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WHAT IS THE GOAL OF PROOF?

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1. *Introduction*

We can try to use means-ends reasoning to explain the methodological choices of mathematicians. Don Fallis (this issue) raises a question for philosophers who advocate this strategy: For the sake of what goal do mathematicians choose the method of proof? Roughly speaking, mathematicians make it harder on themselves by looking for proofs rather than remaining content with approximations or empirical methods. If there is an easier way, what rational reason is there for choosing one that is more difficult?

In this paper, I sketch out a response to Fallis's question. The idea is that in order to explain the point of proof, we need to look at the goals of natural science. While this does not explain strictly mathematical goals of proofs, this does, I argue, answer Fallis's question. Fallis's question thus raises a difficulty for naturalistic views that assess mathematics in its own terms. This differentiates the naturalism of Penelope Maddy from that of Quine, which is not exposed to this difficulty.

2. *Fallis's Example*

It is plausible to think that proof is needed in order to have knowledge of the general case of theorems while empirical checking might suffice for particular cases. We might think here of Fermat's Theorem or Goldbach's Conjecture. But Fallis's example shows this is not the issue. Say we want to know whether a particular number N is prime. Then we might choose to test N by dividing it by all primes less than $\sqrt{N}$. Passing this test would prove N is prime.

But we might choose an easier test. We might choose to test N by dividing it by many primes less than $\sqrt{N}$, but not all. If N is very large, this may be much easier, and yet this method will be very nearly accurate. The method can be made as accurate as we want by applying more tests. If the goal is simply to determine whether N is prime, it would seem that the empirical approximation is the better method. Is there a rational explanation for why mathematicians should choose the more difficult method? (Moreover, why seek proof for general statements, when we might choose instead to test these as hypotheses?)

It is plausible that proof is sought for its own sake. Proof is, on this plausible view, an end in itself. But that is just to say that we have no means-ends explanation for the methodological choice of proof. Fallis then claims that the strategy fails to explain this fundamental methodological choice.

Another idea, however, is that proof may serve the goals of natural science. Among the goals of natural science, for instance, is that of testing hypotheses about the natural world. A hypothesis under interrogation, however, generally cannot be tested alone. Such tests generally require auxiliary hypotheses. If we have a fund of these, this will be useful. Mathematical proof, on this view, serves the function of supplying a fund of auxiliary hypotheses in a controllable way, so this is not *ad hoc*. Among other goals of natural science are to measure things, or to make or find something to solve a problem. I shall use three examples to sketch out the idea that mathematical proof has the function of serving various goals of natural science.

3. *Two Simple Examples*

One goal of natural science is measurement. Say we want to measure the circumference of the earth, following Eratosthenes's method. We assume that light from very distant object reaches earth on parallel rays. Then we measure the angular difference between the position of the North Star in the sky in Buffalo and in Miami. Then we measure the distance on the earth's surface between the two cities. But in order to get our measurement of the earth, we need to assume a further fact of geometry: The angular difference in the sky between the position of the North Star viewed from the two cities is equal to the angular difference between the two cities on the surface of the earth.

How do we know this fact of geometry? We might choose to prove it from more fundamental facts, including an axiom about parallel lines. Or we might choose to measure circles in order to confirm the equality of the two angles empirically. In this case, the difference between the two methods of handling the relevant geometrical facts is that the empirical method seems somewhat backward, while proof seems more in keeping with the methods practiced in science generally. But in this case, I do not think that the empirical method is an error.

One point to note about this example is that we assume either way that we can use circles and lines to represent the earth's surface and light rays. The question is how to make use of this geometrical representation, whether empirically or by proof.

Here's a slightly more complicated example. Another goal of natural science is confirmation of hypotheses. Say we want to confirm Galileo's hypothesis that acceleration due to gravity is at a constant rate of increase according to time, rather than distance. We derive the consequence that the distance traveled by a falling object is in proportion to the square of the time elapsed. That is, we prove that constant acceleration implies that distance traveled is in proportion to the square of time elapsed. This implication is an auxiliary supplementing the initial hypothesis of constant acceleration.

We follow Galileo in not trying to make a direct test of the hypothesis of constant acceleration. But by confirming that distance traveled is in proportion with the square of the time, we also confirm the initial hypothesis of constant acceleration. The proof serves the function of connecting the hypothesis of constant acceleration with our experiments.

We assume, again, that geometry represents the natural world. In this case geometrical line segments represent velocities and times. We further assume that velocity in a given direction is defined as distance traveled divided by time. (Galileo assumes this in his definition of uniform motion. (1954, p. 154)) We represent constantly increasing velocities as line segments perpendicular to a line segment representing elapsed time. Then distances traveled will be represented by the area of a right triangle, one of whose sides is time elapsed and whose altitude is the final velocity.

Galileo then proves simply that the area of the triangle increases with the square of the length of the side representing time, since the altitude increases in the same proportion (pp. 174–175). So distance varies with the square of time. (Actually, Galileo prefers to consider rectangles equal in area with these triangles. He states the theorem on accelerated motion as a consequence of a theorem on uniform motion equal to average accelerated motion (pp. 173–174). But I am not concerned here with Galileo's worries about the idea of instantaneous velocity, and either way we must assume that distance is represented by area.)

Here too, as in the Eratosthenes example, we might proceed in an entirely empirical manner. We might, for example, test the areas of triangles using marbles in frames, like billiard balls in a rack, checking whether we more closely approximate some sort of square law of increasing areas, if the marbles are reduced in size.

In this case, however, it does seem that we are adding to our difficulties, rather than lessening them, if we choose to rely on empirical methods rather than proof. The proof is so simple here. The majority of the reasoning is really in deciding how to represent the problem. As noted above, this decision about representation is neutral with respect to whether we choose empirical methods or proof to support our result.

4. *A More Complicated and Less Familiar Example*

Here is a more complicated example. Yet another goal of natural science is the technological goal of finding out how to make something useful. Say we want to be able to refract light from a point source to a focus point in a single refraction. Knowing how to do this would help explain how the lens in an eye sends light from distinct points in the world to distinct points on the retina. To solve this problem, we need to find or construct a curve such that a lens with a surface in the shape of that curve would refract light in the required way. Descartes solves this problem in his *Geometry*.

In Descartes's work on geometrical optics (1954, 1965), there are a lot of steps that may seem unnecessary. Descartes insists that every curve he investigates, for its optical properties or otherwise, should be constructed in a way that is Descartes's own (1954, p. 43). Descartes describes various compasses, hinged rulers, and ways of using stretched strings to trace curves. He does not accept curves as "geometrical" unless they can be constructed by such methods. I shall not begin to explain Descartes's idea of construction or the reason for his requirement (See Bos 1981). But one result of Descartes's requirement is that for every curve he considers, it is possible to construct it in at least an ideal sense. It is not enough just to write down an equation for a curve. In at least an idealized way, each "geometrical" curve is shown to be a mechanically realizable empirical object.

Descartes also subjects his optical conclusions to experimental tests when it may seem unnecessary. Once he had possession of the sine law of refraction, Descartes is able to show by an easy geometrical proof that hyperbolic and elliptical lenses have important optical properties. But for three years, Descartes worked on lens-grinding machines in order to make empirical tests of what he could easily prove. (Gaukroger 1995, pp. 190–195; Descartes 1991, p. 36)

Descartes was not held back by the need for empirical confirmation, however. One of the most important parts of his *Geometry* concerns the application of his method for finding normals (or equivalently, tangents) at any point of any curve he considers acceptably "geometrical." With his method of normals, Descartes thus claims there is a tangent at every point of every suitably "geometrical" curve (1954, pp. 95–104). Descartes gives an ingenious argument for this claim, but it is easy to see that his argument requires the assumption that limit points exist, and this is explained only by giving a foundation for analysis. Nevertheless he uses the method to prove that a lens in the shape of certain 4th-degree curves, the "Cartesian ovals," refracts light from a point source to a focus point in a single refraction (1954, pp. 131–135).

We can now imagine an attempt to confirm these results empirically. Imagine that Descartes had somehow been able to build a machine to grind a lens in the shapes of his 4th-degree curves. He could construct the curve using mechanical methods (in this case using a stretched string and a pivoting ruler). So perhaps he could figure out how to grind glass in that shape. Then it would be possible to test Descartes's claim about the existence of tangents at every point of his 4th-degree curves. We look for light from a point source focusing at a point. But after spending three years confirming a simpler proof, Descartes did not try this.

Descartes tried very hard to get empirical confirmation of his mathematical reasoning about the world. But he did not want this so much that he made his scientific work depend on this, even in a case where in principle, empirical confirmation of his mathematical reasoning was possible. In this case, natural science would be impeded by a requirement to test Descartes's result empirically. Descartes uses non-empirical methods to solve this problem in natural science, when strictly empirical methods would have been too difficult for him.

Did Descartes learn from experience not to hold mathematical statements up to empirical tests? In this case, it was rational to rely on proof in order to achieve a goal in natural science, whether or not Descartes himself made the decision on this basis.

5. *What Is The Point of Proof?*

Naturalists hope to explain scientific methodological choices by means-ends reasoning because in so doing, we may be able to show that human knowledge seeking activities are similar to the kinds of activities other animals engage in, differing only in complexity. Naturalists thus hope to show that a scientific picture of the world is coherent, and does not require an appeal to any scientifically incomprehensible processes, such as that of an intuitive grasp of some knowledge, in order to make sense of science. So it makes sense to start with our ways of interacting with the natural world in order to clarify how we interact with the inferred world of mathematics.

Fallis asks why empirical methods are not acceptable in mathematics. For the sake of what goal are we required to discard these useful methods? Here I suggest that we might just as well ask why non-empirical methods are acceptable in natural science: Why do we pick out some statements as not requiring empirical tests? I have described several goals of natural science. The goal in the first example is to measure something in the natural world. In the second example the goal is to test a hypothesis. In the third example, the goal is to find or construct something that solves a problem. It is likely that there are many other goals of natural science.

I did not explain what the goal of proof is except to show that some proofs serve the goals of natural science. I appeal to a rough distinction between the method of proof and empirical methods. I did not explain what natural science or mathematics are. I did not explain what proof or empirical methods are. I argue only that non-empirical methods, or proofs, can serve the goals of natural science, and that this answers Fallis's question of what are some of the goals of the method of proof.

Perhaps this is interesting only is we take seriously the naturalistic strategy of means-ends explanation of the methodological choices in science. Naturalistic philosophers of mathematics, however, differ over how to interpret the goals of mathematics. Maddy argues that naturalists should rely on mathematicians' understanding of mathematics as independent of its contribution to natural science. (Maddy draws a contrast between Quine's naturalism, which says that science does not need any justification beyond observation and hypothesis-testing, and her own view that "mathematics is not answerable to any extra-mathematical tribunal and not in need of any justification beyond proof and the axiomatic method." (1997, p. 184)) Certainly we do not want to throw away data that may be useful in our effort to understand the methodological choices of mathematicians and other scientists. But if Fallis's claim is correct, then naturalists cannot rely on a purely mathematical explanation of the fundamental choice of the method of proof.

In this paper, I have explained only that this difficulty does not occur for a naturalism that, like Quine's, is willing to look to the goals of natural science in order to explain mathematicians' methodological choices.

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