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Cratylus' silence. On the philosophy and methodology of Complex Dynamic Systems Theory in SLA

Abstract

Complex Dynamic Systems Theory (CDST) has received considerable attention over the last decades, inspiring a number of second language acquisition studies. This article examines the research from a critical epistemological point of view, starting from the Greek philosopher Cratylus, who concluded that remaining silent is the only way to be entirely coherent with the idea that everything is complex and dynamic. An alternative to this drastic conclusion may consist in 'saying without saying too much', that is, setting some limits to theorizing and empirical inquiry. Problems of description, prediction and generalization in a CDST framework are discussed, pointing to some open issues to be addressed by future research. Finally, some proposals are made for a more constructive research program, which may even involve abandoning the 'CDST' label.

Introduction

In recent years numerous articles have appeared that propose to apply Complexity Theory (CT), Dynamic Systems Theory (DST) or Complex Dynamic Systems Theory (in this article CDST will be used as an umbrella-term, following De Bot, 2017) to the study of Second Language Acquisition (SLA). Its proponents show great enthusiasm for this approach, considering it 'the dawn of a new era' (Dörnyei, 2017: 83), 'a radically new foundation for scientific inquiry' (Hiver and Larsen-Freeman, 2020: 288), that 'opened the door to reconfiguring the field's program of knowledge' (Hiver and Al-Hoorie, 2016: 743) and 'has the power to stimulate our thinking in new directions and to teach us new lessons' (Larsen-Freeman, 2017: 39). The emphasis is such that someone even talks about a crusade: 'Diane Larsen-Freeman started her crusade for complexity' (De Bot, 2017: 53). This article aims to offer a critical overview of the field, with an epistemological approach that starts from Cratylus, an Athenian philosopher whose radical interpretation of Heraclitean ideas is particularly relevant for understanding some consequences of CDST for SLA research.

Briefly stated, the main tenets of the CDST approach to SLA are that language acquisition and use should be seen as complex and dynamic phenomena, that is, produced by a myriad of interacting and constantly-changing factors that cannot be reduced to simple models. The first aim of research in this area is thus to provide accurate and fine-grained descriptions of this complexity, by using several verbal, numerical and graphical forms of representation. It is not clear, and this will be one of the themes discussed in this article, whether this approach is also willing to produce generalized claims and falsifiable predictions about the behavior of complex linguistic systems. Other issues that will be considered include the usefulness of (simple) models, even for CDST-inspired approaches; a discussion of what theoretical statements are original and informative and what are so uncontroversial that it is hardly necessary to repeat them; and how empirical research may be conducted in this area, finding a balance between the theoretical premise that everything is complex and dynamic, and thus irreducible to simple models, and the need to provide accounts of complex dynamic systems that are not limited to a 'chronicle' (Hiver and Larsen-Freeman, 2020: 287) of individual cases.

Heraclitus (5th century BCE) is considered to be one of the forefathers of the view that everything is complex and dynamic: his motto was *panta rhei*, 'everything flows', and one of his most famous aphorisms was that one cannot enter the same river twice, meaning that when you enter for the second time into what you think is 'the same river' the river is no longer the same, as its water, and many other things, will have changed from the first occasion.

Heraclitus had a follower, Cratylus, who is the main character of one of Plato's dialogues, entirely devoted to language. Aristotle wrote about him in his *Metaphysics* (4.5 1010a10-15; transl. Ross):

> It was this belief [that everything is in change] that blossomed into the most extreme of the views above mentioned, that of the professed Heracliteans, such as was held by Cratylus, who finally did not think it right to say anything but only moved his finger, and criticized Heraclitus for saying that it is impossible to step twice into the same river; for he thought one could not do it even once.

Cratylus thus reached the conclusion that in order to be truly Heraclitean one should not speak at all. The reason is that words themselves 'freeze' reality and offer a static, reductionist representation of it. Even the simplest sentence such as *Cratylus enters the river* abstracts away from innumerable details of this constantly-changing world. The very name *Cratylus* reduces the complexity of the spatio-temporal system made of his cells, atoms, actions, their changes and interactions, into a single, simple, static entity. The same holds for the river, and for the act of entering, which, represented by a single word, is a drastic abstraction over all possible acts of entering and their millisecond-by-millisecond deployment.

This intuition has clear affinities with Tao philosophy ('Those who know don't talk. Those who talk don't know'; *Tao Te Ching*), Zen Buddhism and various forms of mysticism, all asserting the infinite complexity and dynamicity of the universe, that human words and concepts limit and constrain, so that the only way to approximate the true essence of things is silent contemplation.

Models are useful: a plea for reductionism

CDST proponents often warn against the risks of reductionism and simplified views of reality (e.g. Hiver and Al-Hoorie, 2016; Larsen-Freeman, 2017; Larsen-Freeman & Cameron, 2008). However, in many cases reducing complexity and dynamism may have positive effects. Suppose you arrive in a city you have never visited before. Getting off the train, who would you prefer to meet: someone offering you a lecture on the infinite complexity of this city, its being a system made up of billions of particles in continuous motion, interacting with one other, and whose behavior can never be predicted exactly, or someone handing you a very simple map? In other words, the map is not the territory, but a reduction of it, and this is not a weakness of the map, but one of its strengths and design features.

Moreover, those who draw two-dimensional maps are aware that these are simplified representations of a multidimensional reality: the fact that they do not preface this remark to every cartography essay does not mean that they do not know it, but simply that they take it for granted. Likewise, those who propose models of learning, or anything else, know that these do not explain the whole process, but only parts of it. A simple model can be a very useful tool, in terms of theoretical insight and practical action, and some of the greatest achievements of humankind, such as Newton's laws or the periodic table of elements, owe their merit precisely to their simplicity.

Reductionism has been long debated in the philosophy of science and applied linguistics (see discussions in Bultè & Housen, 2020; Fulcher, 2015; Mitchell, 2001; Larsen-Freeman, 2017): while everyone agrees that representing reality inevitably involves some simplification, agreement is far from being reached as regards the optimal level of reduction, that is, finding the right balance between over-simplification distorting reality and under-simplification presenting an overwhelming amount of unmanageable information. CDST researchers, in particular, are aware that every time they describe and analyze a complex system and its evolution they are already caught in a 'performative contradiction' (Habermas, 1987), whereby what is done in practice contradicts what is claimed in theory. For example De Bot (2011: 126) notes that 'There is in a way a *contradictio in terminis* in using modeling in a CT/DST approach ... modeling inevitably implies a limitation on the number of variables we want to look at, which is a form of reductionism that a dynamic kind of thinking opposes.'

A first way of facing the dilemma is not considering it a dilemma at all. If one keeps in mind the difference between map and territory, one may agree with Heraclitus on a metaphysical level, recognizing that the territory is infinitely complex and dynamic, without following Cratylus on a practical level, and thus go ahead and produce more and more detailed maps. In this perspective, the study of complex dynamic systems is the continuation of a centuries-old research tradition. Scholars who seek to model complex systems in the natural sciences, such as biology or meteorology, do not accuse of reductionism those who, before them, described relatively simpler systems with simpler models, but recognize an intellectual debt and a substantial continuity of aims and approaches (Mitchell, 2011). In short, as medieval philosophers used to say, they seem themselves as dwarfs on the shoulders of giants.

Cratylism in theoretical statements

If one does not easily accept the idea of feeling like a dwarf on the shoulders of giants, but insists on being original and radically different with respect to previous science, another solution to Cratylus' dilemma could be to write much, in terms of words, while at the same time to avoid saying too much in terms of content. This would amount to producing admonitions about the risks of 'saying too much', or statements that are not falsifiable and/or have little information content, i.e. that are entirely predictable and do not generate any real controversy.

Affirming that everything is dynamic and complex is an example of a metaphysical statement that cannot be falsified and with a surprise value close to zero, given that it has been maintained for at least 25 centuries without anyone ever seriously contradicting it. As Van Geert and Steenbeek (2014: 22) note, 'That education is a complex and dynamic phenomenon is something that hardly anyone will deny', and the same may be said of language acquisition or anything else.

It is equally obvious that all entities are different, as the Latins already said *Si duo faciunt idem, non est idem* ('if two do the same thing, it is not the same thing'). It is thus not very informative nor surprising to read that 'no two brains are alike' (Schumann, 2017: 68); indeed, 'describing the ways "No two people are alike" is more the province of the novelist and the poet than the scientific psychologist' (Nesselroade and Molenaar, 2016: 397). Likewise, an SLA study concluding that 'even identical twins with similar personalities and interests who are exposed to similar input within the same environment may demonstrate different developmental paths' (Chan et al 2015a: 318) does not seem to generate much surprise: could anyone ever seriously argue that monozygotic twins, at a given time, produce sentences exactly of the same length, with the same proportion of subordinate and coordinate clauses, and that these values change in the same way over time?

Other statements found in the CDST literature on SLA that add little or nothing to what we already know are speculations about what might happen. The prototype is the famous butterfly effect, according to which a butterfly flapping its wings in Brazil *might* cause a tornado in Texas. Some examples from SLA research are: 'Does your grandmother's level of proficiency in French when she was young play a role in your learning of Swahili? Probably not, but it is possible' (De Bot and Larsen Freeman, 2011: 10) or 'Hubert's motivation might disappear overnight after an unpleasant experience and Geraldine might come across a language learning approach or methodology that suits her nicely and as a consequence may start making better-than-average progress' (Dörnyei, 2017: 82). The truth value of these statements is equal to that of 'the force of gravity may disappear tomorrow' and of all possibility statements: it is nil, for there is no empirical observation that can falsify them. In some CDST works such statements are particularly frequent: in two pages of the chapter by De Bot and Larsen Freeman (2011: 12-13) there are 16 'may' plus various occurrences of a hypothetical 'will'.

Similarly, it does not seem very informative to criticize, without *verbatim* citations, extreme claims that nobody seems to hold, such as 'the idea of fixed, predetermined stages as part of a grand scheme and single cause of development' (Lowie and Verspoor, 2015: 79), 'outcomes being driven by a single, linear, causal agent' (Hiver and Al-Hoorie, 2020: 72) or

‘SLA studies tend to see interlanguage as a fixed system’ (De Bot et al. 2007: 53). Also, noting that ‘*none* of the learners follow the trajectory of the ‘average learner’ ... an individual curve is quite different from a group curve’’ (Van Dijk et al., 2011: 69, 72), or that ‘the central tendency observed in a group may not be true of any particular person in the participant sample’ (Dörnyei, 2012: 4; quoted in Dewaele, 2019) or ‘wonder to what extent there actually exist “average” learners who develop in similar manners’ (Chan et al 2015a: 320) are not empirical discoveries or methodological puzzles, but mathematical truisms stemming from the very properties of the average, which, being a model, provides a synthetic summary of several cases, without necessarily matching any one of them. The fact that the average, or a regression line, do not correspond to any particular data point is not a limit of these relatively simple models, nor an issue to wonder about; rather, these models help us to solve concrete problems, like that of making sense of a number of sparse observations.

Finally, statements like ‘language development is essentially non-linear and difficult to predict’ (Lowie, 2017a: 5) or ‘not much about language development yields to a simple, linear, causal explanation’ (Larsen-Freeman, 2017: 34) or ‘there are always multiple interacting variables that make the process of development unpredictable to a greater or lesser degree’ (De Bot et al., 2013: 202), and other admonitions on the risks and difficulties of predicting and generalizing, don’t offer a particularly relevant contribution, as everybody is aware of these difficulties. The point is whether, in concrete research practices, these admonitions lead one to abandoning the endeavor or to looking for better ways of pursuing it.

Cratylism in empirical research

Even in empirical research, the CDST approach seems to follow a rather cautious attitude, a sort of ‘saying without saying too much'. In fact, the vast majority of studies have a descriptive character, whose main goal is to ‘chronicle’ (Hiver and Larsen-Freeman, 2020: 287) how different aspects of SLA evolve and interact over time.

What characterizes these studies is a relatively high density of data collection points, with intervals of a few days or weeks at most; some studies on motivation draw samples every few minutes or seconds (e.g. Waninge et al., 2014). The number of data points is certainly higher than in most non-CDST SLA studies, even though 10, 20 or 50 samples are still a strong reduction compared to a reality that varies at infinitesimal intervals. This difference thus does not justify the claim that CDST studies would be concerned with the ‘process’, while other approaches are interested in the ‘product’ (Lowie 2017b; Hiver and Al-Hoorie, 2020). In fact, all researchers try to describe and understand a process (second language development), and to do so they all look at some products, such as texts or reactions to stimuli. The main difference is that for most SLA studies 2-3 products collected at intervals of a few weeks or months are deemed to offer an adequate representation of the process, while CDST studies tend to collect a larger number of products at shorter time intervals.[1] The counterpart to this high number of data collection points is that the number of participants is limited: one or two, rarely more than ten, at least in the parts of the studies analyzing longitudinal data. In these studies, chronological and inter-individual variability is displayed by means of graphs, showing single data points to give an idea of the extreme intricacy of the trajectories, with the addition of trend-lines ‘smoothing out’ jumps between different points to make the general trajectory more readable, and oscillation bands to show how variance around central values can change at different times. Another way of bringing back some simplicity into these rather chaotic representations is to identify discrete stages within the developmental quasi-continuum made up of numerous

1 The oscillation between fine- and coarse-grained observational studies has been present in SLA research from the start. Most early studies in the 70’s were based on detailed longitudinal observations of very few learners; later on, larger samples were collected, which implied fewer colletion points, with the highest abstraction being reached by cross-sectional design on very large samples. Many researchers were also aware of the problem of representing an inherently continuous phenomenon like language development in terms of discrete stages (Sharwood Smith and Truscott 2005).

observations (e.g. Chan et al, 2015). Sometimes correlations between the trends of different variables are observed, or computer simulations are performed to test whether the variability found in empirical data is significantly different from chance. The number and type of observed variables, often taken from the Complexity-Accuracy-Fluency triad (including accuracy, a very traditional and rather questionable construct seeing interlanguages as more or less defective versions of target languages), is normally quite similar to that of other approaches (for reviews of methodological aspects of CDST research, see Bulté and Housen, 2020; Hiver and Al-Hoorie, 2020; Verspoor et al., 2011).

Although even studies with a high granularity inevitably simplify reality, they nonetheless try to maintain a fairly high level of detail and complexity in the representation of phenomena, and this seems to be one of their distinctive features. On the other hand, CDST proponents are aware of the criticism that is usually leveled at highly detailed descriptive studies: once we know that Jim's subordination ratio was 0.88 in the third week and 0.92 in the fourth, while Julia's was 0.76 and 0.89, what do we do with it?

In many areas of science that CDST calls 'traditional', description is seen as a first step followed by generalization and prediction (Jordan 2004). These further steps, however, cause considerable discomfort in CDST research and the positions on the subject are varied and not always clear. Describing the behavior of a single individual is already a reduction of complexity (because only certain aspects are reported, at certain times, with reifying categories), but producing general statements (valid for this and other individuals) leading to falsifiable predictions (valid for the cases observed and also for future ones) implies a further reductionist simplification. Some CDST authors seem to admit the possibility of taking these further steps. For example, Larsen-Freeman (2017: 34) claims that 'contingency does not preclude generalizing', Hiver and Al-Hoorie (2020: 67) assert that 'we are not just describers' and some of the methodological approaches reported in their volume, such as time series analysis or multilevel modeling, contemplate the possibility of making falsifiable predictions. For Lowie and Verspoor (2015: 80), too, 'based on DST, specific hypotheses can certainly be drawn up and falsified'.

On the other hand, the same authors in other passages seem to reach different conclusions: 'prediction is not what the dynamic approach is after' (De Bot et al, 2011: 2) and 'a DST perspective ... undoes the conventional expectation that a good theory is one that describes, explains and predicts. Description and explanation are possible, and these may be good enough. Instead of generalizable predictions, then, *we are content* to point to tendencies, patterns, and contingencies' (De Bot and Larsen-Freeman, 2011: 23; emphasis added). Yet in the very same volume one reads that the aim is to 'test the observations against chance ... [and] set up a resampling model based on a reasonable null-hypothesis' (Van Dijk et al., 2011: 77) because 'the goal of the model is not to describe but to test theoretical assumptions' (Lowie et al, 2011: 119). It is difficult to understand how assumptions and hypotheses may be tested, or tendencies and patterns be identified, if not according to their ability to predict results. Furthermore, it sounds rather odd that an approach aimed at expanding applied linguistics' resources and methodologies can be 'content' to do less than what is done in 'more traditional' research.

To address these dilemmas and contradictions, 'third ways' are sometimes offered that go beyond the binary logic of choosing between two alternatives, such as to predict or not, or to generalize or not. The way some authors present these alternatives resonates with Tao mysticism or Zen paradoxes, as in the following quotes from Larsen-Freeman (2017: 31-32): 'Paradoxes allow both members of a pair to be true, even when that seems to be impossible ... the dialogical principle allows us to "maintain the duality at the heart of unity"' (Cilliers and Preiser 2010: 273)'.

Using more standard scientific language, some have suggested that traditional 'statistical generalization' (Hiver and Al-Hoorie, 2020: 155) be replaced by 'theory-based generalization', in which 'data are checked against theoretical notions and the theory will be strengthened by data supporting it... This implies a soft approach towards falsification, in which single cases are not assumed to refute a theory completely, since there will be individual variation that comes into play' (De Bot, 2011: 126). However, it is difficult to understand how this theory-based generalization can ignore statistical regularity: if, as De Bot says, single cases cannot refute a

theory, couldn't this occur with multiple cases? And, at least from a Bayesian point of view, wouldn't a theory be 'more' strengthened if there are 'more' data supporting it? In sum, even a theory-based approach to generalization cannot easily do without statistical generalizations, and the issue of what evidence is necessary to refute a theory or a statement needs to be tackled more explicitly and rigorously than with figurative expressions like 'soft approach to falsification'.

Likewise, as an alternative to prediction, 'retrodiction' (Chan et al., 2015b) is proposed, which is essentially a form of explanation of what happened based on various theoretical models. It should be noted, though, that also in this case the theory on which the hypothesis is based must be general (*John did x because z* rests on the general assumption $z > x$). Retrodiction thus does not do away with generalization, although it certainly renounces prediction. Yet again, given that the latter is considered by many to be one of the most stimulating and useful parts of science, it is hard to see how such a self-imposed limitation may be called an advance.

Fear of knowledge?

On a theoretical level, therefore, CDST proponents seem to be very optimistic about its ability to provide new and better solutions to SLA research. On the other hand, many warnings are also formulated about the risks of generalizing and predicting, which result in empirical studies whose main finding is that individuals and their evolutionary trajectories are different. The risk is that of producing a series of descriptive 'chronicles' of the form 'sometime, somewhere, someone did something (and this was different from what someone else did)'.

All this seems to be a form of 'fear of knowledge' (Boghossian, 2007), which is one of the main features of relativism and postmodernism. CDST scholars are aware of this potential similarity (discussed e.g. by Larsen-Freeman, 2017 and Dewaele, 2019) and, while acknowledging that all these approaches challenge the reductionism of modern science, they reject the more extreme consequences of denying the existence of an objective reality, professing themselves mostly realists. However, the (partial) rejection of modern science may lead to abandoning some of its most interesting and productive aspects, namely the formulation of general models capable of predicting complex systems' behavior (Jordan, 2004). The point is not to say that these models, if they intend to adequately describe human behavior, cannot be monofactorial, linear and perfectly deterministic, because everyone knows this. The point is where do we go from here: do we strive to develop multifactorial, non-linear and probabilistic models with a better fit to the data than those currently available, or do we renounce the construction of general, predictive, and thus falsifiable models, ending up in the reassuring realm of metaphysical statements and retrospective descriptions of individual cases, both of which escape the risk of falsification and fallibility?

In fact, the explanation of a single event (its 'retrodiction') is unfalsifiable. One may say that the trend of a given variable was caused by, or correlated to, the trend of another, but in reality it could have been related, in a completely random way, to thousands of other data series, to the Brazilian butterfly, the French-speaking grandmother and many other factors, all potentially relevant. By saying 'at this time (and only at this time) A happened because B', one risks committing the *post hoc, ergo propter hoc* fallacy. Yu and Lowie (2020: 875) write for example: 'the relationship between [complexity and accuracy] changed from a clearly competitive relation during the early stage to a supportive relation at later stage. This shows that complexity and accuracy influence each other greatly, which is in line with CDST claiming that all factors within a dynamic system are connected.' However, the fact that two variables vary in different directions does not prove that 'they influence each other greatly'. Of course, if this relationship between variables were to appear systematically in a large number of cases, then one could formulate the falsifiable generalization that complexity and accuracy initially compete and then support each other.

At the present time, to the best of my understanding, it is not clear whether CDST-inspired SLA research is willing to explicitly formulate this kind of general predictions, even if they sometimes appear, more or less implicitly, at least in some authors. The following

statements may be possible candidates of such general and falsifiable claims, based on some citations in the literature.

- *Before an evolutionary change (e.g. the acquisition of a new linguistic structure) there is an above-average level of variation*. 'At moments of transition, degrees of variability are relatively higher, with significant developmental peaks in some measures' (Lowie and Verspoor, 2019: 202) 'An increase in the amount of variability is commonly followed by a developmental jump (Spoelman & Verspoor, 2010)' (Lowie, 2017a: 3; the problem is that the reference supporting the 'commonly' is a study of a single learner).

- *Learners with a higher level of variability initially or in the course of development reach better results in the long run*. 'Higher proficiency gains coincide with higher degrees of variability' (Lowie and Verspoor, 2019: 196). 'High initial within-participant variability tends to be positively related to subsequent learning. ... [L1] studies also showed that children who initially used a wide variety of strategies used more advanced strategies in subsequent tasks' (Verspoor et al., 2008: 229).

- *Different factors and variables interact with one another in systematic and predictable ways*. Besides Yu and Lowie's (2020) previous quote, see also a passage like this: 'there seems to be an interplay between higher motivation, higher aptitude, higher degrees of variability, and greater proficiency gains, but this will have to be investigated further before it can be generalized' (Lowie and Verspoor 2019: 202).

In order to contribute to a productive research program, statements like these should rest on explicit operational definitions of the constructs involved, rather than being formulated as vague metaphysical propositions, analytically true tautologies or admonitions on the difficulty of predicting and generalizing. For example, asserting that 'variability is a necessary condition for change to take place' (Lowie, 2017a: 4) or that 'the amount of variability will be relatively high when the system is reorganizing and low in a more stable system' (Lowie and Verspoor, 2015: 76) says nothing more than what one knows from the dictionary: change is by definition variation over time, and there is little variability in a stable system. Likewise, 'the cause and effect relationship between variation and change is difficult to interpret and is probably multilateral' (Van Dijk et al, 2011: 58) is a generic unfalsifiable statement. On the contrary, operationally defining high and low levels of variation, and how they may be systematically correlated to developmental jumps or high proficiency gains, may lead to a potentially interesting research program.

Similarly, statements like 'L2 development over time can be reliably studied by dense observations in individual cases, but will be unreliable for groups of learners' (Yu and Lowie, 2020: 858), 'the findings from individual cases cannot be generalized to a population of similar learners' (Lowie and Verspoor, 2019: 203) or 'it is difficult or even impossible to generalize about changes in the time dimension for groups of learners' (Lowie, 2017a: 3) all point to limitations in what can be said: the conclusion seems to be that evolutionary dynamics can only be studied for individuals, but there is no way to generalize them to groups, which would be a serious challenge for a field like SLA research. These claims are supported by quoting Molenaar's work, who discusses a number of problems involved in assuming that evolutionary processes are the same for all individuals. However, his conclusions are not a pessimistic retreat into the chronicle of the individual case, but on the contrary aim to build more adequate models, taking into account individual factors without giving up the search for general laws: 'starting with analyses of intra-individual variation does not preclude valid generalization across subjects .... In this way nomothetic knowledge about idiographic processes can be obtained' (Molenaar, 2015: 37; 40). 'From the standpoint of building a science of behavior that emphasizes the similarities in how people behave, we think a strong argument can be made for explanatory models that are common across persons' (Nesselroade and Molenaar, 2016: 409).

Conclusion: *sapere aude*!

After an initial phase in which theoretical statements and criticism of 'traditional' research prevailed, and a second phase producing a number of mainly descriptive empirical studies whose primary goal was to show that developmental trajectories are non-linear and differ across learners, CDST research is now at a crossroads. It may continue to 'chronicle' individual paths, explaining their causes by providing retrospective models, and with that remain in the safe territory of non-falsifiability. This would amount to a sort of partial Cratylism, where the fear of reducing complexity leads one to be very eloquent as regards single-case description, while remaining silent as regards prediction and generalization.

The alternative is to follow the Enlightenment's motto as formulated by Kant, *sapere aude!* ('dare to know!'). As De Bot and Larsen-Freeman (2011: 19) acknowledged, 'Predicting the behavior of dynamic systems [is] a risky business indeed': CDST scholars should state more clearly whether or not they are willing to take this risk, and what their specific contribution can be within a larger scientific community, including researchers as well as practitioners, that sees generalization and prediction as valuable goals.

Cilliers (2005) titled 'Against Arrogance' the final section of his article on 'Complexity, deconstruction and relativism', arguing that 'When dealing with complexity, modest positions are inescapable. This does not imply that they should be relative, vague or self-contradictory, nor does it imply a reason to cringe in false modesty. We can make clear, testable assertions about complex systems.' These lines suggest how CDST-inspired research may give a fruitful contribution to the SLA scientific community. Firstly, it is important to resist the temptation of arrogance: of course, the enthusiasm for one's own approach may lead one to emphasize its novelty and to add a rhetorical touch to the discussion, but this should not lead to caricaturing those who follow other approaches, to claiming to be the first to discover that language learning is variable and non-linear and to label as reductionist anyone who proposes models that try to simplify reality in order to understand it better. These attitudes were rather common a few decades ago, while they seem to have greatly decreased by now, which is certainly positive. This modesty on a theoretical level should be accompanied by greater ambition at the level of empirical research. Many CDST studies have provided useful methodologies for portraying variability in language acquisition, using graphical representations to visualize it and computational models to determine whether it is significantly different from what might be expected by sheer chance. This focus on describing variability naturally brings up questions about its explanatory role. In what sense does variability 'drive development'? How can this be demonstrated empirically, going beyond the truism that there is no development without change? Some of the hypotheses reported in the previous section ask precisely these questions: is it really the case that greater variability precedes developmental jumps? That it leads to better outcomes in the long run? That there are systematic relationships between different types of variability?

The specific contribution of the CDST approach to SLA research would thus be to investigate the role of variability in language acquisition processes, with a particularly fine-grained view. In order to be maximally transparent, one could even think of renaming CDST to something like 'microanalysis of variability in developmental processes', which would have the advantage of making explicit objectives and research methods, while at the same time renouncing terms like *complex* and *dynamic*, now overused and generic, and *theory*, which risks becoming a flag under which to fight not very productive academic crusades.

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