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Jörg J. Dötsch

Theorizing emergence in framing complexity economics. A Reply

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Author's address and affiliation:
Jörg J. Dötsch (joerg.doetsch@andrassyuni.hu)

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This paper provides a theoretical clarification of an important question raised by Olivér Kovács in *Acta Oeconomica* 69, 4 and develops if further. It will clarify what role complexity-theoretical considerations have played in the economic sciences so far and why. It will show where the limits of this approach lie within the discipline and to what extent serious problems of demarcation arise with regard to other disciplines of the social sciences. To this end, it is necessary to address the conditions under which concepts of emergence can and cannot be made fruitful for questions in economics.

Keywords: complexity, complex systems, emergence, evolution

JEL classification indices: A14, B10, B40, B50, B25, D8

1. INTRODUCTION

"What we cannot talk about we must pass over in silence". This applies to the sciences in general. No one will want to ask a proven expert in petrography about metabolism, and no one will expect a first-rate pharmacologist to solve astronomical problems. And even though we acknowledge that there is no such thing as *the* science, only sciences, and thus the fact that division of labour may also have its advantages in the search for knowledge, it is not always easy to find the right correspondent for a question. To what can we expect reliable answers from economics? For Kovács (2019: 571) this question is "the hottest potato of economics today". The answer will also have to include what it must pass over in silence about.

The request is understandable, but I don't think there's a definitive answer to it. The plurality of science also continues within the disciplines. Biology e.g. is divided into many sub-disciplines such as botany, human biology, genetics, etc. This results from a complex process of differentiation. In some cases, progress has been made especially when scientists violated the rules in force. For example, staying with the recent field of molecular biology opened by physicists as Watson and Crick, who were initially viewed extremely critically by the "experts" in biology, it provides a case in point. Examples of this kind abound.

In his contribution "Grounding complexity economics in framing modern governance" (*Acta Oeconomica* 69, 4), Kovács gratefully pointed out that the history of the science of wealth by no means lays out a straightforward career path, but that there are and have been different approaches that make different demands on explanatory patterns. And there is no doubt that under the label of "economics", an explanatory pattern has established itself as the standard in contrast to its obvious lack of explanatory capacity (let's think of Samuelson's factor price equalisation theorem and the observable development of the global economy) and in contrast to obvious failures (see Reinert 2012). Kovács summarised this standard under the label of "mainstream".

If, as Kovács claims, "mainstream economics" does not use the instruments appropriate to the surveyed domain to be explained, is a complexity-theoretical approach a possible building block on the way to a deeper insight? Kovács suggests this: "All of us should consider complexity economics as a research program rather than a singular theory" (Kovács 2019: 589). This proposal is perhaps praiseworthy precisely because it is *not* new and Kovács proves undaunted by the impressive achievements and impressive defensive attempts of the orthodox. Obviously because arguments are closed to the mainstream, he names quite a few authorities as witnesses for his position. Not without reason, he chooses a particularly outstanding one at the end of his contribution to give more emphasis to the justification of a program of complexity economics, and quotes Newton: "Every scientific field shall find a starting point which is per se unexplained" (Kovács 2019: 289). So the question now arises – what is per se unexplained in economics? And in what ways is a complexity-theoretical "research programme"[1] plausible? Kovács calls for this, but he uses only the perplexity of the mainstream as an argument, not how he arrived at this very demand. Perhaps this is why his conclusions for the policy (ibid, 587) cannot be inferred by his own arguments.

[1] Kovács probably refers here to Lakatos, but does not make a more precise classification or delimitation. See Nola/Sankey (2007), 274f.

One can only think about a meaningful application of complexity theory instruments if the surveyed domain, what Kovács calls with Newton "per se unexplained", also suggests this. Nota bene: this does not need to be the case for all phenomena considered to be economically relevant. But it is reasonable to assume that this applies to a number of relevant phenomena. In this respect, Kovács has indeed raised an important question, classified it in terms of dogma history and tried to make its relevance plausible on the basis of a helplessness of the discipline that he has diagnosed. He may succeed in the latter, but the theoretical contours of the required "research programme" remain very vague. This is where the following comments come in. They are contributed to the theoretical clarification of this problem.

In my view, this requires first, a look at some general aspects of the historical differentiation of "the" economic science and its relation to the topic of complexity. Although Kovács offers a brief (and not incorrect) dogma-historical name-dropping, this is on the one hand too imprecise with regard to the economic complexity problem in the narrower sense. On the other hand, the difference in approach between complexity-based economics and "mainstream economics" becomes not sufficiently clear, but is covered under the too-general categories of "Parmenidesian and Heraclitusian economics" (Kovács 2019: 577).[2] These questions are dealt with in the first part, chapter 2 of this article, which clarifies the conditions for the adequacy of answers to appropriate questions. There we will provide a little more clarity on the basis of a few scientific-sociological aspects.

Furthermore, Kovács's call for a complexity-based economics is intuitively comprehensible because of the "ever-more intensifying complexity pervaded by interwoven wicked challenges" (2019: 584) mentioned by him, but what he understands using terms as "socio-economic system" (ibid) or "socio-economic innovation ecosystem" (585), and to what extent different systems could be delimited and categorised from each other, Kovács does not say. This would be necessary, however, if his demand ("shifting towards complexity economics is inevitable", ibid, 583f) is not only to be a general call on the topic, but is also to be integrated concretely into existing theoretical strands (Kovács indeed distances himself – very politely – from the mainstream). There is, indeed, a current discussion of concepts of complexity theory with economic relevance.[3] This discussion clearly shows that it would go beyond the scope of an

[2] According to his brief remarks at the end, Kovács seems to be quite clear about the consequence of the difference, but less about its cause (see his remark on p. 587: "The open, dynamic socio-economic ecosystem does not have by very nature a code filled with elementary and universal laws). Moreover, I am not sure whether his classification as "Parmenidesian" is appropriate at all (nor whether the "Heraclitusian" label is appropriate for his draft programme), but this cannot be discussed here.

[3] Even the attempt to give an overview of this discussion would take longer than the further development of a detail question which is to be addressed here. At the time of the desideratum formulated by Kovács, Victor A. Beker's "Alternative Approaches to Economic Theory" (2020) had not yet been published and offers an appropriate answer to some of the questions raised. With regard to the concrete problems addressed by Kovács, it is important to mention Schwardt's "Path to modern economics. Dealing with complex economic systems" (2017), which – even if it is not very helpful in terms of complexity theory – very clearly captures the plurality demanded by Kovács, and just as vividly addresses the epistemic limitations associated with the choice of methods. Mitleton-Kelly et al. (2018) offer a Handbook of Research Methods in Complexity Science. With a view to the problem of economic modelling, complexity and the possibilities of mathematics, Sarukkai (2012) provides insights here in the informative volume by Zambelli/George (2012). Faggini and Parziale (2014) have developed the topic with mathematical models. However, they are by far not only individual contributions or collective volumes. In 2016 the Springer Publishing has dedicated a whole series to the topic with "Evolutionary Economics and Social

article to fully reflect the many possible starting points of complexity theory considerations within the diversity of economic approaches. But it seems worthwhile precisely for this reason to stick to the basic principles. No matter how one approaches a complex reality, reducing this complexity by explanation means "to decide what is entirely left out of an analysis" (Schwardt 2017, 27). If there are economically relevant phenomena of social reality that can be grasped in terms of complexity theory *as wholes*, then it is worth focusing on a partial aspect: For this purpose, an in-depth discussion of the possibilities for complexity approaches based on considerations of *emergence* theory is necessary. This is dealt with in the second part, chapter 3 of this article. These are theoretical considerations, basic research, from which we cannot claim to be able to move on to concrete policy recommendations in a next step.[4] But neither can Kovács. I draw on considerations that I have partly presented in working papers (Dötsch 2013a, 2013b) and which I formulated in 2014 to a German audience – this concerns especially the third section concerning the problem of emergence in heterodox economics (Dötsch 2014), and which I have taken the liberty of revising in view of the neverending methodological discussion in economics as a theoretical clarification and deepening of Kovács's call. Current developments, however, only seem to lend more weight to the questions raised here.

2. APPROPRIATE ANSWERS TO APPROPRIATE QUESTIONS

2.1 Preliminary remarks on the intradisciplinary differentiation of economics

Just like sport, science is pursued in various disciplines. For the problems considered here, however, it must be borne in mind that until the end of the 19th century, the discourse of the sciences was much less differentiated into hermetically sealed disciplines than it is today. It used, in each case, a much less terminologised and formalised language, which therefore made transitions much easier. A man like Goethe could still be scientifically active in the same field as Newton (see Sepper 1988). The popularity of Ernst Mach's works, for example, illustrates a late stage of this characteristic accessibility of science (Stadler 2018). By the second half of the 19th century at the latest, however, the individual disciplines began to increase rapidly in complexity (!) (see Sarukkai 2012, 69 citing Rescher 1998). Already at the beginning of the 20th century it had become a characteristic of scientific work that its results could no longer be easily communicated and can only be understood by a closed circle of specialists. At the same time, the understanding of individual disciplines among each other is becoming increasingly problematic or completely impossible. Let us note here that the question when something becomes a science at all and what forms sub-areas within the same (reference was made above to biology in this regard) is a partly contingent process. And the requirements *for what is considered an appropriate answer to a scientific question* change over time. This is the *first* important aspect at which the question of the potential of complexity theory considerations for economic science begins.

Complexity Science" and started another one in 2019, "Economic Complexity and Evolution" with a first volume edited by Chai and Baum.

[4] There are already some interesting approaches in this respect, see for example Antonelli (2011), who assembles a series of instructive contributions, but see already Salzano/Colander (2007). See also the volume of Mandl (2019). This line of research cannot be pursued here, as the following article focuses on the phenomenon of emergence in a constructive response to Kovács.

There are of course many answers to this question.[5] Let us look for one as simple as possible for our purposes and choose it from the area that is obvious on the basis of what has been said so far, following Humberto Maturana (1988: 9).[6] For him, a proposal for a scientific explanation of a situation (a phenomenon) is acceptable "(...) if it describes a mechanism that produces that situation or phenomenon as a consequence of its operation."[7] Scientific activity is explicit intellectual construction of explanatory proposals. Scientific answers are proposals that demand collegial acceptance. The sense and nonsense of collegial acceptance does not need to concern us here.[8] An important instrument of such scientific explanations is the construction of analogies. This can be traced within scientific disciplines in a theory-historical way looking at the terms used to clarify certain problem areas. For example, in physics: is light made up of particles, waves or is it a form of energy? An analogous use of terms can also be observed in different disciplines. This can happen when one discipline is particularly successful in solving scientific puzzles and another discipline expects similar success (or similar authority, access to third-party funding, sex appeal, etc.) from the use of the same instruments.[9] It is therefore not surprising that the mathematical methods of the natural sciences were also applied to other domains. The outcome of this process for today's problems of mainstream economics is well known. And indeed, with its conceptual system it provides a watertight answer to "all the phenomena involved in the question" (Maturana 1987: 288). A successful science! Yes – provided a meaningful question has been asked. This is the *second* important aspect that is relevant to questions about the potential of complexity theory considerations in economics. Kovács has first and foremost addressed this aspect.

The construction of scientific answers in the sense of Maturana does not only consist of solving problems by means of known methods, but is of course always accompanied by methodological criticism. The basis for this is the greatest possible explicitness of the methodological arguments. If a question can no longer be answered sufficiently scientifically with the available scientific instruments, a modification or exchange of methods must be discussed. This can lead to a dispute about methods within a discipline or, in Kuhn's words, extraordinary research (Kuhn 1962).[10] Where does economics stand?

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[5] In this respect, the collective term of a "research programme" on complexity is feasible.

[6] In this context, it is still appropriate to consult Maturana for three reasons: Firstly, he poses the theoretical question of the appropriateness of answers against the background of its own concept of emergence, that of autopoiesis. Secondly, it was Maturana who – through a deeper theorisation of a problem in biology –, played a decisive role in preparing the transfer of a concept to the social sciences, especially to sociology. Thirdly, since this was not necessarily further specified in the several disciplines of the social sciences – accordingly in economics – it makes sense to start with the level of generality he set. However, neither an exhaustive discussion of the theory of science (regarding appropriateness of questions and answers) nor an in-depth discussion of the theory of emergence as such can be undertaken here. On this last point, see above all Sawyer (2007).

[7] In a German-language publication published one year earlier, he summarises this sentence more precisely: "Scientific answers, i.e. answers acceptable to scientists, represent proposals for mechanisms (concrete or conceptual systems) which, in their operation (functioning), produce all the phenomena involved in the question". (Maturana 1987: 288, own translation).

[8] Herein lies, however, a source for the contingency mentioned above.

[9] Herein lies another source of contingency.

[10] See also: Kindi/Arabatzis (2012).

2.2 Plural economics, one domain – one economics, many domains?

The situation of the extraordinary research has been familiar to economists for a long time and there is still no prospect of a transition to a "normal science" (Kuhn) under a common name. In the second half of the last century, the dispute over methods was still much more lively than it is today. The (re)discovery of complexity and contingency of economic processes by the Austrian School had been able to revive theoretical work for several decades ("Austrian Revival").[11] The most important motive of these efforts was the question of the sense of the – in itself – watertight economic theory (the second aspect in the previous section). Due to the lack of better and unique ideas and, basically, similar to the emergence of the Neoclassical paradigm, more promising instruments from other disciplines were used. Evolutionary theory, cybernetics, systems theory and perspectives of complexity theory came into focus (Kovács's demand is not new).[12] One result is, that within this "heterodoxy" a representative of institutional economics, for example, is sometimes no longer able to understand what a representative of evolutionary economics is actually researching and vice versa. Nevertheless, both understand themselves very well as economists. Neither direction has been able to develop into a mainstream, i.e. to institutionalise collegial acceptance in the sense of Maturana to a sufficient degree. The answer to the question of the evolutionary economist Ullrich Witt "Self-organization and economics – what is new?" (1997) seems – even more than two decades later – to be brief.[13]

Economists, however, have for a long time dealt with a wide variety of problems in very different ways. Why is it that one group among them, according to Kovács (2019: 573), "ignored what we once knew about the economic system"?[14] To clarify the problem at hand we want to distinguish heuristically and slightly exaggerated between two groups. One type of economists, group A, deals with observable reality using an "economic approach". It is this approach that makes them what they are. Of course, this science does not need to be complemented by complexity theory, because this is not necessary (and theoretically not possible). The other group, group B, considers the answers of group A unsatisfactory, but uses very different approaches (Kovács counts himself among the last). To put it bluntly, one will still have to decide: An economic approach for an infinite domain or a many-voiced discipline of economics for one domain.

Since group A indeed fully satisfies the criterion set by Maturana and is also indifferent to the object area (the sense and nonsense of questions has already been decided, since the path is the goal),[15] we must turn here only to group B. From the point of view of group A, it does not consist of economists, but of other beasts who, using various instruments, create concepts to explain phenomena which they consider to be economic problems. Inverted world! It must

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[11] See the instructive volume by Koppl/Horwitz/Desrochers (2010).

[12] For the period in question see for example Röpke (1977), Nelson/Winter (1982), Dosi et al. (1988) or Witt (2016).

[13] Though, in the new millennium, the general discussion concerning concepts regarding complexity and self-organisation got somewhat more lively, see e.g. the references given in footnote 4.

[14] Grammatically, however, Kovács counts himself among this group. Obviously this is a move in search of acceptance.

[15] Kovács is not without polemics when it comes to the diagnostic competence of this group. He is not alone in this – and according to statements such as Hirshleifer's (1985, 53), having found "the universal grammar of social science" does not come as a surprise. Souter coined the term "economic imperialism" for this remarkable development as early as 1933.

therefore be a decisive question for group B whether it is at all possible to define a real problem area of its own kind that justifies the economic science as a social science of its own kind. The answer to this proves, whether the questions have been asked in a meaningful way (group B accuses group A of not asking meaningful questions) and this is also where it will prove whether these scientists in group B are not rather called sociologists, psychologists or chaos theorists etc. and therefore sometimes should have to pass over in silence. This question is also crucial in view of the collegial behaviour of group B, which quite obviously does not believe in the working principle of a single and exclusive method, but allows for instrumental diversity. The criterion of methodological heterogeneity can in fact be retained by this group. But if it is so, what is its object area?

We cannot yet answer the question here, but must continue to swim in the waters of the extraordinary research of group B for the time being. Let us assume, following Kovács, that complexity-theoretical approaches are a way to access economic problems. If we are prepared to retain instrumental heterogeneity, then the decisive question can be categorised as a subquestion for our purposes, namely: is it possible to define a real realm of problems of its own kind that justifies the use of complexity theory instruments for subquestions of economic science? This would provide justification for a sub-area within group B without immediately demanding the complexity theory revolution in economics. This sub-area could also be pursued in the sense of basic research, and that is what this essay is all about.

The conditions described here already show that processes of inter- and intradisciplinary differentiation show similar patterns and are sometimes not easy to separate. Categorized as a sub-question, the above-mentioned second aspect – if the proposal for a scientific explanation describes a mechanism that produces that situation or phenomenon as a consequence of its operation – would be specified for the theoretical problem raised by Kovács and is now meaningfully delimited on the one hand from group A and on the other hand within group B. But this brings us to the next important problem of principle.

2.3 Problems of (inter)disciplinary demarcation

Indeed, one can only speak about complexity in a way that promotes intradisciplinary differentiation if one is aware of its emergence-theoretical implications. Kovács addresses the phenomenon, but he does not specify it. He calls for a "systems view" (588), but does not specify a concrete systemic level: i.e. the level of emergence. But only a meaningful concept of emergence can answer the question raised in the previous section, what exactly is the relevant object area and can it be conceptualised by means of complexity theory?[16] Emergence itself, however, is a traditionally controversial concept.[17] What would make sense? If we follow the confession of group B, then we should not, like group A, choose an instrument as a fruit from the tree of knowledge as a starting point and use a watertight emergence concept for all questions.[18] According to the confession of group B, it would be rather more sensible to formulate a common problem from which heterogeneous conceptions can take their starting

[16] Of course, this also applies vice versa to the concept of complexity, which is two sides of the same coin. Gräbner (2016: 5) thus formulates the same postulate: "If one advocates a complexity approach to economics, one must be able to answer fundamental questions such as 'What is complexity?' and 'What makes an economic system complex?'"

[17] See, regarding social emergence, the instructive volume of Sawyer (2007).

[18] In this respect, well-intentioned but nonchalant transmission attempts like of Kovács are not without danger.

point. This is therefore the crucial question for the basic research mentioned in the previous paragraph, whether it would be possible to define a real realm of problems of its own kind that justifies the use of complexity theory instruments for subquestions of economic science.

Can a sub-area of the question of behavioural coordination on markets be meaningfully delimited in a way that suggests a complexity-theoretical approach? After what has been said so far, it is not surprising that precisely these problems of delimitation with reference to the object area have always been a unifying element of "heterodox" approaches.[19] Has this broadened the knowledge of economic science, its function as an advisory science for (among other things) economic policy? Or does the discipline perhaps – as Kovács states – fail to remember because diversity led to disorientation? There can be no doubt that non-neoclassical thinking about "economic problems" has produced considerable success in explaining social phenomena. But a real breakthrough, or, again with Kuhn, a paradigm shift, has indeed not been achieved. Robert Sugden (1993: 399) once aptly attested – and this in the context of a heterodox mood of departure almost three decades ago (!) – in Hayek's case that the latter had a "hunch" that certain criteria, such as success or adaptation, must be part of every plausible theory. Hayek gave "hints of several quite different models of group selection, none of which has been developed."(ibid.). Sudgen was right, but that's where it ended up. Clear emergence-theoretical preconditions for what constitutes the object area are not formulated and are usually not problematised (but presupposed) for the object. This is not detrimental per se. There are good arguments for the view that a promising new approach for research requires a certain degree of fuzziness. But modern science cannot accept this. Even a plural economics in the sense of modern science is only possible if its object area or that of its sub-areas is unambiguous. An important characteristic of group B, to admit diverse instruments (or sub-disciplines of a science) for a unifying basic problem, is indeed justified; but its products, i.e. its conceptual systems, should not be less "watertight" for that reason.

This brings us back to the question of the extent to which the economic reality that group B claims as its starting point (and therefore criticises the actions of group A) can be understood as a sui generis phenomenon. We must try to be more precise: what exactly is the phenomenon that is "more than the sum of its parts" and with which the science of economics is most familiar, and what complexity-theoretical concept would be appropriate to explain it? Certainly, this area needs to be much more general than group A tends to be, but also narrow enough to draw a distinction with, for example, jurisprudence, sociology[20] or astrology[21]. By what does something become "more than the sum of its parts" – and of what kind of "parts" can we speak meaningfully? Thus, in the next step we must now devote ourselves more closely to the question of *emergence*.

3. EMERGENCE IN THE ECONOMIC SCIENCES

3.1 Two points of departure of economics as two problems of analogy

The notion of emergence usually refers to the appearance of new properties at a "higher" order or system level. (Hartig-Perschke 2009: 44). The familiar everyday-concept of emergence has

[19] See the footnote 12 on the diversity of approaches among evolutionary researchers.

[20] Kovács 2019 (588) points out that "formal and informal institutions as well as regulations" are relevant factors for economic policy considerations. A clear line to sociology is hardly possible here. See e.g. Fligstein (2001).

[21] An industry with an annual turnover of several billions; it influences the behaviour of billions; it is technically innovative. See for example Griffith (2019). Does it belong in the "socio-economic innovation ecosystem"? Kovács lacks a demarcation criterion.

already been mentioned above. In this case, the properties of a recognised phenomenon can no longer be explained solely by the properties of its parts or by the relationships of these parts to one another. An already long-standing theoretical tradition refers to this phenomenon. However, different disciplines conceptualise this phenomenon differently. In economics, too, the phenomenon has been around for several hundred years and was originally a possible criterion for differentiation from other disciplines.

As the metaphor of the "invisible hand" used by Smith, the phenomenon has gained its greatest popularity in economics. Research has traditionally been carried out under partly implicit emergence-theoretical conditions. The phenomenon of the economy can accordingly be understood as the reduction of the complexity of all possible social coordination actions to that which is relevant to the "system of the economy". This is where the higher level of order comes in.[22] The observation of a phenomenon of emergence as a consequence of the coordination of human behaviour on markets is one way of justifying the claim of economics as an independent science. Admittedly, this starting point is still far too imprecise and, as an investigative concept, is initially no more promising than that of a science of biology, which does not permit the distinction of organs within an organism. It should therefore be possible to specify criteria for differentiating between sociology and economics. Economists have, especially after intradisciplinary differentiation, repeatedly and explicitly dealt with the problem of emergence. F.A. von Hayek made a significant contribution to this, not coincidentally at a time when the systematic analysis of emergent phenomena represented a new scientific challenge. Hayek explicitly referred to insights of biology (see Hayek 1963, 18; 1967a, 292f.; 1978, 39).[23] One of his best-known writings, *The results of human conduct but not human design* (Hayek 1967b) shows this in a striking way by formulating a biomorphistic idea of economic reality.[24]

Hayek did indeed draw attention to the phenomenon of emergence in a way that shaped the history of theory, and it is no coincidence that he is still quoted today when "economics" is recommended to be more realistic. If arguments cannot be asserted for themselves, authorities are obviously needed. It has always been that way. One way or another, economics could not proceed to a uniform understanding of emergence. Parallel to the "economic approach" remained blurred ideas or methodological criticism on side stages. Complexity-theoretical considerations of economists (therefore) are often analysis in the sense of history of science (such as Metcalfe 2010, such as Kovács 2019) without achieving breakthroughs. This suggests that there is a not insignificant problem of demarcation from sociology and that the economics of our group B, if they do not want to be a sub-discipline of sociology, must provide justified added value. For this purpose they obviously have to work on the conceptual system.

Group A uses another way of defining its science. The point of reference is not the phenomenon of open-ended coordination in markets, but rather, normatively and taken from another science, the equilibria that arise in interactions. The difference could hardly be greater. The approach of group B formulates a category of a social phenomenon as a starting point for questions, the

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[22] Up to this point, Luhmann's systems theory provides us with all the necessary instruments. But this level belongs in its generality to sociology.

[23] Of course, among other things. The connecting points for analogies mentioned by Kovács can almost completely be found in Hayek's considerations, eighty years earlier.

[24] As Mundy (2011, p. 64) explains, the concept of biomorphism goes back to art historical considerations of the British anthropologist Alfred Cort Haddon. Remarkably, in 1895 he used the same pair of opposites as Hayek to define "biomorph": "The biomorph is the representation of anything living in contradistinction to the skeuomorph, which … is the representation of anything made …" (ibid.).

approach of group A formulates the goal of interaction according to already known (natural) laws.[25] This is counter-intuitive in every respect, but in itself not a bad quality in a theory. Either way, the representatives of Neoclassicism have succeeded in establishing themselves as the leading mainstream economic science, and one of the reasons (we are excluding the sociological and anthropological aspects) is that they are obviously *better* able to satisfy the criteria of modern science, even if the concrete answers of this science are nonsense in view of the reality perceived by group B.[26] But in this discourse, reality clearly does not play a major role for the science in question. Let us also call a witness with authority: "It is actually strange that people are usually deaf to the strongest arguments, while they always tend to overestimate inaccuracies in measurement" (Einstein 1969: 258, own translation).

However, to get back to the theoretical problems of group B. Invisible hand – that's all very well, but what is exactly the difference between economics and systems-theoretical sociology? One has the impression that a more extensive theorisation of the emergence phenomenon within the discipline is simply absent. One aim of the following considerations must be to examine the extent to which modern concepts of emergence that capture the phenomenon of complexity can be usefully applied to economic questions, i.e. questions to (now using an intentionalistic heuristic) the phenomenon of coordination on markets. Based on this, different sub-areas could then be classified. Of course, they then remain in group B.

3.2 Problems of adequate analogy for a complexity founded economics

Evolutionary economics in particular is often and explicitly orientated on the model of biology (see Witt 1992, 2006).[27] Keywords known from social philosophy and the discussion of evolutionary economics, such as "adaptation", "selection", "problem solving", or the "survival of the tried and tested" form plausible milestones here. In emergence theory, however, a whole series of significant differences between emergent phenomena of the social and chemical or biological systems must be considered.

For social phenomena, the spatial arrangement is irrelevant (Stephan 2011, 137f.). Phenomena of the social, understood as units, cannot be understood as "containers" in which individuals accumulate. The starting point of interest is much more complex (!), since individuals can belong to a multitude of structures and systems at the same time (ibid. 138) and the disposition of individuals can change over time. This situation is not only determined by the respective social systems, but people also determine independently to which system they belong (ibid., 139). The unbundling of selection levels in Hayek's pioneering approach has occupied generations of scientists. However, it is sometimes difficult to understand where evolutionary theory draws the line here. Herrmann-Pillath has to make use of different approaches of knowledge theory for his borderline work (cf. e.g. ders. 2008, 2010) and is (therefore) sometimes difficult to understand as an economist.

The picture becomes even more diffuse when one considers that individuals can also influence the spheres of interaction in which they are involved: "Interactions between the components of a social system (are based) on parameters that can in principle be modified by the members of

[25] It is therefore the reverse of what Kovács states: the "universal laws" are applied to an object area to which they have not been acquired. Of course, the authority of the laws passes to those who teach them.
[26] Or silence – as mentioned by Kovács with respect to the global financial crisis.
[27] The very name alone suggests this. See also Hodgson/Knudson (2004, 2006), Cordes (2006), Knudsen (2002).

the system or by a subgroup of them" (Stephan 2011, 139, here and in the following: own translation). This leads to the theoretical problem of *novelty*.[28] This is a central problem area in the efforts of evolutionary economics (see for example Witt, 2016). If one considers the differences to phenomena of chemistry or biology mentioned at the beginning, however, a new problem appears with regard to the emergent level: it is not possible to specify any parameters "…under which conditions the result of the transformation of a social microstructure should be regarded as a *change of the existing system* and from when on as the *genesis of a new social entity*" (Stephan 2011, 141, emphasis in original). For this very reason it seems worthwhile to take a closer look at individual criteria for emergence. The following section is devoted to this.

3.3 Sensible criteria for economic emergence – some implications

The extent to which emergent quality can be ascribed to a phenomenon depends on several criteria. With regard to social phenomena, different approaches can be distinguished. In the following, some aspects relevant to our question will be briefly reflected upon.

Sawyer (2005: 73ff.) distinguishes between "individualist" and "collectivist theories of emergence": *individualist approaches* assume an ontological irreducibility of the social. At the same time they explain social phenomena on the basis of human action (Hartig-Perschke 2009: 13). Accordingly, psychological aspects gain importance here. Sociology works partly with the concept of *non-reductive individualism*, i.e. the basis of an emergent phenomenon consists of individual actions, at the same time there is an autonomy of the emergent level with respect to this basis. This is one starting point to establish social sciences as independent sciences (Sawyer 2005: 189).[29] *Collectivist approaches* on the other hand, emphasise the ontological and epistemological irreducibility of the social (Hartig-Perschke 2009: 13). The two approaches indicate that differences in emergence theory approaches can usually be traced back to different views of the relationship between parts and the whole.

However, this problem of the relationship between parts and the whole occurs on several levels in the social sciences. Not only with regard to the relationship between emergent entities of the social and the individual, but also between the individual psyche and the material basis. The concepts common to the emergence discussion are found in the philosophy of mind (Greve 2011: 286). Emergent phenomena of the social type show their own dynamics. If social phenomena are attributed their own quality, the problem arises that such phenomena cannot be conceptualised independently of individual actors or their dispositions, decisions, etc. (cf. Hartig-Perschke 2009: 43; Stephan 2011: 139). Sociology discusses how the interdependence of these levels is to be understood under the notion "micro-macro-problem" (see eg. Pawlak 2018). With reference to the discussion, Heintz (2004) points out, however, *that social relations* represent still a third level that must be taken into account.

A distinction is made between *strong* and *weak* emergence. *Weak* variants of the concept of emergence assume that properties of the emergent level can be traced back to properties of the elements, even if they are not the same properties (cf. Greve 2011, 288). All *strong* variants

[28] With some good will, the aspect of innovation introduced by Kovács can be integrated here in terms of complexity theory.

[29] This is particularly attractive for sociology, because of the still relatively high degree of generality, see Greve (2011): 286.

assume that all phenomena can be completely traced back to physical properties. Properties of the emergent entity, which the elements also exhibit, are to be regarded as non-emergent properties. Furthermore, non-emergent are also those qualities which only have the emergent level, but which are achieved by aggregation or composition effects. If *neither of these* is the case, strong emergence is present. A systemic property can therefore be assumed that "…according to the thesis of *synchronous determinacy* it depends on the microstructure of the system *S* which it has, then *irreducible* and therefore *emergent*, if it cannot be deduced from the arrangement which the constituents have in S and the properties which they have in isolation or in systems different from S" (Stephan 2000: 39, emphasis in original).

Novelty plays an important role in concepts of emergence: certain constellations among elements give rise to new entities with new characteristics. Especially evolutionary theories of emergence emphasise the problem of unpredictability in this context (cf. Stephan 2011: 141). This concerns both the qualities of the emergent level and the respective structures: "The emergence of new structures is unpredictable when the formation of the structures follows the laws of deterministic chaos. Likewise, the possibly novel qualities instantiated by those structures are unpredictable" (Stephan 2000: 44).

What relationships of dependence can be described between the macro and micro levels or from the level of elements to the higher level? This can be described as the characteristic of *synchronous determinacy*. The systemic properties of an emergent entity depend on the microstructure of a system (see Stephan 2000: 37, Heintz 2004: 7). Here the complex interaction between elements is decisive, not their properties (cf. Hartig-Perschke 2009: 53). R. Keith Sawyer has a similar view with the concept of "supervenience": "The supervenience relation is asymmetric; an entity cannot change at a higher level without also changing at the lower levels, but an entity could change at the lower levels and retain the same description at the higher level" (Sawyer 2005: 66). The emergent level changes the elements, not vice versa. If systemic properties change, this is *always* due to a change in the properties of constitutive elements and to new forms of interaction (Hartig-Perschke 2009: 53).

The difficulty for the observer is that the macro properties of emergent entities can be realised in different ways. A clear relation of states on the respective order levels is then possibly not possible and with a larger number of possible variations increasingly difficult (cf. Hartig-Perschke 2009: 54). In this respect, the concept of emergence refers to a further dimension based on the relevant processes of the phenomena. In this respect, considerations of emergence theory must always include dynamic aspects; process and quality are simultaneously conceptualised in emergence theory (cf. Ellrich/Funken 1998: 354).

However, the processes that cause the macro properties cannot be sufficiently understood in some cases. The characteristic interplay between emergent and non-emergent levels can partly not be conceptualised. Moreover, this reveals a paradox common to all approaches to emergence theory: emergent qualities are dependent on microstructure in a certain sense. However, a derivation of emergent qualities from microstructure is not possible (cf. Hartig-Perschke 2009: 44). – Quo vadis, economics? Where does the examination of emergence-theoretical assumptions lead and to what extent does it help to formulate more appropriate economic theories? Is the object area of economics an emergent phenomenon? Does economics prefer a weak or a strong concept of emergence? Are there observable differences among subsystems?

First of all, it should be recalled that insights from emergence theory from other disciplines cannot easily be transferred to social science problems. We follow here Stephan (2011: 133) in summing up the problems arising in this attempt to three characteristics of social systems:

(1) *Different social systems are very difficult to distinguish and to typify*. We can safely assume that the economy is a social system; if one does not want to operate with the high degree of generality of sociology, this raises the question for our consideration whether it would not be more purposeful to speak of different economic subsystems. This is by no means trivial, because one would then be faced with the problem of whether an emergence-theoretical approach to phenomena such as the housing market, the financial market or the labour market has a greater capacity for explanation than other approaches. Moreover, this emergence-theoretically oriented approach would then also have to deal with the relations among these subsystems – and then be able to generalise further what "the economy" actually is. It remains questionable whether economics would then still be distinguishable from sociology at this level.

(2) The components of any social system show a high plasticity. Kovács (2019: 573) asks "what in heaven and hell is really going on" – this question will be asked even within a complexity-theoretically watertight approach! For if a derivation of emergent qualities from microstructure is not possible and if elements at the components at micro level potentially change their properties at any time, one will have to be constantly surprised by new complexity and emergence. As a reminder: Kovács (2019: 576) himself speaks of a "socio-economic innovation system". Whether one thinks in terms of individual economic sub-systems or leaves the explanatory approach at a higher level – it is precisely the fact that *competitors* are always faced and *should* always be faced with the question "what in heaven and hell is really going on?" Otherwise we would have a planned economy. However we approach it, "the economy" – as a whole or as subsystems – pushes the need for explanation forward and produces new, unexpected forms of emergence. Every firm can innovate. New firms can pop up. New markets can *emerge*. And a sideways glance at the natural sciences may well relieve the burden on the guild of economists.[30] Even if the Queen (one of Kovács's witnesses) asks emphatically, no oncologist would be able to tell her if, when and which cancer she will get. Nobody therefore would oncologists consider to be dubious. The question embarrasses the queen – not vice versa.

(3) Within the system the forms of interaction among the system components are modifiable. The fact that components can not only change themselves, but also change their behaviour and/or relationships leads to the same conclusion as point two. But, and this is why it is worth mentioning this aspect separately, it also leads to even more complexity! Because this point implies that certain constellations among elements give rise to new entities with new characteristics on the supervenient level. Let us dare to take the same side view into natural science again: what prognosis could our oncologist from the former section be prepared to make if he knew that the relations between the Queen's organs could change at any time? Correct – for one thing, it is very likely that the Queen will soon no longer be his patient. But, for another, perhaps an economist could give him a good guess. As soon as today.

[30] Provocatively formulated by Kovács (2019: 572): "The embarrassing question of the Queen of England 'why did nobody see the crisis of 2008 coming' emblematically signalled the failure of the collective imagination of the entire profession to understand the system and its emerging patterns."

4 CONCLUSION

Careful deliberation of necessary criteria for an appropriate concept of emergence reveal that heterodox economics largely operates with a vague one. From an emergence theory perspective, it is not clear within economics how emergence comes about, what *the whole* is and what its *components* are. One could, somewhat manneredly (and again with important witnesses), say that not "the true is the whole" (Hegel), but "the whole is the untrue" (Adorno). So can a complexity-theoretical approach help to achieve scientific progress? It still seems evident for the economic sciences. The considerations of this paper should at least have made it clear that an expansion of research in complexity theory will perhaps contribute less to answering the question of what we can expect reliable answers to from economics, but will allow us to specify more accurately what we cannot: more precisely, by clearly stating where only the unpredictable can be expected, due to adequately described emergence effects. This is progress, because although this position is more modest and unfortunately does not retain any of the stolen authority of natural sciences, it is better justified. According to Kovács (2019: 572), "Some questions are not only embarrassing, but also unanswerable." This is correct. Some questions cannot be answered without a crystal ball. In section 3.3, we have explained why, using emergence theory considerations, and have illustrated this using the example of one of Kovács's witnesses, the Queen. This is trivial and therefore shows how easily a correct explanation can transform seemingly deep assertions and deep misunderstandings into quite obvious things. Kovács's explanation that we lacked clear answers "due to our limited understanding of their embeddedness into the complex system we live in" is no longer needed now that we have clarified the conditions of this limited understanding. Thus, economics would better satisfy the requirements *for what is considered an appropriate answer to a scientific question*. We have thus clarified the conditions for what we have identified as the *first* important aspect of the potential of complexity theory considerations for economic science in section 2.1. It would be better able to prove its appropriateness to questions that need to be asked more precisely. Of course, we must continue to work on this justification, because it provides us with a more precise answer to Newton's quote mentioned at the beginning: what is per se unexplained in economics?

It should also be clear that the second important aspect mentioned in section 2.1 remains an important homework assignment for basic economic research: to provide proposals for mechanisms (concrete or conceptual systems) which, in their operation (functioning), produce all the phenomena involved in the question. Especially those scientists who are looking for valid explanations outside the boundaries of "economic imperialism", the "mainstream", should be very careful to ask meaningful and sufficiently precise questions. Section four should have made it clear that even for a (complexity-theoretically oriented) branch of economics, a lot of theoretical work still needs to be done, so that at least what can be said is said clearly and that the most explicit methodological instruments possible can become the basis for further, hopefully fruitful dispute about methods. However, one must be clear about one thing: if it cannot be clearly stated what form of emergence is involved, then it must be made clear that one can only continue to talk about *complicated* things and that the concept of complexity is going nowhere. Conversely: if the assumption of emergence is constitutive for the explanans, the concept of emergence must be questioned with respect to its consistency and appropriateness. This must be left to detailed individual considerations and cannot be claimed

as a revolution in complexity theory for the entire discipline. Paragraph 3.3 of this paper should have made this clear. We therefore advocate plural economics making reasonable use of complexity theory. At least a little improvement could perhaps be made by this, but the "framing (of) modern governance" by economics as a science of policy advice, as called for by Kovács in his contribution, should not be subject to any illusions of practicability, precisely because emergent phenomena of the social realm will generate novelty. Some economist should explain this to the Queen. Much will continue to be "merely" complicated and, in parallel, the consequences of complexity are not foreseeable. And we have not yet dealt with the deafness to the strongest arguments mentioned by Einstein. So let us look forward to the unexpected, to further basic research.

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