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System dynamics explanations as *mechanisms* and some implications for theory building

Author name: Camilo Olaya
Organization : University of St. Gallen - Switzerland.
Postal Address : Langgasse 49, EG. 9000. St. Gallen, Switzerland.
Phone : (+41) 071 244 5696
Email addresses **:** colaya@uniandes.edu.co
camilo_olaya@hotmail.com

Abstract

This paper introduces a framework to characterize system dynamics explanations. In order to accomplish this task it shows different ways to explain phenomena and underlines the tendency in management research to use mainly simple causality as the way to do it, illustrating this point with mainstream studies. Presenting the problems linked with causality the article looks for alternatives connecting ideas of Hayek and Russell regarding explanations and structures. Likewise, the paper presents some implications for theory building taking mechanism as a suitable characterization for system dynamics explanations.

Key Words*: philosophy of science, scientific explanation, structure, causality, epistemology, emergence.*

1. Introduction

The system dynamics conference of this year invites us to think about *collegiality*. The 'neutrality' of a system dynamics perspective against social theory is an opportunity to underline some of its benefits for different research programmes. System dynamics aims to answer *why* questions. This is done generally via the development of *dynamic hypotheses*. The main goal of this paper is to explore the positioning and characterization of this kind of scientific explanation. In order to accomplish this task it addresses what is known as the mainstream management research programme and its assumptions about explanations highlighting well known limitations. A system dynamics worldview encompasses a different kind of explanation whose benefits and possibilities for management research have yet to be contrasted and made explicit. Based on this discussion, the paper makes provocative suggestions about the possibilities of theory building. This task demands a different conception of knowledge. Indeed, appreciating the limits of reason, it implies the recognition that only certain kind of knowledge can be achieved, a kind of knowledge based on a structuralist worldview that should use proper approaches to explain phenomena.

John van Gigch, in his book about epistemology and the foundations of knowledge in scientific disciplines[1], identifies the 'neglect of epistemology' as the lack of concern by scientists for epistemological issues. He makes a logical distinction that is indispensable. On the one hand, *Management Science* is about solving the everyday scientific problems of 'normal science'[2]. On the other hand, the *Science of Management* is a metascience which deals with the epistemological issues of the management discipline, i.e. the reflection upon its own reasoning methods and sources of knowledge (van Gigch 2003). This article addresses first and foremost these latter kinds of meta-issues. The paper is organized as follows. After this short introduction, the second section introduces briefly the notion of scientific explanation in order to prepare the discussion about the assumptions in the mainstream management research programme about what an explanation is – fundamentally a causal argument - a task that is developed in the third part. The fourth section addresses some problems related to this causality view. The next section presents a general outline connecting some ideas of Hayek and Russell about scientific explanations in a structuralist framework. The characterization of a system dynamics explanation based on *mechanisms* and structures is presented in the sixth part contrasting the mainstream management approach and introducing some implications for the theories to be built. The last section summarizes the main ideas.

2. Scientific explanations

Fundamentally an explanation aims to answer queries of *why* in order to understand some phenomena in the world. In its more general sense, 'to explain' means to make clear, or to provide understanding (Salmon 1992). To answer this type of questions is perhaps one of the principal goals of science in general. However, this definition is still vague; although a full account of this epistemological concept is beyond the scope of this paper, it will be developed a little bit more by mentioning different kinds of explanations.

The deductive-nomological (D-N) or covering law model of Hempel is a common starting point in order to attempt to characterize what a scientific explanation is (Newton-Smith 2000a). This ideal model is based on deductive reasoning based on general laws and statements (i.e. *explanans*) that enforce, if they are true, to arrive to the *explanandum* - i.e. that which is to be explained (Hempel and Oppenheim 1948)[3]. The other "twin" model is known as the inductive-statistical (I-S) model in which the explanans confer high inductive probability on the explanandum (Salmon 1992), this is, to take the explanandum highly probable given the explanans which contain at least one statistical law (Hitchcock and Salmon 2000). This model introduces the issue of characterizing statistical explanations. A natural subsequent step is to consider the model D-N using statistical laws (therefore known also as the deductive-statistical or D-S model). There is also the mentioned I-S model. Hitchcock and Salmon (2000) provide an illustrative example that will be cited here: "We can explain why atoms of carbon 14 have a ¼ probability of surviving for 11,460 years because the half-life of that species is 5.730 years… We might explain why a particular weed withered by citing the fact that it received a dose of a herbicide, even though we know that the herbicide is not invariably effective" (p. 470). The first case is an example of the D-S model, the second one illustrates the I-S model. The latter model is more problematic because of the general problems associated with inductive arguments[4]; also because "high probability" is neither sufficient neither necessary for a satisfactory explanation (Hitchcock and Salmon 2000) and, furthermore, the probability associated with an event is not an indication of our degree of understanding it (Hitchcock and Salmon 2000). Salmon proposes statistical relevance (S-R model) as the key to explanatory relation - instead of high probability- and also he is the first one affirming that statistical explanations are not arguments (Hitchcock and Salmon 2000)[5].

The main critic to both D-N and I-S models is the lack of causal constraints in them (Salmon 1992). Based on this criticism various alternatives have arisen, for example the idea that one can explain phenomena identifying causes; this approach is known as the causal-relevance (C-R) model in which explanations are not based on arguments but on *causes* that are - at least partially - responsible for what we want to explain, no matter if the reasoning can fit or not in a D-N model (Newton-Smith 2000a)[6]. The association of explanation with causal explanation is common and widespread. However, there are other candidates that may account for reliable explanations. For example, we can explain things by identification, with models and with analogies. It has been suggested also that non causal explanations might be based on formal linguistics, laws of association, laws of co-existence, variational principles and structural laws (see Ruben 1990)[7]. In all of these cases, the explanations are fundamentally non causal regardless of having sometimes causal factors involved because the real explanatory work is not based on these singular aspects but on other essential explanans (Hesse 2000; Newton-Smith 2000a). Another non causal alternative is to expose underlying *mechanisms*, a way that fits better in fields like neuroscience, biology, cognitive science and psychology (see for example Brandon 1984; Machamer, Darden and

Craver 2000). An illustrative example of this distinction is provided also by Salmon (1992) as follows, a death explained as resulting from a cerebral hemorrhage is a causal explanation, the behavior of a gas in terms of the motion of constituent molecules uses underlying mechanisms as a way to explanation.

This is just an overview of the difficulty characterizing what a [scientific] explanation is. As it was shown, there is no agreement and no standard answer. The explanation issue is an open unresolved question in philosophy of science, as Newton-Smith (2000a) condenses: "While we have insightful studies of explanation, we are a very long way from having this single unifying theory of explanation…We would like to be able to explain what it is that leads us to count different explanations as explanatory. This task is made all the more pressing as most philosophers of science hold that a main task, if not the main task, of science is to provide explanation, whatever that may be" (p. 132). Nevertheless, in the mainstream of the management science programme there are some implicit answers to these questions, as the next section illustrates.

3. Explanations in [mainstream] management science

Many of the methods used in mainstream management studies share a similar base in assumptions concerning *explanations*, including both quantitative and qualitative approaches. This common ground is related to the primary role ascribed to *causality*. In the quest to answer queries of *why* it appears an implicit sort of determinism assumed in research.

As it was shown, there are several ways to explain phenomena. But it is clear in management science the emphasis on searching only for causality, there is a tendency to hunt no more than *causes*. A little review to mainstream journals can show this point. Sutton and Staw (1995) expose causal explanations as the way to answer *why* questions[8]. As well, Carlile, Christensen, and Sundahl (2003, p. 5) state firmly: "A theory is a statement of what causes what and why". This propensity is evident in many journal papers where the aim is to explain some variables or behaviors as *caused* by other constructs or variables. The essential basis for explanation and theory building is the causal relationship between constructs and variables and this assumption seems to hold for both qualitative and quantitative methods. Regarding qualitative studies, an example is in the well known work of Yin (1998) about case study research: "To 'explain' a phenomenon is to stipulate a set of causal links about it" (p. 252). Also Miles and Huberman (1994) in their work about qualitative studies emphasize "*causal description* of the forces at work" (p. 4) when answering *why* questions. Likewise, Snow and Thomas (1994) support this idea, "With explanation as a research objective, the investigator must address the issue of causality among variables" (p. 467). Moreover, the setting up of internal validity rests in a well supported relationship between constructs (Eisenhardt 1989). And in quantitative studies – understood in the mainstream only as statistics – this is underlined too, for example: "quantitative studies emphasize the measurement and analysis of causal relationships between variables" (Denzin and Lincoln 2000, p. 8)[9] . It is assumed that an explanatory answer aims to establish causal relationships between variables (Black 1999). And likewise as in qualitative studies, the criterion for internal validity is also based on these cause-effect relationships (Scandura and Williams 2000).

The causality based explanation usually addresses single events or variables and aims for an explanation based primarily on connections between them. The typical question concentrates on the effect or behavior of one or few constructs or variables Y_1, Y_2,…,Y_n in terms of other(s) [causes] variable(s) or constructs X_1, X_2,…,X_n linked by unidirectional causal links (see Fig. 1). Just to illustrate it, some random examples in the management literature of types of questions are: What are the determinants of power? (Finkelstein 1992). What are the factors for successful interpartner learning? (Hamel 1991). What are the determinants of absorptive capacity? (Van den Bosch, Volberda, and de Boer 1999). The search for causes is already embedded in the questions[10]. And the causal relationship becomes the source of the explanatory power (i.e. the *explanans*, that which does the explaining) and the base to build content based theories.

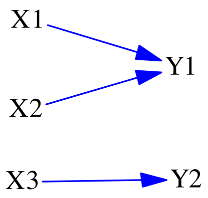

Figure 1. Simple causality

Two points are worth to mention. One is related to the ontic commitment to *entities*, a substantivalist emphasis is present in these causal explanations meaning with this that causes are exclusively entities or their properties (opposed to processes or activities) and it is the change in properties or entities the focus of analysis. But indeed, processes and activities could be better candidates for causes. This point will be developed later but for now a short example used by Machamer, Darden and Craver (2000) may illustrate the concern: “It is not the penicillin that causes the pneumonia to disappear, but what the penicillin does” (p. 6). The substantivalist weight helps to thinks and to develop theories based exclusively on entities and properties of entities, it is a primary feature of these kinds of theories. A process ontology puts the emphasis on activities that may be responsible for what we address. The second point to underline is the inductive nature of reasoning. The assumption of learning starting from particular cases or data with the aim to observe repeated empirical instances (i.e. confirmation) is a methodological pillar of these approaches assuming that tomorrow will resemble today in order to aim for predictive [substantivalist] theories based on statements in *forms* of laws. However, may be there is a different type of knowledge, more abstract, but more restricted, focused on *kind* of phenomena instead of phenomena (see the fifth section).

As it was stated in the subsection concerning explanation, there are several other ways in order to proceed to explain something. Although, as it was shown, there is no agreement on the nature of scientific explanation it would give the impression that in major trends of management studies is taken for granted the assumption that *the* way to explanation is *causality*.

4. The aspiration of causality

The previous section exposed the causality focus in management studies as a major way to explain phenomena – a sort of determinism - and some of its challenges. But, moreover, the concept of causality itself is very problematic. At least since Hume times we know this is a cumbersome topic. The framework of this overview will be the traditional distinction between the probabilistic and deterministic views of causation[11].

A deterministic approach assumes that a cause is always sufficient condition or a part of a sufficient condition for its effect, this *sufficiency principle* (very popular among scientists) is so widespread that might reflect a commitment to the pure strong deterministic thesis that *every* event has a sufficient cause (Hesslow 1981). This seems to be an implicit assumption in management science, at least observing its quest for causes. Somehow paradoxically, this deterministic assumption is also recognizable in the use of statistics (i.e. the search for causes in order to explain effects, see for example Hesslow 1981). One of the first problems with causation is illustrated with the *Humean fork*; based on observation of constant conjunction of events (altogether with temporal priority, i.e. the cause is observed prior to its effect) we can not suppose any causal connection between them; this old argument is still relevant and unresolved (see for example Humphreys 2000; Newton-Smith 2000b). Another challenge is overdetermination - i.e. “an event can have more than one set of independently sufficient causes” (Bunzl 1979, p. 134). Overdetermination cases are frequently taken as arguments to deny that sufficiency (i.e. any cause is sufficient to bring about its effect[12]) is enough for a causal relation to hold because we can not prefer a cause over others based just on observing constant conjunction of events (Humphreys 2000). But also the necessity criterion fails because many (or most) effects can be brought about in a number of different ways (Humphreys 2000).

Additionally, the identification of causal structures based on probability and statistics represents a huge puzzle. Concerning technical and practical aspects, Glymour (1998) addresses social science statistical methods (specifically factor analysis, logistic regression and multiple regression) and identifies major known problems summarized here: little may be known *a priori* about causal relations beyond time order, observed associations may be due to unmeasured or unrecorded common causes and in general all problems associated with "observation and measurement", the vast number of alternative possible hypotheses – larger number of variables - associated with causal structures, the problems associated with getting accurate representative samples, and the complications because of feedback structures. Glymour concludes that "without *a priori* causal knowledge, there is no way to get reliable causal information of any sort from multiple regression" (p. 20). He also questions the basis of factor analysis: "there is no proof of the correctness of any factor analytic procedure in identifying causal structure in the larger sample limit… There is not even a proof that the procedures find a correct simple structure when one exists" (p. 9). He points at the incoherence of pretending to deal with causal structures with tools that lack of formal language for causal analysis (for example opposed to algorithmic methods). The problem becomes worse if the question about where *a priori* knowledge may come from (for example in order to test hypotheses) is answered based on previous statistical based studies.

And about the very possibility of having characterizations of causal relations based on probability and statistics the situation is not better. The basic premise of a probabilistic theory of causation is that "causes raise the probabilities of their effects" (Hitchcock and Salmon 2000, p. 476). This can be summarized this way: Taking that A precedes B in time, A is a cause of B if the probability of B given that A already occurred is higher than the probability of B[13]. The problem is the issue of multiple causality associated with complex phenomena and the fact that probabilities do not enhance our true understanding of *why* things happen. After all probability is just a measurement of our ignorance, like in the quote of Keynes: "judgments of probability do seem to be based on equally balanced degrees of ignorance" (as cited in Settle 1974, p. 728). No ignorance, no probability. The problem related with induction can be illustrated with the next example. In the 19th century it was believed that malaria was produced by the "bad air" produced by swamps (an inductive argument), now we know that it is produced by mosquitoes (that live in the swamps and that carry the *Plasmodium* virus), but breathing "bad air" of a swamp free of mosquitoes will not 'infect' us in spite of the fact that malaria is more prevalent in regions where the air is bad which matches our empirical observations. (Hitchcock and Salmon 2000). An issue associated with this inductive nature is the classic distinction between correlation and causation. If both events A_t and $B_{t'}$ occurred it does not follow that A_t caused $B_{t'}$, even if smoking *genuinely* causes cancer, when both events occur it still does not follow that smoking was the cause[14]. As in the Humean tradition, we only witness constant conjunction - and this time it is even worse because now the probability assigned is usually less than 1. Hesslow (1981) recapitulates this point stating that "if the connection between cause and effect is essentially statistical, it will never be possible to justify an individual causal statement"[15] (p. 598). Additionally, he addresses the well known problem that, based in what we can observe, it is quite conceivable that some causes actually *lower* the probability of its 'effects'[16] because, simply, an event can have different effects in different circumstances[17]. In short, all of these issues are just the reflection of our ignorance (measured with probabilities) in order to accurately comprehend and understand (i.e. to answer *why* questions) phenomena. In general, it is well known the deficiency of causal explanations based on statistical methods[18] (Dowe 1992). Salmon (1994) condenses the debate: "In an illuminating discussion of the possibility of characterizing causal concepts in statistical terms…This enterprise is hopeless" (p. 301) [19].

Does science need causal concepts at all? The point of this section might be underlined with the famous remark of Bertrand Russell (as cited in Psillos 2002, p. 3): "The law of causality, I believe, like much that passes muster among philosophers, is a relic of a bygone era, surviving, like the monarchy, only because it is erroneously supposed to do no harm".

5. Explanations and structures: an inspiration on Hayek and Russell

Are there alternatives to causal explanations? And if so, what would be their implications? These are the motivating questions of this section.

The work of Nobel laureate Friedrich August von Hayek is well known in economics with his general evolutionary picture of society as a system of rules and his view of knowledge as essentially embedded in the values of a society constituted by agents that are not conscious of them (Langlois and Sabooglu 2001). His ideas include also a concern about philosophy of science, aligned in the path of Hume and Popper. As an introduction to his ideas it is worth to start with Hayek's suggestion that "students of social phenomena [should] confine their interest to the elucidation of patterns and other structures of order, abandoning as futile the aspiration to develop a genuinely predictive science of human behavior and social interaction" (as cited by D'Agostino 1992, p. 480). This does not mean a sort of relativism in his ideas. In fact he aimed to show the disastrous political consequences of the epistemological position of relativism because of the easy confusion with pluralism and tolerance (Milford 1994). As he wrote: "If whether a statement is true or false is no longer decided by logical argument and empirical tests, but by examining the social position of the person who has made it…[then] reason has been finally driven out" (Hayek 1944, p. 32). With these ideas in mind he searched for a consistent position between the complexity of social phenomena and the knowledge that we can attain.

In his critic to what Hayek calls "scientism" – the unscientific mechanical and uncritical application of habits of thought to fields different from those in which they have been formed (Hayek 1942) - he makes the sharp distinction of simple phenomena – as the one studied by natural sciences as physics - and organized complex phenomena addressed by social sciences. Hayek associates complexity with the multiple interconnected entities in social phenomena (Hayek 1955)[20]. For instance, complex phenomena that he takes in hand are the nervous system, the interaction of individuals in a social system, evolutionary phenomena and the marketplace in economics. The idea of physics-like predictive theories should be abandoned. Furthermore, for Hayek, concerning complex phenomena, simple cause-effect explanations do not hold. He states:

> *"it would then appear that the search for the discovery of laws is not an appropriate hall-mark of scientific procedure but merely a characteristic of the theories of simple phenomena as we have defined earlier; and that in the field of complex phenomena the term 'law' as well as the concepts of cause and effect are not applicable without such modification as to deprive them of their ordinary meaning… And the prejudice that in order to be scientific one must produce laws may yet prove to be one of the most harmful of methodological conceptions"* (as cited in Weimer 1999, p. 203).

Summarizing these first ideas of Hayek it must be underlined: the rejection of relativism, the inadequate aspirations of 'scientism' attitudes when trying to address social phenomena with simple phenomena studies assumptions, the abandonment of simple causal-effect reasoning, and the study of patterns and structures of organized complexity. Because of the huge complexity linked with multiple dynamic variables he suggests that the knowledge we can attain is inherently constrained. These issues require a different view of the type of explanations we look for when we deal with organized complexity. The next subsections will explore these issues.

Social wholes and explanation

In order to search for a different notion of explanation that can deal with the challenges exposed we have to develop a little bit more their connection with the issue of organized social complexity. A quote of Hayek is worth to make the point:

> *Where we have to deal with such social wholes we cannot, as we do in the natural sciences, start from the observation of a number of instances which we recognise spontaneously by their common sense attributes as instances of 'societies' or 'economies'… What we group together as instances of the same collective or whole are different complexes of individual*

> *events, in themselves perhaps quite dissimilar, but believed by us to be related to each other in a similar manner: they are classifications or selections of certain elements of a complex picture on the basis of a theory about their coherence.* (Hayek 1943, p. 43).

An inductive logic of research deals with single instances and attempts to confirm theories via repeated observation. Hayek points at the fact that this is a way to deal only with single events that maybe we can not observe again in complex frameworks. Fundamentally Hayek conceives *explanations* as modeling (Weimer 1999) and for social sciences he rejects the usual prediction and control aspirations and asks to focus more on models to explain typical processes (Milford 1994), he illustrates it with biology studies: "It deals with pattern-building forces, the knowledge of which is useful for creating conditions favorable to the production of certain kinds of results, while it will only in comparatively few cases be possible to control all the relevant circumstances'" (as cited in Weimer 1999, p. 202). Hayek calls it "Explanation of the Principle". Essentially he means the explanation of *kind* of phenomena instead of particular events. Furthermore, that's all we could achieve given the complexity of social phenomena (Weimer 1999). For example, the theory of evolution by natural selection does not aim at specific predictions of particular events and it is not based on hypotheses in the sense that the statements from which it starts are expected to be confirmed or refuted by observation, this theory explains *kind* of phenomena defined by general characteristics (Hayek 1955). Another example: "A set of equations which shows merely the form of a system of relationships but does not give the values of the constants contained in it, is perhaps the best general illustration of an explanation merely of the principle on which any phenomenon is produced" (Hayek 1942, p. 291). This analogy is a good illustration of the notion of abstract relations that would build an explanation of the principles.

This type of explanation is closer to *mechanisms* as the source of explanatory power instead of causality. The emphasis on *abstractness* and *structures* are key ideas in this type of explanations: "A spontaneous order is the reflection of an abstract system of relations among constitutive elements. Its existence cannot be discerned by simple inspection; our senses cannot take in. We can only mentally reconstruct it" (Langlois and Sabooglu 2001, p. 234). The example of evolutionary economics[21] may illustrate it. An evolutionary-economic explanation is an account of a fact of economic life that may be shown to include: a mechanism of preservation and transmission, a mechanism of variety-creation, a mechanism of selection and a mechanism of segregation between different populations (Andersen 1994). The shift of focus is on structures and the rejection of the aspiration to look for intrinsic properties. "Our physical knowledge is of the structural (rather than intrinsic) properties of nonmental objects known by description in relational terms rather than intrinsic properties known by acquaintance. All nonexperiental knowledge (theoretical, conceptual, commonsensical, etc.) is inferential and relational and is never absolute or intrinsic… [it] is, like all theoretical knowledge, a description of relations. We can never know the absolute properties of experience" (Weimer 1999, p. 224). The ideas of focusing on structures are not new. The next subsection relates these suggestions with the proposals of structuralist theories.

Structures

A view that has been gaining ground in philosophy of science is *structuralism* meaning here the view that there are underlying structures common both to the physical world and to mathematical systems (Brown 2000). The link with mathematics and with the ideas exposed earlier is natural. Hayek himself acknowledges mathematics as "the discipline developed to describe complexes of relationships between elements which have no attributes except these relations" (Hayek 1942, p. 273). To present structuralism in this sense demands to start with the ideas of Bertrand Russell and his theory of relations which "made them as respectable, if not more so, than substances and attributes" (Bradie 1977, p. 441). The structural postulates of Russell can be traced along with his dissatisfaction with empirism and his growing favor for logic. He stated in his seminal work on structural ideas *Human Knowledge* that "If – as I believe… – data, as well as results of inference, may be destitute of the highest attainable degree of credibility, the epistemological relation between data and inferred propositions becomes somewhat complex" (as cited in Bradie 1977, p. 462). The most relevant issue here is the possible implications of structuralism in epistemology and the growth of knowledge in science. Bradie (1977) asserts that “a careful appraisal and articulation of Russell’s structural analysis of scientific knowledge may be able to provide a model for the nature of scientific theories which will provide fruitful in dealing with problems about the growth of scientific knowledge” (p 462).

What is a structure? The basic meaning can be taken by its usual sense. Russell states that to exhibit the structure of an object is to mention its parts and the ways in which they are interrelated (as cited in Clement 1953). In a structuralist view of the world relations play the central role. This is the same claim of disciplines such as systems theory and complexity theory. The emphasis is on relations and on structures, not in the objects. Russell reminds us that we can find causal sequences in which the quality of events may change completely although the structure of the events remains constant (Bradie 1977). To illustrate the whole different view implied by these ideas it can be mentioned, for example, the proposal of Dipert (1997) who presents a formal metaphysical view of the world based on relations. In his paper – with the suggestive title: ‘The mathematical structure of the world: The world as a graph’ – he proposes *relationalism* grounding it on graph theory and showing that most phenomena in the world and mind are ultimately relational: “Every concrete entity ‘in’ the world is a part of this structure and is a structure (subgraph) in its own right. Such entities are individuated (and hence contemplated) solely by their graph-theoretic structural features” (p. 329)[22]. Therefore, we would not be interested in supposed intrinsic properties or qualities of objects because they are not relevant. An illustrative example can be taken from Hayek (1942) using a comparison about the ways in which we might *know* a dead language existing only in inscriptions in peculiar characters:

> *The combinations of different characters of which these inscriptions are composed and which are the only form in which the language occurs correspond to the different combinations of sense qualities. As we come to know the language we gradually learn that different combinations of these characters may mean the same thing and that in different contexts the same groups of characters may mean different things. As we learn to recognise these new entities we penetrate into a new world where the units are different from the letters and obey in their relations definite laws not recognisable in the sequence of the individual letters. We can describe the laws of these new units, the laws of grammar, and all that can be expressed by combining the words according to these laws, without ever referring to the individual letters or the principle on which they are combined to make up the signs for whole words. It would possible, e.g., to know all about the grammar of Chinese or Greek and the meaning of all the words in these languages without knowing Chinese or Greek characters (or the sounds of the Chinese or Greek words). Yet if Chinese or Greek occurred only written in their respective characters all this knowledge would be of as little use as knowledge of the laws of nature in terms of abstract entities or constructs without knowledge of the rules by which these can be translated into statements about phenomena perceptible by our senses*. (pp. 273-274).

Connecting with the explanations of the principles that we look for and the analogy with the set of equations mentioned in the last section, the point to stress is that the value of the models we might build rests entirely on their own because of their specification of abstract relations. The values of the variables – the objects with ‘intrinsic’ properties – are not the essential feature here. A step further would be to affirm that what is knowable of the world is its structure only, as in the ideas of structural realism (See for example Worrall 1989; Psillos 2001). This is linked with the emergent properties of complex systems that will be developed next.

Emergence: the only knowable phenomenon

Emergence is perhaps the distinctive feature of complexity. In spite of multiple characteristics attached to complex systems such as self-organization, far from equilibrium dynamics, non-linearity, feedback, delays, organizational closure, among others[23], the properties arisen in the dynamic interconnection between parts – know as emergence – can be highlighted as a key issue. For example, the well known work of John Holland on complexity – entitled “Emergence” – makes this introduction: “We are everywhere confronted with emergence in complex adaptive systems – ant colonies, networks of neurons, the immune system, the Internet, and the global economy, to name a few – where the behavior of the whole is much more complex than the behavior of the parts" (Holland 1998, p. 2).

To affirm that the only things we can know are structures is related with an epistemological view of emergence - and not as an ontological property of a 'complex' thing:

> *Emergent properties provide the identity of a system. They allow an observer to say 'this is a system of such-and-such and not otherwise' for they act as unifying reference points for the group of interrelated parts which constitute the system… This is an epistemological assertion which comments directly what is knowable about systems. It says, in effect that no matter the point of view taken when observing a system, the emergent properties of the system, acting as they do as illuminating reference point, exist above and beyond the manifold of points of view, are irreducible to such views and hence are on a different dimension to such views. Further, the potentially infinite points of view taken when observing a system all lead to one and the same set of emergent properties, a set which acts as a point of reference without which the particularities of a system are not identifiable, in a word, a set which forms the identity of the system. In general epistemological terms, this is to say that there exists a truth which is knowable and to which all points of view can, indeed will, adhere.* (Georgiou 2003, p. 242).

Therefore, emergence is not only some sort of property ascribed to 'a thing' that we label *complex*. Emergence is also the only knowable aspect of such a thing. The match between the ideas of Hayek about explanations of the principle and the structuralist ideas inspired in Russell is remarkable and, at the same time, natural. A last implication to mention here is for our theories. Grand theories in the sense of general statements based on some attributes, properties or characteristics of objects – in the abstract and general sense of the word - affecting in some way other objects, do not fit quite well in a structuralist view. We could only have structuralist theories i.e. theories that, focusing on relations, make assertions about the implications of this connections in the whole that they form and its environment.

This section introduced a lot of suggestive ideas. The look for explanations of the principle might be directed to the abstract emergent properties of structures in social systems. Focusing on relations we can have the basis for generating a different kind of knowledge, a knowledge not based on objects – whatever they are - or on their properties. This kind of knowledge would neither be based on unique events or instances because, using the picture of a graph, there is no point in addressing the nodes for the reason that the only knowable and relevant issue are the arcs, and this is the starting point to understand emergence which is the source of identity and also a major aspect of complexity. Therefore, there would be room only for structuralist theories. A next question would be: *How* should we address these issues?

6. A structuralist view: system dynamics explanations as *mechanisms*

The current approach to structures, relations, emergence and evolutionary processes uses computer simulation that is acknowledged as a better way to deal with complexity features (i.e. emergence, but also non linearity, non equilibrium dynamics, feedback, and so on) than traditional mathematical tools[24] - see for example Bar-Yam (1997), Sterman (2000), and Georgantzas (2002). Usually a computer simulation model includes multiple interconnections among entities and explores the change through time, i.e. evolution, of the system. A crude definition can be useful in order to make a start: "simulation means driving a model of a system with suitable inputs and observing the corresponding outputs" (Axelrod 1997, p. 23). Not in the narrow sense of 'a tool to solve mathematical problems' neither as a way to attempt – maybe naively – making predictions and forecasting, computer simulation is seen as a technique able to represent, communicate and test theoretical concepts (Liebrand 1998) and it has been used in social science for many years[25]. The emphasis on processes, on patterns of collective action and on the relations between components and its dynamic consequences (i.e. emergence) can be addressed via simulation because of its capacities to represent these issues with less restrictions than other approaches

(Gilbert & Troitzsch 1999). It is the essential method to study *mechanisms* underlying *emergent* properties of complex systems (Holland 1998).

Mechanism

In an earlier subsection concerning explanations *mechanism* was mentioned as one option to answer *why* questions. A quote to introduce the topic may help to frame a context: "Mechanisms are sought to explain how a phenomenon comes about or how some significant process works…To give a description of a mechanism for a phenomenon is to explain that phenomenon, i.e., to explain how it was produced… The explanation renders a phenomenon intelligible. Mechanism descriptions show *how possibly, how plausibly, or how actually* things work" (Machamer, Darden and Craver 2000, pp. 2-3, 21)

But, what is a mechanism? Lately this question has arisen again as a focus of debate in philosophy of science and has been proved hard to address. The proposal of Stuart Glennan can be a starting point: "A mechanism for a behavior is a complex system that produces that behavior by the interaction of a number of parts, where the interactions between parts can be characterized by direct, invariant, change-relating generalizations" (2002, p. 344), focusing on the *interactions* taken here as ocassions on which a "property change in one part brings about a property change in another part" (p. 344) by virtue of the first part capability to do so. Machamer, Darden and Craver (2000) add that they are not only inter-connected entities but also activities producing regular changes from initial to finish conditions[26]. In this sense they include three basic features to describe a mechanism: set-up conditions (as *part* of the mechanism, not inputs), intermediate activities, and termination conditions. They do a critic to Glennan's view arguing that the concept of *activity* is fundamental to understand the changes produced [because of the activities] through the process and not only as the black-box view of change of states or change of properties of the inter-connected entities. Tabery (2004) proposes to integrate these complementary points of view in order to understand what a mechanism is, underlining both features (i.e. the interactions among several parts and the activities associated with these interactions) as an account for a mechanism-based explanation[27].

System dynamics modeling may be one of the best ways to picture this kind of explanations. It is possible to distinguish two main components in the structure of system dynamics models: the physical and institutional assumptions – including the chosen parts/variables and the interconnections between them - and the decision rules of the agents (Sterman 2000). The multiple interplay between the physical structure and the associated decision rules as the explanation for behavior is one foundational aspect for the field. It can be said that the interconnections and the activities needed to account for a mechanism are included in this system dynamic models structures. Specifically, the activities producing change can be referenced in the links of the models and the decision rules describe how the interactions produce certain activities. The whole set of initial and final conditions and the inter-connected parts engaged in producing activities characterize a mechanism. For example, the simplest mechanism is perhaps a single feedback loop. The set-up conditions are the initial values of the variables involved, the termination condition is the endpoint when we do all the way through the loop until the first variable, i.e. the one that we picked as starting point, is influenced. As a mechanism this can be accounted as "the final stage of what is identified as a unitary, integral process" (Machamer, Darden and Craver 2000, p. 12). It is not an output, is part of the mechanism. The intermediate activities are depicted by the links and the application of the decision rules. It is suggested here a process ontology in which activities are the fundamental ontological units; the decision rules, and consistently with the physic structure, give the conditions and the directives to bring the activities out. The explanatory power arises only when the dynamics come into play (i.e. the loop process). Furthermore, a simple positive first order loop can produce exponential behavior beyond the values (i.e. intrinsic properties) of the variables involved[28]. More intricate structures are the source of different behaviors, i.e. the change in the values or patterns of variables through time[29]. This can be seen as an explanation of an abstract principle in the sense of Hayek. The intricate structure is the source of explanation of behavior (i.e. the change in the values or patters of variables through time). This is known in the field as a dynamic hypothesis (Sterman 2000). It can be stated that these kinds of explanations are based on *mechanisms* as explanatory power and not in simple causal relationships as the source of explanation, even less in [substantivalist] causality, i.e. change in singular properties/entities. Furthermore, these hypotheses are developed for each problem according with the structure in hand. This is why system dynamics is not committed to specific theories and only to the explanation of problematic behaviors in terms of structure. Following Lane (2001) "This grand claim of system dynamics is a structural theory — it makes a grand methodological claim about how certain types of phenomena might be explained" (p. 110).

Thus, it is proposed to label system dynamics explanations in the realm of *mechanisms* as the main source of explanatory power in theory building. The theories come from 'playing' with the simulation models in order to understand and describe the mechanisms responsible of the behavior based on the aggregated analysis of the dynamic relations between components and their consequences as time goes by with the aim of building dynamic hypotheses as the primary explanation for phenomena. In spite of its use of causal connections the *explanans* (i.e. that which does the explaining) is based on mechanisms i.e. dynamic hypotheses. Following Glymour: "Remains, however, a considerable bit of science that sounds very much like explaining, and which perhaps has causal implications, but which does not seem to derive its point, its force, or its interest from the fact that it has something to do with causal relations (or their absence)" (as cited in Ruben 1990, p. 212). Furthermore, the theories built with system dynamics are essentially structure-based and not content-based [substantivalist] explanations i.e. they are not associated with intrinsic properties of objects or entities but with the consequences of processes and activities entrenched in the models' structures. Grounded on a structuralist view, these mechanisms provide explanations of abstract principles – in Hayek's sense – that may deal with organized evolutionary complexity. In fact, the abstractness allows to deal with novelty, like in the ideas of Hayek: "such a position enables a society to cope with novel occurrences, to venture into the unknown and unexplored, because the reliance upon abstract principles encompasses the *possible* as well as the *particular* and is not limited to what has already been experienced" (as cited in Weimer 1999, p. 220). This is consistent with the purpose of system dynamics simulation which might be oriented to activities such as theoretical-representations building, articulation and testing in order to learn in and about *complex* systems (Sterman 2000).

This section presented an initial suggestion to characterize a system dynamics explanation as *mechanism*. The popularity of simple causality as the way to characterize the explanation of phenomena (i.e. mainstream management as an example) contrasts with the assumptions made by system dynamics field: structures that generate processes responsible for behavior. *Mechanism* is consistent with a structuralist view and suggests the development of structuralist theories and, at the same time, rules out content theories. Connecting the ideas of Hayek and Russell we can have a ground to develop these claims, and system dynamics suits this vision. But, above all – and following the statement of Machamer, Darden and Craver (2000, p. 23): "thinking about mechanisms gives a better way to think about one's ontic commitments"

7. Conclusion

This article explored the characterization of system dynamics explanations, some of the assumptions behind them and particular implications for theory building. The paper introduced briefly diverse types of scientific explanations. The classic D-N and I-S models were the starting points. Other kinds of explanation were also mentioned. Causality is one of the most popular strategies among scientists in order to achieve their tasks. The case of mainstream management research programme was exposed with its emphasis on causality as the way to do science answering *why* questions. However, there are several well-known puzzles associated with this conception that have been addressed in the philosophy of science discussions. Additionally, the social organized complexity that we deal with presents additional challenges for scientists. The ideas of Hayek may show an initial light on this issue. If we keep on mind the search for explanations of the principles (i.e. directed to kind of events) examining structures (i.e. relations) and not the parts *per se* we can deal with this complexity. This demands to locate the issue of emergence – arisen from structures - at the center of the stage. It was suggested that system dynamics may provide a different non-causal [in essence] type of explanation based on *mechanisms*, an idea that is grounded consistently with a structuralist view and that underlines the nature of the [structuralist] theories that we can only achieve when addressing organized complexity.

The characterization of system dynamics explanations as mechanisms may bring interesting issues to develop furthermore. Some of them will be mentioned next. A first issue is certainly the paradoxical statement that system dynamics explanations are essentially mechanism based and non causal based. The paradox naturally is because of the *causal* connections or *causal* diagrams that we label as intrinsic parts of system dynamics models. Two questions arise: what is the essential [ontic] nature of a causal link in a causal loop diagram? Are these mechanisms causal mechanisms? Some arguments can be outlined in order to attempt to address this issue. The first one is related with the meaning of a 'causal link' in a system dynamics model. Does it really stands for a *cause* or should we enforce the label 'influence link', for example, denoting the effect that the cause produces but without implying a cause per se? i.e. in different conditions the effect may change even may be not effect at all. This would rule out determinism, i.e., "the idea that the cause necessitates the outcome, that only one outcome is possible" (Ruben 1990, p. 35). The opposite view is that the effects depend on their causes but are not determined by them Ruben 1990, p. 36)[30]. So, in principle in system dynamics we would be dealing with nondeterministic causes; in this sense to speak of *influences* instead of *causes* may be more accurate; however this argument does not resolve the paradox, after all nondeterministic causes are still causes (the overdetermination argument exposed earlier does not do the job either for the same reasons). Perhaps a better argument may be found when looking for the essential *explanan*. The explanatory force does not rest only on causal relations per se (whatever they are) but on the intricate structures – physical and decision rules aspects – and the [dynamic] processes and activities that explain the change. If we take the mechanism as a distinct entity from a causal connection then that which does the explaining is certainly a *mechanism* and may help to characterize better (at least more completely) what a system dynamics explanation is.

A next major issue has to do with placing the spotlight in structure as the source of explanation of behavior thinking in abstract terms, i.e. *explanation of principles*. It is abundant the works based on case studies that are very valuable to understand specific phenomena and to address particular issues (finally this is what system dynamics is for). But the development of abstract structural theories may need more emphasis. The focus on understanding behavior in terms of abstract structures (for example dominant loop analysis or archetypes' characterization and formalization) must be a central line of inquiry in order to advance in understanding mechanisms as general explanations. Furthermore, should be the advance of system dynamics assessed by the progress in this type of analysis? This may imply also a deep reconsideration also for inductive-based methods that assume similar patterns in future cases bases on empirical observations of instances. The lines of Hayek may imply different kind of theories with different methodologies that look for more restricted but maybe better kind of theories, abstract principles that describe *kind* of phenomena and not phenomena per se.

A third matter of inquiry is about the nature of theories that we look for in complex settings. Is system dynamics suggesting that there is room only for process/structure theories excluding substantivalist entities/properties based theories? This has to do with the recognition of the complexity we face. Traditional explanations have been recognized as non adequate approaches, indeed this is one of the appealing issues of system dynamics. The inclusion of dynamic aspects that arise only in interaction and emerge as particular characteristics of the systems we deal with may be addressed better keeping in mind the search for mechanisms and not for maybe naive law-like theories. Furthermore, is it possible – using system dynamics - to develop or represent or test theories that place the emphasis in the entities/properties aspects and that develop causal explanations based exclusively on the change of this entities? Does system dynamics fit this approach? Assuming a structuralist approach it wouldn't seem so.

To go into the implications of these lines that concern to the *Science of Management* and the *Science of System Dynamics* is a worth path to follow and explore in full detail. As a final point, and retaking again a quote of van Gigch (2003, p. 217) we must not forget that, after all, “a discipline which neglects its epistemology, risks its own demise”.

Notes

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[1] Specifically he addresses the interface between social sciences, management sciences, engineering sciences and systems sciences.

[2] In the Kuhnian sense: processes of puzzle solving - performed in a particular paradigm, explaining it and expanding it (Kuhn 1962), i.e. making and getting more accurate estimates of existing knowledge (Wisdom 1974).

[3] Formally, the deductive argument takes this form:

$$\begin{array}{c} C_1, C_2, \ldots, C_k \\ L_1, L_2, \ldots, L_k \\ \hline E \end{array}$$

$C_1, C_2, \ldots, C_k$ are statements describing relevant facts, $L_1, L_2, \ldots, L_k$ are general laws, E is the explanandum statement. An argument is an explanation if and only if it is deductively valid given that $C_1, C_2, \ldots, C_k, L_1, L_2, \ldots, L_k$, are true (Newton-Smith 2000a).

[4] Particularly Hempel was troubled with the ambiguity derived from the lack of monotonicity of inductive arguments in the I-S model (i.e. the addition of a premise can invalidate the conclusion, see Hitchcock and Salmon 2000).

[5] Other alternative to the inductive model is the D-N-P model proposed by Railton that uses deductive arguments based on single case probabilities although it is neither a complete argument (see Hitchcock and Salmon 2000).

[6] The main critique to this view is associated with Hume and his view of causality as just merely constant conjunction (see for example Humphreys 2000, Newton-Smith 2000b and the discussion on causality below in the text).

[7] Ruben (1990) develops an argument based on two features of causation: identity (nothing can cause itself) and contingency (it is possible that the cause have a different effect, or even not effect at all, and the effect have a different cause, or even not cause at all), in order to show that not all explanations of singular facts are causal. Particularly he develops the first issue focusing on identities as sources for non-causal explanations.

[8] In their discussion they show the flawed propensity of authors presenting isolated references, data, variables, diagrams and/or hypothesis as sufficient *per se* for theory-building purposes. Some quotes may help to illustrate an assumed tendency for searching causal explanations as the conditions to build theory: “Authors need to explicate which concepts and causal arguments are adopted from cited sources… Observed patterns like beta weights, factor loadings, or consistent statements by informants rarely constitute causal explanations…the reporting of results cannot substitute for causal reasoning…Just like theorists who use quantitative data, those who use qualitative data must develop causal arguments to explain *why* persistent findings have been observed if they wish to write papers that contain theory…Theory emphasizes the nature of causal relationships, identifying what comes first as well as the timing of such events” (Sutton and Staw 1995, pp. 373-378).

[9] An illustrative example of this wide spread kind of work is the statistical based analysis of Coles and Hersterly (2000) in which they investigate the relationship between board composition and the independence of the board Chairman, for example they conclude: “Our results suggest that the independence of the Chair is a strong determinant of how the market views the adoption of a poison pill. The market takes a favorable view of firms that adopt a poison pill when they have an independent Chairman” (p. 211). This sentence illustrates the notion of causal explanation.

[10] Also in traditional economics this type of questions is frequent, for example: “If the Federal Reserve sells $50.000.000 worth of Treasury Bonds, will interest rates rise?” (Hoover 1993, pp. 705-706).

[11] Although deterministic causation can be seen as a special case of the probabilistic one (i.e. a cause gives a probability $p = 1$ to the effect). Sudbury (1976) develops a mathematical formulation of scientific explanations that are neither deterministic nor probabilistic.

[12] To be clear, this does not mean that every event has a sufficient cause (i.e. pure determinism), just that if an event has a cause, then it has a sufficient one (Hesslow 1981).

[13] More precise: “A_t is a *prima facie* cause of B_t, if and only if (i) $t < t'$ (ii) $P(A_t) > 0$ (iii) $P(B_{t'}/A_t) > P(B_{t'})$“ (Suppes as cited in Hesslow 1981, p. 591).

[14] This is also related to the logic of “inferences to the best explanation” in which the explanatory power of a theory is assumed as evidence for its truth. Clearly a self-validating statement (see for example Halfpenny 1987; Nelson 1996).

[15] Hesslow (1981) makes the distinction between generic and individual causal relations but, in spite of this, he arrives to the same conclusion.

[16] Eells (1986) develops deeply this issue.

[17] Succinctly, an example to illustrate this point is the use of contraceptive pills that according to some medical opinion sometimes causes thrombosis. Given that contraceptive pills also lower the possibility of pregnancy – which is a more efficient cause of thrombosis – it would seem that the probability of thrombosis is lowered rather than raised by the pill (see Hesslow 1981).

[18] Economic analysis illustrates also these troubles because of its intense use of statistics. For instance, typical problems with causal explanations in statistics analysis based economics are addressed by Hausman (1988) regarding the difficulty of the ceteris paribus clauses and its association with causation (for example, what if a ceteris paribus factor is cause of “the cause”?). Likewise the work of Hoover (1993) illustrates various problems that macroeconomic analysis faces in its typical studies that are based on temporal asymmetric causal explanations.

[19] The look for alternatives or coherent positions has been a major concern among various scientists and philosophers of science and has proved to be challenging, see for example Menzies (1989, 1996) and Dowe 1992.

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[20] Current complexity studies are based in the relations between multiple entities in which the number of variables itself is not the main issue. Although sometimes Hayek links complexity with a high number of variables his focus is also in the relationships and that's why his ideas are relevant for the discussion.

[21] Although *modern* evolutionary economics is developing and we can not talk about a general consensus these principles are shared by most of the authors of this field. See, for example, the work of Foster and Metcalfe (2001).

[22] In his paper Dipert makes a critique to the influence of traditional logical representation in 20th century science and proposes graph theory as a better way to deal with a structure-world. Specifically he proposes that one, symmetric, dyadic relation is necessary and sufficient for describing the structure of the world. For him a graph has the same compositional *feel* as the world and the *facts* or sentences of first-order predicate logic of logico-metaphysical analysis do not.

[23] See for example introductory works on systems and complexity science such as Flood (1993), Coveney and Highfield (1995) Bar-Yam (1997) and Sterman (2000)

[24] Because, for example, its capacity to deal with higher order differential equations and the use of numerical and algorithmic methods. Computer simulation models are not restricted to linear relationships or only to known mathematical analytical solutions - see for example Gilbert & Troitzsch (1999) and Sterman (2000).

[25] The first formal models can be traced at least half a century ago with the arms race model of Richardson in 1948 and the 'Models of Man' of Simon in 1952, based on a formalization of the theory of Homans of interaction in social groups. The late 1950s witnessed the first computer simulation models (Troitzsch 1998).

[26] Their focus is neurobiology and molecular biology. However they also assert that the approach "may well apply to other scientific fields" (p. 2).

[27] Erroneously, sometimes *mechanism* is identified with reductionism and opposed to holism. The work of Brandon (1984) clarifies these mistakes and shows in fact that *mechanism* leads to a non reductionist ontology.

[28] Taking S as the state of the system and g as the constant fractional growth rate, the linear first order positive loop in formal representation is : $\frac{dS}{dt} = gS$ and it has as solution: $S = S_0 e^{gt}$ (see Sterman 2000).

[29] This type of analysis is not straightforward and it is a developing field in simulation studies. See for example, in continuous simulation, the work of Richardson (1984) as an introductory discussion on dominant structures and the text of Sterman (2000) with several examples of this way of reasoning.

[30] More precisely, we can distinguish some level of influence that the cause may have on 'its' effect. Following Ruben (1990): causal explanations may be determinative, high or low dependent: "a determinative theory asserts that a necessary condition for a cause c fully to explain its effect e is that c physically determines or necessitates e. A high dependency theory asserts that…given the cause, its effect has a high physical or objective probability… A low dependency theory asserts that …given the cause, its effect has only a low chance or physical probability of occurring" (p. 36).

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