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

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Abstract

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

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

The pleasure of writing

In my book *Scientific Research and Discovery*, I wrote: "one of the most efficient ways to develop original scientific ideas is to write […] Writing as early as possible goes against the natural tendency of researchers to consider the data at great length before starting to write, with the hope that the data would somehow generate new ideas. Of course, analysing the data contributes to provide ideas […], but I am convinced that the most original or interesting ideas in a large proportion of studies appear at the time of writing" (Legendre, 2004, 2008).

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

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

The most important scientific paper in my career

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

This paper was the most important in my career not because it was my first, but because of a special series of events. To understand it, one has to know that in Québec in those days, one type of pre-university education took place in *Classical Colleges*, whose curriculum included six years of languages and sciences followed by two years of philosophy and sciences. Graduates from this program could be admitted to the second year of university, whereas those graduating from the regular six-year high-school program could be admitted to the first year. The Classical Colleges disappeared in the 1970s.

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

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

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

Generally, writing a publication follows from previous or ongoing research, but there are instances when a paper is written in response to external stimuli or circumstances, and this paper influences the author's research direction. This happened to me on several occasions, and some of such papers that did not follow directly from the research I was conducting at the time strongly influenced my research directions or my professional activities. Here I provide selected examples of the somewhat counterintuitive situation where writing a paper influenced my research instead of the more usual opposite sequence of events. The papers mentioned here are not necessarily my most cited, but they deeply influenced the course of my activities.

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

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

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

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

In 1989, I was invited to give a lecture at a session on phytoplankton blooms during the *Fifth International Symposium on Microbial Ecology* in Kyoto (Japan). In a way somewhat similar to the Dalhem conference mentioned above, the instructions to invitees were: if you wish to have your expenses reimbursed, you must submit the manuscript of your talk by the beginning of the conference. Since I was interested to attend the ISME and visit Kyoto, I researched the literature on blooms. I already knew that in most publications, the term "bloom" was generally applied to the winter-spring phytoplankton burst, whose initiation was generally explained by a physical mechanism proposed by Sverdrup (1953). I also knew that phytoplankton blooms could occur on time scales ranging from tidal to episodic and annual (Legendre, 1981), and my review of the literature led me to formally define blooms as "rapid increases in biomass, caused by locally enhanced primary production and resulting in abnormally high cell concentrations" (Legendre, 1989, 1990). Using the mechanism proposed by Sverdrup (1953), I distinguished between blooms governed by irradiance or by nutrients, and discussed effects of blooms on marine food webs and their overall significance. I thus became very interested in the mechanisms and effects of blooms on ecosystems and the marine carbon cycle, and published many papers on this topic during the following years in which I referred to the seminal paper of Sverdrup (1953). I later found, to my great surprise, in a review dedicated to Sverdrup's paper (Sathyendranath et al., 2015) that I was the third author who had most often cited Sverdrup (1953) in his publications. My long-term theoretical studies on phytoplankton blooms followed from the ISME invitation.

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

In 2004, I was invited to present a plenary lecture during the *First Symposium on the Ocean in a High-CO_2 World*, held at the Unesco headquarters in Paris (France). I then proposed, together with my long-time collaborator Richard Rivkin from St. John's (Canada; our first joint paper had been Rivkin et al., 1996), a framework for a new class of models that would consider the interactions, in the upper ocean, of functional types of plankton organisms, food web processes that affect organic matter, and biogeochemical carbon fluxes (Legendre and Rivkin, 2005). This led Richard and I to develop and use such models to explore the roles of planktonic ecosystems in ocean carbon fluxes (see section *Long quests through several publications*, below).

In 2009, I was invited to join the SCOR-InterRidge *Working Group on Hydrothermal Energy Transfer and its Impact on the Ocean Carbon Cycles*. This invitation came as a surprise given that I had never worked on hydrothermal systems, but it was explained to me that I was expected to provide expertise on data synthesis. During the following six years, I learned a lot about marine hydrothermal systems, and developed keen admiration for the determination of researchers who collected data with great effort in these hostile environments. I was very pleased to contribute to a geochemical modelling study on hydrothermal iron cycling and deep ocean organic carbon scavenging (German et al., 2015), which got me started in Earth System Science (see also my book Bertrand and Legendre, 2021, in section *The importance of writing books, if possible starting early in the career,* below).

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

I had liked very much the IMBIZO II format, and thus decided to participate in 2013 in the workshop of IMBIZO III dedicated to *The impact of anthropogenic perturbations on open ocean carbon sequestration via the dissolved and particulate phases of the biological carbon pump*, in Goa (India). The workshop targeted interactions between the ocean biological and microbial carbon pumps, the latter being then quite new (Jiao et al., 2010, 2011). Together with collaborators, I developed a quantitative approach to compare the different ocean carbon pumps, and provided the first quantitative estimate of the microbial carbon pump (Legendre et al., 2015). The co-conveners of the workshop were Nianzhi Jiao (China), Farooq Azam (USA), Carol Robinson (UK) and Helmuth Thomas (Canada). I already knew Farooq, Carol and Helmuth well, but it was my first opportunity to meet Nianzhi. Following the Goa meeting, Nianzhi and I developed a strong collaborative program on various aspects of marine microbial ecology and biogeochemistry, and led together to the creation of the *Gordon Research Conference on Ocean Biogeochemistry* in 2016. This collaboration with Nianzhi and other researchers in China was an important factor in my election as Foreign Member of the *Chinese Academy of Sciences* in 2019.

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

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

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

The pleasure of writing with co-authors

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

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

As indicated above, this retrospective analysis of my publications was the first I had ever done. I already knew that I enjoyed creative intellectual interactions with close collaborators, but I had not realized that this had influenced my long-term publication record so much. Writing so many publications with one or two co-authors resulted from a preference, at least on my part, for analysing data and/or developing new ideas with collaborators instead of doing it alone. Why this preference?

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

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

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

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

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

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

Writing as a tool of scientific creativity

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

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

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

What I described in the previous paragraph is the emergence of a new idea, model or hypothesis during the writing of a scientific publication. A key characteristic of the resulting new approach is its potential to explain not only the observations from which it was derived, but also other data of the same type. Hence, the new approach could eventually be rejected if it failed to explain additional data or enough of them (this rejection is called *falsification* in the case of hypotheses). The new approach that emerged at the time of writing thus contributes to the advancement of knowledge. This is very different from the dubious practice of *HARKing* (i.e. Hypothesizing After the Results are Known), which consists in presenting in the introduction of a research report a hypothesis based on or informed by one's result as if it were an *a priori* hypothesis (Kerr, 1998). Such hypotheses are called *ad hoc* (sometimes *post hoc*, a qualifier that in fact refers to the fallacy of arguing from a temporal sequence to a causal relation, as recalled in Legendre, 2004, 2008). An *ad hoc* hypothesis or model (also called a *rationalisation*) is devised for the particular case at hand, without consideration of wider application. Because the *ad hoc* approach does not explain other observations than those under consideration, it does not contribute to the advancement of knowledge.

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

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

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

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

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

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

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

Long scientific quests through several publications

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

First example. I use as starting point the study of Beaugrand et al. (2010), in which I collaborated in investigate changes in phytoplankton and zooplankton biodiversity in the extratropical North Atlantic Ocean between 1960 and 2007. The paper reported a pronounced latitudinal increase in biodiversity during the 47-year period, and a parallel decrease in the mean size of copepods. The analysis led to the conclusion that this decrease had negative effects on the downward biological carbon pump and on demersal Atlantic cod (*Gadus morhua*). These negative effects were evidenced using allometric relationships proposed by Legendre and Michaud (1998), whereby: the minimum turnover time of carbon incorporated in organisms is directly related to the size of organisms; and downward carbon export is directly related to the size of organisms that produce sinking particles, here the sizes of copepods that determine the sinking velocities of their faecal pellets.

I had defined the above allometric relationships with Josée Michaud (Laval University, Canada) in a study where we had quantified the flux of biogenic carbon (BC) acquired by organisms feeding on food particles or other organisms both towards large metazoans and downwards from surface waters (Fig. 2). To do so, we had developed allometric equations to quantify the minimum turnover time of BC in food (τ_{min1}) and marine pelagic organisms (τ_{min2}), and the residence time (τ_s) of BC above the depth below which BC cannot rapidly return to the surface waters or the atmosphere (i.e. sequestration depth, z_s,, e.g. 1000 m). We had used τ_{min1}, τ_{min2} and τ_s in conjunction with the size ratio of consumers to their food particles (ξ) to assess the food-web regulation of BC fluxes, i.e. the consumption of particles or prey (with short τ_{min1}) by larger-sized organisms caused a lengthening of the residence time of BC incorporated in the body mass of larger organisms (longer τ_{min2}) and a shortening of τ_s due to the aggregation of BC in faster sinking faecal material. These two effects cause increased carbon fluxes towards the pools of long-lived organic carbon ($10^{-2} < \tau < 10^2$ year) and sequestered BC ($\tau > 10^2$ year), respectively.

The originality of the above approach was to transform the sizes of organisms, their foods, and the particles they produce (i.e. faecal material) into units of time (i.e. τ_{min1}, τ_{min2}, and τ_s, respectively), which were relevant to the three carbon pools defined by Legendre and Le Fèvre (1992), i.e. short-lived organic carbon ($\tau < 10^{-2}$year), long-lived organic carbon and sequestered BC (τ defined in the previous paragraph). I explained later in Legendre (2004, p. 146-153) how dimensional analysis had helped me to develop this approach, which made it possible to formally connect food-web feeding processes and ocean biogeochemical carbon fluxes.

The approach of Legendre & Michaud (1998) combined four key concepts I had elaborated in previous papers. The *first concept* was the classification of biogenic carbon in the ocean into three pools with the different turnover times cited in the previous paragraphs (Legendre and Le Fèvre, 1992). In that paper, we used our new carbon pool concept to unify food-web related biogeochemical carbon fluxes, and explained how these were largely governed by hydrodynamics. We also identified refractory dissolved organic carbon as a form of sequestered BC, a recognition that underlies the microbial carbon pump proposed later by Jiao et al. (2010, 2011) cited above. The *second concept* was the recognition of five types of pelagic ecosystems based on the relative size structures of phytoplankton production and standing stocks, the latter reflecting the effect (or absence) of grazing by zooplankton (Legendre and Le Fèvre, 1991). The two extreme cases in this typology were ecosystems with phytoplankton production and biomass both dominated by large cells (e.g. ice-edge blooms) and by small cells (e.g. oligotrophic ocean). These five types were used to illustrate the above three biogenic carbon pools. The *third concept* was the following hypothesis (supported by data) that pelagic organisms that package small particles into larger ones lengthen the turnover time of biogenic carbon (i.e. τ) and, in some cases, transfer this carbon from a given carbon pool to a longer lived one; and the lengthening of turnover time is a direct function of the ratio between the size of organisms and that of their food particles (i.e. ξ) (Fortier at al., 1994). We found in that study that the most efficient re-packagers of small particles into larger ones were salps, appendicularians, doliolids and thecosome pteropods, which all feed on particles at least 3.5 orders of magnitude smaller than their own size. In a *fourth conceptual step*, I combined the key concepts from the previous three papers in a theoretical model describing the fluxes of carbon production from phytoplankton in three size classes to the above three carbon pools (Legendre, 1996), and I refined this model in following papers (Fig. 3; Legendre and Rassoulzadegan, 1996, Legendre and Michaud, 1998).

However, the theoretical model in Fig. 3 could not easily be transformed into equations because the physical dimensions of its two components were different, i.e. size (phytoplankton classes, Y-axis) and time (carbon pools, X-axis). I explained above how I resolved this problem by using allometric relationships to transform the size of organisms and their foods into residence time of carbon (τ_{min2} and τ_{min1}, respectively, in Fig. 2; Legendre and Michaud, 1998). In the latter paper, we also used an empirical relationship between the sinking velocity of faecal pellets and the size of the organisms producing them to compute the residence time of faecal pellets above depth z_s (τ_s).

The above paragraphs explain how a 2010 paper used allometric relationships published in 1998, which were themselves rooted in papers written in 1996, 1994, 1992 and 1991. The sequence of studies from 1991 to 2010 extends over twenty years. The study of Legendre and Rassoulzadegan (1996) cited above proposed an approach to determine, using a small number of food-web or hydrodynamic variables, the partitioning of phytoplankton production among three carbon fluxes, i.e. remineralization within the euphotic zone, food-web transfer, and sinking to depth of organic particles. In addition, the approach of Fortier et al. (1994) led us to propose in a subsequent paper a quantitative explanation to the dominance of different parts of the Southern Ocean in different seasons by either salps, krill or some large copepods (Le Fèvre et al., 1998).

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

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

The above paper was a follow-up to a previous study in which we had shown that phytoplankton, microbial heterotrophic plankton and large zooplankton were the three food-web control nodes of five major carbon fluxes, i.e. phytoplankton production, and its partitioning into respiration, transfer to the food web, and downward export as both DOC and POC (Legendre and Rivkin, 2002). Using this approach, we had found that the microbial heterotrophic plankton node was responsible for most of the respiration of organic carbon to CO_2 and the uptake and release of DOC, and the large zooplankton node controlled both the transfer of POC to large metazoans and part of the downward POC flux (i.e. faecal pellets and vertically migrating organisms). We had also identified regions of the world ocean that are net autotrophic and net heterotrophic.

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

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

The two examples of long quests stress that the generation of new ideas and approaches is not always determined by long-term targets, but can develop as an evolutionary process where new papers build upon previous publications. Each new paper is somewhat like a "mutation" in biological evolution when its key ideas had not been foreseen at the time the previous papers were written; "selection" on the proposed new ideas or approaches is exerted first by reviewers and editors, and then by readers who cite the publication or not; and evolutionary "success" is achieved when the key proposal of a publication is used by colleagues in their own work. Success may be complete when one's proposal is cited without reference to the original work because it has become part of general knowledge in the discipline. Some may perhaps fear that long scientific quests are no longer possible in today's funding and academic environment, but I know many successful researchers who cleverly find ways to pursue long-term personal research agendas in today's environment and, thus, contribute significantly to the development of knowledge.

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

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

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

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

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

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

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

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

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

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

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

Researchers could have time to write textbooks when they are involved in fewer projects and responsible for fewer tasks, for example during sabbatical leaves or late in their careers. However, scientists who have not experienced writing books early in their careers generally do not manage to write them later on. As an example, I sadly remember a high-profile colleague I had convinced to write a textbook based on his remarkable teaching notes, who gave up the task after a few months because he was too eager to write more research papers. I realize that the idea of encouraging early-career researchers to write textbooks is counterintuitive, but it could become realistic in some instances if they were encouraged to do so by more senior colleagues. For example, a senior scientist could offer a younger colleague to write a textbook with her/him, the younger colleague being the first author. This could be a great way to combine experience with enthusiasm, and shelter the younger colleague from the criticism of being "too young to write a textbook".

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

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

More recently, I co-authored with Philippe Bertrand an Earth System Science book entitled *Earth, Our Living Planet* (Bertrand and Legendre, 2021). Again, the origin of this book is interesting. Philippe, who is a French marine physical chemist, had written a very original book on the Earth System entitled *Les attracteurs de Gaia* (2008). I had read this thought-provoking book, and suggested to Philippe that he publish an English version of it. After hearing my suggestion a few times, Philippe told me that he wished us to write the English version together. We rapidly decided to write a book less theoretical than Philippe's original, which would be accessible to scientifically literate non-specialists, and looked for a publisher potentially interested in such a book. We found that Springer's *Frontiers Collection* was dedicated to this type of books, and over the next years used a combination of astronomy, biology, chemistry, ecology and geology to explain the progressive takeover of Earth by organisms. We now hope that our book will be well received by non-specialists as well as undergraduates, graduates and researchers wishing to understand the co-evolution of Earth and its organisms.

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

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

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

Publication in peer-reviewed media is often a sweet-and-sour experience. The sweet parts include: successfully submitting the manuscript; reading constructive comments from reviewers; being informed by the editor of the journal or the book series that the manuscript is accepted for publication; and seeing one's work in the published form. The sour parts of the publication experience may include: spending hours submitting the manuscript on a sometimes-intricate website; receiving the message from the editor of the journal that the manuscript is rejected, or from the book series editor that the topic of the proposed book is not appropriate; reading reviewer comments formulated in a nasty way, even if these comments may be useful for improving the manuscript; and receiving proofs full of typos, which is fortunately not common since the widespread use of electronic manuscripts. On the whole, as for sweet-and-sour dishes, even if the first experiences may be unsettling, one develops over time the taste for peer-reviewed publication. Some of the sour parts are unavoidable, such as having manuscripts rejected from time to time, reading reviewer comments that are not always constructive, or having to use different formats for the text and the references in each different journal. Other sour parts can be avoided, such as writing nasty comments when reviewing a paper, even if it is full of errors. In all cases, more experienced researchers should advise and support early-career scientists when these receive feedback from their first manuscript submissions, which is especially true for thesis supervisors with their students.

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

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

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

Conclusion: Giving in to the pleasure of writing

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

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

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

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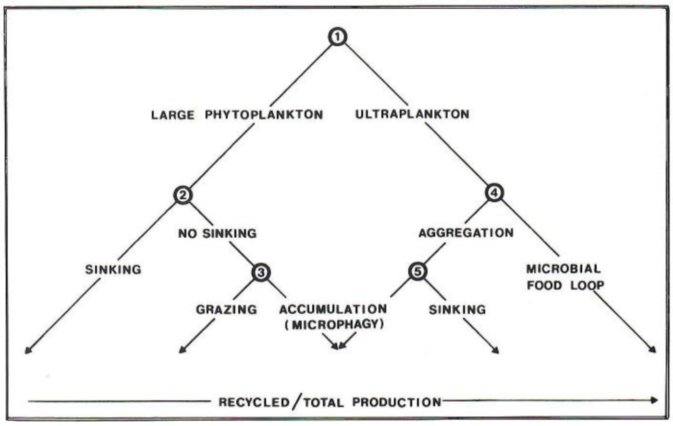

Fig. 1. Conceptual model of export production (downward arrows) in the ocean (Legendre and Le Fèvre, 1989, their Fig. 1). At each bifurcation, part of the primary production may be channelled into export pathways, which does not preclude coexistence with recycling pathways. According to the authors,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

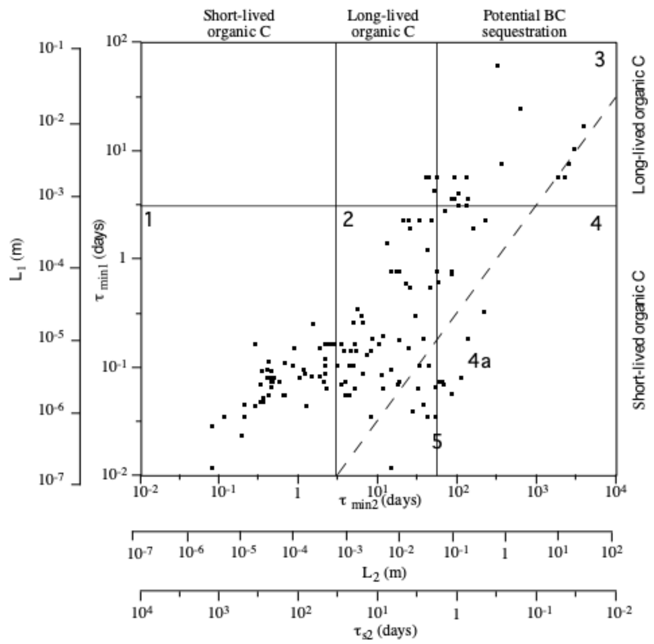

Fig. 2. Fluxes of biogenic carbon in the oceans: size-dependent regulation by pelagic food webs (Legendre and Michaud, their 1998, Fig. 2). Scatter diagram of 139 taxa and developmental stages of marine pelagic organisms. τ_{min2}: minimum turnover time of BC in organisms, as computed from their length (L_2). τ_{s2}: calculated residence time of BC in the sinking faecal pellets of L_2-sized organisms, with z_s = 1000 m. τ_{min1}: minimum turnover time of BC in food organisms, as computed from their length (L_1). Top and right: carbon pools corresponding to different τ. Solid lines: threshold values τ_{min} = 3 days (<3 days: short-lived organic carbon) and τ_S = 2 days (>2 days: potential sequestration of biogenic carbon). Dashed line: $\xi_{21} = L_2 / L_1 = 5\ 10^3$ (to the right: large microphagous zooplankton). Identified on the figure: four main functional groups of taxa and stages (1 to 4) and two additional groups (4a and 5). Figure reprinted with permission from Inter-Research Science Publisher.

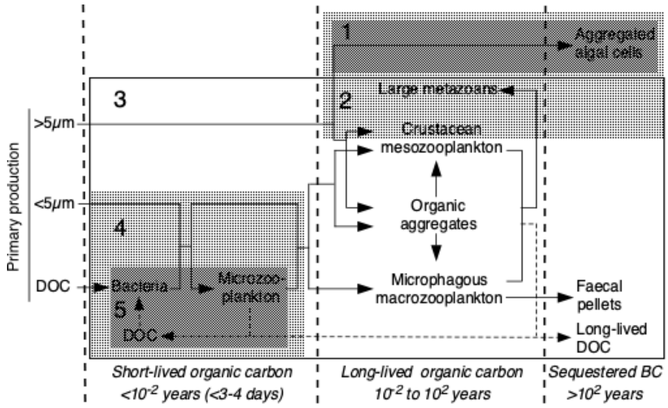

Fig. 3. Conceptual model of food-web mediated carbon fluxes combining the versions of Legendre and Rassoulzadegan (1996, their Fig. 1) and Legendre and Michaud (1998, their Fig. 4). Solid arrows: major flows of biogenic carbon in the euphotic zone of oceans, from phytoplankton production in three size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

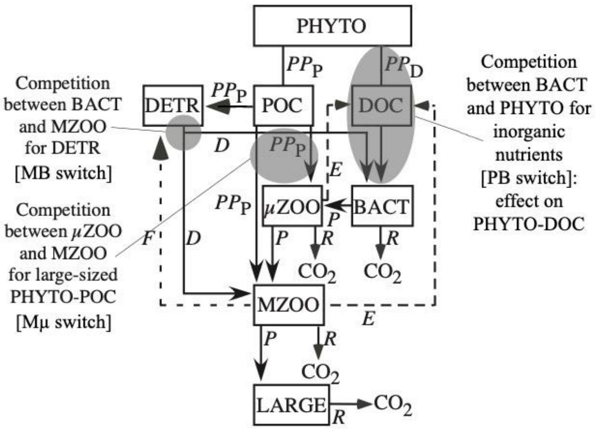

Fig. 4. Schematic representation of the food web model used by Legendre and Rivkin (2015, their. Fig. 1) to estimate the effects of three competition switches, which control the flows of carbon toward either the microbial hub (heterotrophic bacteria, BACT, and microzooplankton, μZOO) or other food web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