Experiments in Mathematics and the Natural Sciences – The Analogy Within the Descriptive Epistemic Context

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Abstract

In this paper my aim is to show that, given the descriptive epistemic context, the analogy between mathematics and the natural sciences holds even when one's epistemic route is experimentation. Experiments are usually taken to be the lynchpin of the natural sciences investigations, which seems to be the domain that does not have much in common with mathematics and the way we grasp the basic mathematical concepts. Putnam, on the other hand, interestingly points out that experiments are perceived in the mathematical investigations too. My goal is to go one step further than Putnam might have wanted to go and, hopefully, show that the analogy holds throughout.

As well known, foundations of mathematics in a more technical sense of the term is the study of the most basic mathematical concepts and logical structure of mathematics. It includes the study of how to organize such concepts into a hierarchy of more and less fundamental ones, of what are the axioms and rules of proof, as well as the study of the properties and limitations of formal systems (Simpson 2000). In a broader sense, "conceived of philosophically, the foundations of mathematics concern various metaphysical and epistemological problems raised by mathematical practice, its results and applications." (Detlefsen 1998)

This paper is about foundations of mathematics in the latter, broader sense of the term and focuses on a topic in the epistemic domain. The goal is to show that, given the context of discovery, the epistemic paths in mathematics are analogous with those in the natural sciences.

In particular, the paper is about the experiment as one of the possible epistemic paths, i.e. modes of initial epistemic access to both mathematical and scientific reality (objects and properties).

At first site, it might be difficult to see how mathematics – as an armchair activity – could be related to the experiments – usually perceived as belonging to the domain of the natural, (hence) empirical sciences.

Putnam, on the other hand, suggests that "the adoption of the axiom of choice as a new paradigm *was* an experiment, even if the experiment was not performed by men in white coats in a laboratory. And similar experiments go all the way back in the history of mathematics." (Putnam 1979: xi) Whatever we might think of the axiom of choice case and its history, when comparing the mathematical procedures with the experiments in the natural sciences the analogy, at least at first, seems implausible.

The two domains are usually found unrelated based on two considerations: firstly, experiments in science are practical procedures, done in laboratories, hence distant from the standard mathematical ones, and secondly, the domains are not mutually analogous due to an insurmountable difference in methodology: the axiomatic-deductive method in mathematics and the inductive/abductive method in the natural sciences.

I will address the former remark by analysing the structure of proofs and experiments, while my reply to the latter remarks will take into consideration a distinction to be made between the pre-formal and the formal development of theories in the mathematical domain.

I shall start by addressing the first consideration, namely that the experiments in science seem to be practical procedures, i.e. that the natural sciences are based on empirical/practical procedures while mathematics is an armchair activity.

How could the domain of mathematical armchair/a priori research be analogous to the predominantly empirical profile of scientific knowledge, in particular when it comes to experiments, often taken to be paradigmatic research tool for scientific discovery and hence *a posteriori* gained knowledge?

Notwithstanding this commonsensical view, let us see if experiments in (the natural) sciences ought to be practical in the first place.

The standard taxonomy, based on Galileo's writings, includes the distinction according to which there are three main types of experiments: the real, the imaginary and the thought experiments (MacLachlan 1973: 374). The real ones are those that have been performed, the imaginary are those that haven't been formed but could have been, while the thought experiments are those that could not be performed due to the lack of technology or because impossible in principle.

Given Galileo's writings, it is a contentious issue which experiments were real, and which imaginary. Koyré, one of the most prominent historian of physics of the 20th century (Stump 2001: 243), take many of Galileo's experiments that are considered to be performed as imaginary ones (MacLachlan 1973: 374). Others have defended the view that Galileo was a great experimenter, and had all the possible resources to actually perform the experiments (Settle 1961) (Drake 1978).

What about the thought experiments? Such experiments played an important role in the development of scientific theories in the work of scientists such as Galileo, Newton, Einstein, Heisenberg (MacLachlan 1973: 375). The list of well-known thought experiments is a long one: Stevin's inclined plane experiment, Galileo's leaning tower of Pisa experiment, Schrödinger's cat, Maxwell's demon, Newton's bucket, Einstein chasing a light beam, Twin paradox and many others (Brown 1991). Thought experiments turn out to be important for our discussion for two main reasons: (1) such experiments are not practical, the objects involved in such experiments are not concrete, and no direct manipulation of the objects is involved, and (2) the structure of such experiments resembles the structure of mathematical proofs.

Let us start with Galileo's thought experiment on falling bodies (or Galileo's leaning tower of Pisa experiment). It is the experiment which, as Galileo says through the words of Salviati,

> shows that difference of weight, even when very great, is without effect in changing the speed of falling bodies, so that as far as weight is concerned they all fall with equal speed: the idea is, I say, so new, and at first glance so remote from fact, that if we do not have the means of making it just as clear as sunlight, it had better not be mentioned... (Galileo 1914: 83)

I shall present Brown's formulation of the experiment (Brown 1991: 1).

Galileo starts from Aristotle's claim that objects fall at speed relative to their mass, having the heavier bodies fall faster than the light ones:

$v(H) > v(L)$

The following step is to get a compound body, more precisely to get a heavy cannon ball attached to a light musket ball.

If we take Aristotle's theory of gravity to be true, it follows that

(1) the combined system is heavier than the heavy ball alone, hence $v(H+L) > v(H)$ and
(2) the light ball acts as a drag and slows up the heavy one, hence $v(H+L) < v(H)$.

Aristotle's theory of gravity leads to a paradoxical result. Galileo's answer consists in making the two velocity equal, which means that the two bodies fall at the same speed: $v(H + L) = v(H)$.

Norton is famously representing thought experiments in physics as arguments in his (Norton 1991). Let us take the example of thermodynamics in which Norton shows how to prove certain assertions by using the structure of the *reductio ad absurdum* proofs in mathematics:

> Thermodynamics lends itself to some of the most effective of all thought experiments. This is because the three laws of thermodynamics are such that they can readily be stated as assertions of impossibilities.
> *First law*: It is impossible to design a perpetual motion machine of the first kind, that is, a machine whose effect is to produce more energy than it consumes.
> *Second law*: It is impossible to design a perpetual motion machine of the second kind, that is, a machine whose sole effect is to transfer heat from a colder heat reservoir to a hotter reservoir.
> *Third law*: it is impossible for any finite process to yield a temperature of absolute zero.
> An easy way to derive consequences from these assertions of impossibility is by a *reductio* argument. To derive a theorem *X*, take one or more of the above laws as premises. Assume not-*X*, the negation of *X*. Show that not-*X* allows the design of a machine prohibited by the premisses. Then conclude *X*. (Norton 1991: 131f)

Interestingly enough, Koyré goes one step further when asserting that "all the good physics is done a priori" (Koyré 1968: 88). I will not pursue Koyré's view any further. Even without endorsing Koyré's Platonism in physics, I consider it possible to defend the analogy between the domain of mathematics and the natural sciences in the epistemic descriptive context.

At his point, someone might complaint (and this is the second remark) that the mathematical domain and the one of the natural sciences are methodologically too different to support any kind of analogy, even in the epistemic descriptive context (i.e. in the context of discovery). Given that mathematical procedures are based on the axiomatic-deduction method, while the scientific ones on induction/abduction.

Let us address this remark. Frege rightly underlines that

> it is in the nature of mathematics always to prefer proof, where proof is possible, to any confirmation by induction. Euclid gives proofs of many things which anyone would concede him without question, and it was when men refused to be satisfied even with Euclid's standards of rigour that they were led to the enquiries set in train by the Axiom of Parallels. (Frege 1884: 2)

However, Frege is referring to the domain of confirmation, that is justification. The historical development of mathematical results, i.e. the heuristics (in the sense of the epistemic paths within the context of discovery) does not generally coincide with the proofs of theories in the context of justification. Lakatos stresses that the *pre-formal* procedures in the development of mathematical results cannot be represented as drafts of the *formal*, deductive proofs and deductive systems that we find in textbooks. The heuristic pattern is not deductive and has a development on its own (Lakatos 1976). Pòlya as well underlines the importance of distinguishing the process of discovery from the one of justification (Pòlya 1945).

Let us take the example of Euler's theorem from 1740. The problem was suggested to Euler by Philippe Naudé, a French mathematician, and it concerns the ways a positive integer can be written as the sum of distinct positive integers. Euler noticed, by analysing many numbers, that all of them could be decomposed into distinct integers in precisely as many ways as they could be decomposed into odd integers.

In the case of number 6, it can be decomposed as the sum of distinct natural numbers in four ways:

6
5+1
4+2
3+2+1

and it can be decomposed as the sum of odd numbers in the following four ways:

5+1
3+3
3+1+1+1
1+1+1+1+1+1.

Euler, as many other mathematicians in similar situations, then asked himself if the same holds for every positive integers. Later on he found an ingenious proof of the theorem (Dunham 1994: 57-63).

Dunham describes such a process of testing the assertion for many cases/numbers as a common practice in which mathematicians "like chemists, gain valuable insight by experimenting with specific cases before trying to formulate and prove general laws" (Dunham 1994: 58), while Pòlya compares the mathematician to "the naturalist who, impressed by a curious plant or a curious geological formation, conceives a general question" (Pòlya 1945: 115).

Throughout the history of mathematic examples like Euler's are legion.

Let me summarize. The contrast between mathematical and physical sciences within the descriptive epistemic context is less dramatic than assumed in the literature. Many important, even crucial experiments in physics are thought experiments, quite similar to their mathematical counterparts. Other are imaginary experiments grounded on knowledge from the laboratory ones. I hence suggest that we talk about analogy rather than about a dramatic contrast.

Symmetrically, to remain within the context of discovery, the method of reasoning in mathematics does not stand in stark contrast to its counterpart in the natural sciences. Namely, mathematical discoveries are often arrived at through inductive varieties of reasoning. The two are again to a significant extent analogous to each other.

To conclude, if we take into consideration the non-commonsensical aspects of experiments in the natural sciences as well as the distinction between the pre-formal and formal development of the mathematical theories, the analogy between the domain of mathematics and the one of the natural sciences in the descriptive epistemic context holds ground.

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