Vol. 15: 63–69, 2015
doi: 10.3354/esep00164

ETHICS IN SCIENCE AND ENVIRONMENTAL POLITICS
Ethics Sci Environ Polit

Published online December 17

Contribution to the Theme Section 'Academic freedom and tenure'

Scientists can be free, but only once they are tenured

Ferdinando Boero[1,2]

[1]University of Salento, DiSTeBA, Via P. le Lecce-Monteroni, 73100 Lecce, Italy
[2]CNR-ISMAR, Via De Marini 6, 16149 Genoa, Italy

ABSTRACT: The frenzy for evaluation metrics recognizes the value of research based on the impact factor of the journals that publish the results. Only some areas of science are conducive to publication in the best tribunes. In biology, for instance, organismal biology, or taxonomy, are not very fashionable, whereas molecular or global approaches are trendy. The citation system to measure the quality of a scientist's work is based on the acceptance of what is published: the more the rest of the scientific community likes it, the higher the value. In this framework there is little space for deviation from norms. Some examples are given here, within the fields of ecology and evolutionary biology, that show how difficult it is to enforce ideas, either new or old, that do not follow mainstream thought. In order to obtain tenure it is advisable to conform to mainstream, and publish your results in journals with high impact factors. New things can be attempted 'after' tenure has been granted; but 'after' is very often 'too late'.

KEY WORDS: Scientific creativity · Metrics · Ecology · Evolution · Biodiversity and ecosystem functioning · Plankton · Benthos · Fisheries biology

INTRODUCTION

For a scientist, freedom means to go wherever his/her own curiosity dictates. If that curiosity has some immediate application, it is a bonus; if it does not, the pleasure is often even greater. Many scientists admit that they work because they are having fun. There is a problem, though. You can dream of earning your living studying tiny jellyfish, if you like that topic, but then you have to find a way to get paid, and if you want to be sure that you will be able to continue to satisfy your curiosity, then you must obtain a steady job which, in many countries, means receiving tenure. In the last few decades the number of people in academia has increased considerably, and the carrying capacity has almost been reached. Selection is strong, and it is not as easy to get tenure today as it once was. Freedom is being channeled into certain directions, whereas other directions are 'unsafe'. Hence, freedom is being restricted. Well, freedom is not as simple as doing what you like, if you cannot afford to do it with your own resources. If you want the support of a research institute, to provide you with a job and laboratory space, and of funding agencies, to supply you with cash, you must give 'them' what they expect. These expectations, however, might lead in the wrong direction or, at least, might prevent exploration in a certain direction that is not within the array of 'allowed' topics, but that might be very fruitful.

CURIOSITY KILLED THE SCIENTIST

Curiosity-driven research is the mother of innovation. By definition, novelties cannot be programmed, and producing them requires inroads into unexplored territory: results are not guaranteed and the risks of failure are great. One might say that only once you are safe can you afford to take the risks that might im-

*Corresponding author: boero@unisalento.it

Publisher: Inter-Research · www.int-res.com

pair your tenure track should the expected results not be obtained. The secret is to follow a line that will guarantee safe results and that, with some luck, might even lead to some breakthrough. If the breakthroughs are not achieved, the work will be valuable anyway. If you dare too much and have the guts to attempt something very risky, but do not reach the expected goal, you might be out of the game. However, there are always exceptions: scientists have been searching for extra-terrestrial life for decades, though they have yet to find such, but they still continue to receive considerable amounts of money for research into this area of study (Mayr 1988).

HOW MUCH FREEDOM DO WE NEED?

Progress in human cultivation could be paralleled to biological evolution. Both can be either gradual (with a slow and somewhat steady rate of change) or saltational (with sharp changes occurring in a restricted time period). The 2 processes often coexist, showing that there is no single universal way to make things happen. Especially the saltational progress requires breaking the rules and the deviation from norms; this can happen only if freedom exists. Rules, however, must be known before being broken and this requires time, especially in biology. In mathematics and physics young researchers, or even children, can have the freshness of mind to solve complex problems through intuition and imagination, whereas old mathematicians may no longer be as creative (of course there are exceptions) (Aiken 1973). In other disciplines, such as biology and ecology, the situation is very different (Fawcett & Higginson 2012, Wilson & Frenkel 2013). In these fields a tremendous body of knowledge has accumulated and the many interconnected facets create a very complex conceptual space that can only be explored based on sufficient knowledge and an understanding of the patterns and processes that underlie biological phenomena.

STORY TELLING VERSUS NUMBER CRUNCHING

It is commonplace that 'serious' science is highly mathematized and that story-telling science is considered to be of lower quality, as demonstrated by the so-called 'physics envy' in biology (Egler 1986) (see Gardner et al. 2007 on story telling in science). Now I have a challenge for those who are confident of this apparent truism: try to translate Darwin's 'On the Origin of Species' into numbers and to be as convincing as he was. No one in bio-ecology has ever had a greater influence than Darwin, but, nowadays, someone who works and writes like he did would have no future in bio-ecology. I am not talking about the prose; I am talking about the approach. Darwin based his work on natural history, but this discipline is out of fashion nowadays, and the chances are good that modern naturalists will not have many opportunities of reaching high positions in academia (with a few due exceptions) (Ricklefs 2012). A scientist who is able to produce mathematical models of pieces of reality that he or she wishes to understand better (for instance the functioning of ecosystems) has a much greater chance of having their work published in high-ranking journals than scientists who try to explore the intimate mechanisms that underlie the relationships between biodiversity and ecosystem functioning (BEF). The same is true for those who recognize species at the press of a PCR button, discovering many new species from their DNA, but without knowing what they look like or what they do (Boero & Bernardi 2014). Traditional taxonomy is a solid warranty of academic failure, with some chances in museum careers. Having been kicked out of the capacity building system (i.e. universities), at least in the so-called 'first world' (i.e. Europe and North America), taxonomy is destined to become an extinct field of research in these countries, since new scientists are trained in universities and not in museums (Boero 2010a).

The link between biodiversity and ecosystem functioning is the most important problem in defining the quality of the environment, as prescribed by the European Union in the Marine Directive Framework Strategy that defines Good Environmental Status. In spite of the stated importance of biodiversity, however, the basic sciences that study BEF (i. e. taxonomy and autoecology) are not conducive to successful careers (Boero 2010a). There is no place for such expertise in academia, and the products of this kind of research find no space in high-ranking journals, those that ensure a successful tenure track at a good university.

Those who might know what they are talking about cannot speak, whereas those who speak often do not know what they are talking about. It is evocative that this very strong statement is the distillate of a presidential address to the American Society of Naturalists, given by an eminent ecologist (Ricklefs 2012). A mathematician (E. Frenkel) attacked in a rather unkind fashion an eminent naturalist (E. O. Wilson) because he dared to say that number crunching is over-valued in the natural sciences, whereas natural history is not valued enough.

THE DOMAINS OF SUCCESS: 3 AREAS THAT WILL LEAD YOU TO TENURE

Fashions are important. Nowadays, in biology, there are several routes that are very promising and others that are more-or-less suicidal (Boero 2015a). The editor-in-chief of Nature, upon his retirement, provided a surprising definition of life in his last editorial: life is chemistry (Maddox 1995). This explains very well the success of molecular approaches to biology, with the dismissal of non-chemical approaches, such as the study of phenotypes, something that can be studied by simple observation. The availability of formidable tools is leading the scientific community to make use of them exclusively and to discard topics that cannot be tackled by the use of machines. This pushed Dyson (2012) to consider whether science is driven by tools or by ideas. Satellites are nice tools, and the whole Earth is the opposite extreme of the size spectrum that starts with chemistry, with the construction of beautiful and multi-colored maps showing the distribution of the feature of your choice. The secret is: either go global or molecular, but stay away from the middle of the spectrum. High-ranking journals (those where one should publish to receive tenure) have a special liking for general things, and there is nothing more general than a map of the world, or the finding of a sequence that explains why something is the way it is (the gene of longevity, or of cancer, you name it). So, if you can produce a map of the world, showing something global, you are a good scientist. If you find a sequence that codes for something, you are good too. What resides in the size spectrum between molecules and the whole planet is not as interesting, even if this is the scale of normal life.

The other secret is to build models that will predict the behavior of some system. We know very well that this is not possible, as the chaos theory has demonstrated (Boero et al. 2004), but scientists continue to produce 'predictive' models of complex systems and, surprisingly, they continue to be successful in publishing them.

THE IMPACT FACTOR AND THE CITED HALF LIFE

The Institute for Scientific Information (ISI) ranks scientific journals according to a set of metrics. The Impact Factor (IF) is the only metric that receives attention in academia. If your IF is high (as inferred from the IF of the tribunes of your choice) then you are a good scientist. If your IF is tiny, then you are worthless. The IF is measured by the citations that articles published in the ranked journal receive 3 to 5 yr after publication. Usually, the 3 yr IF is considered. The Cited Half Life (CHL) is another metric: it measures how long the articles published in a given journal continue to be cited. The highest CHL is >10, equalling infinity. It is very seldom the case that journals with a high IF also have a high CHL. The results of the so-called rapidly evolving disciplines are published in journals with high IFs, and no one cares whether the CHL of these journals is tiny. One might label these disciplines as 'rapidly decaying', since the papers they publish are rapidly forgotten. The obsession concerning these metrics is so great (Fischer et al. 2012) that there is some concern that some journals inflate their IF in order to increase their prestige (Rossner et al. 2007). Well-established disciplines (such as taxonomy) have much lower IFs, but their CHL values are usually >10; they keep being cited forever. The demise of taxonomy is mostly due to the low competitiveness of taxonomists in terms of publication scores, leading to fewer opportunities of ranking high in academia (Boero 2010a).

Biodiversity exploration and assessment cannot be achieved without taxonomy, and biodiversity is universally considered crucial for our well being (see, for instance, the famous Rio Convention on Biological Diversity), but the science of naming species (the core of biodiversity) is in distress. The scientific community working on non-rapidly evolving (and decaying) disciplines should praise CHLs as much as IFs are praised by their counterparts in fashionable disciplines, but unfortunately this is not happening (Boero 2010a). This is strongly affecting the type of topics that are conducive to a successful career in academia.

DEVIATION FROM THE NORM

Without deviation from the norm, progress is not possible. Hence, progress is based on scientific revolutions, stemming from the falsification of previous beliefs. It is not easy to obtain norm-breaking results while also being right. The establishment stubbornly defends its own beliefs, while asking scientists to disprove them, so as to follow Popperian logic. Innovation, in this framework, might be something that proves a truism right, in a way that was never attempted before. Proving what the establishment believes, but in a novel way, is a very safe way of obtaining status in the scientific community. The acceptance of something that disproves previous beliefs and obliges the scientific community to work in a different way, however, is much more difficult.

Not all deviations from norms guarantee progress, that is for sure, but deviations should be encouraged. Every tenure track academician should try to break some rules and propose new ones. The example of Lindeman and his post doc supervision is almost paradigmatic (Sobczak 2005).

BREAKING THE RULES IN BIOLOGY

Rules, in hard science, should be laws. In physics they are, but not in biology. In biology multiple causality exists, and things can happen for many reasons (Mayr 1988). A rule, for instance, is that the structure and function of living matter is based on a DNA–RNA code. But RNA viruses break it. A rule is that living matter evolves and never remains the same, but this evolution can happen in different ways. Sometimes evolution is gradual, sometimes it is saltational. When saltational evolution was proposed, the first impulse of its discoverers was to reject gradual evolution. Strange enough, this revolution was accepted with no problem, but then it became apparent that evolution can be both gradual and saltational. There is not one rule, i.e. a law that cannot be broken. Biological laws, if we really need laws, pertain to the existence of something but not its universality. Saltational evolution rejected the universality of gradual evolution, but this does not mean that gradual evolution does not exist. It does, but saltational evolution exists too. Both exist, and neither is universal.

Biology looks desperately for universal rules, something that could be called a dogma. This stimulated Mayr (1961, 1988) to declare that biology cannot produce definite laws, due to its historical nature. The central dogma of biology was: information flows from genotypes to phenotypes and not vice versa. Epigenetics broke this rule. Information can also flow in the other direction: phenotypes can influence genotypes, activating specific genetic patterns. The dogma has been proven false, as such, it is not a dogma, but this does not mean that information does not flow from genotypes to phenotypes: it does. It can just flow the other way as well.

When the molecular clock was proposed, scientists started to measure the distance in time that separates species from each other. The divergence in millions of years became a widespread feature to be measured. Until the same person who proposed the molecular clock admitted that his 'clock' could tick at different paces (Zuckerkandl 1987). If a young scientist had said the same, he or she probably would have found little approval (and not received tenure).

DISCOVERING HOT WATER

The re-invention of the wheel, or the discovery of hot water, is a widespread sport in bio-ecology. What is the difference between the intermediate disturbance hypothesis and the keystone predator concept, for instance? Many of the concepts that make up the basic notions of ecology are present in Darwin's 'On the Origin of Species', but they have been re-discovered by modern scientists who usually get the credit for having proposed them (Boero 2015b). Famous ecological concepts such as the Red Queen Hypothesis, trophic cascades, the ecological niche, and many others are found in Darwin's work but are usually credited to other scientists who simply re-expressed them in more modern terms. Sometimes, in fact, the re-proposal of something that has been proposed already might be more convincing than the original formulation. What to do then? Admit that the idea is not completely novel and claim anyway that one's own proposal is formally more robust, or propose that one's own idea is original and novel? From my experience, journals do not like the re-formulation of notions that have previously been proposed and that have been forgotten or that have not been given the importance they deserve. Prestigious journals want NEW stuff. So, the best thing to do is to claim that your own results are new and assume that reviewers will not discover the trick. I fear that many authors play this game, which I obviously do not advise.

THE FREEDOM OF REVIEWERS

I served on the editorial board of a very prestigious ecological journal, and, for some time, I was the only editor specialized in marine ecology. All marine papers went through my initial evaluation and, if they passed my scrutiny, I then chose reviewers. I received many papers on BEF, based on very nice mathematical treatments of data (as far as I could understand). Being ignorant of high-level mathematics, I concentrated on the measures of BEF. In many cases (I am tempted to say in most cases) the datasets were not shown. So I asked for them, to discover that the species lists (that represented biodiversity) were strongly biased by the available taxonomic expertise.

In similar systems, biodiversity was either amphipods, or polychaetes, or the meiofauna, and a part of the biodiversity was proposed to represent the whole, with no caveat of the shortcomings of this approach. Ecosystem functioning was even worse, since it was based on the measure of important processes (e.g. decomposition or primary production) that do not account, however, for the real functioning of ecosystems. Measuring them is just like measuring the temperature of a body and then pretending to perform high-level medicine. I rejected these papers without sending them to reviewers, due to insufficient datasets to support the conclusions of the papers (which were invariably rather triumphant, praising their own achievements). After a while, I came to realize that I was cancelling all approaches to BEF in marine systems, whereas the very same approach that I censored in the seas was very successful on land! Just as knowing old literature (even if it is Darwin's 'On the Origin of Species') is a redundant requirement to young ecologists, so is the knowledge of species. Both the study of literature (all the literature) and of species requires a long time, and people are pressed to publish in journals with a good reputation (i.e. those that have a high IF and a low CHL) so, being cynical, I would advise young researchers to stick to literature that you can easily find by pressing the button of a search engine and to pretend to study BEF without knowing species and while having only a vague idea of the functioning of an ecosystem. What is really important is to give a solid mathematical make up to your 'data' so that your papers will be accepted. I have strongly criticized this manner of studying BEF, advising the use of different approaches (Boero 2003, 2010b, 2015b, Boero et al. 2004, Boero & Bonsdorff 2007), but, of course, my advice is being happily ignored, with some exceptions (Guidetti et al. 2014).

THE EQUATIONS EXIST, BUT THEY ARE UNSTABLE…

The quest to translate observations and stories into serious-looking formulas has pushed bio-ecologists into using mathematics to express what they have found. Having an equation that describes the behavior of something in time allows us to measure the considered variables at Time 0, run the equations, and arrive at the variables at Time 1: the future! Thus, descriptive science (usually labelled as soft) can become predictive (i.e. hard). Everything is fine if there are only 2 variables, but when their number increases, the equations become unstable. This means that their results can change, and we cannot be so sure about the value of the variables at Time 1. There may be more values, and not just one. This softens the presumed hard science quite a lot. The so-called hard scientists who play this game do not resign though. They say that their attempts lead to a better understanding of the system and that new insights can be gained just from their efforts. I do not think that this statement has a universal value.

CONSTANTS THAT ARE NOT CONSTANT

It is often the case that poorly known variables are simply ignored by these hard scientists. If somebody insists and tells them that the variables cannot be ignored, then the 'nasty' variables are transformed into re-assuring constants. Take fisheries models, for instance. Fish larval mortality is a very important variable, since the size of what we can catch is the result of successful recruitment. If lots of larvae reach adulthood, then fisheries have large yields, otherwise the yields are lower. Larval mortality is almost ignored in fisheries science, with the presumption that recruitment can account for it. Knowing the rate at which larvae die, and why, provides much insight into the processes that should be managed. Doing so, however, requires a completely different approach from those currently practiced in fisheries science, and so larval mortality is coped with by introducing a constant in the equations. It is presumed, then, that a given quantity of larvae will not reach adulthood, and that quantity is always the same (constant).

Now, take a fisheries biologist who says: hey, but that assumption is not true; there are many different causes of larval mortality, and they are not constant. Larvae can die of starvation due to lack of prey, which might be due to the abundance of competitors. These might be the larvae of other fish that hatched a little earlier and took advantage of an abundant food supply, which they then depleted. But the competitors might be jellyfish, instead, since jellyfish are both competitors and predators of fish: they feed on the food of fish larvae and juveniles, and they feed also on fish eggs and larvae. The link between gelatinous plankton and fish is very important, but for fisheries science jellyfish do not exist (see Boero 2013). The assessment of larval mortality requires placing fish in the ecosystems they live in and connecting them to the rest of the components in that ecosystem, throughout their life cycles. Pursuing the ecosystem approach requires much more than going on board a vessel and measuring the fish that are caught.

THE CLOTHES OF THE EMPEROR

These shortcomings are not only affecting fisheries science. Plankton production in coastal waters is one of the most important ecological phenomena of the whole biosphere. Phytoplankton blooms that take place at a certain time of the year (e.g. spring or the rainy season) are the triggers of other processes throughout food webs, with a flux of energy that passes through herbivorous and then carnivorous zooplankton, to end up in an intricate web of relationships that link many fish, bird and mammal species, including humans. Textbook knowledge teaches that phytoplankton blooms, the basis of ecosystem functioning, depend on nutrient availability, as if biogeochemistry were sufficient to justify biological processes. Phytoplankton do not originate from nutrients: nutrients are necessary, but not sufficient. In coastal systems, most plankters have resting stages that remain in the sediments, sometimes even for centuries, and that form a biodiversity reservoir that accounts for future blooms. Blooms come from the hatching of these resting stages that, then, in their active form, take advantage of nutrient availability. Explaining plankton pulses through nutrient availability is not sufficient, and, to understand the intimate mechanisms of plankton dynamics, benthic systems must also be linked to planktonic systems (Boero et al. 1996, Marcus & Boero 1998). Benthic predation of resting stage reservoirs might even influence the future composition of plankton communities (Pati et al. 1999). Needless to say, the models function perfectly by linking plankton production with nutrient availability, but do not help at prodding scientists to dig into natural history to find resting stages. These models work on correlations, and not on causations.

A plankton ecologist who preached such approaches would have difficulties in finding funds for his/her own research and would find himself/herself obliged to participate in cruises that consider plankton and benthos as separate entities, being an alien in both communities.

TEAM WORK

In the past, most scientists worked in splendid isolation and produced papers that mostly had a single signature, sometimes 2 or 3, not more. Then the scientist thanked some colleagues and technicians who helped him/her to collect the samples, to analyze them, or to discuss the results. Those times are over. Papers are signed by dozens of scientists, and, if huge instruments are needed, operational prices are so high that scientists need to pool resources and package complex projects that, sometimes, do not allow for divergent views. The whole team must conform to a common view. The apex of this tendency is in the field of particle physics, the articles of which are sometimes shorter than the list of the authors (that are in the hundreds and publish hundreds of papers per year, each). Theoretical physics, however, can still allow for academic freedom, but experimental research does not. And this is especially true in bio-ecology, where theoretical work is a pale replica of theoretical physics, with wide use of formulas that, as we have seen, are highly unstable and contain constants that are very variable (so being oxymorons). A person that says so is spoiling the party, and his/her colleagues do not like him/her much, also because he/she cannot be proven wrong!

THE STYLE OF YOUR WRITING

This paper, as many others of mine, experienced the hostility of reviewers for non-conventional writing: a scientific article must be very boring to read. In order to obtain decent results in boring writing, Sand-Jensen (2007) provides a series of precious pieces of advice on how to make your paper as boring as possible, so as to be accepted by the scientific community. In this way, your papers will be considered very serious. The more mathematics you introduce into your paper, the better it will be. Many people will not understand it (Fawcett & Higginson 2012), but they will not dare admit that. Moreover, the mathematics will provide a very solid cover up for your lousy data (Boero 2003), and your papers will go through the revision process very easily. This has a bitter taste, I know, and I do not approve of this trend at all. But this is the way the system is right now. If you want to fight it before you have tenure... good luck.

CONCLUSION

Academic freedom is based on obtained results, and the value of the results is measured by the value of the tribunes they are published in: the higher the

IF, the higher the value of the tribune and, hence, of your papers. The IF is based on citations, and so on the approval of the scientific community (Fischer et al. 2012): scientists are free to write whatever they want, but what they write has to be accepted by the rest of the community, otherwise they will not find a decent journal to consider their work (most 'diverging' work is sacked by the editors, and does not even reach reviewers) and, not having published in important journals, they will not find a place to work or be granted tenure. If they succeed in publishing 'divergent' work, it will not be cited by a multitude of authors, and the impact will be low (in terms, for instance, of the H-index). This means that if you decide to work on a non-trendy topic (for instance the exploration of biodiversity by studying species) your chances of success are low. So, even if this goes against my convictions (and my personal history), I am very reluctant to advise people to attempt a career while tackling problems that are not fashionable. The secret, then, is to play the role of the docile researcher and to only publish novel results that remain within the accepted domain. Keep all of your weird ideas in a drawer and do not take risks. Then, once you are tenured, you can begin to say what you think: now you are free. But maybe it will be too late.

Acknowledgements. Support was received from the European Union (the Network of Excellence on Marine Biodiversity and Ecosystem Functioning, the Projects Jason, SESAME, Vectors of Change, CoCoNet, Perseus, and MedJellyrisk), and the Italian Ministry of Public Education, University and Research (MIUR) (several PRIN projects and the flagship project Ritmare). I thank Kostas Stergiou for having invited me to write this paper and 2 reviewers who forced me to be more 'rigorous', obliging me to cite lots of my papers and, thus, artificially inflating my citation score.

LITERATURE CITED

Aiken LR Jr (1973) Ability and creativity in mathematics. Rev Educ Res 43:405–432

Boero F (2003) Selling beautifully packaged garbage. Mar Pollut Bull 46:921

Boero F (2010a) The study of species in the era of biodiversity: a tale of stupidity. Diversity 2:115–126

Boero F (2010b) Marine sciences: from natural history to ecology and back, on Darwin's shoulders. Adv Oceanogr Limnol 1:219–233

Boero F (2013) Review of jellyfish blooms in the Mediterranean and Black Sea. GFCM Studies and Reviews 92, Food and Agriculture Organization of the United Nations, Rome

Boero F (2015a) F1000Prime recommendation of [Roberts RJ, PLoS Comput Biol 2015, 11(4): e1004084]. In: F1000 Prime, doi:10.3410/f.725533351.793507819

Boero F (2015b) From Darwin's Origin of Species toward a theory of natural history. F1000Prime Rep 7:49, doi: 10.12703/P7-49

Boero F, Bernardi G (2014) Phenotypic vs genotypic approaches to biodiversity, from conflict to alliance. Mar Genomics 17:63–64

Boero F, Bonsdorff E (2007) A conceptual framework for marine biodiversity and ecosystem functioning. Mar Ecol (Berl) 28(Suppl 1):134–145

Boero F, Belmonte G, Fanelli G, Piraino S, Rubino F (1996) The continuity of living matter and the discontinuities of its constituents: Do plankton and benthos really exist? Trends Ecol Evol 11:177–180

Boero F, Belmonte G, Bussotti S, Fanelli G and others (2004) From biodiversity and ecosystem functioning to the roots of ecological complexity. Ecol Complex 2:101–109

Dyson FJ (2012) History of science. Is science mostly driven by ideas or by tools? Science 338:1426–1427

Egler FE (1986) 'Physics envy' in ecology. Bull Ecol Soc Am 67:233–235

Fawcett TW, Higginson AD (2012) Heavy use of equations impedes communication among biologists. Proc Natl Acad Sci USA 109:11735–11739

Fischer J, Ritchie EG, Hanspach J (2012) Academia's obsession with quantity. Trends Ecol Evol 27:473–474

Gardner J, Marsack P, Trueman J, Calcott B, Heinsohn R (2007) Story-telling: an essential part of science. Trends Ecol Evol 22:510

Guidetti P, Parravicini V, Morri C, Bianchi CN (2014) Against nature? Why ecologists should not diverge from natural history. Vie Milieu 64:1–8

Maddox J (1995) On the widespread distrust for science. Nature 378:435–437

Marcus N, Boero F (1998) Production and plankton community dynamics in coastal aquatic systems: the importance of benthic–pelagic coupling and the forgotten role of life cycles. Limnol Oceanogr 43:763–768

Mayr E (1961) Cause and effect in biology. Science 134: 1501–1506

Mayr E (1988) Towards a new philosophy of biology. The Belknap Press of Harvard University Press, Cambridge, MA

Pati AC, Belmonte G, Ceccherelli VU, Boero F (1999) The inactive temporary component: an unexplored fraction of meiobenthos. Mar Biol 134:419–427

Ricklefs RE (2012) Natural history, and the nature of biological diversity. Am Nat 179:423–435

Rossner M, Van Epps H, Hill EJ (2007) Show me the data. J Exp Med 204:3052–3053

Sand-Jensen K (2007) How to write consistently boring scientific literature. Oikos 116:723–727

Sobczak WV (2005) Lindeman's trophic-dynamic aspect of ecology: 'Will you still need me when I'm 64? Limnol Oceanogr Bull 14:53–57

Wilson EO, Frenkel E (2013) Two views: How much math do scientists need? Not Am Math Soc 60:837–838

Zuckerkandl E (1987) On the molecular evolutionary clock. J Mol Evol 26:34–46

Editorial responsibility: Konstantinos Stergiou, Thessaloniki, Greece

Submitted: November 11, 2014; Accepted: July 29, 2015
Proofs received from author(s): December 8, 2015