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1 **The Pleasure of Writing, Being Published, Appealing to Readers, and Contributing to**
2 **the Advancement of Knowledge**

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11 **Abstract**

12 In this *Food for Thought*, I use my experience of writing scientific publications to stress some
13 aspects of the process that were especially significant for me, and from which I try to derive
14 some general suggestions. These aspects include: strong interactions (co-evolution) between
15 paper writing and some of my research directions; the pleasure of writing with co-authors;
16 writing as a tool of scientific creativity; long scientific quests through several publications;
17 the importance of writing books, if possible starting early in the career; being published,
18 reaching readers, and contributing to the advancement of knowledge; and giving in to the
19 pleasure of writing. I explain that I often seized unexpected opportunities that led me to
20 develop ideas and write publications that influenced the course of my career, but I do not
21 necessarily suggest that anyone proceed as I did. My motivation was the enjoyment of
22 exploring new topics, and I wholeheartedly recommend that everyone give in to the pleasure
23 of writing.

24 **Keywords:** pleasure of writing, scientific papers, textbooks, scientific creativity, co-authors

25 **The pleasure of writing**

26 In my book *Scientific Research and Discovery*, I wrote: "one of the most efficient ways to
27 develop original scientific ideas is to write [...] Writing as early as possible goes against the
28 natural tendency of researchers to consider the data at great length before starting to write,
29 with the hope that the data would somehow generate new ideas. Of course, analysing the data

30 contributes to provide ideas [...], but I am convinced that the most original or interesting
31 ideas in a large proportion of studies appear at the time of writing" (Legendre, 2004, 2008).

32 I concluded this section of the book as follows: "Writing can be a great joy. When this skill
33 leads to discovery, it provides extraordinary pleasure. The individual pleasure of discovery is
34 enhanced by peer recognition, and by reaching readers all over the World. I personally never
35 tire of the pleasure of writing scientific texts, of having manuscripts accepted for publication
36 and of hearing colleagues sometimes tell me 'I enjoyed reading that paper of yours!' I wish to
37 offer here a few suggestions to enhance the overall pleasure in the scientific community: as an
38 author, start writing early during the course of projects; as a reviewer, be fair and open-
39 minded; as a fellow human being, tell colleagues whenever appropriate: 'I enjoyed reading
40 that paper of yours!'."

41 I had derived the above views on the pleasure of writing from published opinions of scientists
42 and writers and also from my own experience. What I read and experienced during the two
43 decades since I had written the above confirmed my earlier views. In this *Food for Thought*, I
44 will use my rewarding experience of writing to stress some aspect of scientific publications
45 that I find especially significant.

46 **The most important scientific paper in my career**

47 The most important scientific paper in my career was my first one (Lacroix and Legendre,
48 1964). This paper described changes in the abundance and species composition of
49 zooplankton in the estuary of a river flowing in a large bay of the Gulf of St. Lawrence, in
50 Canada. I was the co-author of this early paper because I had done a summer internship in
51 1962 under the supervision of the senior author, Guy Lacroix, who was then working at the
52 *Grande-Rivière Marine Biological Station*, located on the shore of the Gulf of St. Lawrence.
53 During my internship, I had actively participated in the collection of zooplankton samples at
54 sea and their laboratory analysis, but the paper itself had been written by Guy Lacroix. Later,
55 I had the pleasure of being Guy's colleague at Laval University during many years.

56 This paper was the most important in my career not because it was my first, but because of a
57 special series of events. To understand it, one has to know that in Québec in those days, one
58 type of pre-university education took place in *Classical Colleges*, whose curriculum included
59 six years of languages and sciences followed by two years of philosophy and sciences.
60 Graduates from this program could be admitted to the second year of university, whereas

61 those graduating from the regular six-year high-school program could be admitted to the first
62 year. The Classical Colleges disappeared in the 1970s.

63 In the classical college where I was studying, there were two options for the first six years,
64 i.e. studying Latin, classical Greek, French, English and sciences, or alternatively Latin,
65 French, English (no Greek) and more sciences. For some reason, I was enrolled in the Latin-
66 Greek option, which suited me as I liked classical Greek a lot, although I also liked
67 mathematics. I did not really care about my future career at the time, and was instead deeply
68 involved in several of the extracurricular activities organized in my college, namely the
69 science club, the cine club, and the student newspaper. However, at the end of my first six
70 years and after my summer internship in marine biology, I decided to target admission in
71 sciences at the university, and realized that my background in mathematics and physics was
72 not strong enough. I thus switched for the last two years to the option that prepared students
73 for engineering. Because I was far behind my schoolmates in physics and mathematics, I had
74 to study very hard and my marks in these two key disciplines were quite average, lower than
75 my marks in such subjects as philosophy where I was not at a competitive disadvantage.

76 The scientific paper of which I was the second author was published in January 1964, and I
77 applied to enter the second year of Biology at the University of Montreal in the spring of
78 1964. My application was reviewed by the professor in charge of admissions to the Faculty of
79 Sciences, and I guess that the poor man had to handle hundreds of applications for the
80 different scientific disciplines (he was himself a gruff chemist). When I met him for my
81 admission interview, he told me abruptly: "Sorry, we cannot admit you in the Faculty of
82 Sciences because your marks in mathematics are not high enough ... whereas your marks in
83 philosophy are quite high". I answered him hoping to sound convincing: "Please check my
84 file, and you will find that I have already published a scientific paper in a peer-reviewed
85 journal". He took the reprint of my January 1964 paper out of the file, looked at it briefly, and
86 told me: "Then, I have no choice but admitting you. Good bye." My first paper had
87 determined my career as a scientist. Perhaps a great loss for philosophy! In any case, the event
88 imprinted on my mind the importance of scientific publications.

89 **Strong interactions (co-evolution) between paper writing and some of my research**
90 **directions**

91 Generally, writing a publication follows from previous or ongoing research, but there are
92 instances when a paper is written in response to external stimuli or circumstances, and this

93 paper influences the author's research direction. This happened to me on several occasions,
94 and some of such papers that did not follow directly from the research I was conducting at the
95 time strongly influenced my research directions or my professional activities. Here I provide
96 selected examples of the somewhat counterintuitive situation where writing a paper
97 influenced my research instead of the more usual opposite sequence of events. The papers
98 mentioned here are not necessarily my most cited, but they deeply influenced the course of
99 my activities.

100 In 1979-80, I was in Villefranche-sur-Mer (France) for my first sabbatical leave from Laval
101 University, and I saw the announcement of the *Twelfth International Liège Colloquium on*
102 *Ocean Hydrodynamics* in Belgium. I decided to participate in this multidisciplinary
103 colloquium as I had heard good things about the meeting, and had never visited the city of
104 Liège before. A few months before the colloquium, I began to think about the topic of my
105 presentation, and decided to synthesize results my students and I had obtained on
106 phytoplankton production in the Estuary of the St. Lawrence (Canada) during the previous
107 years. Studying phytoplankton in this physically very dynamic environment, with strong tidal
108 mixing, had been quite challenging, and we had published a number of unusual results.
109 Putting these results together and combining them with those from phytoplankton studies by
110 other authors led me to identify what I called the "paradox of stability", and to propose a
111 general mechanism by which the phytoplankton production potential of marine ecosystems
112 was characterized by their frequency of stabilization-destabilization of the water column
113 (Legendre, 1981). This was my first theoretical paper, which led me to combine observations
114 and theories in several publications during the remainder of my career. I could not have
115 imagined in 1980 that I would attend many Liège Colloquia over the years, become part of
116 the organization of the colloquium during the 1990s, and receive an honorary doctorate from
117 the University of Liège in 1997.

118 In 1984, as a follow up to my 1981 theoretical paper, I was invited to give the *Stevenson*
119 *Memorial Lecture* during the *Canadian Conference for Fisheries Research*. In this lecture I
120 proposed, together with my former PhD student and then colleague Serge Demers, that
121 hydrodynamics was the driving force of aquatic ecosystems, and different hydrodynamic
122 processes and biological responses occurred on different spatial and temporal scale (Legendre
123 and Demers, 1984). This paper confirmed my involvement in theoretical research on the
124 effects of hydrodynamics in biological oceanography.

125 In 1988, I was invited to attend the *Dahlem Workshop on Productivity of the Ocean: Present*
126 *and Past*, in West Berlin (West Berlin was then still encircled by a wall, one of the most evil
127 feature I ever experienced). As usual in Dalhem conferences, some of the participants were
128 requested to write a "food for thought" paper in advance, and I was among them (instructions
129 from the organizer: make sure I receive your paper by the stated date, or forget your
130 participation). Together with my French colleague Jacques Le Fèvre, a zooplankton biological
131 oceanographer from Brest (France), we devised a conceptual model in which hydrodynamic
132 singularities controlled the recycling of phytoplankton production in the surface layer of the
133 ocean *versus* its export to depth (Fig. 1; Legendre and Le Fèvre, 1989). Different versions of
134 our model's diagram were published by other authors in following years (e.g. Cullen, 1991,
135 Cullen et al., 2002). This was my first step in marine carbon biogeochemistry.

136 Also in 1988, I was invited to become member of the *SCOR Working Group on the Ecology*
137 *of Sea Ice Biota*. This followed from my interest in sea-ice biota, which got back to the
138 collection of my first samples of sea-ice algae in Hudson Bay (Canada) in the winter of 1978.
139 During a meeting of the working group in Bremerhaven (Germany) in 1990, the participants
140 undertook the writing of two collective papers, which became quite influential (Horner et al.,
141 1992, Legendre et al., 1992). An additional outcome was the creation by two members of the
142 working group, Steve Ackley and I, of the *Gordon Research Conference on Polar Marine*
143 *Science* in 1997, which continues to meet every second years to this day. The experience I
144 then acquired led me to contribute to the creation of a second Gordon Research Conference
145 almost twenty years later, in 2016, as explained below. When I collected my first sea ice
146 samples in 1978, I could not have imagined that it would lead me to be part of the creation of
147 two international Gordon Research Conferences, twenty and forty years later.

148 In 1989, I was invited to give a lecture at a session on phytoplankton blooms during the *Fifth*
149 *International Symposium on Microbial Ecology* in Kyoto (Japan). In a way somewhat similar
150 to the Dalhem conference mentioned above, the instructions to invitees were: if you wish to
151 have your expenses reimbursed, you must submit the manuscript of your talk by the
152 beginning of the conference. Since I was interested to attend the ISME and visit Kyoto, I
153 researched the literature on blooms. I already knew that in most publications, the term
154 "bloom" was generally applied to the winter-spring phytoplankton burst, whose initiation was
155 generally explained by a physical mechanism proposed by Sverdrup (1953). I also knew that
156 phytoplankton blooms could occur on time scales ranging from tidal to episodic and annual
157 (Legendre, 1981), and my review of the literature led me to formally define blooms as "rapid

158 increases in biomass, caused by locally enhanced primary production and resulting in
159 abnormally high cell concentrations" (Legendre, 1989, 1990). Using the mechanism proposed
160 by Sverdrup (1953), I distinguished between blooms governed by irradiance or by nutrients,
161 and discussed effects of blooms on marine food webs and their overall significance. I thus
162 became very interested in the mechanisms and effects of blooms on ecosystems and the
163 marine carbon cycle, and published many papers on this topic during the following years in
164 which I referred to the seminal paper of Sverdrup (1953). I later found, to my great surprise,
165 in a review dedicated to Sverdrup's paper (Sathyendranath et al., 2015) that I was the third
166 author who had most often cited Sverdrup (1953) in his publications. My long-term
167 theoretical studies on phytoplankton blooms followed from the ISME invitation.

168 In 1993-94, I was in Villefranche-sur-Mer (France) for my third sabbatical leave from Laval
169 University. On this occasion, Fereidoun Rassoulzadegan and I, who had exchanged ideas for
170 many years but had never published together, decided to present our new views on planktonic
171 ecosystems during the *Symposium on Nutrient Dynamics in Coastal and Estuarine*
172 *Environments* held in Helsingør (Denmark) in October 1993. The proceedings were later
173 published in the journal *Ophelia*, and we proposed in our paper the concepts of multivorous
174 and microbial food webs to complement the already known herbivorous food web and
175 microbial loop (Legendre and Rassoulzadegan, 1995). This was the beginning of a long-term
176 collaboration with Fereidoun, which sparked off my interest for the various types of
177 planktonic systems, their connection with environmental conditions, and their food-web and
178 biogeochemical effects.

179 In 2004, I was invited to present a plenary lecture during the *First Symposium on the Ocean in*
180 *a High-CO₂ World*, held at the Unesco headquarters in Paris (France). I then proposed,
181 together with my long-time collaborator Richard Rivkin from St. John's (Canada; our first
182 joint paper had been Rivkin et al., 1996), a framework for a new class of models that would
183 consider the interactions, in the upper ocean, of functional types of plankton organisms, food
184 web processes that affect organic matter, and biogeochemical carbon fluxes (Legendre and
185 Rivkin, 2005). This led Richard and I to develop and use such models to explore the roles of
186 planktonic ecosystems in ocean carbon fluxes (see section *Long quests through several*
187 *publications*, below).

188 In 2009, I was invited to join the SCOR-InterRidge *Working Group on Hydrothermal Energy*
189 *Transfer and its Impact on the Ocean Carbon Cycles*. This invitation came as a surprise given

190 that I had never worked on hydrothermal systems, but it was explained to me that I was
191 expected to provide expertise on data synthesis. During the following six years, I learned a lot
192 about marine hydrothermal systems, and developed keen admiration for the determination of
193 researchers who collected data with great effort in these hostile environments. I was very
194 pleased to contribute to a geochemical modelling study on hydrothermal iron cycling and
195 deep ocean organic carbon scavenging (German et al., 2015), which got me started in Earth
196 System Science (see also my book Bertrand and Legendre, 2021, in section *The importance of*
197 *writing books, if possible starting early in the career*, below).

198 In 2010, I was invited to present a tutorial during the workshop of IMBIZO II about *Large-*
199 *scale regional comparisons of marine biogeochemistry and ecosystem processes – research*
200 *approaches and results*, in Heraklion (Greece). Together with Nathalie Niquil, then in La
201 Rochelle and now in Caen (France), we created a typology of methods and approaches for
202 comparing large-scale marine ecosystems, based on two criteria, i.e. four ecosystem
203 properties and three different roles played by field data and conceptual models, respectively
204 (Legendre and Niquil, 2013). This created twelve types to which we could assign all existing
205 methods. Nathalie and I thought that such a typology of methods and approaches currently or
206 potentially used for large-scale ecosystem comparisons would generate interest in the
207 community of researchers in that field. However, this interest is still to come. In any case, my
208 participation in IMBIZO II proved to be very important for my future research as explained in
209 the next paragraph.

210 I had liked very much the IMBIZO II format, and thus decided to participate in 2013 in the
211 workshop of IMBIZO III dedicated to *The impact of anthropogenic perturbations on open*
212 *ocean carbon sequestration via the dissolved and particulate phases of the biological carbon*
213 *pump*, in Goa (India). The workshop targeted interactions between the ocean biological and
214 microbial carbon pumps, the latter being then quite new (Jiao et al., 2010, 2011). Together
215 with collaborators, I developed a quantitative approach to compare the different ocean carbon
216 pumps, and provided the first quantitative estimate of the microbial carbon pump (Legendre et
217 al., 2015). The co-conveners of the workshop were Nianzhi Jiao (China), Farooq Azam
218 (USA), Carol Robinson (UK) and Helmuth Thomas (Canada). I already knew Farooq, Carol
219 and Helmuth well, but it was my first opportunity to meet Nianzhi. Following the Goa
220 meeting, Nianzhi and I developed a strong collaborative program on various aspects of marine
221 microbial ecology and biogeochemistry, and led together to the creation of the *Gordon*
222 *Research Conference on Ocean Biogeochemistry* in 2016. This collaboration with Nianzhi

223 and other researchers in China was an important factor in my election as Foreign Member of
224 the *Chinese Academy of Sciences* in 2019.

225 In 2013, I was invited to participate in an international workshop that involved coral reef
226 specialists at the *Okinawa Institute of Science and Technology* (Japan). I had previously done
227 work on plankton in coral reef lagoons (Sakka et al., 2002), but this was my first involvement
228 with coral specialists. I was very impressed by the large amount of data collected on the
229 recruitment of coral larvae in tropical waters of the various oceans using settlement tiles.
230 These data were then scattered in many databases, and one of the tasks of the workshop was
231 to favour the creation of a comprehensive database. We initiated the analysis of this rich
232 information during the workshop and continued it after, which led us to find that coral
233 recruitment had been progressively shifting from the equator poleward in the two hemispheres
234 since the 1980s (Price et al., 2019). I think this is a major discovery at a time when coral reefs
235 are under threat in tropical waters. We will see in coming years if this paper will raise interest
236 in the coral reef community. Who knows? It may also influence the course of my career in
237 years to come.

238 Other examples of publications resulting from an unexpected, external events are the books I
239 wrote, i.e. *Écologie numérique* (Legendre and Legendre, 1979, 1984), *Numerical Ecology*
240 (Legendre and Legendre, 1983, 1998, 2012), *Scientific Research and Discovery* (Legendre,
241 2004, 2008), and *Earth, Our Living Planet* (Bertrand and Legendre, 2021). I explain in a later
242 section (*The importance of writing books, if possible starting early in the career*) the unusual
243 genesis of each of these books.

244 The word that best describes the above events may be *serendipity*, which is the occurrence of
245 events by chance in a beneficial way. This word is most often applied to circumstances
246 leading to discoveries or inventions, but I think it could also be applied to the above chains of
247 events I experienced. There are at least two ways to react to such occurrences when they
248 happen. The most reasonable is perhaps to dismiss all opportunities that distract from the
249 course of one's ongoing research, and move resolutely on the path already marked out. A less
250 reasonable reaction may be to take advantage of some opportunities to explore new avenues,
251 and follow them if they seem promising, thus adding a new line of research to those already
252 underway. I often chose the second approach, but I would think that the first is more career-
253 safe, although perhaps not as exciting as giving in to exploring new areas. In fact, all
254 researchers continually acquire new skills as science progresses. However, depending on their

255 personality and the institution that they work for, some researchers will prefer to limit
256 themselves to their current research line, whereas others will enjoy gradually broadening their
257 fields of research.

258 **The pleasure of writing with co-authors**

259 I like writing papers with colleagues. Before starting to write this *Food for Thought* I did, for
260 the first time, a retrospective analysis of my publications. I thus discovered that my preference
261 for joint publications had strongly influenced my long-term publication record. Indeed, I
262 found that out of my almost 300 papers, books and book chapters, I had single-authored less
263 than two dozen, written more than 100 with one or two co-authors, and participated in about
264 150 written by more than three authors.

265 About one third of all my publications and also of my publications with one or two co-authors
266 followed from thesis work of graduate students who were, of course, the first authors of these
267 publications. Without considering these publications, I first-authored about two thirds of my
268 publications written with one or two co-authors, and my collaborators first-authored the
269 remaining third. My publications involving more than three authors mostly resulted from
270 large collaborative projects, or collective brainstorming sessions during conferences,
271 workshops or working group meetings as is often the case in modern research. I enjoyed
272 exchanging ideas with the co-authors of these publications before and during the writing
273 period, but I generally did not first-author such publications except about a dozen.

274 As indicated above, this retrospective analysis of my publications was the first I had ever
275 done. I already knew that I enjoyed creative intellectual interactions with close collaborators,
276 but I had not realized that this had influenced my long-term publication record so much.

277 Writing so many publications with one or two co-authors resulted from a preference, at least
278 on my part, for analysing data and/or developing new ideas with collaborators instead of
279 doing it alone. Why this preference?

280 I have no simple answer to the above question. One important aspect is that the co-authors of
281 my two- or three-author publications were often specialists of other disciplines or fields than
282 mine. These co-authors included physical and chemical oceanographers; specialists of
283 microbial ecology (heterotrophic bacteria and archaea), micro-, meso- and microzooplankton,
284 flow cytometry, and marine optics; fish ecologists; numerical ecologists; and food-web
285 modellers. In the case of multi-authored publications, I also collaborated with specialists of:

286 climatology; sea-ice physics; marine robots; marine chemistry, geochemistry and geology;
287 marine viruses; seaweeds; photobiology; coral ecology and physiology; remote sensing; and
288 limnology.

289 I enjoyed collaborating with specialists from other disciplines and fields, and especially
290 writing publications with one or two of them, for several reasons. One of these is that such
291 collaborations provided access to a wider range of scientific tools and a broader intellectual
292 framework to analyse the data and/or develop new ideas than I would have had if I had
293 written the publications alone. Another, probably more enticing reason, is that writing
294 together gave me deep understanding of the ways of thinking and procedures used by
295 collaborators belonging to other disciplines or fields. Finally, I think that publications
296 combining multidisciplinary approaches were often richer and more interesting for readers
297 than if the same information had been published in individual papers by specialists in the
298 different disciplines.

299 However, there is more for me to the writing of publications with one or two co-authors than
300 the complementarity of expertise. Indeed, deep collaboration with colleagues from other
301 disciplines or specialties is often not easy because of differences in training, vocabulary and
302 concepts. For example, the ocean is seen differently by specialists of different disciplines and
303 fields, e.g. the view of the ocean may be dominated by water movements for physical
304 oceanographers, by reactions of substances dissolved in seawater for chemical
305 oceanographers, by billions of interacting bacteria and viruses for marine microbiologists, by
306 complex planktonic food webs for plankton ecologists, by the biological diversity of coral
307 reef species for coral ecologists, by fish schools for fish biologists, or by fights of cachalot
308 and giant squids in ocean depths for whale specialists. The development of mutual
309 understanding contributed to attract me to the collaborative writing of publications, which
310 often followed from research projects conducted in collaboration with the co-authors.

311 I know that the opposite may be true, i.e. becoming frustrated with difficulties communicating
312 or writing multidisciplinary studies with some co-authors. I have occasionally seen colleagues
313 so frustrated that they bitterly fought with co-authors. When this threatened to happen to me, I
314 put the pleasure of writing first, and found a way to iron out my relationship with the difficult
315 co-author, or completed the ongoing manuscript with this co-author and avoided writing with
316 him/her again. Indeed, writing is such a great pleasure that it should not be spoiled by poor
317 personal relationships.

318 An example of the above is my long-term collaboration with physical oceanographer R. Grant
319 Ingram. Grant and I were born the same year, and he died much too young in 2007. We wrote
320 together many papers that combined hydrodynamics and biological production, sometimes
321 under the form of series in which we successively investigated physical and biological
322 oceanographic processes and their ecological effects (e.g. in sea-ice covered Arctic waters:
323 Ingram et al., 1996, Legendre et al., 1996, and Fortier et al., 1996). Grant and I enjoyed
324 learning from each other and discovering together.

325 **Writing as a tool of scientific creativity**

326 It is generally assumed that the new ideas and discoveries found in publications are
327 descriptions by researchers of achievements they had made before these were put on paper.
328 However, I have often experienced a different sequence of events linking writing and
329 discovery, i.e. some important ideas and concepts I published were generated at the time of
330 writing, and did not exist before. Several colleagues have told me that they had experienced
331 the same.

332 I described and analysed this phenomenon in Legendre (2004, 2008), where I explained that
333 although most writers proceed from a seed idea, the final work is often very different from the
334 initial concept. For example, authors of novels have reported that characters in some of their
335 books had acquired a life of their own as the writing of the book progressed. This process is
336 called *inspiration*, which is defined as the creative drive of artists and writers, and I suggested
337 that it could also be experienced by researchers when writing scientific works.

338 Indeed, it sometimes happens when writing a scientific paper that a new idea seems to emerge
339 from the text itself, and the new angle it provides becomes the main thrust of the work. I
340 purposely used the word *emerge* in my book both as the image of an idea rising from the text,
341 and by reference to systems theory where new properties generally appear, i.e. emerge, as one
342 goes from a low level of organization in a system to a higher one (e.g. Bertalanffy, 1968).
343 Hence within the context of systems theory, the act of writing could be seen as a progression
344 towards higher organisation of ideas, which sometimes favours the emergence of original
345 thoughts that could not have occurred by simply putting observations together.

346 What I described in the previous paragraph is the emergence of a new idea, model or
347 hypothesis during the writing of a scientific publication. A key characteristic of the resulting
348 new approach is its potential to explain not only the observations from which it was derived,

349 but also other data of the same type. Hence, the new approach could eventually be rejected if
350 it failed to explain additional data or enough of them (this rejection is called *falsification* in
351 the case of hypotheses). The new approach that emerged at the time of writing thus
352 contributes to the advancement of knowledge. This is very different from the dubious practice
353 of *HARKing* (i.e. Hypothesizing After the Results are Known), which consists in presenting in
354 the introduction of a research report a hypothesis based on or informed by one's result as if it
355 were an *a priori* hypothesis (Kerr, 1998). Such hypotheses are called *ad hoc* (sometimes *post*
356 *hoc*, a qualifier that in fact refers to the fallacy of arguing from a temporal sequence to a
357 causal relation, as recalled in Legendre, 2004, 2008). An *ad hoc* hypothesis or model (also
358 called a *rationalisation*) is devised for the particular case at hand, without consideration of
359 wider application. Because the *ad hoc* approach does not explain other observations than
360 those under consideration, it does not contribute to the advancement of knowledge.

361 I remarked in my book that the role of inspiration in scientific writing is not something that
362 researchers generally admit or recognize. I suggested that this is because inspiration seems to
363 bring into the process of scientific research an irrational component, and also to operate
364 outside the accepted framework of the scientific method. I provided an explanation
365 reconciling inspiration with the process of scientific discovery, which requires reading what I
366 wrote in my book about the latter. In any case, I also remarked that the end product of
367 inspiration may not be the end of the story, as reviewers may disagree with the interpretations
368 of authors, thus forcing additional iterations. I also argued that the concealment by researchers
369 of the role of inspiration in the production of scientific works (voluntarily, or because they are
370 not aware of it) prevents the public and young people from recognizing that research is a
371 creative activity, which contributes to keep them away from science.

372 My interpretation of inspiration in scientific writing was that interactions between
373 researcher's intuition, the act of writing, the pleasure of interpreting the data and developing
374 theoretical explanations, and the unfolding of the discussion create conditions required for the
375 emergence of new ideas. I also suggested that an alternative, simpler interpretation could be
376 that the conditions favourable to the emergence of a novel idea are the result of the extreme
377 focusing of the mind engendered by the act of writing. In any case, I insisted that one of the
378 most efficient ways to develop original scientific ideas is to write, being convinced that in a
379 large proportion of studies, the most original or interesting ideas appear at the time of writing.

380 Here I use three of my publications as examples of the above. To do so, I purposely chose
381 publications that I had single authored in order to avoid involving co-authors in my
382 interpretation of writing as a tool of scientific creativity.

383 I already summarized in a previous section the content and theoretical contribution of my
384 paper on the paradox of stability (Legendre, 1981). Although writing this paper goes back
385 forty years, I vividly remember the excitement of experiencing the emergence of the idea of a
386 general mechanism governing phytoplankton production as I was synthesizing my studies and
387 those of other researchers on various characteristics of phytoplankton. Once the key idea of
388 the paper had taken shape, i.e. that the phytoplankton production potential of marine
389 ecosystems was characterized by their frequency of stabilization-destabilization of the water
390 column, I reorganized the available information on phytoplankton according to the
391 hydrodynamic characteristics of the environment. This led me to describe in the publication a
392 logical progression from observations to theoretical model, and apply the model to a wide
393 range of phytoplankton structures in the ocean. It was not useful to explain in my paper that I
394 had reorganized the data after the model had emerged from their analysis, and I presented the
395 model as following from the data as was indeed the case. As explained in Legendre (2004,
396 2008), "because science is the universal knowledge acquired through discoveries [...], and not
397 the compilation of the personal quests of discovery of individual researchers, what we find in
398 scientific literature are always reconstructions, not reports of how discoveries actually took
399 place".

400 I also summarized in a previous section my first theoretical papers on phytoplankton blooms
401 (Legendre, 1989, 1990). Again, when I was working on these papers, there was a strong
402 interaction between the development of their main ideas and my writing activity. Indeed, I
403 had started to write a review of the literature on phytoplankton blooms, and about one-third of
404 the way in this task, I saw that the different types of blooms I was reviewing were different
405 realizations of a general mechanism, at which point I bifurcated from a review to a paper
406 organized around a new theoretical development. I was then able to arrange the information I
407 had gathered in the literature into a progression that went from published observations to a
408 theoretical model, and from the model to the effects of blooms on marine food webs and
409 ocean carbon fluxes. As in the case described in the previous paragraph, the emergence of a
410 general mechanism from observations at the time of writing was very thrilling.

411 In closing this section on writing as a mean of scientific creativity, I wish to report an
412 approach that I used when I wrote *Scientific Research and Discovery* (Legendre, 2004). This
413 book has 12 chapters divided in a total of 36 sections (the 2008 web edition is shorter). I
414 decided early when I was writing the book that each of the 36 section should contain at least
415 one figure or one table. The purpose of this self-imposed rule was to help readers grasp the
416 content of all sections, some of which were quite theoretical. As I was writing, I sometimes
417 regretted my self-imposed rule because several of the sections did not lend themselves easily
418 to creating a figure or a table. I nevertheless persisted, and generated at least one figure or one
419 table for each section even if the result was only marginally related to the text I had written.
420 In almost all such cases, the creation of the figure or table provided me with a new insight
421 into the content of the section, which I then rewrote, and the latter sometimes led me to
422 improve the figure or the table. Self-imposed writing rules can be a tool to enhance creativity
423 when they force the writer to "look outside the box".

424 As already indicated at the beginning of this *Food for Thought*, my own experience led me to
425 think that the most original or interesting ideas in a large proportion of studies appear at the
426 time of writing, although I cannot document this opinion because researchers have very
427 seldom reported this process. In any case, writing can be a great joy, especially when it leads
428 to new ideas or discoveries.

429 **Long scientific quests through several publications**

430 Some concepts I have developed matured over several years through successive publications,
431 each of them building on previous ones. I describe here two examples showing that writing
432 each publication was a key step towards a product that was not foreseen at the beginning of
433 the process or even during it. In each example, I begin with a study that implemented the
434 (provisionally) final product, and trace its origin backwards in my publications.

435 *First example.* I use as starting point the study of Beaugrand et al. (2010), in which I
436 collaborated in investigate changes in phytoplankton and zooplankton biodiversity in the
437 extratropical North Atlantic Ocean between 1960 and 2007. The paper reported a pronounced
438 latitudinal increase in biodiversity during the 47-year period, and a parallel decrease in the
439 mean size of copepods. The analysis led to the conclusion that this decrease had negative
440 effects on the downward biological carbon pump and on demersal Atlantic cod (*Gadus*
441 *morhua*). These negative effects were evidenced using allometric relationships proposed by
442 Legendre and Michaud (1998), whereby: the minimum turnover time of carbon incorporated

443 in organisms is directly related to the size of organisms; and downward carbon export is
444 directly related to the size of organisms that produce sinking particles, here the sizes of
445 copepods that determine the sinking velocities of their faecal pellets.

446 I had defined the above allometric relationships with Josée Michaud (Laval University,
447 Canada) in a study where we had quantified the flux of biogenic carbon (BC) acquired by
448 organisms feeding on food particles or other organisms both towards large metazoans and
449 downwards from surface waters (Fig. 2). To do so, we had developed allometric equations to
450 quantify the minimum turnover time of BC in food ($\tau_{\min 1}$) and marine pelagic organisms
451 ($\tau_{\min 2}$), and the residence time (τ_s) of BC above the depth below which BC cannot rapidly
452 return to the surface waters or the atmosphere (i.e. sequestration depth, z_s , e.g. 1000 m). We
453 had used $\tau_{\min 1}$, $\tau_{\min 2}$ and τ_s in conjunction with the size ratio of consumers to their food
454 particles (ξ) to assess the food-web regulation of BC fluxes, i.e. the consumption of particles
455 or prey (with short $\tau_{\min 1}$) by larger-sized organisms caused a lengthening of the residence time
456 of BC incorporated in the body mass of larger organisms (longer $\tau_{\min 2}$) and a shortening of τ_s
457 due to the aggregation of BC in faster sinking faecal material. These two effects cause
458 increased carbon fluxes towards the pools of long-lived organic carbon ($10^{-2} < \tau < 10^2$ year)
459 and sequestered BC ($\tau > 10^2$ year), respectively.

460 The originality of the above approach was to transform the sizes of organisms, their foods,
461 and the particles they produce (i.e. faecal material) into units of time (i.e. $\tau_{\min 1}$, $\tau_{\min 2}$, and τ_s ,
462 respectively), which were relevant to the three carbon pools defined by Legendre and
463 Le Fèvre (1992), i.e. short-lived organic carbon ($\tau < 10^{-2}$ year), long-lived organic carbon and
464 sequestered BC (τ defined in the previous paragraph). I explained later in Legendre (2004,
465 p. 146-153) how dimensional analysis had helped me to develop this approach, which made it
466 possible to formally connect food-web feeding processes and ocean biogeochemical carbon
467 fluxes.

468 The approach of Legendre & Michaud (1998) combined four key concepts I had elaborated in
469 previous papers. The *first concept* was the classification of biogenic carbon in the ocean into
470 three pools with the different turnover times cited in the previous paragraphs (Legendre and
471 Le Fèvre, 1992). In that paper, we used our new carbon pool concept to unify food-web
472 related biogeochemical carbon fluxes, and explained how these were largely governed by
473 hydrodynamics. We also identified refractory dissolved organic carbon as a form of
474 sequestered BC, a recognition that underlies the microbial carbon pump proposed later by Jiao

475 et al. (2010, 2011) cited above. The *second concept* was the recognition of five types of
476 pelagic ecosystems based on the relative size structures of phytoplankton production and
477 standing stocks, the latter reflecting the effect (or absence) of grazing by zooplankton
478 (Legendre and Le Fèvre, 1991). The two extreme cases in this typology were ecosystems with
479 phytoplankton production and biomass both dominated by large cells (e.g. ice-edge blooms)
480 and by small cells (e.g. oligotrophic ocean). These five types were used to illustrate the above
481 three biogenic carbon pools. The *third concept* was the following hypothesis (supported by
482 data) that pelagic organisms that package small particles into larger ones lengthen the
483 turnover time of biogenic carbon (i.e. τ) and, in some cases, transfer this carbon from a given
484 carbon pool to a longer lived one; and the lengthening of turnover time is a direct function of
485 the ratio between the size of organisms and that of their food particles (i.e. ξ) (Fortier at
486 al., 1994). We found in that study that the most efficient re-packagers of small particles into
487 larger ones were salps, appendicularians, doliolids and thecosome pteropods, which all feed
488 on particles at least 3.5 orders of magnitude smaller than their own size. In a *fourth*
489 *conceptual step*, I combined the key concepts from the previous three papers in a theoretical
490 model describing the fluxes of carbon production from phytoplankton in three size classes to
491 the above three carbon pools (Legendre, 1996), and I refined this model in following papers
492 (Fig. 3; Legendre and Rassoulzadegan, 1996, Legendre and Michaud, 1998).

493 However, the theoretical model in Fig. 3 could not easily be transformed into equations
494 because the physical dimensions of its two components were different, i.e. size
495 (phytoplankton classes, Y-axis) and time (carbon pools, X-axis). I explained above how I
496 resolved this problem by using allometric relationships to transform the size of organisms and
497 their foods into residence time of carbon ($\tau_{\min 2}$ and $\tau_{\min 1}$, respectively, in Fig. 2; Legendre and
498 Michaud, 1998). In the latter paper, we also used an empirical relationship between the
499 sinking velocity of faecal pellets and the size of the organisms producing them to compute the
500 residence time of faecal pellets above depth Z_s (τ_s).

501 The above paragraphs explain how a 2010 paper used allometric relationships published in
502 1998, which were themselves rooted in papers written in 1996, 1994, 1992 and 1991. The
503 sequence of studies from 1991 to 2010 extends over twenty years. The study of Legendre and
504 Rassoulzadegan (1996) cited above proposed an approach to determine, using a small number
505 of food-web or hydrodynamic variables, the partitioning of phytoplankton production among
506 three carbon fluxes, i.e. remineralization within the euphotic zone, food-web transfer, and
507 sinking to depth of organic particles. In addition, the approach of Fortier et al. (1994) led us to

508 propose in a subsequent paper a quantitative explanation to the dominance of different parts
509 of the Southern Ocean in different seasons by either salps, krill or some large copepods (Le
510 Fèvre et al., 1998).

511 *Second example.* I use as starting point the study of Giering et al. (2014), in which the authors
512 addressed the carbon budget in the ocean's twilight zone at a long-term sampling station on
513 the Porcupine Abyssal Plain in the eastern North Atlantic Ocean. Contrary to many studies of
514 vertical ocean carbon fluxes where there is a large discrepancy between known carbon
515 sources in the euphotic zone and carbon sinks at depth, this study reconciled the carbon
516 budget in the twilight zone. The authors achieved carbon balance by following the
517 recommendation from Legendre and Rivkin (2008) to base carbon budgets on community
518 respiration estimates instead of carbon demand as usually done. The reason for using
519 respiration instead of carbon demand is that each atom of carbon within organic matter is only
520 respired once and thus balances the carbon sources, whereas atoms of carbon can be recycled
521 many times before being respired and can thus generate carbon demand much larger than the
522 carbon sources.

523 In Legendre and Rivkin (2008), the above principle is stated as follows: "respiration is the
524 only additive property of the ecosystem, and can thus be used as a metric for assessing trophic
525 conditions or comparing food-web compartments". Similarly, Anderson and Ducklow (2001)
526 had remarked that "bacterial respiration, in conjunction with zooplankton respiration, cannot
527 exceed the supply of organic carbon". In our study, Richard Rivkin and I grouped
528 heterotrophic microbes in a "microbial hub" and larger heterotrophs in a metazoan
529 compartment and we found, by applying the microbial-hub approach to a wide range of food
530 webs in different zones of the world ocean, that heterotrophic microbes always dominate
531 respiration in the euphotic zone, even when most particulate primary production is grazed by
532 metazoans.

533 The above paper was a follow-up to a previous study in which we had shown that
534 phytoplankton, microbial heterotrophic plankton and large zooplankton were the three food-
535 web control nodes of five major carbon fluxes, i.e. phytoplankton production, and its
536 partitioning into respiration, transfer to the food web, and downward export as both DOC and
537 POC (Legendre and Rivkin, 2002). Using this approach, we had found that the microbial
538 heterotrophic plankton node was responsible for most of the respiration of organic carbon to
539 CO₂ and the uptake and release of DOC, and the large zooplankton node controlled both the

540 transfer of POC to large metazoans and part of the downward POC flux (i.e. faecal pellets and
541 vertically migrating organisms). We had also identified regions of the world ocean that are net
542 autotrophic and net heterotrophic.

543 In our 2002 study, we had used a previous numerical relationship we had developed to
544 estimate bacterial respiration as a function of bacterial production and temperature (Rivkin
545 and Legendre, 2001). In that paper, we had shown that bacterial growth efficiency in the
546 ocean is an inverse function of temperature, and bacterial respiration generally accounts for
547 most of community respiration.

548 The above paragraphs explain how a 2014 paper used a concept published in 2008, which was
549 itself rooted in papers written in 2002 and 2001. The sequence of studies from 2001 to 2014
550 extends over almost fifteen years. We also used the approach of Legendre and Rivkin (2008)
551 in a paper showing that microbes are key components of marine pelagic food webs and
552 biogeochemical cycles not only because of their physiological characteristics (e.g. high
553 specific metabolic rates) coupled with large standing stocks, but also because of their unique
554 positions in pelagic food webs where they concurrently produce, consume and remineralize
555 organic matter (Legendre and Rivkin, 2009). We used the same approach in a paper where we
556 investigated the controls exerted by food-web "competition switches" on the flows of carbon
557 toward the microbial hub or other food web compartments (Fig. 4; Legendre and Rivkin,
558 2015). The three switches were: competition for inorganic nutrients between bacteria and
559 phytoplankton; competition for detritus between bacteria and mesozooplankton; and
560 competition for large-sized phytoplankton production between microzooplankton and
561 mesozooplankton. We found that competition for resources between the microbial hub and
562 other food web compartments plays a crucial role in controlling the flows of biogenic carbon
563 in the euphotic zone.

564 The two examples of long quests stress that the generation of new ideas and approaches is not
565 always determined by long-term targets, but can develop as an evolutionary process where
566 new papers build upon previous publications. Each new paper is somewhat like a "mutation"
567 in biological evolution when its key ideas had not been foreseen at the time the previous
568 papers were written; "selection" on the proposed new ideas or approaches is exerted first by
569 reviewers and editors, and then by readers who cite the publication or not; and evolutionary
570 "success" is achieved when the key proposal of a publication is used by colleagues in their
571 own work. Success may be complete when one's proposal is cited without reference to the

572 original work because it has become part of general knowledge in the discipline. Some may
573 perhaps fear that long scientific quests are no longer possible in today's funding and academic
574 environment, but I know many successful researchers who cleverly find ways to pursue long-
575 term personal research agendas in today's environment and, thus, contribute significantly to
576 the development of knowledge.

577 **The importance of writing books, if possible starting early in the career**

578 For me, each book I wrote was a unique experience, entirely different from writing papers,
579 reviews or book chapters as explained below. I had the privilege of writing the widely used
580 textbook *Numerical Ecology* with my brother, Pierre Legendre, Professor at Université de
581 Montréal, relatively early in my career (Legendre and Legendre, 1979). In the following
582 decades, Pierre and I published four additional editions of our book (Legendre and Legendre,
583 1983, 1984, 1998, 2012). I understand that our book has been widely used by researchers and
584 students in ecology, based on the >20,000 citations it received so far in the scientific literature
585 (Google Scholar).

586 The origin of *Numerical Ecology* was a bit unusual, as described by Pierre Legendre in the
587 *Encyclopedia of Ecology* (Legendre, 2019). Briefly, neither Pierre nor I had been trained in
588 numerical ecology, for the very reason that the expression "numerical ecology" first appeared
589 in the first edition of our textbook. In addition, neither Pierre nor I were biostatisticians, as
590 Pierre's Ph.D. had been in evolutionary taxonomy and biosystematics and mine in biological
591 oceanography. However, we had both independently used published methods of numerical
592 data analysis in our studies, methods that we had struggled to understand, master and program
593 (on mainframe computers, which became available in universities and research centres in the
594 late 1960s). In May 1975, Pierre and I were invited independently to join a small 3-day
595 workshop of a dozen or so ecologists at the *Station marine de Villefranche-sur-Mer*, in
596 France, to discuss a new trend in ecological research, namely the statistical analysis of
597 multivariate ecological data. On the evening of the closing day of the workshop, Pierre and I
598 had dinner together on the terrace of a restaurant in historical Villefranche, with an inspiring
599 view on the Bay of Villefranche. During our meal, we thought that we should share with
600 fellow researchers and students the knowledge on the use of numerical methods in ecology we
601 had acquired through hard work. We decided there and then to write a textbook in a way that
602 would be understandable by non-mathematically oriented ecologists, based on ecological
603 questions, in which numerical methods would be introduced to address these questions, and

604 would be illustrated with real ecological examples from the literature. We wrote on a paper
605 place mat a list of subjects that became the table of contents of the first editions of *Écologie*
606 *numérique* and *Numerical Ecology*.

607 When we decided to write a textbook together in 1975, Pierre and I could not have imagined
608 that our work would go through two French and three English editions over the next 40 years,
609 launch the new scientific discipline of *numerical ecology*, and become a "classic" reference in
610 ecological research. I did not know either that the young Faculty member of Canada's *Laval*
611 *University*, where I was at the time, would become Director of the French *Villefranche*
612 *Oceanography Laboratory* a quarter of a century later. However, Pierre and I then knew that
613 we would write and publish our book whatever the obstacles. And obstacles there were,
614 including the responses of some Publishers we approached that we were too young to write a
615 textbook, but we persisted until we held in our hands printed copies of the first editions of
616 *Écologie numérique* in 1979 and *Numerical Ecology* in 1983.

617 In my experience, writing textbooks is very different from writing scientific papers or
618 reviews. One aspect of this difference is the use of the information drawn from the literature.
619 In a paper, the information from the literature is cited in support of the substance of the study
620 and its specific objectives. In a review, the substance of the work is the analysis and synthesis
621 of information from the literature. In a textbook, the information extracted from the literature
622 is fully digested and blended into the narrative of the work. My experience of writing chapters
623 in multi-authored books was mid-way between that of writing a paper and a review
624 concerning the use of the information from the literature. When writing a textbook, the choice
625 of cited papers is determined by the overall concept of the work, the chosen papers are
626 analyzed deeply, and their content is carefully explained to readers. Contrary to this in-depth
627 analysis of cited papers, I sometimes experienced seeing one of my publications cited in a
628 paper in support of a point that I had not even mentioned in my publication, and I would think
629 that most readers of this *Food for Thought* had the same experience. In the same vein, I guess
630 that most of us have sometimes cited publications in some of our papers without having fully
631 analyzed their contents.

632 In a textbook, each cited publication – paper, review, chapter, or other book – is deeply
633 analyzed, and when conducting this analysis, some points that sometimes looked at first
634 glance to be small pebbles finally prove to be large boulders as one digs deeper into the
635 matter. For example, when Pierre and I wrote the first editions of *Écologie numérique* and

636 *Numerical Ecology*, we carefully checked all the attributions of methods used in ecological
637 papers, and sometimes found that the paper cited as being at the origin of a method (which
638 could have been written in another language than English) had nothing to do with the method
639 under consideration. The incorrect attribution might have been made in one paper years
640 before, after which all researchers using the method simply repeated the incorrect citation. In
641 such cases, we not only had to debunk the error, but we also had to find the paper in which the
642 method had really been proposed, which often was not easy. Days of work at digging out a
643 large, hidden boulder often produces a single sentence in the book. Nevertheless, such work is
644 rewarding for the authors, who learn much, and very useful for the users of textbooks, who
645 obtain "clean" information.

646 Another major difference between textbooks and other types of scientific publications is that
647 textbooks often provide comprehensive developments of their main topics, which can be of
648 great benefit to both the authors and the readers. The broad, coherent approach of textbooks
649 explains why some of them become very influential, probably more than any scientific paper.
650 For example, according to Google Scholar, our book *Numerical Ecology* has been cited more
651 times than any scientific paper written by Albert Einstein. In Legendre (2008), I wrote:
652 "Imaginative textbooks stimulate the curiosity and creativity of undergraduate and graduate
653 students. High-level syntheses provide both general ideas and specialised information that
654 facilitate discovery to graduate students and professional researchers. This is especially
655 important because, in the midst of an information explosion, scientists have over-emphasized
656 production and neglected digestion and foresight; hence the need for syntheses."

657 Given the above, one would think that most researchers would wish to write one or several
658 textbooks during their careers. However, this is not the case and many researchers hesitate to
659 write textbooks, and some even think that textbooks are inferior to scientific papers or
660 reviews. There are many cultural and institutional reasons explaining why many researchers
661 do not write textbooks. Indeed, in some countries or research environments, writing textbooks
662 is not encouraged and may even be discouraged. However, a textbook is among the best ways
663 to influence the long-term development of a scientific field, and I believe that institutions that
664 do not actively encourage their scientists to write textbooks are missing out on one of the best
665 means to be among the main long-term players in the progress of science. In any case, my
666 experience of unsuccessfully trying to convince both rising stars and well-established
667 colleagues to write textbooks combined with that of writing *Numerical Ecology* with Pierre
668 Legendre led me to think that one of the main factors is lack of time.

669 To illustrate the last point, let us look at the publication years of the two editions of *Écologie*
670 *numérique* (Legendre and Legendre, 1979, 1984) and the three editions of *Numerical Ecology*
671 (Legendre and Legendre, 1983, 1998, 2012). The publication dates show that in the 10 years
672 that followed our 1975 Villefranche dinner, Pierre and I published three editions of our
673 textbook (i.e. 1979, 1983 and 1984), after which it took us 30 years to publish two additional
674 editions (i.e. 1998 and 2012). This was largely because, as our careers progressed, we had less
675 and less time to dedicate to writing books because of increased involvements in many
676 research projects and other professional activities. In contrast, within 8 years of completing
677 our PhDs (both in 1971), we had written from scratch and published the first edition of
678 *Écologie numérique*, in 1979.

679 The above led me to believe that the best period in the career to start writing textbooks is
680 early on, when one can still devote time to the demanding project of writing a book. Once a
681 researcher has mastered the art of book writing, enjoyed the pleasure of holding one's book in
682 her/his hands or seeing it on-line, and found the influence exerted by his/her book in the
683 community, it may be easier to write other textbooks during the course of the career. Indeed,
684 many people who have only written short publications (i.e. papers, reviews, or chapters) find
685 it very difficult, if not impossible, to undertake the long-time task of writing a textbook.
686 Hence, in lectures to young researchers I gave in different countries during the last decades, I
687 tried to convince the young colleagues to launch important scientific projects, such as writing
688 a textbook, early in their careers, when they still have enough time to do so. Doing so is very
689 important because writing a comprehensive textbook gives the author a unique in-depth
690 knowledge of the topic of the work. I thus encourage young researchers to take the plunge,
691 possibly in collaboration with a more experienced colleague as suggested in the following
692 paragraph.

693 Researchers could have time to write textbooks when they are involved in fewer projects and
694 responsible for fewer tasks, for example during sabbatical leaves or late in their careers.
695 However, scientists who have not experienced writing books early in their careers generally
696 do not manage to write them later on. As an example, I sadly remember a high-profile
697 colleague I had convinced to write a textbook based on his remarkable teaching notes, who
698 gave up the task after a few months because he was too eager to write more research papers. I
699 realize that the idea of encouraging early-career researchers to write textbooks is
700 counterintuitive, but it could become realistic in some instances if they were encouraged to do
701 so by more senior colleagues. For example, a senior scientist could offer a younger colleague

702 to write a textbook with her/him, the younger colleague being the first author. This could be a
703 great way to combine experience with enthusiasm, and shelter the younger colleague from the
704 criticism of being "too young to write a textbook".

705 From my experience with colleagues, I found that the following two aspects sometimes stand
706 in the way of writing or publishing books. On the one hand, some potential authors may be
707 overwhelmed by the magnitude of the task, and therefore hesitate to start writing the book or
708 become discouraged along the way. One way to get over this problem is to take one chapter,
709 one section, one paragraph, and sometimes one sentence at a time. The presence of a co-
710 author often helps overcome difficulties. On the other hand, some authors who have
711 succeeded in writing a manuscript cannot resist improving it as new information continually
712 appears in the literature. One way to go from manuscript to published book is to decide to
713 stop at some point, and publish a revised edition a few years later if it becomes necessary to
714 take into account the new literature. Indeed, the value of a book for readers is in the vision
715 and the concepts it conveys and not in the review of the latest literature.

716 I explained above that *Numerical Ecology* resulted from the independent invitations, in 1975,
717 of Pierre Legendre and I to a workshop at the end of which we decided to write the textbook
718 together. In addition to the five editions of *Écologie numérique* and *Numerical Ecology*, I
719 wrote two editions of *Scientific Research and Discovery* (Legendre, 2004, 2008). As in the
720 case of *Numerical Ecology*, the writing of this book resulted from an unexpected, external
721 event. Indeed, I was awarded in 2001 the *International Ecology Institute (ECI) Prize*, which
722 was accompanied by the commitment to write a book to be published in the *Excellence in*
723 *Ecology* collection. I explained in the Preface of that book how I had progressively developed
724 my interest in the process of scientific research and discovery and its consequences through a
725 suite of largely unexpected, external event. Without the incentive provided by the ECI Prize
726 and accompanying book, I may never have written a book on the philosophy of science.

727 More recently, I co-authored with Philippe Bertrand an Earth System Science book entitled
728 *Earth, Our Living Planet* (Bertrand and Legendre, 2021). Again, the origin of this book is
729 interesting. Philippe, who is a French marine physical chemist, had written a very original
730 book on the Earth System entitled *Les attracteurs de Gaia* (2008). I had read this thought-
731 provoking book, and suggested to Philippe that he publish an English version of it. After
732 hearing my suggestion a few times, Philippe told me that he wished us to write the English
733 version together. We rapidly decided to write a book less theoretical than Philippe's original,

734 which would be accessible to scientifically literate non-specialists, and looked for a publisher
735 potentially interested in such a book. We found that Springer's *Frontiers Collection* was
736 dedicated to this type of books, and over the next years used a combination of astronomy,
737 biology, chemistry, ecology and geology to explain the progressive takeover of Earth by
738 organisms. We now hope that our book will be well received by non-specialists as well as
739 undergraduates, graduates and researchers wishing to understand the co-evolution of Earth
740 and its organisms.

741 As a closing remark on books, I recommend those planning to write a textbook to do it alone
742 or with only one co-author. For me, writing books with one co-author was great, as one
743 helped or encouraged the other when he ran out of steam. I realized, through personal and
744 observed book failures, that the combination of three authors was often difficult, i.e. when one
745 ran out of steam, s/he sometimes relied on the other two, and when two of the authors or the
746 three did the same, the writing of the manuscript stalled. However, I know that three or more
747 co-authors have produced great textbooks in some circumstances. The community needs good
748 textbooks, and I found that writing them was a wonderful, rewarding experience.

749 **Being published, reaching readers, and contributing to the advancement of knowledge**

750 A key purpose of scientific writing, in addition to the great pleasure of organizing one's own
751 thoughts and often generating new ideas, is to be read and thus contribute to the advancement
752 of knowledge. In order to reach readers, scientific manuscripts need to be published. It is
753 possible nowadays to bypass traditional journal or book publishers, and display one's work
754 directly on the web, either as a preliminary or parallel step to submission to a publisher
755 (preprint) or as the final version of the work (self-publication). However, science mainly
756 advances through the publication of peer-reviewed papers and books, and most researchers,
757 therefore, wish to publish their works in such media.

758 Publication in peer-reviewed media is often a sweet-and-sour experience. The sweet parts
759 include: successfully submitting the manuscript; reading constructive comments from
760 reviewers; being informed by the editor of the journal or the book series that the manuscript is
761 accepted for publication; and seeing one's work in the published form. The sour parts of the
762 publication experience may include: spending hours submitting the manuscript on a
763 sometimes-intricate website; receiving the message from the editor of the journal that the
764 manuscript is rejected, or from the book series editor that the topic of the proposed book is not
765 appropriate; reading reviewer comments formulated in a nasty way, even if these comments

766 may be useful for improving the manuscript; and receiving proofs full of typos, which is
767 fortunately not common since the widespread use of electronic manuscripts. On the whole, as
768 for sweet-and-sour dishes, even if the first experiences may be unsettling, one develops over
769 time the taste for peer-reviewed publication. Some of the sour parts are unavoidable, such as
770 having manuscripts rejected from time to time, reading reviewer comments that are not
771 always constructive, or having to use different formats for the text and the references in each
772 different journal. Other sour parts can be avoided, such as writing nasty comments when
773 reviewing a paper, even if it is full of errors. In all cases, more experienced researchers should
774 advise and support early-career scientists when these receive feedback from their first
775 manuscript submissions, which is especially true for thesis supervisors with their students.

776 The successful publication of a paper or a book is a key step towards reaching readers and
777 contributing to the advancement of knowledge, but it is only one step. For example, Garfield
778 (2005) found that of more than 38 million scientific publications, 48% had never been cited,
779 and 9 and 13% had been cited once and between 2 and 5 times, respectively. Hence, the
780 likelihood of no or a small number of citations is very real for any publication, and low
781 numbers indicate low interest from readers, at least in the short term. Also, some works
782 become highly cited many years after their publication, these being called by bibliometricians
783 "sleeping beauties" (e.g. Ke et al., 2015). One example in marine sciences is the paper of
784 Hjort (1914) which has been cited more than 900 times between 1945 and 2013, an
785 exceptional fate for a 100-year-old scientific article (Aksnes and Browman, 2014). In any
786 case, how could one enhance the likelihood of reaching potential readers and contributing to
787 the advancement of science?

788 Certainly not by artificially inflating one's citation rate by heavily self-citing his/her own
789 works, as is done by some scientists (Van Noorden and Chawla, 2019). Indeed, artificially
790 high citation rates do not increase the dissemination of one's results and ideas in the
791 community. In the section *The Pleasure of Communication* of Legendre (2004, 2008), I
792 provided suggestions to produce high-quality scientific manuscripts, which include writing
793 not only interesting science, but also using a precise and pleasant style, and generally making
794 sure that each manuscript has a clear focus. The aim of my suggestions was for readers to
795 discover the new publication with pleasure. Indeed, a recurring theme developed in my book
796 was that pleasure is among the key criteria of quality at all steps of scientific research, from
797 the initial development of concepts and hypotheses, to their testing and until their publication.

798 my idea being that while good research without pleasure may be possible, pleasure is a solid
799 guideline for good research.

800 **Conclusion: Giving in to the pleasure of writing**

801 I explained in the above sections that there was often a strong interaction (co-evolution)
802 between the papers I wrote and my research directions, and on several occasions writing
803 papers strongly influenced my research. I also explained my joy of writing with co-authors,
804 and how writing was often a tool of discovery for me. I described some of my long quests
805 through several publications, which I compared to biological evolution. I finally stressed the
806 importance of writing books, and encouraged early-career researchers to write textbooks,
807 while suggesting that more senior colleagues help them in doing so.

808 What I explained in this paper was never planned at any point in my career, i.e. it just
809 happened. In retrospect, I was able to present here in an organized way events, ideas and
810 publications that unfolded according to their own logic. I guess that I was, unknowingly at the
811 time, taking advantage of the idea of the founder of microbiology Louis Pasteur (1822–1895)
812 whereby "fortune favours the prepared mind". In my case, I often seized unexpected
813 opportunities that led me to develop ideas and write publications that influenced the course of
814 my career, but I do not necessarily suggest anyone to proceed as I did. My motivation was the
815 enjoyment of exploring new topics, and I wholeheartedly recommend everyone to give in to
816 the pleasure of writing.

817 **Acknowledgements**

818 I would not have thought of writing such a paper as this *Food for Thought* without the
819 invitation from the Editor of this journal, Dr. Howard Browman, in November 2015. I
820 hesitated for more than five years before I started writing what I had in mind, mostly because
821 I was not sure if it would be appropriate for a *Food for Thought*. However, as colleagues
822 progressively published excellent *Food for Thoughts* with a wide variety of approaches, I
823 decided to take the plunge. The ideas I put on paper were a follow-up to *Scientific Research*
824 *and Discovery*, cited in the text. I am very grateful to Howard and my brother Pierre for their
825 comments and suggestions on the manuscript. The imagination, expertise and support of my
826 collaborators over the years have greatly contributed to my pleasure of writing.

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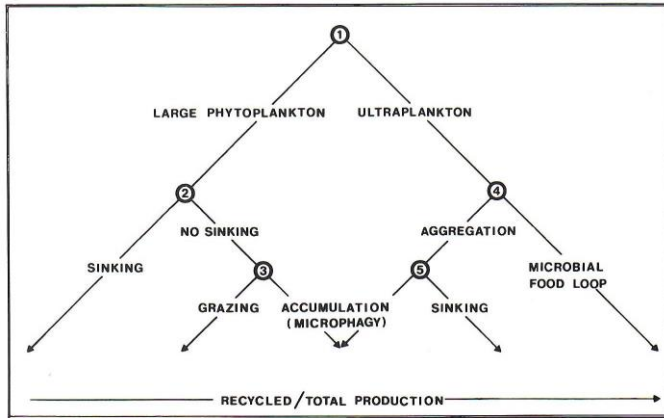
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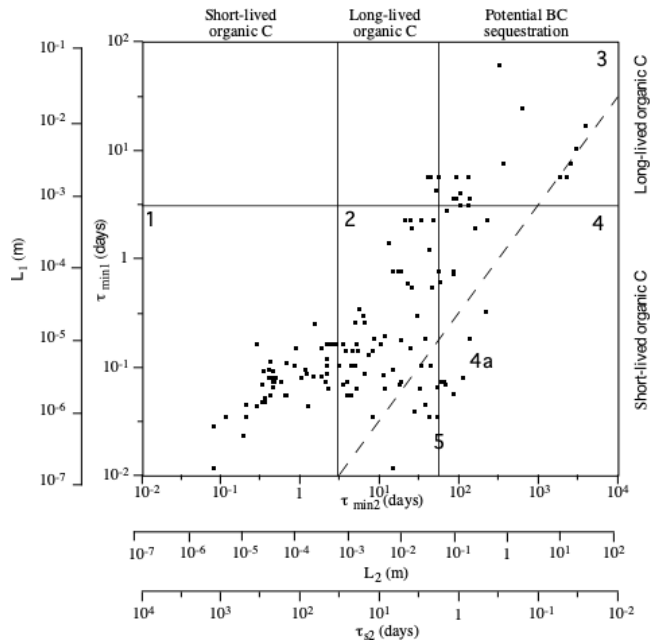
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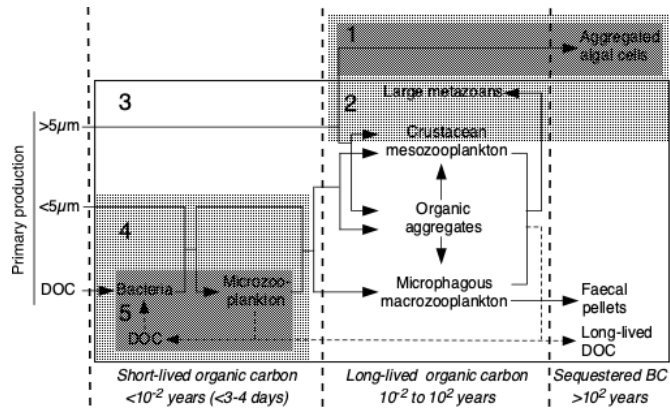
969

970 Fig. 1. Conceptual model of export production (downward arrows) in the ocean (Legendre
 971 and Le Fèvre, 1989, their Fig. 1). At each bifurcation, part of the primary production may be
 972 channelled into export pathways, which does not preclude coexistence with recycling
 973 pathways. According to the authors, hydrodynamical singularities controlled the five
 974 bifurcations. Figure reprinted with permission from John Wiley and Sons.
 975



977

978 Fig. 2. Fluxes of biogenic carbon in the oceans: size-dependent regulation by pelagic food
 979 webs (Legendre and Michaud, their 1998, Fig. 2). Scatter diagram of 139 taxa and
 980 developmental stages of marine pelagic organisms. $\tau_{\min 2}$: minimum turnover time of BC in
 981 organisms, as computed from their length (L_2). τ_{s2} : calculated residence time of BC in the
 982 sinking faecal pellets of L_2 -sized organisms, with $z_s = 1000$ m. $\tau_{\min 1}$: minimum turnover time
 983 of BC in food organisms, as computed from their length (L_1). Top and right: carbon pools
 984 corresponding to different τ . Solid lines: threshold values $\tau_{\min} = 3$ days (<3 days: short-lived
 985 organic carbon) and $\tau_s = 2$ days (>2 days: potential sequestration of biogenic carbon). Dashed
 986 line: $\xi_{21} = L_2 / L_1 = 5 \cdot 10^3$ (to the right: large microphagous zooplankton). Identified on the
 987 figure: four main functional groups of taxa and stages (1 to 4) and two additional groups (4a
 988 and 5). Figure reprinted with permission from Inter-Research Science Publisher.

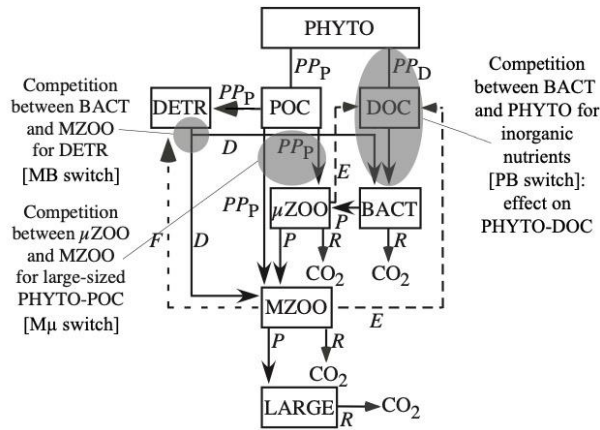


989

990 Fig. 3. Conceptual model of food-web mediated carbon fluxes combining the versions of
 991 Legendre and Rassoulzadegan (1996, their Fig. 1) and Legendre and Michaud (1998, their
 992 Fig. 4). Solid arrows: major flows of biogenic carbon in the euphotic zone of oceans, from
 993 phytoplankton production in three size classes — dissolved organic carbon (DOC), and
 994 phytoplankton $<5\mu\text{m}$ and $>5\mu\text{m}$ — to three biogenic carbon pools (short-lived, long-lived,
 995 and sequestered). Dashed arrows: food-web recycling of DOC, from consumers to
 996 heterotrophic bacteria (as consequence of viral lysis, sloppy feeding, excretion by herbivores,
 997 and degradation of faecal material and other detritus) and carbon sequestration as long-lived
 998 DOC. The 5 identified subsets of flows (rectangular boxes) correspond to known pathways of
 999 biogenic carbon: (1) sinking of ungrazed phytoplankton, (2) herbivorous, (3) multivorous and
 1000 (4) microbial food webs, and (5) microbial loop. Each number corresponds to a given
 1001 pathway: box 5 is within box 4, which is within box 3; box 1 is within box 2, which is partly
 1002 within box 3. Figures used with permission from Inter-Research Science Publisher.

1003

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1005

1006 Fig. 4. Schematic representation of the food web model used by Legendre and Rivkin (2015,
1007 their, Fig. 1) to estimate the effects of three competition switches, which control the flows of
1008 carbon toward either the microbial hub (heterotrophic bacteria, BACT, and
1009 microzooplankton, μZOO) or other food web compartments. The seven compartments of the
1010 model are (1) PHYTO-POC, particulate organic carbon (POC) produced by phytoplankton
1011 (PHYTO); (2) dissolved organic carbon (DOC) from PHYTO and excreted by both μZOO
1012 and mesozooplankton (MZOO); (3) BACT, which use DOC and detrital POC (DETR);
1013 (4) μZOO, which consume POC and BACT; (5) MZOO, which consume POC and DETR;
1014 (6) large animals (LARGE), which consume MZOO or food that is derived from MZOO; and
1015 (7) DETR, which comes from PHYTO and metazoans, mostly MZOO. The arrows represent
1016 carbon flows into and out of compartments: primary production (PP particulate, PPP; PP
1017 dissolved, PPD) and heterotrophic detritus consumption (D), excretion (E), egestion (F),
1018 production (P) and respiration (R). Solid arrows show forward flows; dashed arrows show
1019 backward flows. Shaded areas identify the locations of the three competition switches. Figure
1020 reprinted with permission from Inter-Research Science Publisher.