Studies in History and Philosophy of Biological and Biomedical Sciences 46 (2014) 44–54

Contents lists available at ScienceDirect

Studies in History and Philosophy of Biological and Biomedical Sciences

journal homepage: www.elsevier.com/locate/shpsc

The normative structure of mathematization in systematic biology

Beckett Sterner, Scott Lidgard

Field Museum of Natural History, Chicago, IL, United States

ARTICLE INFO

Article history:
Received 25 September 2013
Received in revised form 24 February 2014
Available online 6 April 2014

Keywords:
Numerical taxonomy
Classification
Species
New Systematics
Methodology
Logical positivism

ABSTRACT

We argue that the mathematization of science should be understood as a normative activity of advocating for a particular methodology with its own criteria for evaluating good research. As a case study, we examine the mathematization of taxonomic classification in systematic biology. We show how mathematization is a normative activity by contrasting its distinctive features in numerical taxonomy in the 1960s with an earlier reform advocated by Ernst Mayr starting in the 1940s. Both Mayr and the numerical taxonomists sought to formalize the work of classification, but Mayr introduced a qualitative formalism based on human judgment for determining the taxonomic rank of populations, while the numerical taxonomists introduced a quantitative formalism based on automated procedures for computing classifications. The key contrast between Mayr and the numerical taxonomists is how they conceptualized the temporal structure of the workflow of classification, specifically where they allowed meta-level discourse about difficulties in producing the classification.

1. Introduction

Is mathematizing practice the best way to achieve the aims of science? Answering this is crucial to evaluating how computer technology is changing science. More frequently, though, philosophers and scientists have sought to answer the question, "Why *is* mathematics so useful for science?" The physicist Eugene Wigner famously attempted to account for the "unreasonable effectiveness of mathematics" in terms of metaphysical correspondences between nature and reason (Wigner, 1960). Philosophers of science have also examined the "indispensability" of mathematics for science and the implications this may have for the existence of mathematical objects (Colyvan, 2014).

Although superficially similar, the two questions we posed differ profoundly in the assumptions they bring to understanding the place of mathematics in science. The second question views mathematics as a body of knowledge and practice more or less autonomous from science. Penelope Maddy, for example, has argued that we should treat the standards for research in mathematics as distinct from science (Maddy, 1997). Applying math to science then typically depends on mapping an abstract mathematical structure onto a concrete empirical scenario. Baker (2012), for instance, presupposes this sort of mapping relationship in evaluating what it means for mathematics to be indispensable for a scientific explanation. Given this starting point, explaining the usefulness of mathematics becomes a problem of explaining why and how this mapping holds between pre-existing mathematical and scientific objects.

Yet this view of math as autonomous from science is in fact a fairly recent historical development, and represents only a partial account of the overall relationship between math and science. Our present image of mathematics as a pure, abstract, and autonomous activity originated out of particular epistemic problems facing mathematicians a hundred years ago, such as confusions over the nature of physical space in conjunction with geometric reasoning (Corry, 2006; also see Wilson, 2006). Similarly, historian Jeremy Gray has argued that math underwent a modernist transformation in the early twentieth century analogous to modern art or music (Gray, 2008). It would be a mistake to take this image of math as eternal, or to emphasize

E-mail address: bsterner@uchicago.edu (B. Sterner)

http://dx.doi.org/10.1016/j.shpsc.2014.03.001
1369-8486/

the successes that motivate it without paying attention to the failures that continue to drive math to change and grow.

By contrast, the first question we posed foregrounds how mathematization is an inherently normative, dynamic, and institutional activity that alters the proper conduct of science. The question focuses on how the *work* of mathematization changes the *doing* of science and does not presuppose facts about the general success or failure of mathematization. Rather, it highlights how mathematization transforms the way scientists themselves judge success and failure. Influence can also flow in the other direction, as mathematics changes from its interaction with science—consider, for example, the importance of genetics (and eugenics) and Brownian motion for the development of statistics. (For the case of genetics, see Stigler, 2010).

From this practical perspective, mathematization is a project of institution-building or reform carried out by certain scientists within a community with regard to certain aspects of their work, often in opposition to other scientists within the community. It requires making the case that things should be done *this* way, i.e. mathematically, and not as they were done in the past. In this manner, mathematization is an historical process that incorporates cognitive work by scientists to interpret, articulate, and argue for mathematical methods in a concrete organizational context. Studying scientists' practices of mathematization therefore offers a way to investigate its pros and cons: how do its advocates and opponents make their cases, what resources do they draw upon, and how are their efforts are judged over time by other scientists? We believe this represents a rigorous way of investigating the ongoing relationship between math and science, including where they are indistinguishable or overlap.

The normative structure of mathematization is thus organized around ideal and realization. Scientists draw on outside conceptual resources, such as a positivist theory of reason, to specify a normative ideal for their practices. In the case we will consider from systematic biology, the ideal describes what should hold true of classifications as a result of how they are made. Given this, there remains the task of realizing it in practice. Ensuring this happens is the charge of methodology. We can separate this into at least two parts: (1) stipulating how the ideal should be realized and (2) providing means to validate that it has. The way that methodology represents practice reflects both of these subtasks, in that the actions that are most important to stipulate are also the most important to validate (not that they are always possible or easy to track). Moreover, the development of new tests reflects scientists' growing knowledge about sources of failure in the stipulated method that have to be recognized and corrected.[1] In this way, we can track the process of mathematization by studying how scientists revise their methodology to account for important sources of error that obstruct their ability to realize the ideal. We draw here on recent work by James Griesemer, who analyzes theories as tracking devices (Griesemer, 2006, 2007, 2012).

In fact, this normative relationship between methodology and practice is quite general, and we use it as a way of investigating what changes are introduced into the relationship by mathematization in particular. We characterize the distinctive features of mathematization here using a comparison between two efforts to reform the practice of biological taxonomy between approximately 1940 and 1965. Our focal contrast is the numerical taxonomy movement in the 1960s with Ernst Mayr's contribution to the New Systematics in the 1940s. Both Mayr and the numerical taxonomists sought to formalize the work of classification, but Mayr introduced a *qualitative* formalism based on human judgment for determining the taxonomic rank of populations, while the numerical taxonomists introduced a *quantitative* formalism based on automated procedures for computing classifications. Regarding mathematization, we will argue that the defining contrast is how each movement conceptualized the temporal structure of the workflow of classification: more specifically, where and whether they allowed meta-level discourse about problems that occur in the process of producing the classification. We suggest that numerical taxonomy used a widespread strategy for coping with failure, "complete first-order linearization," that attempts to exile meta-level discourse from the classification process, relegating it to before and after the work of the process itself.

We begin by introducing the historical and conceptual background to biological classification in the early twentieth century. We also introduce Griesemer's notion of tracking devices and show how it helps us analyze mathematization in a comparative framework. We then discuss Mayr's efforts to reform classification using a theory of evolution in his 1942 book, *Systematics and the Origin of Species*. Afterward, we consider Sokal and Sneath's parallel effort to reform classification in their 1963 book, *The Principles of Numerical Taxonomy*.

2. Rules of the game

"The methods and techniques of a field of science are often like the rules of a game. It was Linnæus's principal service to biology that he established a set of rules by which to play the taxonomic game" (Mayr, 1942, p. 108). This comment from Ernst Mayr sets our scene, in which Mayr and later systematists raised the stakes on the taxonomic game so high that the field shook with debates reaching from the metaphysics of species to the organization of the life sciences (Hull, 1990).[2] Although these arguments often reached unprecedented levels of mathematical and theoretical abstraction for systematics, their character was different from more familiar stories of mathematical modeling in biology (e.g. Abraham, 2004). The point of all this theorizing was not to model or simulate processes of evolution *per se*, although the nature of evolution was an important factor. Instead, the effort was primarily methodological: to specify how scientists should classify organisms into groups. Hence ours is a story of the difference that introducing mathematics into "the rules of the game" made for systematists' practice of classification.

Mayr's choice to talk about the rules of "*the* taxonomic game" takes on particular significance against the fractured institutional history of systematics and its predecessor, natural history. Emerging from the 19th century, taxonomy was fragmented geographically and across groups of organisms. There were no methodological standards across the whole of taxonomy in the sense of agreed-upon, explicit rules for how to select and analyze specimens in order to produce a classification. Indeed, instituting international rules about nomenclature—how to name a species and designate specimens as material representatives—led to protracted arguments over many years in subfields such as zoology (Johnson, 2012, pp. 216–218). Practical training predominantly focused on what worked in a particular group of organisms rather than on a uniform approach across the kingdoms of life. As Mayr wrote in 1942, "the best textbook in most systematic groups is some particularly good monograph in that group which, by its thoroughness and lucid treatment, sets an example of method" (Mayr, 1942, p. 11).

The project of standardizing classification is fundamentally an institutional one: getting every scientist in the field to reliably classify their organisms in the same way (Gerson, 2008). A number of

[1] It is a general requirement for any robust methodology that it have techniques for addressing cases where following the method does not lead to the realization of one's aims. The best methodologies use failures as sources of knowledge in their own right, which William Wimsatt has discussed under the slogan "metabolism of error" (2007).

[2] To be precise, the issue shifted strongly away from taxonomic classification toward phylogenetics over the 1970's and 80's while larger debates continued to rage.