The Mathematician Qua Matematician Makes Value Judgments

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Abstract: In this article, I aim to show that there is a type of argument for the necessity of value judgments in mathematics exactly analogous to a type of argument for value judgments in science based off of risk assessment. I develop the argument out of the conception of mathematics posited by Imre Lakatos, and I show the analogy between the two arguments by explicitly highlighting the most important structural features of both. Then, in a final section, I show some of the implications that my argument for value judgments in mathematics may have in philosophy of mathematics and in philosophy of science.

I. Introduction

In contemporary work on philosophy of science, there has been much discussion about the roles of value judgments in the scientific method and in the project of science in general. Many such debates have centered on the ontological status of scientific objects and on the epistemic nature of scientific justification. Such debates are often complex, tedious, and highly contentious. However, some philosophers have advocated an additional, simpler route by which social values might enter into the scientific method. In the literature, this avenue has been termed *risk assessment*, and argues that at almost every step in the scientific process, scientists must engage in risk assessments, i.e. judgments about the likelihood of a given choice producing an incorrect answer, that are arbitrated by values and thus are themselves value judgments. In this paper, I will argue that there is an argument for the importance of value judgments in pure mathematics analogous in many ways to the arguments for value judgments in science made by way of risk assessment. I will do this first by giving a brief characterization of the main structural features of risk assessment arguments, although I will not attempt to give in-depth paraphrases of the arguments themselves. For the purpose of this paper, I will take it for granted that they obtain. Then I will set out my argument, drawing heavily from the work of Imre Lakatos. Last, I will discuss some of the implications that follow from my conclusions, namely that taking my argument seriously requires us to move away both from a strict Platonic conception of mathematical truth and the view that mathematical theories offer a special tool for determining the nature of reality that is able to function in the absence of empirical evidence.

II. The Risk Assessment Argument

Risk assessment arguments have a long philosophical history. To my knowledge, the first risk assessment argument was proposed by Richard Rudner in the 1950s. In *The Scientist Qua Scientist Makes Value Judgments*, he argued that the scientist in her role as a scientist makes value judgments because it is the task of the scientist to confirm or reject hypotheses. Rudner claimed that since the basis of scientific argument is induction from a pool of data, no hypothesis would ever be fully confirmed.[1] Instead, induction provides only supporting evidence. As such, the scientist must decide what level of supporting evidence is sufficient for confirmation in a given case. Rudner then claims that setting this level involves a straightforward ethical judgment of the importance of the hypothesis to be confirmed or denied. Thus, the level will be different in different cases. In particular, many authors have thought that it will be at its highest in cases where a mistake may have significant human costs. Rudner's argument elicited criticism from Richard Jeffrey, who claims that on Rudner's conception of science, the scientist *qua* scientist does not need to make value judgments. Instead, he may content himself with simply assigning probabilities of correctness to scientific hypotheses. It is only in the scientist's role as a citizen of a larger society and as a human being in general that he actually makes the ethical judgments necessary to assign truth-values to hypotheses.

This line of reasoning seems correct to me for as far as it goes, but it does not go very far. Those who have taken up the argument against this counter-attack have pointed out that varieties of risk assessment occur not just in the final step of confirmation, but throughout the entire process of the scientific method. These philosophers argue that scientists are called upon to make use of value judgments about risk assessment when determining scientific methodology, for example when picking statistical methods. One subclass of this argument claims that any experimental analysis will contain the risk of coming up both with false positives and false negatives, and that there is a direct trade-off between these risks, such that no method will allow the scientist to minimize both. It is this expanded risk assessment argument that I accept, and for the purposes of this paper, I will assume that it obtains. So that later we will be able to clearly see the structural resemblance between this arguments and my argument about mathematics, it will be useful to outline the general structural features of arguments for value judgments in science based off of risk assessments. We can do so in the following manner:

S1. There is no objectively determined a priori correct methodology for a given scientific project.
S2. The scientist qua scientist must determine the methodology to be used in a scientific project.
S3. The determination of scientific methodology ineliminably requires certain kinds of value judgments.
S4. The types of value judgments required are straightforward ethical judgments of importance.

Based on this characterization of the standard risk assessment arguments, I will now argue that there are arguments with an analogous structure in mathematics that lead to an analogous conclusion.

III. Imre Lakatos' Conception of Mathematics

In his seminal *Proofs and Refutations*, the philosopher of science Imre Lakatos outlines his vision of how mathematics proceeds as a discipline. His picture of the subject rejects the axiomatic, logically certain approaches of both logicists like Hilbert and logical empiricists like Carnap for whom mathematics establishes theorems about the relationships of pre-existing concepts. In addition, Lakatos rejects the related idea that mathematical proof establishes the absolute, unqualified certainty of the proven conjecture. Instead, Lakatos argues that we do not really understand the realm of mathematical inquiry until we being to inquire into a given concept. In fact, Lakatos suggests that in many cases we may never have a fully explicit and well-understood formulation of a given concept. This is because, according to Lakatos, mathematical concepts, be they polyhedra or sets or tensors, take shape only when we begin to study them. For him, this study consists of the method of proofs and refutations, by which we formulate naïve but educated conjectures about relationships between imprecise mathematical concepts. The conjectures become less naïve as we find counterexamples to them. The finding of exceptions to a conjecture requires us to modify either our conjecture or the domain over which we quantify it. According to Lakatos, if we do the former, we can be seen as reducing the content of the conjecture, i.e. we make it applicable only in certain special case, for example we may change a theorem about all polygons to a theorem only about squares. If we choose to take the latter path, we do not reduce the content of the theorem but the scope of inquiry.[2] We might, for example, decide that for some methodically specifiable reason the counterexample we have come upon ought not to count as part of the domain of the theorem, for instance if we have a theorem about polyhedra and decide that for our purposes a cylinder is not a polyhedron. Lakatos argues that we will not want to reduce either the scope of the theorem or the content of the theorem unduly, and so will strive for a balance between these two sorts of activities.

Based on his overall conception of mathematics, Lakatos formulates a set of heuristic rules for going about mathematical investigation.[3] It is worth noting that these rules are both descriptive and prescriptive. That is, Lakatos thinks that they describe, and in fact are derived from, the actual practice of mathematics as it is performed by mathematicians, and indeed, as it must be practiced if mathematics is to remain recognizable. Additionally, Lakatos thinks these rules provide a prescriptive heuristic for productively engaging in the principle activities of mathematical investigation and that one ought to attempt to follow them in such an activity, just as his imaginary class does in *Proofs*. Since I am lifting these rules directly from the text, it will be helpful now to clarify a bit of terminology. Where Lakatos speaks of “global counterexamples”, he means counterexamples that refute the entire conjecture under investigation, not one particular lemma. Likewise, where he says “local counter examples”, he means counterexamples that falsify one lemma, but not necessarily the entire conjecture, since they might easily be replaced. Lakatos believes that it is possible for a given counterexample to be both local and global. By “proof-analysis” he means the explicit list of lemmas that serve as the proof of a given conjecture. These lemmas themselves might undergo proof analysis, and depending on how precise we would like to be, so on *ad infinitum*. Where he says "monster", he means a counterexample that seems to be a perverse interpretation of a given concept, to take an example, Russell's set,[4] the set of all sets that do not contain themselves, might be seen as a monstrous interpretation of the concept of sets. By "deductive guessing", Lakatos means an educated guess founded on deductive reasoning which is itself based on the understanding of the concept under investigation gained through rules 1-4. Having clarified this, I can now state the rules as formulated by Lakatos:

> Rule 1. If you have a conjecture, set out to prove it and to refute it. Inspect the proof carefully to prepare a list of non-trivial lemmas (proof-analysis); find counterexamples both to the conjecture (global counterexamples) and to the suspect lemmas (local counterexamples).
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> Rule 2. If you have a global counterexample discard your conjecture, add to your proof-analysis a suitable lemma that will be refuted by the counterexample, and replace the discarded conjecture by an improved one that incorporates that lemma as a condition. Do not allow a refutation to be dismissed as a monster. Try to make all "hidden lemmas" explicit.
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> Rule 3. If you have a local counterexample, check to see whether it is not also a global counterexample. If it is, you can easily apply Rule 2.
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> Rule 4. If you have a counterexample, which is local but not global, try to improve your proof-analysis by replacing the refuted lemma by an unfalsified one.

Rule 5. If you have counterexamples of any type, try to find, by deductive guessing, a deeper theorem to which they are counterexamples no longer.

It is also worth noting that Rule 5 represents an alternative to the continuous investigation and growth of a given mathematical concept that occurs from following Rules 1-4 insofar as it scraps the original conjecture and tries to come up with something deeper, which in the context seems to mean something more elegant or with greater explanatory power. However, insofar as some of the concept formation that takes place from following Rules 1-4 before proceeding to Rule 5 need not necessarily be undone, a conjecture arrived at through Rule 5 may still owe some intellectual debt to those concepts.

IV. Lakatos' Picture as an Argument for the Important of Value Judgments

In *Proofs and Refutations*, Lakatos' aim seems to be to come up with a descriptive picture of mathematics that is consistent with its actual practice. Thus, by looking at how mathematicians actually go about their thinking in non-idealized cases, Lakatos arrives at the conclusion that math is done rather differently than philosophers normally suppose. He undertakes this investigation primarily to dispel the misconceptions about mathematics that have arisen in the last few centuries, and to show the relationship between the mathematical process of discovery and the scientific process of discovery. To my knowledge, Lakatos nowhere discussed the fact that his book could provide the basis for an argument for the importance of value judgments in mathematics, although at several places in his book he brushes, perhaps unwittingly, against this possibility. It is difficult to believe that he missed this possibility since both the tone and content of the book lend themselves so clearly to it. It may have been that he did not consider such an argument worth stating explicitly, or that it only seems obvious now in light of subsequent developments in science studies. Whatever may be the case, it seems to me that, despite its simplicity, the argument is well worth making explicit.

I will begin by outlining the structure of the argument by stating its main contentions in exactly the manner that I outlined those of the risk argument, with the implication that the corresponding propositions are directly analogous.

M1. There is no *a priori* correct methodology for a given project in pure mathematic.
M2. The Mathematician *qua* mathematician must determine the methodology for any given project.
M3. The determination of scientific methodology ineliminably requires certain kinds of value judgments.
M4. The sorts of value judgments required are straightforward judgments of importance.

These propositions provide a roadmap for the argument itself, and serve to keep the analogy between my argument and the risk assessment based argument at the front of the mind. It seems to me that the analogy itself is obvious, so I will now spend no time attempting to make it more explicit. Instead, I will turn to justifying the above propositions, beginning with (M1).

The key implication of the Lakatos-derived picture of mathematics is that this understand of mathematics undermines the old idea, going back to Euclid and Plato, that mathematics might be axiomatic, i.e. that we could drive the major theorems of all areas of mathematics from a set of propositions which were obviously true according to strict logical principles. This picture of mathematics has been highly alluring, not simply because it offered the potential for a rigorously stateable foundation for mathematics or because it suggested the potential to automate mathematical theorem-making, but also because it is a picture on which all mathematical ideas appear already well-formed, just waiting to be discovered. By accepting this assumption that we have precise ideas and concepts in mathematics and precise logical ways of relating them, one places oneself in a position from which it is not at all a far jump to grandiose conclusions about the objectivity of mathematical truth, for example the claims that mathematical truths are eternal and immutable or that mathematical truths represent deep truths about the world.

The Lakatos picture undermines this older picture of mathematics for several reasons. Perhaps the most important is that it shows that mathematics gives us no predetermined starting place. To borrow the discipline's common language, Lakatos points out to us that when developing a mathematical theory, it is rarely clear where to start. What is even less clear is whether the point we choose to start at was the only point or the best point. In fact, Lakatos demonstrates that we might productively begin in many different places, that where we begin will effect the course of our investigation and the theorems we come up with, and that there is no objectively mathematical way to say that some starting places are better than others regardless of your perspective on them. Indeed, Lakatos shows that we usually do not even have clear concepts with which to formulate axioms at the very beginning of our investigations. In those cases where it most naturally seems like we do, (in his case with the concept of polyhedron), we actually are dealing only in vague abstractions based off either common-sense concepts or concepts derived from previous investigations. This vagueness, of course, is not an accidental feature of any particular concept, but rather a necessary feature of our language. We deal most of the time, as Wittgenstein said, in family resemblances. In day-to-day use, I share Wittgenstein's view that this is as it should be—actually as it must be—but this fact poses a problem in mathematics. This is because mathematics as a discipline demands methodological precision and rigor. I contend that we are willing to regard a concept as mathematically relevant only if it has a stateable definition. Perhaps some would disagree with this claim, but even a cursory look at the actual practice of mathematics vindicates it. Mathematicians always operate with definitions, even if those definitions are sometimes vague and not fully explicit. Mathematics without definitions is not mathematics. Thus, the content of my claim is not that mathematical concepts are uniquely precise, but that the practice of pure mathematics uniquely requires precision, and so mathematicians do their best to sand off the rough edges of concepts and to state as much of their essence as they can. It is important to note, however, as Wittgenstein does in the *Philosophical Investigations*, that our normal concepts do not entirely satisfy this demand for precision. In fact, outside of mathematics, Wittgenstein argues that this is one of a number of fundamental errors that are central to philosophy as an undertaking. For almost any given concept, there are boundary cases. However, since it is necessary to have precise definitions in doing mathematics, the mathematician is forced to decide on these boundary cases in formulating his definitions. In turn, that formulation will affect the sort of theorems that are true. To illustrate this with a mathematical example, ask yourself whether a cylinder ought to count as a polyhedron. The answer is unclear, and one could reasonably argue it either way. What would arbitrate this example is context. We can place the cylinder accurately only when asked the following question: "Is a cylinder a polyhedron for the purposes of X?" where X is some task whose goal we are familiar with. If we consider, Lakatos' example of the Descartes-Euler theorem for polyhedra, namely that for all polyhedra the number of vertices plus the number of faces minus the number of edges is always equal to two, i.e. V+F-E=2, and let X stand for "proving the Descartes-Euler theorem", we can begin see how this works out in practice. If the definition of polyhedra includes a cylinder, then the theorem is not a theorem at all because it is false. If we want to rescue the theorem, and we do because of how we have defined X, we have to formulate a definition of polyhedra that does not include cylinders, or indeed any of the other fatal counter examples that we might be able to come up with.

This insight raises a few questions. One of which is whether we will ever reach a stable definition of a given mathematical concept, in this case whether we will ever reach a stable definition of the concept of polyhedra. One of Lakatos' central discoveries is that for any conjecture there will always be an infinite number of counterexamples, and thus we can continue the process of refining our definitions indefinitely. In the end, it is only through the process of mathematical investigation that we come to have any real idea of what our concepts are and which boundary cases they include and which they do not. That is not to say, however, that the concepts contained the decision procedure for arbitrating the boundary cases all along. They did not. Rather, we extend or contract our concepts in accordance with the counterexamples that we find. Some might claim that we are really dealing the creation of new concepts, but I am not convinced that this is the case. At any rate, I see no reason to be fussy about it. What this all amounts to is the conclusion that there is no *a priori* correct starting place to begin any particular mathematical investigation. We can also see that there is no *a priori* correct way to proceed in any mathematical investigation because, as I have just observed, there will always be a certain arbitrariness to the definitions that we choose to set. At the very least, there will always be a live possibility that we could have defined our concepts otherwise, even if only slightly. Taken together, both of these conclusions seem to me fit to justify (1), the statement that there is no *a priori* correct methodology in mathematics because any methodology with pretensions to objectivity would have to be able to specify a starting point and a manner of proceeding from there.

Statement (2) is more readily evident. Having accepted (1), all we need for (2) is to come to the conclusion that mathematicians cannot escape choosing a methodology. Recall that in the early risk assessment arguments, Jeffrey was able to respond that we do not have to give scientists the task of accepting or denying hypotheses. Instead, we can simply give them the power to assign probabilities of being correct to the hypotheses. Now, we ought to ask if there is a plausible way that we can construe the role of the mathematician *qua* mathematician such that he or she does not have to adopt a methodology that is arbitrary in the mathematical sense, i.e. is not grounded in true reasoning about best practices. I think it is pretty clear that the mathematician cannot without giving up the idea that it is the job of the mathematician to prove theorems. This is because we need a methodology to get from any starting point to any theorem. We may even need a methodology to pick a starting point. We have just shown that this methodology will not be justified in the way often supposed. Well, what about that conclusion that mathematicians does not prove theorems? This seems analogous to Jeffrey's conclusion. Yet, there is an asymmetry between mathematics and science. There is nothing else for mathematical work to consist in. It is the nature of mathematical propositions that they require binary truth-values. There simply is nothing for the probability of the truth of a theorem to consist in, and this is because mathematics is not quantified over the domain of physical phenomena in the way that science is. Certainly this is true for at least some propositions. Considering the statement that *there is 50% chance that all squares have four sides* highlights what taking this seriously would mean. Even if one disagrees about the possibility of probabilistic truth-values in mathematics, one would have to provide a theory of how mathematicians could come to have access to these probabilities. But, since we cannot survey any broad swath of the mathematical domain, this seems impossible. Thus, as this conception of mathematics is highly dubious and as mathematics seems to be getting along fine as is, I think that we are justified in concluding that it is the business of mathematicians as mathematicians to prove theorems.

Statement (3) follows easily from the first two. We see that mathematicians must pick a methodology and that this methodology is not objectively grounded in the fullest sense. Thus, we ask what reason they have for picking their methodology. Of course, they could pick one arbitrarily, by selecting two methodologies and flipping a coin or something along those lines. Yet, given the standard folk view of human psychology, the decision itself to pick randomly has its basis in a preference, i.e. in a value judgment of some sort—likely in a preference for objectivity that leads one to try and reintroduce it into the situation. What is more, it is not clear that methodologies can be designed without value judgments without mathematics thereby devolving into unproductive anarchy. Thus, based on these considerations, I think it reasonable to conclude that randomness will neither remove value judgments in mathematics nor lead to fruitful results. So what about methodologies picked non-randomly? Well, as

Lakatos shows, in mathematical practice we start with the goal of exploring a concept by attempting to prove or disprove something about it. We form the concept by accepting or rejecting counterexamples along the lines that I described in the previous section. I now contend that this is the pivotal point at which value judgments enter mathematics, for the mathematician will determine which counterexamples to accept or reject based on two things. The first is how strongly she wants the conjecture to become a theorem and the second is what she desires the range of the theorems content to be. Both of these are subjective and rely on value judgments. Thus we arrive at (3). What is more, it is easy to see that these judgments rely on considerations of importance, i.e. how important it is for the larger body of theory that a theorem be true, or how gratifying it would be given our epistemic preferences that a theorem come out true in the case of the former, and how important it is that the theorem have a broad range in the case of the latter. This is exactly what (4) claimed. Thus, we now see that Lakatos' argument provides a simple analogue to the risk assessment arguments that provides us with an analogous mathematical conclusion.

V. Applications of the Conclusion to Philosophy of Mathematics and Science

In this final section of the paper, I would like to show that the above argument has implications outside of the field of pure mathematics. Actually, this is where the implications are likely to be most important. Historically, Mathematicians have not been keen to change their behavior based on the work of philosophers, and at any rate, Lakatos only purports to describe how mathematics already works. There would likely be very little to change in its actual practice. Indeed, any change would amount to an increase in a certain kind of efficiency, rather than any sort of paradigmatic shift. Now, I will look at two examples of applications outside mathematics: one in the philosophy of science, and one in the philosophy of mathematics.

By applying Lakatos' conclusion to the philosophy of mathematics, we can see how it makes a certain theory of mathematical truth hard to accept. This theory is Platonism, which can be characterized by the belief that mathematical truths correspond to abstract truths and that mathematical objects are abstract objects. Thus, mathematical truth is analogous to ordinary theories of correspondence truth. Something is true mathematically just in case the relevant mathematical objects really do relate in the way described by the mathematical proposition. How is it that Lakatos calls this into question? Well, to begin with it is rather difficult to think that there are precise abstract objects that correspond to mathematical concepts when we have just seen that those concepts are shaped by human inquiry. A commitment to both these claims would entail a commitment to the idea that humans shape the abstract realm of objects through their mental actions. This is not something that could not be held, but it goes against the spirit of Platonism. The Platonist wants mathematics to be purely objective and to have clear truth standards so that we might have clear epistemic standards for evaluating mathematical propositions. If value judgments are crucial for determining which theorems are true, truth cannot possibly be objective in the normal sense. Furthermore, it is not really all that clear that the realm can qualify as abstract if it can be shaped by human action. Some philosophers would think that this requires causal completeness between the abstract and physical realms, in which case they really are one physical realm. This would not allow for Platonism in any recognizable sense.

A related problem becomes apparent in philosophy of science. It has often been held, in particular by the theoretical physics community, that the truths of mathematics are deep truths about reality. Thus, many theoretical physicists conceive of mathematics, as providing an instrument by which they can reach into the unknown, determining in the absence of empirical evidence what is true about the world. This view has been supported by the eventual conformation by evidence of theories like Relativity and the Standard Model of Particle Physics. These cases provide the motivation for holding mathematical theories to be true when they have no empirical support. A clear example of this kind of thinking is String Theory, which continues to have a large number of adherents, despite its inability to predict virtually anything. The attack on Platonism made possible by the uncovering of value judgments' role in mathematics provides some reason to abandon this view as well. For one thing, if all mathematical truths will be based on considerations of importance and are thus non objective, and truth about the universe is considered to be objective (it is by most scientists, if not by all philosophers), then it is not clear how those realms of truths could correspond. Additionally, when we give an intellectual genealogy of our mathematical concepts and their formation, we can see that we shape them to be useful. We are thus able to give an account of the usefulness of mathematics that explains it as the product of a type of selection. We prove theorems that are most useful by restricting concepts and domains in such a way that these theorems come out true. If by chance we prove theorems that do not have useful applications in the natural sciences, we may be less likely to hold fast to our previous restrictions on the concepts. Thus, we may accept counterexamples that lead to these theorems no long being theorems at all.

Conclusion

In this essay, I have shown the importance of value judgments in the process of engaging in pure mathematics, i.e. in proving theorems. I have revealed that this argument is analogous in important ways to the types of arguments that have been made about value judgments in the scientific process of risk assessment.

For this, I have relied heavily on the work of Imre Lakatos, to whom I owe a debt of gratitude. Finally, I have shown that my conclusions about mathematics have far reaching implications for both the philosophy of mathematics and the philosophy of science. It is my hope that the reader has found these arguments persuasive, or at least illuminating.

Notes:

1. It seems to me that this is Lakatos' way of taking that statement. The first is a sort of Humean skepticism about induction, in which inductive reasoning never provides sufficient evidence for rationally holding a belief, and the second is that inductive reasoning is able to provide a form of evidence, and that the strength of such evidence is proportional to the sample size of the inductive judgment. On the latter construal, inductive reasoning can provide strong support for an argument, but is never conclusive. As the sample size rises, the supporting power of the inductive argument will asymptotically approach confirmation. Although he never explicitly chooses between these two interpretations, it seems to me that Rudner's assumption that it can be rational to hold a scientific proposition as a belief presupposes the latter.
2. In Proofs and Refutations, Lakatos waffles a bit about exactly which responses to a given counterexample count as increasing content and which count as decreasing it. This difficulty is compounded by the fact that he presents his argument in the form of a dialogue between mathematics students. The result is that except in the few cases where the teacher character draws sharp lines, it is always a bit unclear which stance Lakatos himself takes. However, it is my view that it does not much matter here. The question of whether an adaptation increases or decreases content seems to me to be a matter of perspective. For the sake of argument, we need to observe that our choices following a mathematical counterexample will have differing effects with respect to content, and that to some extent these affects represent a trade off.

3. As in the footnote above and for similar reasons, it is rather unclear whether Lakatos thinks these rules represent the final word on the subject. They are presented rather early on in the text by a character that is not Lakatos' teacher, although they do receive his tacit approval. Three rules are stated together, with the last two being added at later points. One might take some of the later discussion as going against these rules, and while it is certainly true that some of Lakatos' characters take issues with them, it is not clear that Lakatos himself abandons them. His teacher does not seem to, and I see no reason to believe that he is committed to the objections. At any rate, these rules present a good picture of Lakatos' conception of science as it strikes me at this time, and so they are sufficient for the task I have in mind. They may not represent that totality of Lakatos' thought and that is quite alright.
4. Asking the question "Does this set contain itself" of this set entails a famous paradox, which prompted the strict reformulation of set theory on axiomatic lines, with the goal being to formulate a coherent conception of sets that did not allow for the Russell Set. Russell himself attempted this with his theory of classes, but his work did not outlast him. Zarmelo-Fraenkel set theory is the most ubiquitous modern formulation of the subject.

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