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Marine Sciences: from natural history to ecology and back, on Darwin's shoulders

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The naturalist Charles Darwin founded modern ecology, considering in a single conceptual framework the manifold aspects regarding the organization of life at various levels of complexity and its relationship with the physical world. The development of powerful analytical tools led to abandon Darwin's natural history and to transform naturalists, as Darwin labelled himself, into the practitioners of more focused disciplines, aimed at tackling specific problems that considered the various aspects of the organization of life in great detail but, also, in isolation from each other. Among the various disciplines that stemmed from the Darwinian method, ecology was further split into many branches, and marine ecology was no exception. The compartmentalization of the marine realm into several sub-domains (e.g., plankton, benthos, nekton) led to neglect of the connections linking the various parts that were separated for the ease of analyses that, in this way, prevented synthetic visions. The way marine sciences were studied also led to separate visions depending on the employed tools, so that ship-based biological oceanography developed almost separately from marine station-based marine biology. The necessity of putting together such concepts as biodiversity and ecosystem functioning is rapidly leading to synthetic approaches that re-discover the historical nature of ecology, leading to the dawn of a new natural history.

Keywords: Ecology; evolution; biological oceanography; marine biology; biodiversity; ecosystem functioning

Introduction

Human culture probably started when early man acquired and elaborated knowledge about nature, having to learn how to recognize species, how to find or avoid them, and how to transmit this knowledge from one generation to the next, through learning from teaching. Only thereafter did man elaborate more abstract concepts. The exploration of nature proceeded in every culture, during human history, but it reached its apex in the western world, with both the invention of a universal way of naming organisms (the Linnean nomenclature) and the formalization, through ecology and evolutionary biology, of the patterns and processes that can describe and interpret the living world around us, including ourselves.

Europe, along with ruthless colonialism, started to explore the rest of the world in a systematic way, organizing the knowledge of nature into a single, grand system. During Victorian times, Charles Darwin changed the world of both science and philosophy,

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showing that nature evolves, and that we are part of it. Darwin labelled himself a naturalist and his discipline was natural history. The great museums that sprouted in those times were, and still are, Museums of Natural History.

Darwin was also a paleontologist: he showed that life has a history and that the species that inhabit the world today are not the same as those that inhabited the world in the past. Life, and nature in general, thus, does have a history. The present is the product of the past, and cannot be understood if the past is unknown.

The disciplines studying nature

Physics is often taken as the queen of sciences, being the discipline that formulates the basic rules that govern the natural world. Etymologically, physics derives from the Latin *physica*, and the Greek *phusika* 'natural things', both stemming from *phusis:* 'nature'. So, physics is a science that studies nature, but its rules (laws) are not related to natural history, since they identify regular behaviours of natural things, that invariably occur whenever a given set of conditions is met. Quantum physics and chaos theory demonstrated that this is not the case even in the world of physics, but the label of 'predictive' was and still is often boldly attached to ecology as a science, as if inherent unpredictability did not exist. A law, in fact, predicts the behaviour of a system, once the initial conditions are known. History has little to do with this exercise, and the disciplines allowing for the identification of laws are usually labelled as ahistorical. The laws of physics are always valid, otherwise they would not be laws: they are necessary but not sufficient to understand nature [1]. If nature were obeying just to the laws of physics, there would be no history, and the universe would remain always the same, or it would change in a very predictable fashion. In a way, this is true, even if we cannot be completely sure about the validity of these predictions. Physicists, in fact, have calculated the date of the origin of the universe, and are even proposing a date for its end. They can calculate when the sun will be exhausted, or where Mars will be in the future, so that our rockets will reach it. There is no history when everything is predictable. The origin of life introduced history in a world that would have been understandable just by physics, with a completely predictable history (besides the unpredictable phenomenon that led non living matter to a living state). This lack of explanation justifies religious faith in many scientists. Living matter evolved in a very varied way, and led to a myriad of forms that started to interact with each other and with the physical world in such an intricate way to make the outcome of their reciprocal influences inherently unpredictable. If physics were sufficient to understand the world, chemistry would not have been elaborated, and the same can be said for biology when confronted with chemistry, even though biology (and especially zoology and botany) was probably the first science that man elaborated.

The more the organization of matter becomes complex, the more we are confronted with historical rather than ahistorical domains.

The tasks of historians

Historians describe past historical patterns, and try to infer about the processes that led to their occurrence; they are not asked to produce predictions about future history. We know that the history of the future cannot be predicted, even if we would like to know about the future [2]. The knowledge of history, however, confers a wisdom that allows for the elaboration of possible future scenarios, not envisaged as precise forecasts of the course of events but only as general pictures of future situations. Of course, historians can identify patterns and processes that occur in a more or less repetitive way during the course of history: for instance, all great empires are destined to decline. But this 'discovery' equals the consciousness that all living beings are destined to die. The difficult task is not to predict that great empires will fall, it is difficult to predict when they will fall, and how, and why. Some historian predicted the end of history after the USSR fell, but this prediction was soon contradicted by such 'contingencies' as September 11. This should teach us that history can be properly understood only after some temporal distance from the events of the past, whereas the present is more unfathomable due to our emotional involvement.

Some crucial questions, at this point, are:

Are the properties of natural history so different from those of human history?

Does natural history have the same limits that we recognize to human history, in terms of prediction of the future?

Why human historians are not asked to predict future history whereas modern natural historians (now known as ecologists) are continuously asked to predict the future of the history of natural systems?

Why do historians do not even dream of predicting future history, whereas natural historians do not find it dishonest to predict the future of natural systems?

Is the 'selling' of historical predictions a matter of over-self-confidence, or is it a matter of committing the epistemological mistake of considering historical systems are behaving as if they were ahistorical?

Scientific natural history

Elton [3] wrote that ecology was simply 'scientific natural history'. Hence, ecology replaced natural history at the beginning of the past century, trying to evolve from a descriptive (and explanatory) to a predictive stage. The rationale to achieve this goal was borrowed from physics: reductionism. Complex, inextricable problems (the proverbial entangled bank used by Darwin as an example of natural system) were divided into many sub-problems that allowed a deeper analysis of reality or, better, of portions of reality that were extracted from their context. The extraction can be accomplished either by selective observation, or by experiments. In both cases, complexity is reduced (almost eliminated) with the goal of finding general rules that regulate the functioning of subsets of natural phenomena: the laws of nature. The born again natural historians, once transformed into ecologists, became more similar to physicists (and chemists) than to anything else. They abdicated to zoologists and botanists the study of the details of nature, considering these sciences as merely descriptive, and searched for 'laws'. Physic's envy [4] started in that period, and triumphed in ecology.

Ecology and economy

Both human and natural history are descriptive and interpretative, and are not aimed at performing predictions. The practitioners of these sciences, however, felt the urge of being more precise and predictive, developing new approaches that passed from description to prediction, using mathematical tools to arrange facts and their interactions according to algorithms that are intended to lead, once initial conditions are known, to predict future conditions [5]. Economists used these tools to predict the outcome of the exchanges of goods and services among humans, ecologists did the same for the rest of nature. Recently, with the ecosystem approach on the one hand, and with the advent of ecological economics on the other hand, the inclusion of man into nature (and vice versa) led to some merge of the two disciplines.

In the last century, and especially during the final decades of the second half of it, with the advent of computers, an unprecedented power of performing calculus became available to both ecologists and economists. Ready-made computational programmes became available and future telling was at hand for those who were able to insert some data and press the right button. The outcome of these exercises is under the eyes of everybody. Economic systems are in severe distress, ecological systems are collapsing or, anyway, are not behaving according to our expectations. Of course, some economists and some ecologists predicted the troubles, whereas others did not, providing magic recipes that promised effective management and governance of both human and natural affairs. The practitioners of history never promised the future, whereas economists and ecologists did. Maybe they were promising the impossible.

A psychological constraint

Both stakeholders or politicians, when asking scientists about something, like to have answers that will depict what will happen if something will be done. Or, in alternative, what is going to be done to achieve a desired goal. The more re-assuring will be the answers, the happier the questioners will be. Those who have magic solutions are better accepted than those who suggest care and warn about uncertainties. Solutions, furthermore, must be rapid and economically advantageous. One might ask: would you accept to travel to the moon with a rocket that has been built with this philosophy? This is the way we deal with our trip on this planet!

The short-term solutions invariably lead to long-term problems that are worse than the initial problems, but those who caused the new problems will propose magic solutions again and, paradoxically, they will be followed by stakeholders with inefficient long-term memory. Psychologically, we like solutions better than problems: our inclinations towards unjustified optimism led to great advances that, however, are now revealing that our 'success' is making this world almost inhospitable for our species (nature is not jeopardized... we are!).

Short-term successes of the physics-like way of performing science also in historical systems, in fact, led to the conviction that this was The right way of performing any science, and epistemologists set rigid rules about how to perform science, taking physics as the model for all other sciences.

An epistemological mistake

Popperian approaches to science have been extremely successful, and they pervade the way of performing science today. Such approaches advised scientists to produce hypotheses and then to try to falsify them. It is impossible to prove such hypotheses right, but if they are not proven wrong (falsified), they are temporarily taken as valid; if they are proven false, however, even in a single case, then they must be rejected. And new hypotheses must be elaborated so to replace them.

This attitude implies that scientists should search for hypotheses that work as laws: they must be verified, albeit provisionally, every time they are tested. These laws, of course, predict the future, and can be phrased in a 'if... then' fashion. If the conditions are these, then the outcome will be that. The most famous natural laws are the laws of physics. We have seen, however, that physics is rather ahistorical and that the outcomes of interactions of objects falling in the domain of physics are rather predictable. Once history is introduced into the system, predictions are liable of failure, due to contingencies. That's why the laws of physics are valid if 'nothing strange' happens. As it happens with perfect or ideal gases, or with frictionless systems. You cannot let weights fall from the Leaning Tower of Pisa (as Galileo did) during a storm, otherwise the laws about their falling down will not be respected with precision (i.e. they will be proven false, at least under that circumstance, since their predictions will be hindered by the storm, even though gravitation will eventually prevail, but not with precisely predictable outcomes, besides the obvious one that, sooner or later the weights will arrive to the ground). History obeys laws, but 'strange' things (the storm) can break them. These 'strange things', that we call contingencies, however, do not allow for the formulation of better laws, once the ones that do not cover contingencies are rejected.

It is an epistemological mistake to ask historical systems to behave as if they were ahistorical, trying to formulate laws in a domain that does not allow for laws. It is also an epistemological mistake, furthermore, to take physics as the best representative of how science should be carried out. If this were true, as argued before, then physics should be sufficient to understand the world, and all the other disciplines would be useless (as a matter of fact, many physicists are secretly convinced that this is the case and, which is even worse, many biologists tend to believe that this is almost true, developing physics' envy). These disciplines become useless if they mimick physics, trying to obtain its results (the production of laws) by using its methods (mathematization and reductionism) in domains where these methods do not work (when contingencies take their toll). In these cases, the methods and the philosophy of physics are not sufficient, in spite of being, of course, necessary. Laws explain a great deal of the world but, in some cases, they are not enough. This does not mean that we have to reject the laws (and that physics is useless), we have only to apply them loosely. We need physics to understand the basics of the organization and behaviour of matter, but then we need more than its rigid laws, even if we will get less from the new tools. More means that physics is not enough, less means that we will not get as much as we get from physics, in terms of predicting the future (with mathematical precision). This is not due to the faults of the tools, but to the features of the investigated systems.

More than one law

In ecology, for instance, there are at least three models that predict the development of a community: the facilitation, the inhibition, and the tolerance model [6]. Of course, when one model is validated, the others are falsified. What is rejected, however, is not the model but, instead, its universality. There is not a universal law regulating the patterns and processes that lead a community to change with time. The rules are loose, and they can be valid in combination, so that no community will ever devolop like another one, even though some regularity in the way communities develop might be recognized and some loose predictions can be made. The development of a community is not like the behaviour of a weight that falls from some height (in absence of wind). A formula can easily describe the behaviour of the weight and predict it once some variables are known, but no formula can describe with the same precision the patterns and processes that will concur to lead a community from an early stage of development to a subsequent one.

If this is valid in ecological time, this is even more true in evolutionary time. Evolution, however, went through the very same philosophical troubles. When Eldredge and Gould proposed a saltational model of evolution, some claimed that Darwinian gradualism had been falsified, but the same was true each time gradualism was demonstrated. The only falsification was that of the universality of evolutionary processes. The evolution of species, just like the evolution of communities, can occur with different patterns and processes. There are many rules (laws?) governing these complex phenomena, Mayr [1], hence, coined the concept of multiple causality. What is falsified is the universality of the rules that, thus are not laws: their application leads to weak predictions.

The hard sciences lead to precise predictions, but they apply to simple systems, whereas the soft sciences do not lead to predictions, because they apply to complex systems.

This is known since a very long time, Darwin explained it very well when he wrote, in the Origin of Species: 'Throw up a handful of feathers, and all must fall to the ground according to definite laws. But how simple is this problem compared to the action and reaction of the innumerable plants and animals which have determined, in the course of centuries, the proportional numbers and kinds of trees now growing on the old Indian ruins!'. With these sentences, Darwin compared the law of universal gravitation to the description of the patterns and processes involved in the development and functioning of a community. In spite of this authoritative statement, made by the greatest scientist of all times, ecologists became afflicted by physics' envy, and tried to find out laws that did not exist, or that inevitably became the laws of physics.

The demise of natural history

Darwin's ideas were temporarily forgotten at the beginning of the 19th century, and the Modern Synthesis later revived them by the pervasive use of genetics. Strange enough, the founding disciplines of Darwin's theory (i.e. ecology and embryology) did not contribute much to the Modern Synthesis. The rise of ecology as a modern science, in the meantime, was linked to the mathematization of the phenomena described by natural history, leading to predictions [5]. Natural history was labelled as a 'story telling' discipline and, as phrased by Elton [3], it lost its reputation.

For almost a century, ecology tried to become like physics, or chemistry, and the exercise was very fruitful in terms of insight. Reductionism has its merits, and many facets of the functioning of nature became clearer, once extracted from the intricacies of natural phenomena. The understanding of portions of nature was a necessary passage to acquire knowledge about key phenomena that increased our insight into the functioning of nature. The role of ecology was to answer precise questions, leading to the identification of general rules that were supported by experimental evidence. Natural history was mostly aimed at describing nature, at recording presences (and absences) of species, their interactions and life cycles. This approach was labelled autoecology, and was carried out by either zoologists or botanists [7]. Natural history, being focused on species, was reductionistic, whereas ecology was holistic, at least in principle.

Biological oceanography vs. marine biology and ecology

The quest for general principles, and the need for experimental hypothesis testing, often by manipulation, led marine scientists to focus on easily accessible environments. The choice fell on intertidal habitats. Intertidal rocks, or mudflats and salt marshes, became the arena of experimentation, and many general principles of modern ecology originate from studies in these environments. The advantages of operating in the intertidal are great. Low tides allow easy access to the field, with ample possibilities of manipulation without the hassle of underwater work. Marine stations proliferated all over the world, and marine scientists chose study sites in their vicinity, to perform experiments and observations. These tendencies led to very careful knowledge of restricted areas, over long periods of time. At the opposite end of the spectrum, oceanography was mainly performed from ships, so leading to the exploration of vast areas that, however, were only seldom visited. Oceanography, furthermore, was mainly observational, whereas marine ecology was mainly experimental and often manipulative. Biological oceanography covers wide spatial scales, but over narrow time windows (the time of sampling stations during cruises), whereas marine biology and ecology cover small spatial scales but over long temporal scales, since experimental stations are visited regularly. This led to two ways of studying the same system (marine life) that were, and still are, almost completely separated. The separation of biological oceanography from marine biology and ecology, with different journals and different scientific societies, is a result of these research patterns and processes.

Born-again natural history

The last century was marked by powerful analytical efforts, and reductionistic approaches led to prodigious understanding of how natural systems are made and how they function. Reductionism is of course necessary, now we have to accept that it is not sufficient. The comprehension of how the 'pieces' of nature work was a necessary step towards the understanding of what they do once they are assembled. It is commonplace, at this stage, to remind that the whole is more than the sum of the parts. The challenge of the new century is to pass from analysis to synthesis, but this will require a widening of the scopes of the new approach, while treasuring the conquerors of the old ones. Some scientists used the term holism to label the alternative to reductionism. But holism looks for generalities by disregarding the details yielded by analytical approaches. Holism and reductionism are the two extremes of a continuum, and depend much on the adopted spatial and temporal scales. Macroecology, and also landscape ecology, try to apply a multiscalar approach that, however, is also to be scaled in time, falling within the domain of historical ecology (in the short term) and paleoecology (in the long term). The enterprise of putting all this into a single conceptual framework is almost hopeless, as remarked by Darwin. But our only hope for understanding passes through these views.

Natural history vs. environmental sciences: in search for a theory

Words convey concepts. Natural history is singular: there is just one history of nature. Environmental, or natural sciences are plural, and they split nature into many sciences. Of course, modern natural history cannot be like the old one. Old natural history was not blurred by all the details that we now know, all being extremely important to understand how nature works. Darwin's theory of natural selection is still the basis of our approach to nature. The theory has been remodelled many times, exceptions have been incorporated into it, and the present grand view of nature is rather baroque. Much more than at Darwin's times. Loreau [8] proposed a unifying ecological theory, aimed at joining community ecology and ecosystem ecology into an evolutionary perspective. The aim is surely the right one, and this is what is to be done. But how? Boero [9], commenting on Loreau's proposal [8], wrote:

> Ecology is divided into many branches, usually disconnected from each other. Community ecology and ecosystem ecology, for instance, are conceptually separated and their integration is much needed, to better respond to the current needs of nature conservation and management, based on proper understanding of the patterns and processes that characterize natural systems.

Loreau [8] argued that these branches of ecology must converge into an eco-evolutionary approach that is still lacking, proposing some theoretical ways to build models that might lead to this achievement, stemming from recent attempts to link biodiversity and ecosystem functioning. The analysis of how ecology was developed in the last century, and in the first decade of this one, leads to the idea of putting evolution into ecology (besides the much-considered individual level).

The lag between field, experimental, and theoretical ecology is evident and is exemplified by the size of biodiversity in Loreau's [8] fig. 2, where species numbers accounting for biodiversity are, respectively, 32 and 16. Any biodiversity survey, even in a desert, probably would yield a higher number of species (if performed by experienced taxonomists, a rather rare breed). Presuming that such low species numbers represent biodiversity is optimistic. Experimental manipulations, however, are limited by the number of tractable species, and this is possibly a limit also for theoretical ecology. It is still a mystery, then, what is the definition of biodiversity adopted in these exercises. Also ecosystem functioning is a rather elusive concept that, if defined in one of the current ways (see [8] for references), has little to do with biodiversity, being linked more to biogeochemistry than to anything else. The urge of merging these aspects, and to put them into an evolutionary (i.e. historical) framework is crucial. But it seems that we still have to agree upon the definition of the words and concepts we are using, since they are far from being univocal. In my experience, biodiversity is often made of hundreds of species, even at a small spot. So, when I use this word I have a different definition in mind than other types of ecologists (even if most of us use species numbers as a measure of biodiversity). Are experiments on a limited number of species of a single trophic level (e.g. prairie grasses) really handling biodiversity? What about pollinators, parasites and herbivores? Can a portion of biodiversity represent biodiversity? Or is it the opposite of biodiversity, not covering all the diversity of life? And hence, is primary production a reliable estimate of the functioning of that ecosystem? Loreau [8] rightly refers to Darwin [10] as the first to link species diversity (or the diversity of varieties in monocultures) to ecosystem functioning. In the Origin of Species, Darwin refers to two kinds of experiments about this link. In the first one, he surveys all the seedlings that originate on a plot of ground and sees that the greatest majority of them is grazed upon by both small and large herbivores. Also Darwin [10] takes plant production as a measure of ecosystem functioning, and considers it as being greatly dependent on grazing (i.e. predation). In another experiment, further in the book, Darwin finds, again with plot experiments, that many co-occurring plant species yield more dry herbage (a proxy of the functioning of the ecosystem) than a single species and that, if just one species is present, the co-occurrence of many varieties gives a higher production than when a single variety is present. Hence, biodiversity enhances production because species groups can take better advantage of the possibilities offered by the environment in terms of nutrients. Since, in agriculture, we use both insecticides and fertilizers to enhance production, it is obvious that both biotic and abiotic pressures act on the way species interact in making communities and ecosystems function. The issue of diversity-stability is also discussed by [8], reporting again on opposite views: one stating that diversity enhances stability, and another stating exactly the contrary, and that diversity leads to instability. Loreau [8] cited many papers, supporting either view, but maybe Connell's [11] proposal that instability (i.e. intermediate disturbance) leads to higher diversity than stability is worthwhile being mentioned as just another way of discussing this issue from a radically different standpoint. Ecological theory is still replenished with apparent contradictions. Many textbooks report about climax communities, where long-lived K species dominate a given portion of the environment. These species (e.g., the redwoods) are very stable, a feature usually considered as positive. From another point of view, however, they are biodiversity depressors that monopolize the environment, preventing the expression of higher diversity. A keystone predator (or an intermediate disturbance, but the two are synonyms) removing them might lead to greater biodiversity. If this happens with mussels in the intertidal it is considered as a good thing (as hinted in [12]), but if it happens with the redwoods it is seen as a catastrophe! Paraphrasing Hamlet: there are more things out there, than are dreamt in our ecology! The proposal of Loreau [8] to integrate all these facets of ecology and to put them into an evolutionary framework is probably the best way to evidence all the contradictions (is stability generated by diversity, or is it instability that generates diversity?) and convergences (e.g. intermediate disturbance and keystone predation). From an evolutionary point of view, for instance, the Red Queen Hypothesis postulates that diversity is enhanced by instability, and that every evolutionary novelty triggers further novelties. Also this concept, however, is present in the Origin of Species. Both ecology and evolution, furthermore, are historical disciplines, governed by both identifiable laws and unpredictable contingencies. The more they are intertwined, the more they become inherently unpredictable, just like history. Unless the 15 million species inhabiting the planet become just 32. But, then, are models and experiments really representing the real world? Probably they do, but covering just very small portions of it, both in space and time.

Where are we?

Jackson [13] showed that experimental marine biology and ecology considered mostly the intertidal, dealing with very different organisms from those living in the subtidal, and that ecological 'principles' identified there might reflect more the peculiarities of this environment than the generalities in the marine domain. This problem is common also to the model animals chosen for experimental biology. These are often exceptional for some features that are conducive to experimentation, being exceptions that become rules! The subtidal is much less 'experimented' than the intertidal, not to speak about experiments in the water column, and the ambiguity of experimental marine ecology persists. The shift from the field to mesocosms, as sites for experimentation, is also leading research towards simplifications that help hypothesis testing but that might oversimplify both the structure and the function of the investigated systems.

Biological oceanography, with the advent of satellite observation, is providing much information about some aspects, such as temperature and primary production at the surface of the sea. But this information is more and more scant under the surface. Such instruments provide information about the variables that the instruments can 'see', so that we find only what we expect to find, when we set up the instrumentation. Gelatinous plankton, for instance, is becoming more and more dominant at global scale [14] but this does not result from satellite observations and is often unreported by plankton samples with traditional plankton nets.

This lack of appreciation of important phenomena such as gelatinous plankton outbreaks (that can lead to completely different ways in which ecosystems function) is linked to the demise of observation of 'what is happening' in favour of 'problem solving' approaches, focused on specific topics. If a bloom of gelatinous plankton is not envisaged in a research project, there will be no work about it, even if it will be encountered during the investigation. This is also linked to the bureaucratization of scientific research [7]. If funding is received to carry out a project on a given topic, no other topic is to be treated, since it will not become the object of deliverables and will not lead to the fulfilment of milestones. Oceanographic long term series, such as the Continuous Plankton Recorder, are usually demised, and, paradoxically, long-term oceanographic measurements are taken in the vicinity of marine institutions, such as Naples, Helgoland, Plymouth, merging the philosophy of marine biology and ecology with that of biological oceanography. Furthermore, the compartmentalization of approaches is still too extreme, and there is a lack of integration among disciplines: the more research is focused on a topic, the less are the connections that link it to other topics.

Theoretical marine science

Theoretical physics uses mathematics as its standard language. Story telling (i.e. verbal models) is transformed into formalized equations that describe the world unambiguously, so that overwhelmingly important concepts can be conveyed in a very synthetic way (as exemplified by the famous and elegant $E=mc^2$). Do we have a similar equation for evolutionary theory? No, we don't. Can we hope to explain in mathematical terms the portion of nature including living beings? Chaos theory demonstrated that we cannot. Theoretical ecologists are becoming aware that important things like regime shifts can occur with no warning [15] and that, hence, cannot be predicted by a model. The foundations of modern theoretical ecology are in Darwin's Origin of Species [10], since natural selection deals more about ecology than about evolution. Darwin, in fact, was right in proposing the ultimate causes of evolution (investigated by ecology) but failed in proposing the proximate causes, since he did not use genetics to explain how organisms change. He understood the why (ecology), but failed about the how (genetics).

Instead of searching for laws, environmental disciplines (while considering physics and chemistry, of course) must investigate about principles of general occurrence, and are more inductive than deductive. Life is more a collection of many different ways of performing some fundamental processes than a single, grand and uniform concept. Biologists, for a while, thought that the decodification of genomes might have revealed all mysteries of life (hence the Human Genome Project, somehow speeded up by some mathematical simulation... physics strikes back every once in a while), to become aware that epigenetics is as important as genetics in determining the organization of living matter. And that the environment has a great bearing on organisms (Darwin discovered again, with great surprise, and in the company of Lamarck).

Theoretical ecology, thus, might be split in two parts. One is mathematized and it can develop freely so to achieve results that raise as much as possible the number of considered species (the current 32 are not so impressive, though) and their interactions with the physical and biological world. But another part should be dedicated again to the old subjects of natural history. How many species are actually out there? Strange enuough, we do not know. What are their roles? We do not know either, even for most of those we know the existence of. What are their life cycles? Again, we do not know, besides some noticeable exceptions. How can we base a theory on such a great number of assumptions about such a great number of things that we do not know yet? How comes that we became more interested into building up very complex formulas (full of constants that are arbitrary, by the way, or based on a very limited number of observations) than in actually becoming aware of the real variables that we should include in the models (i.e. species and interactions)?

What is the use of an abstract model if we do not have the concrete data that might allow for its testing? The usual answer is: the use is to gain more insight about the involved processes. Right. But maybe some more insight might be gained also by singling out the variables and finding out the relationships among them. Some more insight might be gained by taking record of what is happening (historical ecology, through long-term series) and then trying to find regularities, trends, and regime shifts into them, so to acquire wisdom from experience, maybe without the arrogance of being able to predict the future from the inspection of the past, but being nonetheless aware that the things happening today might be conducive to something else in the future, even if we cannot predict it with certainty.

Ecological modellers, however, became so bold to dare publishing sentences like this one: '*An ecosystem theory could also facilitate the use of ecology in environmental management, because it is possible with a good theory in hand to make statements and prognoses with none or very few observations*'. I prefer to let the author of this sentence unnamed because I respect his (or her) intelligence and I suppose that s/he wrote this sentence in a moment of bold optimism. Presuming not to need data and to rely on own theory only is the apex of self-referential statements.

Sometimes 'theories' are so detached from reality that their following can lead to disasters, as it happened with the economic 'laws' that were developed and enforced as if the laws of nature did not exist!

What economists want, what ecologists show

Economists want growth. Everything has to grow: production, consumption, gain, income. There is a little discrepancy between this grand plan and the real world. Our world is finite, and we cannot expect infinite growth in a finite system. If something grows (our economy) something else degrows (usually nature, or the well being of some humans at the benefit of others). Ecologists made a big mistake when they tried to explain this to non ecologists: they made predictions. In the Seventies, for instance, Captain Cousteau predicted that the Mediterranean would have died within 20 years. Captain Cousteau died in 1997 and the Mediterranean is still alive. Does this mean that Cousteau was wrong? Obviously he asked too much to his experience and feelings and he dared making precise predictions in a domain, history, were precise predictions are impossible. We can surely say that we cannot continue to exploit and impact on the Mediterranean Sea (and on every other sea or ocean) the way we do because, if we will persist in pursuing the crazy expectation of infinite growth, the things that are of interest for us will not persist anymore. This catastrophe will not affect the Mediterranean as a whole. We cannot destroy life, we are not so powerful. We cannot destroy rats and cockroaches, for instance. But we can easily destroy all whales. So we might end up with a Mediterranean with less and less fish and more and more jellyfish. Without having the power of destroying the jellyfish as efficiently as we did with the fish. This is happening right now, under our eyes. No mathematical model about fisheries led to predict that jellyfish would have taken the place of fish. Of course jellyfish scientists warned about this possibility long time ago (e.g., [16]) and they continue to do it [14], but their claims did not look very scientific. They reported about what they were and are actually seeing, but if this cannot be expressed with a cumbersome formula, the claims are not scientific enough, and will not end up in highly impacting journals. The failure of fisheries models in predicting a shift from a fish to a jellyfish ocean stems from a wrong philosophy. The same that led our economy to the recent collapses. But now fisheries scientists have become aware of jellyfish [17], and are predicting that they will become very abundant in the future! It is interesting to notice that this prediction is not performed by a formula but, instead, by a drawing, with an arrow that goes from a situation with lots of fish to a situation with lots of jellyfish, passing through periods of decreasing fish populations. This is the most accurate description of the trend that we can produce. And everybody can understand the message in that drawing, being more informative (and precise) than any formula produced so far to describe these phenomena. Now we can go at sea and try to find out what species of jellyfish are out there, and how many they are, maybe to discover that the future depicted by fisheries scientists is already with us [18].

Ecology shows that the expectations of economy are ill-based, and that the economists' childish picture of the world is wrong. The so-called laws of economy cannot be enforced if they are in conflict with the laws of nature (starting from the precise ones of classical physics and ending with the loose and probabilistic ones of ecology and evolution). Unfortunately, however, there are ecologists that behave like economists, daring to perform precise predictions about the fate of historical systems!

Too many breeds of ecologists

When ecology started to become mathematized, losing perception of its historical nature, a group of ecologists started to depict the environment in very abstract terms, refraining from looking at it, and having more faith in the insight of their models than in the real world. These scientists dared performing predictions about the future, often without really looking at the systems they were modelling. Another group of ecologists persisted in studying the real world, by looking at it and by making experiments on it. They did not dare performing precise predictions, even though they tested hypotheses in very rigorous terms (sometimes).

These two breeds of ecologists did not speak much to each other, and they even split in further branches, like ecosystem ecologists, community ecologists, evolutionary ecologists, molecular ecologists and, in the sea, like plankton ecologists, benthic ecologists, fish ecologists, fisheries ecologists, biogeochemistry ecologists, microbial ecologists, larval ecologists, parasite ecologists, and so on. In this was, ecology split into a host of branches, losing its foundational feature: the appreciation of interactions.

Vellend [19] related the many patterns that community ecologists did describe so far with the four processes determining them: selection, speciation, drift, and dispersal. Acting together or in various combinations at a time, they explain the observed patterns, providing a conceptual toolkit to understand the intricacies of community ecology, one of the most comprehensive biological disciplines, second only to ecosystem ecology [8].

Vellend [19] showed that some researchers considered some of these processes as being important, but that none considered them all together. Maybe Charles Darwin did: his masterpiece [10] is about speciation (the origin of species) and selection (natural selection), and many arguments about the two processes are based on dispersal (see the paragraphs on the coupling of seed survival in marine water and their dispersal to distant islands by the currents, to explain the patterns of island communities). Of course, speciation, selection, or dispersal do not always occur with the same ‘strength’, and when their bearing on species assemblages (many experimental ecologists do not use the term ‘community’ and prefer ‘species assemblage’, by the way) is loose we can have drift, which is a process occurring when the other three are not working much. The toolbox proposed by [19] covers all the possibile processes that might lead to the observed patterns and, thus, cannot be falsified, because there will always be at least one of the invoked processes to explain the observed patterns. Seen from the already discussed Popperian viewpoint, this ‘theory’ explains everything and, thus, explains nothing, because it can never be falsified. In a way, Vellend [19] invoked multiple causality and listed all the possible processes that lead to the observed patterns, without saying that one is better than the others, just as Connell and Slatyer [6] did with community development. Vellend [19] finds examples for each process (with some difficulties for drift) and the examples ‘demonstrate’ the existence of the process, without claiming that the process is always acting. This theory postulates the existence of processes leading to patterns, but it does not consider any of them as universal (i.e. always explaining the observed patterns). Popperian science asks for universal statements (*all crows are black* is Popper’s favorite universal statement), rejecting them if they are falsified even once (*the white crow*). The explanation of the existence of the pattern *white crow* by the process of *albinism* is considered an *ad hoc* explanation that should not prevent the scientist from rejecting the universal statement about the color of crows. Of course, since albino crows exist, there is no universal statement about the color of crows, and science shifts from universal statements (applicable to simple phenomena) to existential ones (dominating the disciplines of complexity). This view is applicable to community ecology, evolution, and to any other complex discipline: that’s why there are no laws, in a Popperian sense, in ecology, but this does not make it a mess, it just accounts for its complexity. This anti-Popperian way of making science is timely. Community ecology is a historical discipline, just like evolution, and historical patterns are determined by many processes, some are governed by constraints (the attractors of chaos theory) and some are governed by contingencies (the inherent unpredictability of complex systems of chaos theory).

After a very long tour in Popper's land, here we are with Darwininan natural history again!

Time for a synthesis: back to natural history

Maybe it was right to split ecology into many branches. And the reductionistic approach will surely continue to yield beautiful results, as it did so far. But, maybe, we should also start to think in a more synthetic way. As Loreau [8] advised, we must join ecology and evolution, or, as suggested by McGill et al. [20], we should return to the appreciation of species' functional traits versus abiotic environmental traits, plus the consideration of the bearing of biotic interactions in determining the realization of niches. The appreciation of the role of life cycles in ecology has been invoked by various authors [14,21–23], since compartments such as plankton and benthos are often posing barriers that are non existent when species are not considered as just adults but as life cycles. The same is valid for nekton, since many fish are planktonic in their early life. The paths of life cycles determine much more crossroads among species than those determined when considering adult paths only. To accomplish the much desired synthesis, thus, we must join plankton and nekton to benthos, onshore environments with offshore ones, the water column and the sea bottom, plus the coast (and the rivers), connecting biogeochemical cycles, with life cycles, with trophic networks (as proposed in [23]). This synthetic view of the ocean, of course, is not the simple sum of the results of all the approaches to single aspects of a highly interconnected system. The pieces of the puzzle are starting to fit together, and a grand picture of patterns and processes is taking shape. With the timely recognition that there are no shortcuts to the understanding of complexity and that all processes, even apparently negligible ones, might be conducive to important patterns, sometimes, accounting for the non-linearity of biological disciplines.

This grand picture is the future challenge of marine sciences, and of environmental sciences in general, linking all facts of life from molecules to ecosystems and landscapes. The approach is that of Darwin, who kept ecology and evolution together, who made observations, formulated hypotheses stemming from them and tested them with experiments, practising both ecology and evolutionary biology, and many other disciplines that he reconciled under a single umbrella: natural history. This vision is more modern than ever, to give a meaning to the many novelties still enriching a conceptual landscape that requires continuous adjustment, sometimes with the rejection of general rules such as the impossibility for any metazoan to lead its whole life in anoxic environments. We just learnt that some can [24].

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