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 plausible 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.

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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 mechanicist 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 plausible 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.

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1 In 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

2 For the sake of briefness, I will refer to Anderson’s career as a condensed matter physicist, even though it’s the unification of a variety of fields such as solid-state, electronic physics, many-body problems, etc, which didn’t converge into a single discipline until — (see J. D. Martin).

3 In 1978 he is invited to a neurosciences meeting in Keystone by the American physiologist Eugene Yates, where he enjoys lively discussions about reductionism in science with a variety of characters [OBB01]. It is plausible that his first contact with the term of emergence is in this meeting.

4 The first to identify Anderson’s views in MID with emergence was the British ethologist and amateur philosopher William H. Thorpe as early as 1974, two years after the publication of MID [AD74].
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.5

In what follows, I will present these two readings and comment on their limitations, not without 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

5By this I don’t mean that the case of thermodynamics and statistical mechanics was not paradigmatic in the debates of reductionism in the sciences, in the philosophical community for instance, but that it didn’t stimulate the physicists to engage on those debates until after MID (with the exception maybe of Polanyi –see note 1.).

6For further reading on this topic, see [RHK15] and [Mar18].
at the time were there. 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.

An especially painful instance of disappointment for him were a series of three publications by the particle physicist Victor Weisskopf in 1965-67 [Wei65, Wei66, Wei68]. In Anderson’s words [OBB01, p. 2]: “I had always considered him a friend (and do now)—he sat on my thesis committee, and much of the early work in my thesis field was his—and this made it particularly hard to take.” Weisskopf’s 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 [OBB01, p. 2]: ““More is Different” is a reply to [Weisskopf’s] article dividing science into “intensive” (for practical purposes, particle physics) vs “extensive” (the rest).” This reply took place 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 reacted: “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 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). 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.

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7This 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].

8https://www.the-scientist.com/opinion-old/the-case-against-the-ssc-63734
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. 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 I 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 (as we read in the previous Stevens’ quote). 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 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. In this sense, I believe Stevens depiction of the narrative about the condensed matter opposition to the grand unification projects in fundamental physics and their use of broken symmetry is conflating the chronological order of cause and effect: there was not a homogeneous usage of broken symmetry in the solid-state community during the 50s, 60s or even 70s, neither was it connected to any anti-fundamentalist message until, as far as my research has proven, Anderson connected both ideas. Therefore, Anderson is not the culmination of broken-symmetry-against-fundamentalism movement that was bubbling under the scenes, but rather the origin of it.

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 arised as an analogy to superconductivity gauge symmetry-breaking for particle physics through the work of Nambu and Jona-Lasinio and that it was

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9High-energy physics.
10find article
coined for the first time by particle physicists Baker and Glashow in their article “Spontaneous breakdown of elementary particle symmetries” of 1962\textsuperscript{11}. 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, 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 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 2.2.1)– Cat concludes that An-

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 work. 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-constructionist 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.”

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 there is a discrepancy between what the observational properties of the molecule and what we know about its structure: the asymmetric pyramidal structure of the molecule would imply a net-dipole-moment which is not observed phenomenologically. The reason is that the stationary state of the molecule is in fact in a quantum superposition of the two asymmetric states with inverse dipole moment, resulting a no-net-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 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

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.
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 thermodynamics 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

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14 Again, with the exception of Polanyi, who we are excluding from the discussion since he failed to become the first emergentist in physics.

15 Further details on Anderson’s immersion to the philosophical debates on emergence and reduction in biology will be given in an upcoming paper.
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”.

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 the origins of “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 and philosophy and philosophy of science. 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 \( x \)”, for example, does a successful reduction also imply the opposite, i.e. that \( y \) could have in

\[ \text{From now on I will refer to } x \text{ and } y \text{ as, respectively, the lower level and higher level entity or property or law, or anything that can be subject to reduction or emergent relations.} \]
principle been derived or predicted from \( x \)? 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 mechanicist 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 distinction between laws that are a composition of more basic laws and laws that violate this composition rule, such as “homopathic vs. heteropathic laws” [Mil43] or “intra-ordinal vs. trans-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\(^{17}\). As McLaughlin explains in his wonderful revision of the rise and fall 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 from the late 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 paper is embedded. I chose Paul Mainwood’s thesis “Is more different? Emergent properties in physics” of 2006.

\(^{17}\)It 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 aggregation of laws or properties and can very well take into account the interactions of the different parts of a whole. For a critique on the very strict and implausible meaning of reduction as mere aggregation see Wimsatt (2006) [Wim06]
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”, in analogy to the British Emergentists discussed previously. This group, precisely inspired after Anderson’s “More is different”, is stimulated by developments in twentieth century 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 (what I will call thesis 1). 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 paradigm in the study of condensed matter physics (what I will call thesis 2) [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. It is precisely this second thesis which my aim is to criticise.

Before that, I would like to briefly go through Mainwood’s first argument, which reconciles two apparently contradicting positions taken by the New Emergentists: their adherence to microphysicalism and their belief in the novel metaphysical status of higher-level properties. His main thesis in this respect is, as mentioned above, that the New Emergentism qualifies (contrary to the mainstream commentators’ opinion) as a metaphysical position. And not only that but it qualifies as an original metaphysical thesis which fails to be accounted for in the pre-existing metaphysical positions on emergence in the traditional debates in philosophy of science and mind, thus rendering it more valuable than usually considered. I will not go through the standard metaphysical approaches to “microphysicalism” and “novelty” that, according to Mainwood, fail to characterise the New Emergentists view; I shall directly focus on Mainwood’s approach to reconcile both positions endorsed by the New Emergentists in a coherent and valuable metaphysical picture.

In view of Mainwood’s analysis, two are the basic claims of the New Emergentists [Mai06, p. 20]:

1. Microphysicalism (both property and entity varieties) – All entities are composed of microphysical entities, and all their properties are fixed by the microphysical properties, which evolve according to microphysical laws.

2. Novelty – Some systemic properties are importantly novel: so different to the microphysical that they and the laws that govern them can be recognised as having a metaphysical status in no way inferior to the microphysical.
The way in which these two propositions can be mutually consistent is by not falling into the deceitful corollary that microphysical properties and entities are more (metaphysically) fundamental. As Anderson puts it in MID, there is a hierarchy in the sciences and higher-level sciences obey the laws of lower-level ones, but that doesn’t mean that the former are just a mere application of the latter. In this sense, he refuses talking about lower-level sciences as “more fundamental” since, even if consistent with them, higher-level laws cannot be derived or predicted from them. This is where the core of the debate on how to interpret Anderson’s (and the New Emergentists’) position lies: is this non-predictability/non-derivability an in principle or an in practice one? The majority of philosophers seem to agree on the latter being the position defended by the New Emergentists –which is often named epistemological emergence, while they accuse them of making claims as if they supported the former –ontological emergence, therefore excluding their position from a coherent metaphysical one. Mainwood finds disingenuous of them to accuse them of this confusion and proposes a way to reconcile the New Emergentists’ support to epistemological emergence and their claims about the novel ontological status of higher-level laws. In order to do that, Mainwood distinguishes two kinds of microphysicalism that are usually conflated together [Mai06, p. 55]:

1. Microphysicalism$_1$ – a sense associated with microphysical supervenience, which admits as “microphysical” all of those properties describable under a recognisably microphysical theory – say, the Standard Model. These may include properties of very large systems.

2. Microphysicalism$_2$ – a sense associated with British Emergence, which admits as “microphysical” only those properties of systems below a certain size, which they display in isolation, or when they are parts of systems other than the one in question.

Conflating these two kinds of microphysicalisms results in two prejudices [Mai06, p. 71-2]:

1. Microphysical prejudice 1 – the belief that supervenience physicalism grants a privileged place to properties of the very small.

2. Microphysical prejudice 2 – the belief that the laws of microphysical theories –such as the Standard Model– are privileged a priori with respect to higher-level ones.

The first prejudice arises from only conceiving microphysicalism$_2$, the one that only admits as microphysical properties those who correspond to parts in isolation, lone particles. If you include however systemic properties into your metaphysical picture of microphysicalism (that would be microphysicalism$_1$) –i.e. properties (that arise from the interaction amongst the parts, but that equally correspond to elemental properties for the construction of the given microphysical theory– then there is no reason to grant a more fundamental status to lone-particle properties vs. systemic ones. Having dissembled this prejudice, we come to the second one: if it’s not the small size or the isolation of the particles which renders a theory microphysical (since it can also include macroscopic or systemic properties), why should they be considered as more fundamental than higher-level ones in the hierarchy of the sciences? As Mainwood points out, every science has their own sets of properties which are considered fundamental at that level of description in a pretty obvious way (such as valence in chemistry or heritability in biology) regardless of the conviction that they supervene on physical properties in a non-straightforward way. However, when it comes to higher-level properties within physics, we generally don’t accept this “fundamentality” status of other properties than microphysical (type 2) ones. Mainwood defends that this prejudice comes from conflating physics and microphysics, which is precisely the picture the New Emergentists deny. In this way, their aim is to disentangle two notions that have been wrongly conflated together: scale and fundamentality.

To sum up, Mainwood’s main point is that the New Emergentists certainly enter into metaphysical debates on the status of physical laws. They are not only offering a new methodology
for the study of condensed matter systems, but they are justifying why this methodology exists and is successful to explain such higher-level phenomena and they are redefining the concept of fundamentality in a way in which the properties of the very small is no longer privileged with respect to large scale ones.

We come now to what I have dubbed the second of Mainwood’s thesis, which directly relates with our subject at hand, i.e. the origins of Anderson’s “More is different”. In a nutshell, my position against Mainwood will be that it is not only the physics which inspired Anderson’s MID (the fountainhead of the New Emergentists movement in his own words [Mai06, p. 4]) into “discovering” emergent properties in physics, as he defends. Before going to the points in disagreement with Mainwood, I shall comment on the common assumptions we both share, which will exclude the trivial reasons for discrepancy.

The first one is that, as many authors and me, Mainwood places “More is different” at the origin of the new emergentist movement in physics and science in general [Mai06, p. 4]: “Much of this startling ‘resurgence of emergence’ can be traced back to a short 1972 paper by the Nobel Prize winning physicist Philip Anderson, entitled ‘More is Different’.” It is important that we both share that premise since it gives Anderson a leading role in being the first one to associate physical properties to emergent properties, and all of what that involves, i.e. creating new concepts and vocabulary –such as the term “constructionist hypothesis” with which he believes reductionism to be incorrectly conflated— or associating existing concepts to new meanings –such as associating “symmetry breaking” to “scale change”, as I will explain in section 3. Nevertheless, this is the point in which our interpretation of the story differs: despite attributing this leading role to Anderson, Mainwood suppresses his creative power and considers all the inspiration to come from the observation of the phenomena themselves. As I already quoted, he believes his concerns about funding and status “are separate from his position on emergence” (p. 113), while I think they certainly contributed to intensify his search for a general physical mechanism which would render condensed matter physics autonomous from particle physics. Despite Mainwood’s defense that Anderson’s claims were solely “based on well-understood physical theories”, there is an obvious tension in the 1972 article –which I will describe shortly– that forces him to admit that “his position was, and remains, a controversial one” (p. 4). I will argue that this tension between what he believes to be Anderson’s message in “More is different” and the actual message presented in the paper is due to the gap between the physics and the interpretation of the physics, which –in my opinion but contrary to Mainwood’s opinion– was filled by historico-biographical factors.

But before delving into that, I will quickly comment on the other common assumption to Mainwood, which is placing symmetry-breaking as the more basic (although not unique) mechanism through which the derivation of the higher-level laws of condensed matter physics from the particle physics laws is practically impossible [Mai06, p. 93]:

The New Emergentists stress that there is no single mechanism lying behind all instances of anti-constructivism nor behind all emergent properties and laws. [...] However, there is one class of mechanisms which they judge to be particularly basic, important, and which may provide the basis for a widely generalisable framework – symmetry-breaking.

It is in our analysis of the role of symmetry-breaking in “More is different” that Mainwood and I differ. Let’s see in which respect.

I believe there is a tension between the usual definition of symmetry-breaking before “More is different” and what Anderson claims to be its meaning in “More is different”. On the attitude towards this tension is where Mainwood’s analysis and mine differ. To put it shortly, I decided to investigate the reasons behind that tension whereas Mainwood decides to omit it appealing
to “confusion” and “unfortunate choices” from Anderson’s side. I find it striking that, while he has been careful enough to grant the New Emergentists with sufficient intellectual clarity, he has given up on finding that clarity in Anderson’s “More is different” which, in the end, he admits to be the origin of the New Emergentist movement. In admitting the whole movement to be based in a paper which is “confusing” and “controversial” but not trying to find the reasons behind that controversy, he is acting as if that tension should not be regarded as important when interpreting the later homogenous emergentist movement in physics. In doing so, he creates an impersonal and atemporal New Emergentist philosophy which most probably is not really instantiated by any of its actors. He is therefore projecting this philosophy to Anderson in retrospective, a philosophy which he doesn’t consider that he is capable of defending in the 1972 paper. It is as if it was the physics which were dragging Anderson to the correct coherent philosophy (that he calls New Emergentism) and “More is different” was only his awkward and “premature” début. My position couldn’t be more opposed: I believe MID, with all its confusion and awkward examples, is what shaped what will later characterise the whole emergentist enterprise in physics. Because I find Mainwood’s position to be oddly common in commentators, it is my aim here to bring the clarity that MID has been denied. In order to do that let’s now finally delve into the content of that tension.

The basic problem resides in the way Anderson’s presents the concept of symmetry-breaking in “More is different”. In the paper, Anderson is concerned with defending the idea that one can “array the sciences roughly linearly in a hierarchy” [And72, p. 393] but that doesn’t mean that the higher-level ones are just the application of the lower-level ones. They are autonomous from them and have their own concepts and laws which he considers as fundamental as the ones at the bottom of the hierarchy. The novelty of his message is that he accepts this hierarchisation to take place also within the discipline of physics which, as Mainwood rightly points out, has been typically conflated with microphysics, not allowing this differentiation in subdisciplines to play anything more than a practical role. Anderson claims that the impossibility of deriving the laws of condensed matter physics from the more fundamental laws of particle physics defies the popularised idea of a final theory of everything. This is what he calls the anti-constructionist hypothesis. Once we have this hierarchisation of the sciences, what differentiates the different levels, according to Anderson? The degree of scale and complexity [And72, p. 393]:

The constructionist hypothesis breaks down when confronted with the twin difficulties of scale and complexity. The behaviour of large and complex aggregates of matter is not to be understood in terms of a simple extrapolation of the properties of a few particles.

And once we know what distinguishes the different levels, how does this differentiation happen? Through the physical mechanism of symmetry-breaking [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. [...] The general rule [...] is that the large system is less symmetrical than the underlying structure would suggest.

We arrived to the crucial point. In my opinion, there are two concepts Anderson is conflating here: scale differentiation (which is essential for him to defend the autonomy of condensed matter with respect to particle physics) with the mechanism of symmetry-breaking. First of all, it is important to mention that, although he doesn’t explicitly name it, Anderson is talking about spontaneous symmetry-breaking, i.e. the fact that a system’s state presents a different symmetry than the one it should have according to its equation of motion. This is opposed to an explicit symmetry-breaking, in which the asymmetry comes from an additional term (an
external field, for instance) that is introduced by hand in the equation describing the state of the system. The spontaneous asymmetry need not be related with arguments of scale or complexity of the system, although it can be one of the reasons for the asymmetry to take place since, in a large and complex system, the interactions between the different parts can force the whole into an unstable state with respect to the one envisioned by the state equation. However Anderson presents symmetry-breaking as a *general* mechanism for understanding scale differentiation and, most importantly, which is specific of his field of condensed matter physics. There is no need saying that this is not the case since, as a matter of fact, the concept of *spontaneous* symmetry-breaking was first introduced in the 1962 paper by Baker and Glashow which discusses the asymmetries in elementary particle physics. This paper was indeed influenced by the analogies worked by Nambu and Jona-Lasinio between the BCS theory for superconductors and the asymmetries in elementary fields. The fact that this work was inspired by works in solid-state physics doesn’t imply that the concept of symmetry-breaking is unique to that field, quite the contrary! However, in MID, Anderson accuses particle physicists of “borrowing” the term as if the original meaning only belonged to solid-state physics [And72, p. 394] “A second source of confusion may be the fact that the concept of broken symmetry has been borrowed by the elementary particle physicists, but their use of the term is strictly an analogy, whether a deep or a specious one remains to be understood.”

I believe Anderson’s ardent desire to attribute the originality of symmetry-breaking to his field rather than share it with the particle physicists is precisely influenced by the socio-economic factors mentioned in section 2.1, that also led him to write “More is different”. His already quoted contribution to the APS meeting in 1987 on the history of superconductivity is a proof of his not so honest intention in MID not to share the patent of symmetry-breaking with his particle physics colleagues [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.

Mainwood is of course also aware of this conflation [Mai06, p. 95]: “But this distinction between laws and their solutions is not directly related to the emergentist distinction between large and small systems: Anderson called his 1972 paper ‘More is different’, not ‘Solutions are different’.” And he adds that this is precisely this confusion that leads him to consider MID as a “controversial” work and to omit further discussion on the examples presented on the paper in order to back-up Anderson’s argument [Mai06, p. 100]:

> Anderson’s own discussions give some impression that symmetry-breaking can be discussed within a single theoretical framework. However, he moves very quickly between cases of quantum and classical symmetry-breaking, which are handled in rather different ways; indeed most of his case studies, as well as his suggestions for general frameworks are specific to the quantum context. [...] Therefore, I shall not follow Anderson’s own case studies, nor his discussions of more general frameworks for symmetry-breaking since, despite their promise, they raise controversy that does not affect the philosophical points we will need.

How can they not affect the philosophical discussion at hand if they are the origins of that discussion? I take a different stance: I believe there are reasons behind Anderson’s conflation of the concepts of symmetry-breaking with scale differentiation and his attempt to make it unique to condensed matter physics which are not merely physical or a “confusion” on his part,

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18 It is funny how he proceeds by trying to give this deeper meaning to broken symmetry in the rest of the paper, even proposing a generalisation of the mechanism which would go beyond physics, to the life sciences and above.
as Mainwood defends. And, contrary to him, I consider crucial to go through the examples presented by Anderson to understand what are the ingredients that led him to defend that conflation as a reasonable one in MID. This is the aim of the next section.

Before that, let me summarize this whole section against the purely philosophical reading of “More is different”, that is Mainwood’s thesis that it is purely the physics which inspired Anderson (the fountainhead of the New Emergentist movement) into discovering emergent properties in physics. The two arguments I have put forward are:

1. if that would be the case, it wouldn’t explain the why then, why him questions, i.e. why did emergence only appear in physics in the 70s through MID and by the hand of a single actor? I find these two questions important because I am trying to avoid simplistic stories about a single genius having a divine inspiration and discovering a new paradigm out of the blue. I do believe Anderson had a very original contribution which was completely his own: putting together the concepts of anti-constructionism and broken symmetry. But, despite being the motivation for the new school of condensed matter emergentists, I do think his creative role in putting together those concepts goes beyond the mere observation of the physics. In this point is where Mainwood and I differ, although in a non-trivial way: Mainwood does seem to agree in the fact that Anderson’s arguments and examples in MID (what he agrees to be the origin of the New Emergentist movement) are somewhat confusing and forced, but then he is perfectly comfortable in dismissing them alleging that “they raise controversy that does not affect the philosophical points we will need.” I think this position is contradictory: how are they not important for the philosophical discussion about emergence in physics if they constitute the origin for such a discussion? My belief is that Mainwood, when talking about the New Emergentism, is talking about a useful but unrealistic label in which he is mixing different authors and different phenomena that qualify as emergent into a single coherent picture. I certainly agree that there are many points in common between all of them and that his work is immensely valuable in recognising all these commonalities and trying to put them in a coherent metaphysical picture, but I don’t think is completely reasonable to say that “this movement originated solely from the physics” along with “the originator raised a controversy in his most iconic piece which is the landmark of this movement for which I will dismiss the examples in it”. Again, because Mainwood’s goal was not to make an historico-epistemological work on emergence in physics, my criticism here is only concerning this point of view.

2. the fact Anderson uses the concept of “broken symmetry” to counter-intuitively demarcate condensed matter from particle physics (which also very much uses this concept and helped in a crucial way to its development) by conflating its meaning with “scale differentiation”, which was not a standard pairing of concepts before MID− seems to indicate that there was in fact some desire of claiming the autonomy of his field with respect to the more traditionally considered as fundamental, precisely coinciding with an historical context of rivalry in prestige and funding between those two fields. Needless to say, the most substantial weight of Anderson’s claim comes indeed from a great understanding of the physics, but it is not disconnected from the historico-sociological context that he makes this claim at that precise moment and choosing the available physical ingredients that he had at hand at that moment. In fact, he recognises in an interview in 2002\(^\text{19}\) that he “really only got [broken symmetry] properly formulated when [he] went off in 1980 to a meeting in Paris in honor of Pierre Curie”. So it is fair to say that his conclusions about the generality of broken symmetry in MID as the mechanism which brings about the qualitative differentiation between levels are rather premature, as Mainwood correctly points out. What it is not fair to say then, in my opinion, is that contrary to other

\(^{19}\)https://www.aip.org/history-programs/niels-bohr-library/oral-histories/24312-2
emergentists movements, this one originated solely from a mere observation of the physical phenomena.

3 New reading: Anderson’s diverse trajectory at the root of MID

3.1 An unfortunate choice of examples?
3.2 Beyond physics, piling up speculations
3.3 A hierarchy of broken symmetries

4 Conclusion

References


