new properties, can appear which cannot be understood through the mere composition of the more basic laws that govern its parts. In Anderson’s later words, “the whole becomes not only more than but very different from the sum of its parts” [And72, p. 395]. The British Emergentists motivation was to create a framework which would retain “vital qualities” such as reproduction, consciousness, or simply the distinction between being alive or dead, avoiding the necessity to appeal to a “vital substance”. In this sense, they were materialists and the novelty was at the level of the laws, properties or powers of a given whole. Different names were given to this notion, such as “heteropathic laws vs. homopathic laws” [Mil43] or “trans-ordinal vs. intra-ordinal laws” [Bro25], until George Henry Lewes [Lew77] coined the now famous term “emergent” laws, which he opposed to “resultant”, i.e. which result from the composition of other laws and properties. As McLaughlin explains in his wonderful revision of the rise and fall

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16From now on I will refer to \( x \) and \( y \) as, respectively, the lower level and higher level entity or property or law, or anything that can be subject to reduction or emergent relations.

17It is worthy to point out that the term emergence was opposed by all these authors to a notion of aggregativity, composition, result, and not to the more sophisticated notion of reduction, which is broader than a mere
of British Emergentism [McL92, p.31]: “The introduction of the term “emergent” is, I should note, Lewes’s main contribution to Emergentism. “Emergence” captures the imagination in ways that “heteropathic law” and “heteropathic effect” do not. There is something in a name.”

According to this current of British philosophy, the sciences are arranged in a hierarchy of levels of organization in which physics lies at the bottom, very much in tune with Anderson’s views in “More is different”. However, they never questioned the aggregative or composite character within the laws of physics, which didn’t allow these discussions to permeate into the field until Anderson’s claims started attracting attention. As McLaughlin clarifies, it was certainly the advents of the quantum mechanical revolution which degraded the popularity of emergentism in the 1920s. The British Emergentists voted for at least three distinct kinds of forces: the physical, the chemical and the psychological forces. But with the quantum mechanical success in describing chemical molecular bonding, it became clear that physical forces could be, after all, all there is. Emergentist views were mostly forgotten until their renaissance in the 1950s in biology. The debates have been acquiring an increasing level of sophistication from the second half of the twentieth century on and, due to its multidisciplinarity, they beg for clarity and formality, specially from the side of the scientists. In this search for philosophical clarity is where the next papers are embedded. I chose Paul Mainwood’s thesis “Is more different? Emergent properties in physics” of 2006 and Paul Humphrey’s paper of 2015 “More is different? Sometimes: Ising models, emergence, and undecidability”.

2.2.1 “Is more different? Emergent properties in physics”

Paul Mainwood’s thesis is a philosophical analysis of the views on emergence espoused by the group of physicists dubbed by him as “New Emergentists”. This group, precisely inspired after Anderson’s “More is different”, is inspired by developments in condensed matter physics to claim the autonomy, or non-reductive character of this branch of physics with respect to the more fundamental ones. Mainwood’s main thesis is that some of the “New Emergentists” claims about emergent properties in physics are clearly metaphysical, not only methodological as the mainstream opinion suggests. He also puts much emphasis on the “honesty” of their claims, arguing that this group of physicists was not intentionally looking after the phenomena which would verify certain pre-established emergent features, but that they were the phenomena themselves which inspired them to consider emergence as a new paradigmatic way of considering the study of condensed matter physics [Mai06, p.7]:

Similar parochial concerns can be expected to appear in any approach which first constructs a firm list of desiderata for emergence, then searches for examples which conform to it. [...] I claim that the New Emergentists deserve special attention for taking the opposite tack: looking within well-understood physical sciences for simple phenomena that seem to display a few core emergentist characteristics, illustrating fundamental laws, or principles operating on a large scale, and then examining the theoretical descriptions to see whether they might suggest criteria for a more general approach.

In this way, Mainwood deposits all the justificatory weight of the entrance of emergence into physics on the phenomena themselves, leaving no space for the before mentioned historico-sociological context to play any role. I have already argued why the historical context is essential to understand the chronology of the appearance of emergence in physics.
2.2.2 “More is different? Sometimes”

3 An unfortunate choice of examples?
3.1 A diverse trajectory at the root of MID
3.2 Beyond physics, piling up speculations
3.3 A hierarchy of broken symmetries

4 Conclusion

References


