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► **To cite this version:**

Fabrizio Li Vigni. Regimes of evidence in complexity sciences. *Perspectives on Science*, 2021, 29 (1), pp.62-103. 10.1162/posc_a_00361 . halshs-03648769

HAL Id: halshs-03648769

<https://shs.hal.science/halshs-03648769v1>

Submitted on 21 Apr 2022

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1 **Regimes of evidence in complexity sciences**

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12 The author wishes to thank Sebastien Dutreuil, Franck Varenne and two anonymous
13 reviewers for their comments and advice.

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22 **Abstract**

23 Since their inception in the 1980s, complexity sciences have been described as a revolutionary
24 new domain of research. By describing some of the practices and assumptions of its
25 representatives, the present article shows that this field is an association of subdisciplines
26 laying on existing disciplinary footholds. The general question guiding us here is: On what
27 basis do complexity scientists consider their inquiry methods and results as valuable? To
28 answer it, I describe five “epistemic argumentative regimes,” namely the ways in which
29 complexity scientists argue the credibility of their research, and five “ontological views,” that
30 is the ways in which they interpret the material and formal causes of their study objects and
31 models. Finally, the article proposes the term of “regime of evidence” to designate the
32 specific combination of one ontological view with one or more epistemic argumentative
33 regimes.

34

35 **Keywords**

36 Complexity sciences, Santa Fe Institute, epistemic argumentative regimes, ontological views,
37 Science and Technology Studies, pragmatist approach

38

39 **1. Introduction**

40 From 1982 to 1984, a group of senior physicists from the Los Alamos National Laboratory,
41 the University of Illinois, Princeton, and Caltech gathered regularly in order to discuss about
42 their common objective of founding an interdisciplinary research institution. In 1984 they
43 decided to establish a private research center called the “Santa Fe Institute” (SFI) in Santa Fe,
44 New Mexico (Li Vigni 2020c). At the same time, they launched the “science of complexity”
45 conceived of as an overarching theory of all complex adaptive systems (Cowan et al. 1994).

46 While several researchers had already been talking about complexity and complex systems
47 both in Europe and the US since the 1970s (e.g. Nicolis and Prigogine 1989), the SFI has
48 played a fundamental role to institutionalize and publicize the field – which is more than a
49 sheer “catchy umbrella name”¹ and less than a discipline or subdiscipline. Complexity should
50 rather be described as an interdisciplinary and transnational association of subfields, whose
51 aim is to model natural and social “complex systems,” namely ensembles of heterogeneous
52 elements whose interactions produce emergent properties (Waldrop 1992; Mitchell 2009;
53 Cowan 2010; Williams 2012; Li Vigni 2018). Their main tools are mathematical, physical and
54 computational – i.e. dynamical systems, statistical physics, agent-based models, network
55 simulations, cellular automata, genetic algorithms, spin glasses, Big Data statistics, etc. Like
56 SFI founders, some observers of this field, both journalists and social scientists, have
57 qualified complexity as both a unitary and groundbreaking field. For example Waldrop (1992)
58 considered “complexity” as a “revolution in science as a whole” (p. 327). British sociologist
59 John Urry has talked about a “complexity turn” in knowledge (Urry 2005) and claimed that
60 “complexity sciences seem to provide the means to overcome [the] divisions between nature
61 *and* society and between the natural/physical sciences *and* the social sciences” (Urry 2006, p.
62 115). Historians Schweber and Wächter (2000) have also written that, thanks to the computer,
63 sciences have underwent a “Ian Hacking revolution,” which has transformed scientific
64 practices of several disciplines, created new scientific institutions and vehiculated a new feel
65 to the world. In their article, Schweber and Wächter have focused on one of the components
66 of such a revolution, what they call the “complex systems computer modelling and simulation
67 revolution.” While computer simulation gives a “new perception of the notion of scientific
68 model” by allowing an interactive and visual character (p. 606), the SFI is, according to them,
69 “perhaps the paradigmatic institutional manifestation” of such a revolution (p. 588).

¹ As a reviewer of this article has suggested.

70 Today only very few complexity scientists believe in the actual or yet-to-come unity of a
71 science of complexity. They are generally scientific entrepreneurs who struggle for the
72 institutionalization of the field (e.g. Bourguine and Johnson 2006; Weisbuch 2007). But the
73 vast majority of complexity practitioners and observers do agree on its internal socio-
74 epistemic heterogeneity. As for the novelty of complexity on the contrary, there seems to be
75 less discussion and critique. In the light of a careful analysis, observers' descriptions about the
76 revolutionary power of this domain seem to echo complexity founders' (often grandiloquent)
77 promises and claims according to which, because of its anti-reductionist, anti-deterministic,
78 post-Newtonian and holistic approach (Mitchell 2009), complexity science (in the singular)
79 was going to be the "science of the XXI century" (SFI 2009; IXXI 2005). Nevertheless, if we
80 adopt a pragmatist and Science & Technology Studies (STS) approach based on the empirical
81 analysis of scientists' practices, we are soon brought to confirm the intrinsic heterogeneity
82 and looseness of complexity as well as to contest its radical novelty. Its representatives rather
83 adopt a multiplicity of ways to accumulate scientific credibility in a plurality of subdisciplines
84 gathering together intermittently, in visiting institutes and conferences, under the generic term
85 of complex systems. Some of these subfields existed before the birth of the SFI, some others
86 have appeared afterwards but have mutualized epistemic tools, styles and frameworks from
87 established disciplines.

88 While several normative articles exist about complexity epistemology, works on what
89 complexity scientists actually do are rarer (e.g. Varenne 2012, 2017). This article offers to fill
90 the gap by proposing the concept of *regimes of evidence* as the articulation between one or
91 more *epistemic argumentative regimes* with one *ontological view*. The first term refers to the
92 ensemble of numerical, narrative, material and visual arguments that scientists mobilize in
93 order to increase the credibility of their research, by claiming the solidity of their methods or
94 by establishing an evidence. Five of these are described: the parsimony, the toy model, the

95 mathematical, the statistical and the proof of concept. The term of *ontological view* refers to
96 the conception of the essence that scientists have of their study objects, of the simulations
97 supposed to study them, and of the articulation between the two. Five of these are described:
98 the dualist, the agnostic, the homomorphic, the vitalist and the monist. Such categorizations
99 are the result of an inductive empirical and analytical work presented in the following pages.
100 The theoretical positions and the social groups of complexity scientists preexist the analysis,
101 while the analytical terms proposed to apprehend them come after it.

102 The general question of this article is epistemological: On what basis do complexity scientists
103 consider their inquiry methods and results as valuable? The method chosen to answer the
104 question is sociological: Based on empirical material, I propose to describe what complexity
105 scientists actually do instead of prescribing them what they should do. In the following, some
106 extracts of interviews, scientific texts and archives are gathered in order to show the
107 methodological practices of complexity scientists, and to deconstruct the claim of their
108 novelty. The second section presents the materials and methods, the third and the fourth are
109 dedicated respectively to expound the five *epistemic argumentative regimes* and the five
110 *ontological views* that operate within the subdisciplines gathered around the label of
111 complexity. The fifth section briefly discusses the concept of *regime of evidence* and in
112 particular the question of the possible postures that scientists can have in extra-academic
113 activities. Three configurations are thus described: the heuristic, the productive and the
114 consulting. The conclusion summarizes the main contributions of the article.

115

116 **2. Materials and methods**

117 The materials which this article is based upon are composed by 115 interviews with
118 complexity scientists from the US and Europe and some dozens texts of two types: scientific
119 publications and institutional archives.

120 Interviews were semi-structured – partly open and individualized, and partly following a
121 general framework. Such framework contained a dozen questions about personal pathway,
122 view of complexity sciences, main epistemological practices and methods, personal and
123 disciplinary ontological positions, definition of computer modeling and simulation, as well as
124 relation with policy makers, NGOs and private actors. In order to bring the interviewees to
125 make their *epistemic argumentative regimes* explicit, the questions focused essentially on the
126 way they validate models and conceive error. Similarly to what linguists and sociologists of
127 argumentation do when they classify arguments into general categories (assertoric, apodictic,
128 hypothetical, epistemic, normative, etc.), our take here is to propose some categories of the
129 ways in which complexity scientists defend their inquiry practices and results. In order to dig
130 up their metaphysical assumptions and to describe their *ontological views*, the questions
131 convoked Aristotle’s distinction between formal and material causes. If very few interviewees
132 were familiar with the Stagirite’s terms, all of them had either one or another of the five
133 postures described in section four. The categorizations proposed here were built along the
134 way of the fieldwork research, since the meetings with the scientists were dialogic
135 experiences in which to formulate and test hypotheses with the interviewees; the writing and
136 outreach phases have refined such categorizations. Now one may ask: What is the causal
137 relationship between the epistemological and the ontological levels? Science and Technology
138 Studies scholars show that epistemic, moral, material, technical and political questions always
139 imply ontological ones, and that holding an ontological view of an object always brings about
140 epistemic, moral, material, technical and political questions about it (e.g. Vermeulen et al.
141 2012). In other words, it is impossible to establish the priority of epistemology over ontology
142 or vice versa, for they are coevolutive and multifactorial constituents of the work of scientists.
143 Interviewed researchers were mostly confirmed ones, but roughly a third were either PhD or
144 postdoctoral. Among the scientists, about two thirds were men. The interviews lasted two

145 hours and a half on average (few were \leq one hour long and \geq four hours long), they were
146 always recorded and have been conducted most of the time in face-to-face settings within the
147 researcher's lab, more rarely elsewhere (classrooms, cafes or private houses), and in a dozen
148 cases by telephone or video-conference. The bulk of the interviews was determined by the
149 choice of the pivotal institutions taken as study objects – the SFI and the Parisian Complex
150 Systems Institute which depends from the French *Centre National pour la Recherche*
151 *Scientifique* (CNRS) – in order to explore the hub of the American and international
152 community on one side, and the hub of the French community (one of the largest in the
153 world) on the other side. The rest of the actors come from other laboratories inscribed in
154 complexity sciences: These institutions will be mentioned along with the disciplinary
155 belonging of the people cited. Other interviewees were contacted at conferences, seminars and
156 through the snowball effect, in order to have the widest diversity of disciplines, countries,
157 career degrees and institutions.

158 As for the texts, the scientific publications mentioned in the rest of the paper have been
159 chosen for their representativeness to illustrate the matter of each section. Secondary literature
160 can also be cited, when a specific subdiscipline or group of complexity specialists has already
161 been studied by other social scientists. Philosophical literature is mentioned to reinforce the
162 findings presented here, which can sometimes permit to interpret the former. As for the
163 archives, a support is particularly of help here: the SFI main outreach document. From 1986
164 to 2014, the American institute published 40 issues of its Bulletin. The articles it contained
165 were written by SFI staff members, resident scientists and freelance journalists. It was
166 addressed to the members of the Board of Trustees, the research officers, the SFI advisors, the
167 scientists, the donors (past and potential), as well as to university, industrial and governmental
168 directors. Its aim was to inform such a public about the scientific and administrative programs
169 of the institute. The Bulletin was published once to twice per year. Printed in 5000 copies, it

170 was available for free upon request. Later, its publication became uniquely electronic and
171 old issues were digitalized. In the mid-2010s, the bulletin was suppressed because it was
172 considered too expensive and less efficient than other supports. The Bulletins are an excellent
173 material in order to explore some of the main research projects, tools, frameworks and
174 protagonists of the American (and in some part international) complexity community.

175 The approach followed here is mixed. With regard to the general question addressed, the
176 matter is clearly epistemological, for it deals with the conditions of validity of statements,
177 models and theories. In what concerns the data collection and interpretation, the method is
178 sociological. Each quote relates to scientists' declarations of intent and methodological
179 practices or to their metaphysical views and assumptions. In both cases, citations have been
180 chosen in order to show the way complexity specialists defend the idea that their science is
181 heuristic and that it deserves credit. The answer to the epistemological questioning provided
182 here is pragmatic and interpretive. Pragmatist and STS approaches in sociology, which are
183 historically deeply intertwined, refer to "following the actors," to "taking the actors seriously"
184 and to "take the scientists as informers about whom to doubt" (Latour 1987, 1996; Boltanski,
185 1990). In France, American pragmatist philosophers (such as William James, John Dewey,
186 Charles Peirce and George Mead) have inspired an entire sociological family – alternative to
187 the hegemonic Pierre Bourdieu's one – which, according to two of its representatives, follows
188 a series of maxims such as: The "primacy of experience;" the processual and gradual
189 "dynamics of arguments formation" through abduction, experience and controversy; the
190 "primacy of reasoning through consequences" (for example, it is today cogent to study
191 conspiracy theorists and followers not because their theories are true, but because they do
192 have tangible social effects); the "orientation toward action" instead of privileging intellectual
193 essentializations; and the theoretical and political "pluralism" (Chateauraynaud and Debaz
194 2017, pp. 604-606; see also Barthe et al. 2013 for another presentation of pragmatic

195 sociology). These maxims do not imply the withdrawal of philosophical, sociological,
196 political and ethical critiques, but they do bring the social scientist to privilege – at least in the
197 first place – more a comprehensive than a normative approach to the study object. While
198 philosophers of science aspire to be “hybrids” and to contribute to the endeavor, practice and
199 advancement of knowledge (Grüne-Yanoff 2014), here the posture is descriptive and
200 empirical. The aim is to explicit and categorize the often implicit or unconscious ways in
201 which scientists fabricate the credibility of their academic internal and public work, starting
202 by what they actually do and say. As Chateauraynaud has efficaciously synthesized,
203 pragmatist sociology “consists in taking seriously the situations, devices and processes in
204 which people act and think, by taking a close look at their resources and constraints” (2003, p.
205 26, my translation). In this sense our approach here resembles to what some philosophers now
206 practice and call the “philosophy of science-in-practice,” in which thinkers “use empirical
207 methods drawn from the historical or social sciences (such as archival research, ethnographies
208 or interviews) to acquire insights into and evidence of scientists’ research behaviour”
209 (Boumans and Leonelli 2013, p. 260). One of the drawbacks of such approaches is that they
210 often step aside from the epistemological debate about complexity (or any other subject), for
211 they do not aim at adding another set of prescriptive tenets to the discussion. On the other
212 side, their advantages are to inform philosophers and social scientists about what scientists
213 think and practice on the ground and, by this very fact, to pave the way both to classical
214 normative epistemological debates and to the “philosophy-of-science in practice” approach,
215 which consists of being “directly engaged with scientific research through interaction with
216 scientists about philosophical problems [...] which often result[s] in joint publications and the
217 organisation of joint events” (ibid., pp. 259-260).

218

219 **3. Epistemic argumentative regimes**

220 Complexity sciences have been studied more by philosophers (Wimsatt 1994; Rescher 1998;
221 Cilliers and Spurrett 1999; Knyazeva 2005; Aziz-Alaoui and Bertelle 2009; Hooker 2011;
222 Fraioli 2012; Taborsky 2014; Zuchowski 2018) than by social scientists (Helmreich 1998;
223 Williams 2012; Li Vigni 2020b). To establish the five regimes listed in this section, I have
224 gathered the verbal and written testimonies from complexity scientists about the way they
225 arrange and mobilize data, models, graphs, objects and discourses in order to defend the
226 solidity of a dataset, a technique or a result. *Epistemic argumentative regimes* are indeed both
227 discursive and practical. Discursive elements are illustrated by the scientists' reflexive and
228 normative statements, while the practices by the examples they give of their methods of
229 validation. The regimes can cross and combine, some are operating more generally most of
230 the time, others operate specifically and intermittently. To put it in other words, the fact that
231 such regimes may overlap logically does not constitute a problem for the analysis, since this
232 one aims at establishing the presence, within the different subgroups of complexity sciences,
233 of different hegemonic styles of epistemic argumentation: Some put the accent more on the
234 development of the simplest model, others on the mathematical elegance of their theories,
235 others yet on the cumulation of statistical results. These regimes can also characterize one
236 subdiscipline more than another. It is in this sense that complexity is far from being a
237 scientific revolution, since it does not really propose a thoroughly new set of methodological
238 standards. These can be either very general principles in science like the *parsimony regime*, or
239 very old scientific practices such as the *toy model* and the *mathematical regimes*, or modern
240 practices linked to the *statistical regime*, which has at least three sub-regimes (the *gold-*
241 *standard*, the *Popperian corroboration* and the *backcasting*), or engineering and industrial
242 tools like the *proof of concept*, which is also present in economics, design, and so forth.

243 The following list does not pretend to be exhaustive but to show the internal diversity of this
244 interdisciplinary domain² and the preexisting nature of such frameworks and practices as they
245 belong to well established disciplines.

246

247 *3.1 The parsimony regime*

248 The Ockham razor – another name for parsimony – is admitted as a general principle of all
249 sciences, comes from medieval Christian philosophy and has been widely studied by
250 philosophers of science who have proposed different methodological criteria for a fruitful
251 usage of it (Hoffmann et al. 1997; Huemer 2009; Sober 2015). This general epistemological
252 principle is found at the basis of all modeling activity and consists in explaining a
253 phenomenon by invoking the smallest possible number of causes. If it is implicitly and
254 explicitly mentioned in basically any kind of “scientific” text, it becomes a constant worry for
255 many complex systems modelers, who often ask how much complexity they are supposed to
256 insert into their models to make them at the same time reliable and manageable (e.g. Pumain
257 1997; Batty and Torrens 2001). Such principle is often convoked along with logical, esthetical
258 and pragmatological corollary arguments such as “simplicity,” “elegance” and “efficacy” (e.g.
259 Holland 1996).

260 A computer scientist from the Parisian Complex Systems Institute illustrates the principle as
261 follows: “with very few mechanisms, I have to explain something which seems very complex
262 to apprehend. [...] the least mechanisms you have, the most you simplify the process, and the
263 more – I think – you can understand it” (interview, 13.02.17). As a bio-informatician from the

² One of the most important contributions of STS is the deconstruction of the positivistic account of science as a unitary approach to knowledge. Science pluralism has been variously apprehended by STS scholars through concepts like “epistemic communities” (Haas 1989), “epistemic cultures” (Knorr-Cetina 1999), “ways of knowing” (Pickstone 2001), “styles of knowledge” (Kwa 2011) and “epistemic commitments” (Granjou and Arpin 2015). Fewer works exist about interdisciplinary fields and their internal diversity. This article can also be seen as a contribution to the exploration of diversity in interdisciplinary fields of study such as complexity sciences.

264 Complex Systems Institute of Lyon explained, “what is important in a model is not what you
265 put inside. It’s what you leave out” (interview, 15.08.15). The approach of these researchers
266 consists in eliminating any non-indispensable hypothesis for the production of a pattern or
267 behavior that resembles the patterns and behaviors of the target system. Some researchers
268 from the same institute develop a platform or meta-model which, of specific models, “in the
269 end says: this parameter is useless. Or it’s useful, but only between this and this value”
270 (interview with a computer scientist, 13.02.17). What fascinates complexity specialists as for
271 the practical effects of such principle is its explicative power: “the phantasy we have is to be
272 able to say [...] that, if we leave out that parameter, it is compensated by other explicative
273 parameters which lead to the same dynamics” (ibid.). In other words, simplicity is both a
274 method and an end on the way toward efficient and reliable models.

275 Nevertheless, the frequency with which the Ockham razor is cited by researchers depends on
276 the situation. A group of computational epidemiologists from Paris based at the French
277 national health institute (INSERM) qualify parsimony as a contextual choice (Ajelli et al.
278 2010). Depending on the aims of a given research and on the data available, scientists choose
279 to apply a simulation approach instead of another. For example, agent-based modeling (ABM)
280 is very precise, for it can model a territory street by street, building by building and agent by
281 agent. But since such information is not always available everywhere and since to compute
282 such details for the entire globe is computationally prohibitive, ABM is (for now) applicable
283 only to a national or continental basis. It is generally privileged when the aim is to provide
284 Northern countries policy makers with accurate prescriptions, say, about which schools to
285 close where and when in order to avoid the spread of a contagious disease. But since network
286 meta-population models are applicable to the entire world (for they are less coarse-grained,
287 computationally affordable and less dependent on informational gaps), this kind of simulation
288 is privileged when researchers wish to predict the general dynamics of a pandemics country

289 by country (and not neighborhood by neighborhood). Interviewees generally agree on the fact
290 that the choice of model complexification is a matter of gradations, but a fixed point remains
291 even when the Ockham razor is not explicitly mobilized: A model which is not parsimonious
292 is simply not considered as a functional one because, if too many mechanisms are represented
293 into it, the scientist may become unable to distinguish which ones are the really important
294 ones in the understanding of the target system.

295

296 *3.2 The toy model regime*

297 According to one of the most prominent members of the SFI, computer scientist Melanie
298 Mitchell, there are four ways of doing science. Consider the following exert from one of the
299 SFI Bulletins where the researcher is interviewed:

300

301 Mitchell [...] explained that there are four approaches to science – theory, laboratory
302 experiments, Gedanken experiments (thought experiments), and computer experiments.
303 Computer experiments, she contends, allow the scientist to incorporate theory and thought
304 in a sort of digital laboratory. (SFI 1995, p. 4)

305

306 The thought experiment or, in German, the *Gedanken-Experiment* has an old tradition. Using
307 intuition, it considers a hypothesis, theory or principle for the purpose of thinking through its
308 consequences, without the need, or in the absence of the possibility, to back it up with an
309 empirical experiment. Historically, complexity sciences appeared when the computational
310 power of PCs was not as performant as today. Between the beginning of the 1980s and the
311 end of the 1990s, complex systems specialists used to work almost only on “toy models,” a

312 sort of computational instantiation of the Galilean-like thought experiment³. Lenhard (2018)
313 distinguishes the thought experiment *stricto sensu* from the simulation on the basis of two
314 criteria. Both explore hypothetical worlds, but while the first is “epistemically transparent”
315 because it takes place in cognition, the second is “epistemically opaque” because it makes use
316 of algorithmic procedures whose calculations are not always accessible to the modeler
317 (Lenhard on this point bases himself on Humphreys 2004): “Simulations are not opaque
318 because it would be unclear how one step follows from its predecessors. On the contrary, it is
319 the multitude of interrelated steps that can render the overall process opaque” (Lenhard 2018,
320 p. 489). Also, they lay upon two different kinds of iteration, the “convergence” and the “atlas”
321 type: the first “is involved in cases like exploring a new pathway that eventually becomes
322 your routine way. At the beginning, there is much uncertainty and back-and-forth, but after a
323 couple of repetitions the pathway begins to stabilize” (ibid.); the second “works rather by
324 exhausting the possibilities and thereby creating a compendium, or atlas. [...] Iteration here is
325 used for exploring a set of options under controlled variations, gathering the results and thus
326 obtaining an overview” (p. 490). According to Lenhard, “Thought experiments are tied to the
327 convergence type of iteration. When you perform a thought experiment, repeated execution
328 eliminates initial intransparency or ambiguity. Simulation experiments, on the other hand,
329 involve the atlas type of iteration. Repeated, and slightly varied, model runs do not eliminate
330 opacity, but rather explore the space of possible model behavior” (ibid.).

331 Nevertheless, in this text Lenhard treats simulation experiments as an indistinguished whole.
332 The “toy model” – an expression that comes from physics, definitely the dominant discipline
333 within complexity sciences (Deffuant et al. 2015; Grauwin et al. 2012; Li Vigni 2020c) – is a
334 specific kind of model which aims at exploring the causal reasons at work in a given

³ This is due to at least two socio-technical reasons. On the one hand, the lack of computer power and of theoretical knowledge for parallel computing; on the other, the lack of captors, efficient archival methods and statistical tools to analyze big data.

335 phenomenon in a very simplified and abstract way (Axelrod 1997). It is built starting from the
336 elements which appear to be essential to the working mechanism at the basis of a given
337 system. Moreover, this type of models is fabricated regardless of “real world” data, and
338 sometimes does not even have a concrete target, but only a “hypothetical” one (Weisberg
339 2013, p. 121). By means of this simplified procedure, scientists try to show that it is possible
340 to seize the general principles of a given phenomenon, reason why no realistic description or
341 prediction is ever pretended by researchers working on toy models. In the light of the
342 typology of explanations proposed by Grüne-Yanoff and Weirich (2010), toy models serve to
343 propose “partial explanations” which describe “the operation of some factors behind a
344 phenomenon’s occurrence” by isolating such factors (p. 38) and “potential explanations”
345 which make “more precise the possible worlds in which [a given] conjecture holds” (p. 43).
346 Ylikoski and Aydinonat (2014) go further in the analysis of this kind of models. They write
347 that while the “common objection is that [toy model] is too abstract to be explanatory,” they
348 defend it for it is “a device for thinking about theoretical possibilities” (p. 21-22). The
349 important thing for them is to consider such tools in the context of “a family of related
350 models, not as isolated representations” and “in the context of competing theories” (p. 23-24)
351 so to permit them to complement and correct each other. Their value would lay in the fact
352 they allow to formulate “possible causal scenarios” which can help “narrow the space of
353 possibilities, that is, to eliminate as many of the competing scenarios as possible” (p. 25-26),
354 as well as “causal mechanism schemes” which can constitute the “building blocks of causal
355 scenarios” (p. 28).

356 Among the first and most active epistemic communities present within complexity sciences in
357 the 1980-90s, one can mention complexity economics (Palmer et al. 1994), the physics of
358 materials (Stein 1989; Solomon and Shir 2003), computational social sciences (Epstein and
359 Axtell 1996; Dean et al. 1999), computational theoretical biology (Kauffman 1993; Holland

360 1996), as well as *Artificial Life* (Langton 1997). Despite the theoretical and methodological
361 differences between all these researchers, they share the fact they have worked, and
362 sometimes still work, with toy models. To illustrate the philosophy behind this practice, let us
363 consider an example from a complexity economist. The following interview extract is from an
364 American researcher who was among the first post-doctoral students at the SFI back in the
365 1990s. The fragment is interesting inasmuch as the interviewee contextualizes the toy model
366 into the larger spectrum of simulation styles in economics and compares it to the thought
367 experiment:

368

369 *Question: Is simulation a kind of experiment?*

370 Answer: Yes. Or really isn't it an extension of theory? It's a theoretical model. One of the
371 reasons why ABM got troubles in economics is that when you turn it on a computer,
372 simulation is the attempt to get really close to reality. Lots of people try to build realistic
373 models. In Washington there's people doing very detailed models on tax system. In these
374 complex models you're never going to understand what's the key detail. On the other side
375 you got these computer experiments, much simpler, toy models, not intending to be all the
376 reality. They try to understand the basic components. The problem with economists is they
377 believe that models should fit very well with reality to be good. And I don't think that's
378 right. ABMs should be treated as other pencil and paper economics models, which are sort
379 of thought experiments. (Interview, 14.09.16)

380

381 In other words, for some complexity scientists the toy model is the best way to push forward
382 the understanding of systems. Now, toy model users often mention the parsimony argument in
383 their texts and interviews. But to think that the two regimes are always and only present

384 together would be wrong. For example, the quantitative geographers and the computational
385 epidemiologists encountered in France and the US aim at complexifying their models as much
386 as possible, but they also constantly mention the need to remain parsimonious.

387

388 *3.3 The mathematical regime*

389 Within complexity sciences, the mathematical formalization is a very important tool of
390 inquiry and, for some, an objective in itself as for the power it bears in attributing credibility
391 to research. In one of the bulletins published by the SFI, one could read that the first president
392 George “Cowan’s vision [was] to apply the quantitative tools of the physical sciences, such as
393 mathematics, to the social sciences” (SFI 1994, p. 3). The motivation of such mission of
394 naturalization of social sciences lays on a positivistic conviction, condensed in Ernest
395 Rutherford’s famous sentence that Cowan cites, according to which “All science is either
396 physics or stamp collecting” (Birks 1963). At the beginning of the 2000s, Cowan led a project
397 of formalization of cognitive sciences (SFI 2002), while Murray Gell-Mann associated to two
398 linguists in order to quantitatively study the genealogical tree of human tongues (SFI 2004).
399 In SFI’s view indeed, non-mathematical social sciences are “pre-scientific,” so that only
400 numerical formalization can make of them “real science.” But humanities are not the only
401 target of this positivist project. The same attention has been given to life sciences, as it is
402 testified by the workshops organized to transfer numerical tools to biologists (e.g. SFI 2001),
403 as well as by the approach followed by certain complexity specialists seeking for general
404 theories. Take the study conducted by two ecologists and one physicist about the allometric
405 scaling of animal metabolic ratios: “They propose that the origin of this distinctive biological
406 scaling arises from the fact that cardiovascular systems, respiratory systems, plant vascular
407 systems, and insect tracheal tubes all exhibit the same fractal-like branching structure” (SFI
408 1997, p. 7). For certain complexity scientists, *mathesis* is definitely *universalis*. The

409 *mathematical regime* refers to all those who believe that numbers and equations are not only
410 working tools, but also the very structure of the cosmos, the altar of scientific evidence and at
411 the same time the only meaningful language to tackle their study object. In other words they
412 believe that scientists should mainly or only produce simple mathematical theories to describe
413 systems behavior. These researchers – often physicists, mathematicians and other disciplines
414 representatives with a strong training in numbers – use mathematics to argue the dignity of
415 their research, and sometimes the superiority of it compared to more discursive or
416 computational approaches.

417 Theoretical ecology and quantitative geography, today well represented at the SFI, inscribe in
418 such regime. According to the tenants of these fields, mathematics are important in the
419 context of theory. Such an aspiration comes from the physicist culture that, at one of the
420 founding meetings of the SFI, Gell-Mann famously synthesized as follows: “Surface
421 complexity arising out of deep simplicity” (Pines 1988, p. 3). Such way of thinking has been
422 shared by most complexity scientists, and the international members of the Network for
423 Ecological Theory Integration (NETI) – a group of ecologists from the US, Europe, Australia
424 and Chile – are no exception. In their view – explicitly turned toward physics –, science has to
425 produce not only hypotheses and local models, but also general theories, where theory is
426 defined “as a hierarchical framework that contains clearly formulated postulates, based on a
427 minimal set of assumptions, from which a set of predictions logically follows” (Marquet et al.
428 2014, p. 701). According to these SFI affiliates, 1) theories “should be built on first
429 principles,” the “bedrock of science,” in order to be “efficient;” 2) they should also “entail as
430 few assumptions and free parameters as possible” (see the *parsimony regime*); and, above all,
431 3) they should be written in mathematical language:

432

433 Mathematically formulated theories are generally preferable, because the logic is more
434 transparent, less subject to alternative interpretations, and more amenable to rigorous
435 empirical testing. Increasing the number of imprecise, qualitative terms and meanings will
436 do little to advance ecology, but using mathematics to reduce ambiguity will. (Ibid., p.
437 703)

438

439 The NETI common research efforts, embodied in several international workshops and some
440 collective publications, aim at an integration of several mathematical theories available in
441 ecology – such as the Fisher’s sex ratio theory, the optimal foraging theory, the metabolic
442 theory, the maximum entropy theory, and the neutral theory of biodiversity. All these bodies
443 of concepts condense in relatively simple equations the complex dynamics of ecosystems or
444 of parts of them. The challenge of the NETI group is to combine and unify them in an
445 overarching general theory of ecosystems.

446 Quantitative geography – our second example – is a “nomothetic” kind of geography, seeking
447 to detect general laws of urban development that transcend regional particularisms (Varenne
448 2017, p. 30). Once again, within this community physics plays a role model (Bettencourt and
449 West 2010). Some quantitative geographers are indeed ex-physicists that propose to do a
450 science of cities with the tools they learned in their precedent career, many others are trained
451 geographers with a strong inclination for quantitative tools who aim at making their discipline
452 “more scientific” by the yardstick of formalization (Cuyala 2014). SFI researchers apply
453 power laws to world city databases and argue macroscopic general properties of urban centers
454 independently of the continent, such as the fact that social phenomena increase at the same
455 path as the demographic expansion (Bettencourt et al. 2007; Bettencourt et al. 2010).
456 According to them, GDP, wages, art and patents follow the same scalar rhythm as the number
457 of crimes. On the other side, these scientists remark that basic needs like housing, electricity

458 or water usage stay proportionally the same – no matter the size and location of the city.
459 Thirdly, they highlight the fact that infrastructures increase proportionally less than the
460 increase of population, because of economies of scale which allow more people to use the
461 same infrastructures.

462 Some philosophical works have commented the intrinsic, often concealed, contradictions of
463 the complexity sciences epistemic project – officially anti-reductionist and anti-determinist,
464 but actually as reductionist and determinist as the “classical science” they say they wish to
465 counter (Israel 2005; Morin 2007; Li Vigni 2020a). In particular Israel (2005, p. 505) wrote:

466

467 there is no doubt that the mathematical-formal approach remains the heart of the
468 theoretical elaborations of the science of complexity. And this is precisely where the
469 ambiguity lies: since, in this way, the science of complexity runs the risk of becoming an
470 updated version of reductionism, that is, of the attempt to subordinate every aspect of
471 reality to the same interpretative key.

472

473 Exceptions must be noticed however. For example some researchers who inscribe in
474 complexity sciences write books about the impossibility of “putting society into equations”
475 (Jensen 2018) or criticize “the considerable influence exerted by physics in the domain of
476 complex systems modeling” (Banos 2016, p. 45).

477

478 *3.4 The statistical regime*

479 The toy model is opposed to what we can call the *simplified realistic model*, which is
480 theoretically more complex but still stylized and metaphorical (Edmonds and Moss 2005). Its
481 relative realistic significance concerns the material with which it is nourished – empirical

482 data. Yet, this type of model does not reach the morphological accuracy and the ambition of
483 exhaustivity that some recent simulations possess. In computational epidemiology for
484 example, complexity specialists propose accurate agent-based simulations of the entire United
485 States in order to predict the epidemic peaks region by region. In such a case we can talk
486 about *complex realistic models*, which are validated through more complicated
487 epistemological procedures making use of statistics. Graebner (2018) distinguishes between
488 *verification*, that is testing the internal consistency of a model, and *validation* in the strict
489 sense, which aims at testing whether the model reasonably represents the target system.
490 Statistics are important in both operations, because they are used to verify the model by doing
491 data sensitivity analysis, compensating the absence of data or exploring the possible variations
492 of each parameter taken alone and in interaction with other parameters, namely during
493 “sequential modeling” (building the model block by block in the perspective of an increasing
494 complexification); but statistics are also used to validate the model by fixing the free
495 parameters of a simulation starting from available data or by launching several runs of the
496 model in order to obtain a faithful representation of the target system. Today statistical
497 operations on simulations are run up to million times thanks to international (public and
498 private) grid computing. In general, complexity scientists using *complex realistic models* and
499 statistics aspire at predicting and providing “full explanations,” which entail to illustrate “why
500 a certain fact occurred at a certain time in a certain way, either by providing its causal history
501 or by identifying the causal relations that produced it” (Grüne-Yanoff and Weirich 2010, p.
502 37).

503 At least three sub-regimes can be identified within the *statistical regime*. Firstly, the *gold-*
504 *standard data sub-regime* (an expression that comes from medicine and statistics) concerns
505 the reliability of data: In order to establish their validity, scientists take as a comparison the
506 best available reference test, which is thus constantly updated and amended over time. In a

507 French laboratory of microscopic and computational embryology using ABM to simulate *in*
508 *silico* embryos, there are two sub-teams. One is focused on the biological part (caring for the
509 animals, leading the experiments, analyzing the data) and the other is focused on engineering
510 (developing mathematical tools, writing code, managing the microscopes). In this context, the
511 biological data used to feed the computer simulations must be validated upon gold-standard
512 data which, as the director of the laboratory explained,

513

514 is the perfect data set which is going to serve as a milestone, so that you can later compare
515 the efficacy of your algorithms. Other people call it the “ground truth.” This expression is
516 in use in computer science. It’s funny, because [in biology] we consider that what we can
517 do best is with our eyes, the expert’s eye. Yet, if you charge different people for building
518 the same gold-standard, they won’t agree on the result. (Interview with a biochemist from
519 BioEmergences lab at CNRS, 24.04.15)

520

521 Even if this researcher explains that in the biological community “it is customary to say that
522 the gold-standard must be validated by two independent experts,” she admits that “this is very
523 difficult to realize for material reasons and temporal constraints” (ibid.). The manual and
524 visual validation of a dataset indeed presupposes a long perceptual work that is extremely
525 energy and time consuming, for embryological datasets imply, during a 24 hours’ time lapse,
526 the displacement of thousands of cells that the researchers have to track down one by one.

527 A second sub-regime can be called the *Popperian corroboration*. It is well exemplified by
528 two computer scientists from the Paris Complex Systems Institute and one quantitative
529 geographer from Géographie-Cités (CNRS-Sorbonne, Paris), who have developed an
530 algorithm to fortify specific models written with different programming languages. Starting

531 from the epistemological assessment according to which the simple corroboration of models
532 is not sufficient to validate them (also known as *equifinality* [Oreskes and Belitz 2001; Beven
533 2019]), the authors appeal to an algorithmic technique to explore the space of parameters in
534 order to falsify models following Popper's prescriptions (Chérel et al. 2015). According to
535 these researchers, an algorithm that can produce unexpected patterns provides a method to
536 falsify a model and at the same time to generate predictions through it. The unexpected
537 patterns are sought by the algorithm

538

539 through the stochastic perturbation of the parameter values and through the pursuit, among
540 all possible parameters, of the different behaviors that I can have as an outcome. And if,
541 among these different behaviors, there are some I wasn't expecting, I can say they are
542 going to be either counter-examples, which allows us to bring down the model, or
543 predictions of the model which I can later empirically test or compare to some database.

544 (Interview with a computer scientist, 06.07.16)

545

546 The process is seen as iterative. Once a model is ready, scientists can explore the space of
547 parameters, then they can search for unexpected patterns in order to re-adjust the model, and
548 finally the process can start over again until they get to a model considered satisfactory.
549 Through their genetic algorithm, conceived to produce innovation, the three researchers
550 generate an indefinite variation of patterns in order to reinforce models by challenging them.
551 French sociologist of statistics Alain Desrosières used to describe statistical techniques as
552 "things that hold" (Desrosières 1998, p. 9). Inversely, the genetic algorithm of these
553 complexity specialists is conceived to explore the "things that do not hold" into the models.

554 A third sub-regime has a pattern-based validation and corresponds to what Graebner (2018)
555 calls the *descriptive output validation*, which consists of asking “to what extent the output of
556 the model can replicate existing data.” In the case of realistic models indeed, the exploration
557 of parameters is mostly done in comparison with the target-system data. The *backcasting sub-*
558 *regime* consists in considering a simulation as valid as soon as its output patterns resemble the
559 patterns that have been established through the analysis of “real world” databases. While
560 forecasting aims at predicting the future by means of the analysis of present data, backcasting
561 implies to test model hypotheses over a past event of which initial and final data are available,
562 in order to test both the “dynamical sufficiency” and the “mechanistic adequacy” of the model
563 – that is, respectively, the “model’s capacity to reproduce patterns observed in the target” and
564 “its capacity to represent the mechanistic structure of the target” (Graebner 2018). As
565 Augusiak et al. (2014) wrote, “Mechanistic modelling simplifies real-world processes to
566 understand driving mechanisms well enough so that forecasts of a system’s response to
567 certain conditions become feasible” (p. 118). For the scientists who work on explanatory or
568 predictive realistic modeling, this sub-regime is fundamental.

569 The term of backcasting appeared in the 1980s in the field of future studies, sustainable
570 development and planning in a normative fashion (Robinson 1982; Dreborg 1996; Holmberg
571 and Robèrt 2000). It is now used within complexity sciences by quantitative geographers and
572 computational epidemiologists, among others, in order to validate their models (Cottineau
573 2014; Van den Broeck et al. 2011). An ex-PhD candidate in quantitative geography from
574 Sorbonne in Paris efficaciously explained that “The meaning of backcasting is to verify
575 whether, starting from all the data we possess, we achieve to put together and reconstruct a
576 system of cities which is coherent from a holistic viewpoint” (interview, 13.07.15). A
577 computational epidemiologist based at INSERM of Paris illustrates this regime with an
578 example of model validation in epidemiological predictions:

579

580 We have done what-if tests, for example by simulating that the network is homogeneous,
581 and we have seen that the consequences would indeed be very different. We have validated
582 the model on the SARS epidemics in a historical manner. We have adjusted the model only
583 with data concerning Hong Kong epidemics. With this fit we have seen whether the model
584 was capable of reproducing the propagation at a spatial level. And it did, with a good
585 agreement relatively to our data. It is an easy technique that we put in place with post-hoc
586 studies, and by using already collected, confirmed, updated and validated data. (Interview,
587 24.07.15)

588

589 The match of the outcomes is not only numerical (statistical). It is also visual (graphical).
590 Philosopher Julie Jebeile has studied the way scientists draw on visual representations to
591 explain with simulations and gave three reasons for this: 1) the synoptic view allows to have
592 access to complex phenomena at a single glance; 2) images have a revealing power insofar
593 that they highlight the relation between variables which the opacity of models generally
594 conceal; 3) and they permit to single out relevant information out of lots of non-pertinent data
595 (Jebeile 2018). Besides the *synoptic*, *revealing* and *selective functions*, there is a fourth one
596 which can be called *pattern identification function*. As a computer scientist from the Parisian
597 Complex Systems Institute explained, to ensure that his colleagues from quantitative
598 geography validate a model it is necessary for them to see the “right plot” at the end of a
599 simulation run (interview, 31.01.17). According to the same researcher, his geographer
600 colleagues aim at “finding the rules which reproduce the paths they observe [in available
601 databases]; [for them] the knowledge is in the model and not in its specific outputs” (ibid.). In
602 both these subdisciplines, backcasting serves to validate the model, but while computational

603 epidemiologists wish to anticipate actual disease diffusions, quantitative geographers only
604 wish to “tell a history” based on a “theoretical foundation” (ibid.).

605 *Nota bene:* While scientists using the statistical regime employ mathematics of course, they
606 do not try to condense the entire behavioral complexity of a system into one simple equation
607 or set of equations. Mathematical formula are embedded into the several mechanisms
608 included into the simulation, but in this regime what is convoked to prove the validity of a
609 model is the statistical outputs of the simulation, not the mathematical equations producing
610 them. It is no coincidence if in the eyes of mathematical regime tenants simulations are
611 devalued. For example, the NETI group wrote that they see ABM “only as a stage in the
612 process of understanding that may lead to the identification of first principles and, eventually,
613 to the development of efficient theories” (Marquet et al. 2014, p. 707). On the other side,
614 complexity scientists’ use of simulation can be intended to overcome the impossibility of
615 formally mathematizing certain systems. In most cases, simulation tools are meant to bypass
616 the linearity and stationarity of equations in order to capture the non-linearity and the
617 dynamism of complex systems. For example, in one of his books, Artificial Life founder
618 Christopher Langton explained the interest of simulating life by agent-based models precisely
619 because coding allows to study systems that escape the mathematical harnessing: “it would
620 take tens to hundreds of lines of equations to express even a simple model of an organism’s
621 behavior [...] and there are simply no mathematical tools for dealing with equational systems
622 of that complexity” (Langton 1997, p. 5). In a position paper, a computational social scientist
623 from the SFI explains that ABM can be a way to solve unsolvable equations or to replace
624 equations at all: “there are important classes of problems for which writing down equations is
625 not a useful activity. In such circumstances, resort to agent-based computational models may
626 be the only way available to explore such processes systematically” (Axtell 2000).

627

628 *3.5 The proof of concept regime*

629 The “proof of concept” – or the equivalent formula of “proof of principle” – designates the
630 practical realization of an idea, a method or a process in view of demonstrating its feasibility;
631 it “describes research in the beginning stages, at the cutting edge of new applications or
632 technologies, and is a buzzword used to mark out scientific research as potentially extendable
633 and/or scalable” (Kendig 2016, p. 737). Proof of concept is a common approach in industrial
634 production, engineering and design, and is used for its relative inexpensiveness with the aim
635 of gaining confidence in the practical potential of a hypothetical scheme.

636 Interestingly, while most complexity scientists are driven by theoretical research questions,
637 some are (or collaborate with) engineers and industrials in view of the fabrication of artefacts.
638 In this case, they convoke the industrial or robotic feasibility of their models as an argument
639 for their validity more than mathematics or other criteria. Let us take the example of a
640 chemical engineer from the Center for Complex Systems and Dynamics, at the Illinois
641 Institute of Technology in Chicago. According to his account, he could never obtain the trust
642 of his collaborators if he considered his simulations as something more than a simple
643 exploration of potential “regions of interest.”

644

645 I collaborate with people who do experiments. I just try conditions in simulation to show
646 how blood vessels are being formed and, based on those results, they sometimes say, “We
647 didn’t think that that could have been something of interest” and they start thinking about
648 it. [...] The only way I could convince others to collaborate with me is when I tell them
649 that I respect the real experiments being the final judge. (Interview, 16.09.16)

650

651 The *proof of concept regime* is an empirical argumentative regime, where simulations are
652 seen as useful forerunners, but nothing reliable in and by themselves: “if the simulation was
653 as good as reality, many things that have been simulated and worked fine but fell apart when
654 tried in real life shouldn’t have fallen apart” (ibid.). This is a very different argumentative
655 regime compared to the precedent ones, where mathematics or simulations are considered as
656 having the last word: What constitutes a proof here is the material concretization of a
657 computational model.

658 In the swarm robotics stream of Artificial Life, the proof of concept is a necessary step before
659 realizing bigger developmental projects (Bonabeau et al. 1999). Artificial lifers promise a
660 variety of “applications” of their expertise to technology, management, production and even
661 art (Bonabeau and Théraulaz 1994). Today, the most advanced and spread implementations
662 are to be found in machine learning algorithms, as they are employed for problem solving and
663 decision making. If several representatives of this field patented some algorithms of this kind,
664 few have founded a company to develop personalized consulting services for other companies
665 with the aim of optimizing industrial production and business management. “Icosystem” –
666 founded in 2000 by French physicist Eric Bonabeau in Cambridge (MA) – is one of these. Its
667 domains of application are corporate diversity, product design, marketing, operational strategy
668 and network analysis. The company has worked for corporations from different sectors
669 (telecommunication, pharmacy) and for governmental agencies (defense, healthcare). In an
670 article entitled “A More Rational Approach to New-Product Development” (Bonabeau et al.
671 2008), Bonabeau and his coauthors highlighted the limits of the classical new-product
672 development that they qualify as a “monolithic process.” Alternatively, they propose to split
673 the process “into two distinct stages: a truth-seeking early stage, focused on evaluating novel
674 products’ prospects and eliminating bad bets, and a success-seeking late stage, focused on
675 maximizing the value of products that have been cleared for development” (Bonabeau et al.

676 2008). In the rest of their article, the authors tell the story of a device called Chorus – a unit of
677 R&D using a computer software provided by Icosystem –, which has been used by the
678 pharmaceutical company Eli Lilly with the objective of finding the best candidates for drugs
679 late-stage development, “in an environment where development costs and failure rates are
680 extremely high” and where less expensive and more traditional approaches cannot work. The
681 Chorus genetic algorithm is conceived to create a portfolio of molecules and test their
682 feasibility *in silico*. The evidence is first made with a “proof of mechanism” supposed to
683 provide “an acceptable therapeutic index, a practical dose regimen and a manageable degree
684 of [pharmacokinetic/pharmacodynamic] variability” (Owens et al. 2015, p. 19). Before the
685 phase III, which is dedicated to industrial development, the phase II has to provide the proof
686 of concept evidence: “Chorus defines ‘early stage’ as the work of determining proof of
687 concept (POC) for a drug candidate. Researchers must show [...] that the drug is likely to be
688 effective and not to have obvious serious side effects” (Bonabeau et al. 2008). As sociologist
689 Benjamin Raimbault (2018) summarizes, “The proof of concept is not uniquely the final
690 material realization of a project [...] but a way to reason and to experiment halfway between
691 science and industry, in which feasibility is the decisive criterion” (p. 280). Table 1 sums up
692 the five *epistemic argumentative regimes* and the hegemonic disciplines/subdisciplines in
693 each of them.

694

695 **4. Ontological views**

696 Philosophers of science have much debated about the relation between the world of the model
697 and the world of the target system (Gelfert 2017). The question they ask is, “How does
698 working with [models] tell us anything about the world?” (Morgan 2012). While some
699 scholars have defended the superiority of laboratory experiments over computer simulations
700 (Grüne-Yanoff 2009; Roush 2018; Symons and Alvarado 2019) and/or have defined the

701 simulation as an “argument” (Beisbart 2012), others have argued the equal epistemic dignity
702 of digital modeling (Humphreys 2004; Elsenbroich 2011) and either compared computer
703 simulations to “mind experiments” (Farge 1986) or to “simulated experiments” with the same
704 value as that of laboratory ones (Winsberg 2003). What about the onto-epistemological
705 assumptions of the scientists themselves? The pragmatist approach adopted here leads us to
706 examine their reflexivity about their scientific practices and metaphysical views. Similarly to
707 the first section, philosophers positions will be mentioned next to complexity scientists’ ones.

708 Since the beginning, STS scholars have struggled to show the complexity of the
709 sociotechnical world, the mixing up of nature and culture, science and technology, humans
710 and non-humans, theoretical and material practices, as well as the indissolubility of
711 epistemological, ontological and axiological spheres and dimensions (Hackett et al. 2008;
712 Law 2010; Woolgar and Lezaun 2013). This does not mean that, in the STS view, social
713 scientists have to explicitly analyze all spheres together all the time. If the epistemological
714 and the ontological questions are often separated by philosophers, here to talk about ontology
715 permits to better understand the different epistemological postures at presence in complexity
716 sciences. During the fieldwork research, it became clear to me that scientists could share the
717 same *epistemic argumentative regimes*, but still behave very differently as for the extra-
718 academic applications of their models and results. The codified and the “spontaneous
719 epistemologies” (Althusser 1974) of complexity specialists were not enough to explain the
720 observed differences in what concerns their attitudes about the scientific and sociopolitical
721 use of their simulations. This is because epistemological postures are deeply related to
722 ontological ones. For some interviewees, the computer runs in another dimension compared to
723 the “real world,” and the digital simulation can never be a proof of anything occurring in the
724 analogical realm. For others, the world of the computer and the world of the target system
725 share important similarities, or are seen as ontologically coincidental.

726 As the whole panoply of evidence production techniques that complex systems specialists put
727 in place comes from existing disciplines and consolidated epistemic practices, in the same
728 way complexity sciences *ontological views* come from existing disciplines, except one: The
729 *vitalist view* (section 4.3) in vigor within the community of Artificial Life (Helmreich 1998).
730 How many ontological postures are there? Hacking (2002) has highlighted the plurality of
731 ontologies at work in sciences, as well as their historicity. In an article published on *Social*
732 *Studies of Science*, Sismondo (2015) has added that the ontologies at work in sciences are not
733 individual and contextual perspectives, but world visions shared by entire communities that
734 are the result of material and sociotechnical practices coevolution. Such an anti-platonic
735 movement implies that the number of extant ontologies is not infinite, as Sismondo himself
736 explains: “a focus on interwoven practices that can be described on the pages of journals like
737 this one means that there will always be at most a handful of ontologies at issue” (ibid., p. 5).
738 The field of complexity scientists confirm such claims.

739

740 *4.1 The dualist view*

741 For the dualists, the digital simulation can only suggest possible lines of inquiry and help
742 pinpoint questions to ask and questions to discard. Within this framework, simulation is
743 purely heuristic – it does not prove anything. An anthropologist from the SFI illustrates this
744 ontology as follows: “I think [simulations a]re used in most of the cases, certainly in social
745 sciences that I’m aware of [...], to show the likelihood. And so they don’t prove something
746 but they show that a more abstract hypothesis is at least feasible on the ground” (interview,
747 23.09.16). The metaphysical reasoning here is based on the fact that physical, biological and
748 social realms are very different from that of ideas and computer simulations. The sphere of
749 symbols – which simulations are an instantiation of – runs on a parallel and separated rail
750 compared to that of the target system. The epistemological position that is linked to this

751 *ontological view* is prudential. If the two worlds never touch or cross each other, the computer
752 cannot say anything “true” about the system under investigation. In this sense, simulation
753 serves to test hypotheses, but more like a game than like a “synthetic laboratory,” since such
754 hypotheses have to be confronted with *in vivo* experiments.

755 Often, those who share this view come from life and social sciences, and use toy models. An
756 evolutionary biologist affiliated to the SFI told me he always reproaches his students when
757 they qualify their computer simulations as “experiments:”

758

759 I have a bunch of computer scientists in my group [...] [At] one of our group meetings
760 [somebody said] something like, “Last week I did this experiment.” And the first thing I
761 told him is, “Never call something you did on a computer an experiment.” An experiment
762 is something you do in the lab or in the field or in the real world. [...] the reason I say this
763 is because I want them to have the distinction between whatever they do in a computer and
764 what they do in the lab or in the field. Because if you go talk to biology audiences and say,
765 “This is an experiment,” they’ll just laugh you out of the room. (Interview, 07.10.16)

766

767 Similarly, a quantitative geographer from Sorbonne in Paris claims the following:

768

769 I think of [simulation] as a sort of heuristics, or in some cases even as a maieutic, for it
770 helps you give birth to new ideas. After that, you have to throw the model away [...] and
771 you have to step to a further level. [...] For me, we must avoid a reification of the models,
772 you see? A model is simply an intermediary step toward something else. (Interview,
773 25.04.17)

774

775 The *dualist view* can also concern new entrants in the field of computational sciences – for
776 they lack of confidence in the digital models –, or complex systems specialists at the
777 beginning of the history of the field – when simulations were not as accurate as today’s. For
778 the latter case we can think of the many artificial lifers, computational archeologists,
779 sociologists and economists that used to play with toy models back in the 1980s and 1990s.
780 Yet it would be a mistake to think that all toy model users are necessarily dualists. Some
781 scientists sharing other views use very simplified models in the await for more complex and
782 realistic ones.

783 Some philosophers have similar conceptions of computer models (e.g. Longo 2013; Opitz
784 2017). According to Oreskes et al. (1994, p. 641), “Models can only be evaluated in relative
785 terms, and their predictive value is always open to question. The primary value of models is
786 heuristic.” According to Morgan (2012) – who is probably the most explicit in such matter –
787 “by experimenting with the model, economists can gain understanding and provide
788 explanations of *how the economic world in the model works* and use these in an informal way
789 to *reflect on the workings of the real economy* that the model is taken to represent” (pp. 36-
790 37). According to her view, in the case of laboratory experiments in biology for example, the
791 deductive leap between the model species and the target species is lesser, since both share the
792 same material ontology. Even more clearly, she affirms that “The model world is at most a
793 parallel world. [...] economic models are only *representations of things* in the economy, not
794 the things themselves” (Morgan 2012, pp. 286-287).

795

796 4.2 *The agnostic view*

797 The scientists that hold this position have a pragmatic argument about the essence of the
798 target and of the simulated systems. Such an argument consists in claiming that simulations

799 sometimes “work,” and for this very reason it is useful to employ them in different situations,
800 no matter *why* they work.

801 Two researchers from the Los Alamos National Laboratory collaborating with computational
802 epidemiologists show such suspension of judgement about ontological issues. One is a
803 statistician and says that statistical results are more or less “defendable” depending on some
804 collectively established criteria (interview, 29.09.16). He is indeed conscious of the social
805 codes he needs to respect in order to “validate” a model in the eyes of the scientific
806 community: “Basically the way you judge disease models is whether their assumptions seem
807 reasonable, that’s one, and then two, do they fit the data” (ibid.). Yet, what is interesting for
808 him is to see whether the model allows the modeler to learn something: “is error sufficiently
809 small that you can still learn something from the model? [...] So if you have a specific
810 epidemic outbreak, can you reproduce that outbreak in your simulation?”

811 Analogously, his colleague from computer science thinks that the question of the
812 “truthfulness” of models does not even make sense. If the model reproduces the expected
813 pattern, this allows to continue to reflect upon the underpinning hypotheses, without asking if
814 there is an *adequatio rei intellecti* between theory and target systems:

815

816 *Question: Yours is then a pragmatic approach: You just ask “does it work or not?”*

817 *Answer: Right, exactly. So whether the model is true... I don’t know if that question is*
818 *even meaningful” (interview, 29.09.16).*

819

820 In philosophy of science, such a position is known as “pragmatist” and claims that the
821 “predictive power [of simulations is the] mark of success” (Symons and Alvarado 2018). Yet,
822 compared to the laconic interviewees cited before, who strongly avoid the “truth” question,

823 some philosophers propose a “pragmatic theory of truth and error in computer simulation,”
824 whose argumentative foundation is very loquacious and complex (Hubig and Kaminski 2017).
825 Both in complexity sciences and in philosophy, the agnostic/pragmatist position remains rare.

826

827 *4.3 The homomorphic view*

828 For the representatives of this view, the world of simulation and the world of target systems
829 do not share the same material cause, but do share the same formal one. In this view, the
830 ontological homology between the formal arrangement of the target system and the one of the
831 simulated system allows researchers to go way further than dualists and agnostic in the kind
832 of performative power they attribute to computer simulations. These are not seen just as an
833 heuristic, for they are taken as sound proofs of what goes on in the “real” world.

834 A computer scientist from the Parisian Complex Systems Institute claimed that, since the
835 target system and the simulation model share the same structure, “to work on the model
836 means working on the real system” (interview, 03.02.17). But the clearest illustration of the
837 *homomorphic view* comes from a computational epidemiologist based at the INSERM:

838

839 [The simulation] is an abstraction of the form. If a real form exists, the form of the
840 simulation is an abstraction of the real form. [...] [In the modeling process you have to]
841 lose some details [...] some mechanisms [...] that you don’t consider as indispensable in
842 the process [...] What I capture with [my model] is the set of ingredients I believe have an
843 important role within the epidemic propagation phenomenon, and they are *real* ingredients
844 [...] we write [mobility, gravity, diffusion] laws [...] and the starting hypothesis is that
845 these laws [...] are a precise and reliable description of what’s going on in reality. [...]

846 When [the simulation] works, it means that the phenomena I have captured within it are the
847 real ones. (Interview, 09.05.17)

848

849 As Israel (2005, p. 504) noticed, “The success of physics is due to the fact of having chosen as
850 its guiding principle the Galilean approach of ‘pruning the impediments’ [...] that is the belief
851 in the fact that there is a mathematical order underlying nature which is simpler than it
852 appears, and that represents the essence of phenomena.” It is no coincidence if most of the
853 homomorphic representatives, as well as the one just cited, are mainly physicists by training,
854 no matter the subdiscipline to which they contribute in the second part of their career. This
855 category has a considerable amount of expressive nuances – sometimes the accent is on the
856 “structure,” sometimes on the “underpinning hypotheses,” some others on the “formal
857 organization.” Yet, they all share the same belief about the power of the models: These can
858 definitely explain and predict “real” systems evolution. Admittedly, homomorphic researchers
859 trust their simulations because these “work,” but they also explain *why* they work: In their
860 view, the rules inscribed into the computer code “capture” the very rationales of the target
861 systems.

862 Some philosophers share the same *ontological view* as these interviewees. Computational
863 models are described as representing “analogues” of “real” phenomena (Sismondo 1999), or
864 as having a “mimetic quality” relatively to the target systems (Winsberg 2003), or as being
865 more or less “isomorphic” compared to them (Suppes 2002; Da Costa and French 2003).
866 Galison (1997) has also distinguished between a “homologous” tradition in physics, which
867 “make[s] statistical arguments for the existence of a particle or effect,” and a “homomorphic”
868 tradition, which “has had as its goal the representation of natural processes in all their fullness
869 and complexity – the production of images of such clarity that a single picture can serve as
870 evidence for a new entity or effect. These images are presented, and defended, as *mimetic*” (p.

871 19). Similarly, Norton and Suppe (2000) assimilate (climate) computer simulations to
872 experiences, since algorithms mimic “real” physical systems. While other examples could be
873 given (Elsenbroich 2011; Winsberg 2009; Peschard 2013), one of the clearest endorsements to
874 this view comes from Humphreys (2004), who talks about the possibility that “the
875 computational model used by S correctly represents the structure of the real system R” (p.
876 110).

877

878 *4.4 The vitalist view*

879 At first sight, the *vitalist view* seems to be a subcategory of the *homomorphic*, for both views
880 share the same idea according to which the formal structure of simulation can be the same as
881 the formal structure of the target system. But the accent that vitalists put on the very essence
882 of their simulations distinguish them neatly from the homomorphic. Vitalists indeed push to
883 the extreme the symmetry between the world and the model, by claiming that their
884 simulations live in the literal meaning of the word. For vitalists, models are detached from any
885 sake of realism and are subject to the sake of plausibility. But as soon as a digital agent is
886 capable of feeding, defending and reproducing, we are facing a concrete instantiation of
887 Darwinian evolution. Thus for them, simulations can be studied as real biological systems.

888 This view is present in Artificial Life. This domain parallels Artificial Intelligence in that the
889 distinction between hardware (matter) and software (form) is sharp and hierarchical, since the
890 identity of an artificial being is given by its organization, not by the specific body in which it
891 is incarnated nor by the specific combination of the two. As the founder of this field
892 Christopher Langton once wrote, “[Artificial Life] is an attempt to abstract the principles of
893 life from its material organization, and recreate them in other materials” (SFI 1990, p. 17). In
894 this perspective, matter is indifferent, almost causally transparent: “The objective [of
895 Artificial Life] is not so much to explain in particular natural phenomenon as to claim that a

896 certain kind of phenomenon will be robustly exhibited by a broad variety of systems having a
897 certain abstract or formal structure” (SFI 1997, p. 20). The form is the only criterion to
898 establish life identity: “[life] is a property of the *organization* of matter, rather than a property
899 of matter itself” (Langton 1988, p. 74). The conviction of artificial lifers is that, if they
900 manage to capture the rules of life organization into computer code, the software which
901 results out of it will not only be an imitation of living beings, but will be a *silico* living being.
902 In their view, simulations are alive for they have an autonomous existence, not in the sense
903 that they self-program their code, but in the sense that their evolutive stages are unpredictable
904 to the programmer.

905 The social scientist who has the best described artificial lifers’ perspective is MIT
906 anthropologist Stefan Helmreich (1998, 2004). One of the many things that he highlighted in
907 his work is that, since the inception of the field, its representatives started claiming that
908 computers are parallel worlds compared to the physical one in which we live: “Computers are
909 candidates for *being* worlds, not just describing them” (Helmreich 1998, p. 103). As one of
910 Helmreich’s interviewees once told him: “After a while, the analogy between self-replicating
911 programs and living organisms becomes so perfect that it becomes perverse to call it merely
912 an analogy. It becomes simpler just to redefine the word *organism* to apply to both chemical
913 and software creatures” (ibid., p. 123).

914 Some philosophers have worked with artificial lifers on the philosophical foundations of their
915 field (Bedau 1992, 2007; Boden 1996). Among the subjects they have dealt with, they
916 essentially focused on the theory of evolution, on the definition of life, on the relationship
917 between life and mind, as well as on the ontological status of the simulated living beings.

918

919 *4.5 The monist view*

920 The conviction according to which a “key property of complex adaptive systems is their
921 ability to process information – to compute – in order to adapt and thrive in an environment”
922 (SFI 2014, p. 18) is widely shared by homomorphic and vitalists. The monists differ from all
923 the others insofar as the entire universe is seen by them as a computer or as a computational
924 process. The term “monism” refers to the fact that the parallel rails of “real” world and
925 simulation make one and the same thing, the material rail being completely determined by the
926 formal one.

927 For one of the tenants of this view – physicist and entrepreneur Stephen Wolfram – all natural
928 and social phenomena can perfectly be reproduced and explained by a processor. If the
929 universe is a computing process, then the simulation contained in a PC has a thorough
930 ontological continuity with the former. Simulation hence becomes a real system or a universe
931 into the universe, since their essence is the same. Also influenced by his personal interest for
932 cosmology, Wolfram has come up to produce an original cosmogony: “And could it even be
933 that underneath all the complex phenomena we see in physics there lies some simple program
934 which, if run for long enough, would reproduce our universe in every detail?” (Wolfram 2002,
935 p. 465). According to him, the fundamental physical laws are not that fundamental, since they
936 are actually an emergent phenomenon of a single and simpler computational law which
937 expresses the entire universe and all it contains. In particular, he rejects to qualify the universe
938 as a computer, for he prefers to define it as the result of a computational process:

939

940 What I would say is we have a thing that is computational, just like we can have a thing
941 that is mathematical and, as we could say the Universe is mathematical, the Universe is
942 computational. That means there is a computational process which is equivalent to the
943 Universe, which reproduces the Universe. (Interview with Stephen Wolfram, 13.09.16)

944

945 His position does not imply to reject the importance of matter: “if you’re trying to study
946 snowflakes and you don’t believe in matter, you are doing something stupid. Because
947 snowflakes are made of ice [...] there is not really a conflict or difficulty there” (ibid.). Still,
948 computation is in this perspective the most important principle of the cosmos:

949

950 if we are living in the world and we’ve got tables and chairs, then matter is important. But
951 if we are trying to find a broader theory of the world, then probably mathematics is more
952 important than the specifics of this crystalline solid or something. So similarly it is with
953 computation, it’s more important, it’s a broader category of mathematics. (Ibid.)

954

955 Another monist is MIT quantum physicist Seth Lloyd, who advocates for a cosmology and a
956 cosmogony which are similar to Wolfram’s, that he knows and cites. In his book entitled
957 *Programming the Universe*, he starts the narration with a Biblical-like citation: “In the
958 beginning was the bit” (Lloyd 2006, p. IX). According to Lloyd, information is the basic
959 principle of the cosmos next to matter and energy, for he claims that the universe “computes,”
960 “is governed by the laws of quantum mechanics” and thus is “a quantum computer” that has
961 “compute[d] itself” (ibid., p. 3) since the “Bit Bang” (ibid., p. 46). According to Lloyd,
962 “Energy makes physical systems do things. Information tells them what to do” (ibid., p. 40).
963 His monist position is even more straightforward than Wolfram’s: “the universe and a
964 quantum computer have the same information-processing power: they are essentially
965 identical” (ibid., p. 149). The question does not focus on whether simulations can say
966 something about the target system. The point is that if the cosmos is a computational process,
967 then the computer simulation have all the important characteristics of the world.

968 Table 2 summarizes the five *ontological views*, the disciplines hegemonic in each of them, as
969 well as their view of the relation model-world under the material and formal causes
970 framework. As the table shows, the views locate over a continuum going from dualists to
971 monists, passing through agnostic, homomorphic and vitalists.

972

973 **5. Regimes of evidence and extra-academic activities**

974 With the term of *regime of evidence*, I refer to the combination of an *ontological view* with
975 one or more *epistemic argumentative regimes*. Such combinations are not endless.
976 Complexity sciences are composed by a finite number of epistemic cultures and – despite the
977 existence of some “dialectal” variations, transient postures and exceptions within each one of
978 them – every epistemic culture has its own prevalent *regime of evidence*. For this very reason,
979 *regimes of evidence* can be seen as one of the components of Karin Knorr-Cetina’s *epistemic*
980 *cultures* – which are defined as “amalgams of arrangements and mechanisms” or as the set of
981 theoretical, practical, technical, institutional and normative practices which “make up how we
982 know what we know” (Knorr-Cetina 1999, p. 1).

983 One question remains to be addressed in this last section: What are the effects of the different
984 *regimes of evidence* on the way complexity scientists interface with policy makers and civil
985 society? The degree of conviction with which researchers use their science as an expertise
986 with practical effects for enterprises, NGOs and administrations can vary a lot (Li Vigni
987 2020b). Their attitudes can indeed go from a prudent approach, which considers their tools
988 and results as a sheer heuristic way to explore theoretical questions, up to a remarkable self-
989 confidence with the explanatory and predictive power of their models. As for the pragmatic
990 consequences of this, while some complexity specialists never adventure to address private
991 and public actors to offer them expert consulting services, others do not hesitate to do so.
992 Many reasons can of course be adduced – a gap in scientific confidence in one’s models, the

993 lack of culture of decision making support within a given subdiscipline, as well as the specific
994 institutional conjuncture of a scientific community in a given country at a given historical
995 moment. But if we focus on the specific articulations between the *epistemic argumentative*
996 *regimes* and the *ontological views*, at least three different positions emerge from the material
997 presented here:

998 1) A merely *heuristic regime of evidence*, where scientists refuse to support any decision
999 making process for the lack of confidence in their tools. It is often the case of those
1000 aligning to the *dualist view* and the *toy model regime*. In their case, models can aid
1001 research but not more, for these can neither prove a theory nor predict a given
1002 phenomenon. The dominant epistemic cultures here are life and social sciences, two
1003 domains in which researchers tend to be cautious in terms of prediction because of the
1004 remarkable complexity of their study objects. Accustomed to trial-and-error and to
1005 other qualitative inquiry techniques, biologists and social scientists who affiliate to the
1006 complexity field certainly have a strong interest in regularities detection and
1007 formalization, but tend to bring in their scientific culture traditionally suspicious of a
1008 *mathesis universalis*;

1009 2) A *productive regime of evidence*, where scientists aligning with the *vitalist* and the
1010 *monist views* and exploiting the *proof of concept regime* offer to produce software and
1011 robots in view of a practical, often entrepreneurial application of their tools and
1012 models. Mostly engineers and entrepreneurs (or physicists with a strong interest in
1013 engineering and entrepreneurship), the tenants of this regime are by training turned to
1014 the material realization of artefacts. When they do not build something tridimensional,
1015 they work on simulations that they tend to consider as autonomous beings. Theory for
1016 them is not an end in itself, but a step toward something useful and/or capitalizable for
1017 society;

1018 3) And, finally, a *consulting regime of evidence*, where scientists carrying a
1019 *homomorphic view* and laying upon the *mathematical* or the *statistical regime* – and
1020 particularly on the *backcasting sub-regime* – do not hesitate to offer their services to
1021 enterprises and governmental policy makers. Mostly physicists by training, the tenants
1022 of this regime practice interdisciplinarity and, often through complexity institutional
1023 spaces and networks, turn toward other disciplines and domains where they feel they
1024 can bring some theoretical and practical contribution. They may indeed predict an
1025 epidemic peak or anticipate the effect of an infrastructure implantation over an urban
1026 system in view of planning. In either case, their conviction in a *mathesis universalis*
1027 or, said otherwise, in the formal equivalence between the target and the simulated
1028 systems, give them the necessary confidence to use the computer modeling to claim
1029 something about “reality.”

1030 These are non-exhaustive generalizations and the analyst should avoid the charm of easy,
1031 logical correspondences between the different argumentative regimes and the different
1032 ontological views. Notable exceptions exist indeed which are explainable by taking into
1033 account, besides pure logic, also contextual and cultural facts. For example, some researchers
1034 from ecology, computer science and social sciences, who are dualists using toy models,
1035 propose to communities and administrations sophisticated devices of action research, based
1036 on serious games, debates and ABM among other tools, in order to pacify conflictual
1037 situations in the management of public natural goods (Collectif ComMod 2005; Étienne
1038 2012). In this case, simulations are simplified and yet expected to reproduce the interrelations
1039 linking stakeholders between them and with their environment. The reason why a toy model
1040 is, according to them, capable of modifying a sociopolitical status quo resides in the scope of
1041 these complex systems specialists, which is not predictive, but pedagogical and political. For

1042 them, the simulation plays the role of a serious game through which the different stakeholders
1043 in conflict can discuss and rethink their interactions and impact on natural resources.

1044

1045 **Conclusion**

1046 Back in the 1980-90s, the SFI and some of its epigones in Europe launched the project of a
1047 new, unitary and revolutionary discipline with the aim of “defin[ing] the principles of a
1048 general science of complexity” (SFI Arch. #1, p. 3), but decided to abandon it after journalist
1049 John Horgan criticized, on the pages of the *Scientific American*, complexity science for being
1050 “flaky” and the SFI for being “fact-free” (Horgan 1995). Nevertheless, along the lines of the
1051 founders of this domain, some observers have argued its unity and radical novelty. But by
1052 describing complexity scientists’ *epistemic argumentative regimes* and *ontological views*, this
1053 text has disconfirmed the alleged unity of complexity and minimized the claim of its radical
1054 novelty. Indeed, the five *epistemic argumentative regimes* – parsimony, toy model,
1055 mathematical, statistical and proof of concept – and the five *ontological views* – dualist,
1056 agnostic, homomorphic, vitalist and monist – draw from physics, statistics, mathematics, life
1057 sciences, as well as design and engineering. The last section has shown that the different
1058 articulations between the regimes and the views produce different postures in relation to
1059 extra-academic actors. Whether scientists show a prudential approach about their results or
1060 propose their consulting to companies, NGOs and policy makers, it depends, among other
1061 things, on their different views of the world and the kind of knowledge they think they can
1062 produce. An *heuristic*, a *productive* and a *consulting regimes of evidence* have thus been
1063 identified.

1064 Interestingly, the socio-epistemic space that complexity founders have contributed to open up
1065 has produced new subdisciplines instead of a singular overarching discipline – econophysics,
1066 Artificial Life, complex networks and computational epidemiology all stemmed from this

1067 space. Whereas Waldrop (1992) insisted in employing the singular “complexity science,” the
1068 interlocutors featured in his book always used the plural. What is more, if until the mid-1990s
1069 some of the founders were engaged in the project of a general theory of all natural and social
1070 complex systems (Cowan et al. 1994; Holland 1996), other illustrious protagonists of the
1071 beginnings rather emphasized the onto-epistemic difference of the layers which physical and
1072 social nature is made of (Anderson 1972). Today very few representatives of the field believe
1073 that a science of complex systems exists or may someday exist (in my corpus, only four over
1074 the 115 interviewees and three of them have been active in the institutionalization of the
1075 field).

1076 A remark on the limits of the present article and about its possible openings deserve to be
1077 done before concluding. Since my primary objective was to qualitatively clearing brush
1078 within the famous but poorly understood domain of complexity, the quantitative backup of the
1079 theses defended here has been left out of the picture. The fact that the *agnostic* and the *monist*
1080 *views* are very rare in my corpus (each being <2%) is indicative of their probable rarity in the
1081 entire complexity community. It is also possible to claim that the *homomorphic view* is more
1082 common than the *dualist* one (respectively 83 and 9% of my corpus). But the *vitalist* is
1083 underrepresented (<4%) because, since Artificial Life has already been studied by
1084 anthropologists and philosophers, I decided to neglect it, and this bias – along with the
1085 snowball method – does not permit to establish the exact ratio between *homomorphic*, *dualists*
1086 and *vitalists* in the complexity community. It is however easy to conceive of a short
1087 questionnaire with both closed and open questions to send to as many complexity scientists as
1088 possible in different countries, so to confirm/amend the different regimes and views identified
1089 here and to quantify them more precisely. The number of interviewees from the different
1090 epistemic cultures at presence should follow a representative logic. Lastly, the reader who
1091 may be willing to conduct such an inquiry could definitely find useful the few, but

1092 comprehensive scientometric studies that have been conducted on complexity
1093 subcommunities by some of their own representatives (Cointet and Chavalarias 2008;
1094 Grauwin et al. 2012).

1095

1096 **Funding** Not applicable.

1097 **Conflict of interest** The author declares that he has no conflict of interest.

1098 **Availability of data and material** Not applicable.

1099 **Code availability** Not applicable.

1100

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1102

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1396

1397 **Table 1. The epistemic argumentative regimes within the subdisciplines gathered under the**
1398 **label of complexity sciences**

Epistemic argumentative regimes	Hegemonic epistemic cultures
Parsimony	All sciences, but especially life, social and computer sciences working on complex realistic models and validating them through statistics
Toy model	Physics, computer science, Artificial Life, economics and other social sciences
Mathematical	Physics, mathematics, chemistry, theoretical ecology, quantitative geography and computational epidemiology
Statistical	Physics, statistics, computational life and social sciences

Proof of concept	Engineering, design, robotics, computer science and Artificial Life
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1399

1400 **Table 2. The *ontological views* within the disciplines gathered under the label of**
 1401 **complexity sciences**

Ontological views	Hegemonic epistemic cultures within each view	Material cause in the real system and the simulation	Formal cause in the real system and the simulation
Dualists	Life sciences, social sciences, engineering	Different	Different
Agnostic	Statistical sciences	Whatever works	Whatever works
Homomorphic	Physics, computer science, quantitative geographers, computational epidemiologists	Different	Coincidental
Vitalists	Artificial Life (physics, computer science, theoretical biology)	Different	Coincidental & lifelike
Monists	Physics, computer science	Coincidental	Coincidental

1402