Visual Thinking in Mathematics: An Epistemological Study
by Marcus Giaquinto
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reviewed by Jeremy Avigad[1]

Published in 1891, Edmund Husserl's first book, *Philosophie der Arithmetik*, aimed to "prepare the scientific foundations for a future construction of that discipline." His goals should seem reasonable to contemporary philosophers of mathematics:

> ... through patient investigation of details, to seek foundations, and to test noteworthy theories through painstaking criticism, separating the correct from the erroneous, in order, thus informed, to set in their place new ones which are, if possible, more adequately secured. [7, p. 5][2]

But the ensuing strategy for grounding mathematical knowledge sounds strange to the modern ear. For Husserl cast his work as a sequence of "psychological and logical investigations," providing a psychological analysis

> ... of the concepts *multiplicity*, *unity*, and *number*, insofar as they are given to use authentically and not through indirect symbolizations. (*ibid.*, pp. 6–7)

This emphasis on psychology is a reflection of Husserl's training. As a teenager studying in Leipzig, he attended the lectures of Wilhelm Wundt, a seminal figure in the field of experimental psychology. Wundt held that, via introspection, we can study and classify our inner experiences, in much the same way that scientists study the natural world.[3] People working in his laboratory were therefore trained in procedures for observing and reporting on their own thought processes, as a means of gathering scientific data regarding our cognitive faculties. Bridging the gap between psychology and epistemology, Wundt felt that the results of such inquiry could have normative consequences, since the principles of reasoning employed in the

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[2]Here and below page numbers refer to the English translation indicated in the bibliography.

[3]Wundt himself provided a readable overview of the method [27], which is still available in English translation today, as well as a longer work [26] which describes the method in greater detail.

sciences not only have their origins in psychological processes, but, moreover, are justified by the fundamental role they play in thought. His two-volume work, *Logik* (1880/1883), thus combined empirical considerations with a Kantian emphasis on the way that knowledge depends on our cognitive faculties.[4]

In *The Philosophy of Arithmetic*, Husserl applied something akin to Wundt's analysis to the the fundamental notions of arithmetic, to develop a genetic account of concepts like "something," "unit," "one," "collective combination," "multiplicity," and "number." What makes the work interesting are the dynamic terms that are used to characterize the way these concepts arise and the role they play in thought. For example, a collective combination arises from "focusing attention" on the relationship between objects in a group, and "noticing" that they share something in common; a "multiplicity" then arises from "seeing" the objects as units, and "disregarding" their individual nature. A number arises from the process of "thinking of" a multiplicity as an answer to the question, "how many?" Husserl's exposition is discursive and imprecise, but his lush account of concepts interacting in a lively mental realm found resonance with cognitive scientists a century later. One can get a sense of the project from his account of abstraction:

> To disregard or abstract from something means merely to give it no special notice. The satisfaction of the requirement wholly to abstract from the peculiarities of the contents thus absolutely does not have the effect of making those contents, and therewith their combination, disappear from our consciousness. The grasp of the contents, and the collection of them, is of course the precondition of the abstraction. But in that abstraction the isolating interest is not directed upon the contents, but rather exclusively upon their linkage in thought – and that linkage is all that is intended. (*ibid.*, page 83)

Thus abstract concepts arise from a process of "disregarding" certain aspects of a particular perception, "directing" attention at certain others, "isolating" those aspects, and "intending" the concept to be the result of those acts.

The task of reviewing the work fell to Frege, who could hardly be expected to be sympathetic. In his *Grundlagen der Arithmetik* of 1884, he had railed against psychologistic views of the philosophical enterprise, but his scorn for the work was extreme, even by Frege's standards. His critique of Husserl's account of abstraction, in particular, is a marvel of philosophical sarcasm:

[4]Here I am relying on the characterization of this work in Alan Kim's article [9].

> We attend less to a property and it disappears. By making one characteristic after another disappear, we get more and more abstract concepts… Inattention is a most efficacious logical faculty; presumably this accounts for the absentmindedness of professors. [5, pp. 84–85]

The extent to which Husserl's early philosophical work is susceptible to Frege's criticisms, as well as the extent to which these criticisms has substantial influence on Husserl's later work, is still subject to debate (see, for example, [18]). But Husserl clearly felt the need to respond to Frege's invective and distinguish his project from brute psychology, and the Prolegomena to the first volume of his *Logische Untersuchungen* [8] contains long passages renouncing psychologism. Husserl's later transcendental idealism reinforced the distinction between his philosophical program and psychology by characterizing the former as a determination of the essential capacities of an idealized mind, rather than the determination of our own mental capacities.

With Frege, analytic philosophy took a different tack. When one reads *his* characterization of the foundational project in the *Grundlagen*, there is a striking absence of explanation as to whence the concept of number that he presents derives its normative force. Rather, the work can be read as an analysis of the concept of number as it is used in our scientific and informal practice, and a clarification of the norms that govern that use. However one interprets the project of the *Grundlagen*, Frege made it clear that psychology has nothing to do with it: his first fundamental principle is that there "must be a sharp separation of the psychological from the logical, the subjective from the objective" [4, p. 90]. That attitude has held firm, at least in analytic philosophy, to the present day.

But a lot has changed since the turn of the twentieth century. Traditional research in the philosophy of mathematics has begun to stagnate, and the various metaphysical and foundational "isms" on offer have by now sprouted so many prefixes, modifiers, riders, and caveats that it is often hard to tell what the discussion is about. Many now feel that if the philosophy of mathematics is to have any bearing on mathematics itself, the subject has to attend to the kinds of value judgments that govern everyday mathematical practice, and provide a more realistic description of the goals and purpose of mathematical activity. In this last respect, the goal of navigating a complex world with limited cognitive resources seems to be a reasonable candidate, in which case the nature of those cognitive resources becomes relevant.

Moreover, psychology now seems much better equipped to deliver. Cognitive science offers a robust vocabulary and methodology for analyzing our cognitive capacities, and experimental psychologists have made great strides in mapping out our fundamental systems of numerical and spatial cognition. In particular, the nature of visualization and visual reasoning has been an especially rich topic, yielding not just interesting experimental data but also spirited methodological debates as to how such data should be interpreted and understood.[5] Thus it is not surprising to come across a book like Marcus Giaquinto's *Visual Thinking in Mathematics*, a timely exploration of the relationship between contemporary psychological theories of cognition, on the one hand, and issues in the philosophy of mathematics, on the other.

The core methodology is presented in the first five chapters. After a brief introduction in Chapter 1, Chapter 2 surveys recent findings in cognitive science, which support a model of visual perception in which "perceptual recognition" yields representations that have a kind of conceptual content. In Chapters 3 and 4, Giaquinto uses this model to make the case that visualization can secure mathematical knowledge. In Chapter 5, he considers the role of diagrams and visualization in geometric proof. Having established the basic approach with respect to geometry, Chapters 6 through 8 present a similar analysis of arithmetic. Finally, Chapters 9 through 12 branch out to consider other types of mathematical reasoning where visualization seems to play a role, including reasoning in analysis, algebraic and symbolic reasoning, and reasoning about mathematical structures.

The historical context I have provided is relevant insofar as it is impossible to provide a meaningful assessment of Giaquinto's project without addressing the broader question of the role that psychological data can or should play in the philosophy of mathematics. Since my remarks will be generally critical, I want to make one point clear up front: Giaquinto has written an important book. The philosophy of mathematics has of late shown a disappointing indifference to scientific developments that shed light on many aspects of mathematical activity, lacking the temerity to engage issues that are of interest anywhere other than academic philosophy journals. Giaquinto is to be commended for forcing us to think about how the philosophy of mathematics relates to important developments in experimental psychology, and if some of the aspects of the work are problematic, we need to find a better synthesis, rather than throw up our hands and go back to business as usual.

What is interesting about Giaquinto's approach is that although it pushes the boundaries of the philosophy of mathematics, it maintains a traditional

[5] The topic has sustained a healthy dialog between experimental psychologists, computer scientists, and philosophers. See, for example [6, 11, 15, 22].

focus on the appropriate means of obtaining mathematical knowledge. As Giaquinto explains in the preface:

> This book is not a mathematical text..., not a psychological investigation..., nor a How-To manual.... It is a work of epistemology. But unlike almost all other writing in the epistemology of mathematics, it is constrained by results of research in cognitive science and mathematics education. (p. v)

One's initial impulse may be to agree with Frege that psychology and epistemology should be kept distinct. We tend to feel that our subject matter is *mathematics* in some abstract or transcendent sense, rather than human cognitive capabilities. One way to clarify the difference is to distinguish between the appropriate methods of inquiry: experimental psychologists rely on things like verbal reports and timing data gathered as subjects perform various cognitive tasks, whereas philosophers tend to rely on conceptual analysis and other forms of argumentation. Another sense in which bringing in cognitive data seems to constitute a change of subject is that we usually take philosophy, in contrast to psychology, to support normative claims. When Boole portrayed the logician's task as that of determining the laws of thought, he was quick to distinguish this from a description of how people actually think. After all, people make mistakes, and while the mistakes that people make can provide useful insights into our cognitive functioning, the very notion of "making a mistake" only makes sense relative to a normative stand as to what it means to "get it right." Philosophy has traditionally been concerned with the latter.

Giaquinto is fully aware of these conventional views, and argues forcefully that they should be rejected. But once the usual methodological barriers are lifted, his project suffers from an uncomfortable vagueness of purpose, and it is ultimately unclear what the epistemological story is supposed to do. The book's opening chapters do not distinguish between diagram use and visualization, that is, between the use of physical images and diagrams in our reasoning, and the act of seeing such images and diagrams "in the minds eye" (whatever that may mean). This indicates that Giaquinto's primary concern is not whether a diagram or an image can play a justificatory role in a proof, but, rather, whether the perception of a diagram or image can play a role in obtaining knowledge. This shifts the task from determining the appropriate public warrants for mathematical assertions to something more hazy and subjective, namely, the legitimacy of certain types of thought. In this sense, it is possible for someone to have obtained mathematical knowledge without being able to justify the claim to anyone else.

Having shifted the focus in this way, Giaquinto adopts a broader epistemological stand whereby a true belief is considered knowledge if it is obtained as a result of a reliable epistemic process.[6] This is the sort of justification one wants if knowledge is to be understood as a basis for rational action. But in the case of mathematics, such justification factors naturally into a mathematical and empirical component: if a mathematical belief is to justify a decision to design a bridge in a certain way, surely it should be *true*, and so, for Giaquinto, the central question is whether visualization provides us with a reliable means of obtaining true mathematical beliefs. This is the sort of question that psychology and educational research are well-equipped to handle: first one develops a set of instructions or pedagogical interventions that is designed to get subjects to use the method of visualization in question, and then one develops a series of assessments to measure the method's success. Performing the intervention on a group of subjects and assessing the results relative to a control group yields statistical data that can support claims as to the reliability of the method.

But Giaquinto is interested in reliability of a stronger sort. He takes "visualization" to involve the use of inferential dispositions, related to our perceptual faculties, that cause us to hold certain beliefs once certain input conditions are triggered. Giaquinto then takes such an inferential disposition to be *reliable* if "the output belief would be true for any input that satisfied the given condition" (p. 41). These inferential dispositions can be chained to yield more complex belief-forming dispositions: "a belief-forming disposition is reliable if it is reliable by this criterion or if this disposition results from other dispositions all of which are reliable by this criterion" (*ibid*).

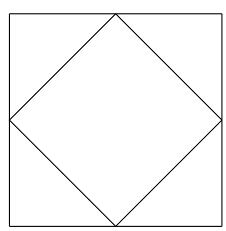
Giaquinto's goal is to show that our visual faculties provide us with belief-forming dispositions of this sort. At the risk of oversimplifying, the story goes something like this. Marrying contemporary cognitive models of visualization with a model of concepts advocated by the philosopher Christopher Peacocke, Giaquinto suggests that visual perception can trigger certain acts of "recognition" that cause us to represent our perceptions with a type of conceptual content. These representations can then trigger belief-forming dispositions that reliably result in true mathematical beliefs. Consider, for example, the diagram below. In Chapter 2, Giaquinto proposes that per-

[6] Giaquinto notes (p. 40) that, in addition to reliability, there may be other rationality constraints as well. In Chapter 4, he also considers the question as to whether visualization makes it possible to "discover" mathematical knowledge, where the word "discover" is used in a technical sense to describe a process that is not just reliable and epistemically rational, but also independent. But his discussions of epistemic rationality and independence are brief, so I will focus on the reliability requirement.

ceiving the outer figure *as* a square means representing it with a certain "perceptual concept," which registers that the perceived object is a plane figure bounded by straight edges, that the opposite edges are parallel, and so on. According to Giaquinto, the only difference between this and the *geometric* concept of a square is that instances of the latter are required to be perfect exemplars of their kind. The story is thus very similar to Husserl's, but any acts of "noticing" or "disregarding" needed to represent the perception as a square are carried out by our basic perceptual hardware, which is configured to do the work for us automatically. Similarly, on Giaquinto's account, perceiving the inner figure as a tilted square, and perceiving the figure as bearing certain symmetries, means representing those with certain perceptual concepts. In Chapter 4, he argues that our belief-forming dispositions can act on the content of these perceptual concepts to deliver the following justified true belief:

> If c_i ("the inner square") is the square whose vertices are midpoints of the side of a square c ("the original square"), then the parts of c beyond c_i ("the corner triangles") can be arranged to fit exactly into c_i, without overlap or gap, without change of size or shape.

Combined with other types of reasoning, this can provide the geometric knowledge that in the configuration depicted by the diagram, the area of the inner square is half the area of the outer square.



Giaquinto allows that his proposals are speculative, pending cognitive data that support the claim that we employ perceptual representations that are distinct from the linguistic representations that are more traditionally taken to underwrite inferences involving squares and areas. But even modulo the legitimacy of the cognitive model, it is not clear what the account is supposed to tell us. Suppose we were to come across a subject who somehow drew a false conclusion from the diagram above, but showed perfectly ordinary behavior in the types of visualization experiments that produced the type of data Giaquinto describes in the opening chapters of his book.

Would we conclude that the subject has a system of visual perception that is different from ours? We would more likely conclude that the subject had "misunderstood" the image, and reasoned about it in an incorrect way; or, in Giaquinto's terminology, that the wrong belief-forming dispositions were triggered. But now consider the contrary case where the subject comes to believe the theorem in question on the basis of the diagram. Is this belief knowledge? Well, perhaps — but only if, in this case, the *right* belief-forming dispositions were triggered. But how can we tell whether this is the case? The fact that the right dispositions were triggered has to amount to more than the fact that the subject ends up with the right answer, because otherwise having the knowledge is no different from having the belief (modulo, perhaps, the assumption that *some* belief-forming disposition was triggered in obtaining the belief). But in the absence of any independent scientific or philosophical account of what it means for the "right" process to have occurred, we can only conclude that an epistemically significant event may have taken place, without any tangible effects.

The problem is that the story lays two theoretical domains side by side without sufficient linkages between them. Perceptual categories like "edge," "region," and "surface" in the cognitive scientist's vocabulary are theoretical terms designed to explain experimentally observed cognitive abilities and behaviors. They do not refer to the same sorts of things as "line," "square," and "plane" in the mathematician's vocabulary, which describe abstract objects, governed by mathematical rules. It seems fanciful to think that the cognitive scientist's theoretical posits will line up neatly with the basic mathematical notions. But even if they do, the problem is that Giaquinto has done nothing to connect the two domains, beyond postulating a "correspondence" between the two types of concepts. For a cognitive representation to "correspond" to the mathematical concept it has to trigger a belief-forming disposition which results in a normatively correct mathematical inference; for, in the face of an incorrect "output," we simply conclude the "wrong" belief-forming disposition was triggered. But without grounding talk of belief-forming dispositions in empirical terms or providing an account of which belief-forming dispositions are triggered by external stimuli, Giaquinto's account tells us little more than the fact that visualization (whatever it is) can, possibly, provide us with mathematical knowledge, *provided* that it employs processes that reliably give us the right answer.

Put differently, we don't "see" numbers, squares, and functions. We often use *representations* of numbers, squares, and functions in our reasoning, and what we perceive are the representations. The figure above can represent a square even though the lines are pixilated and wide and imperfect, and could continue to represent a square even if there lines were to grow squiggly and splintered, *provided* the representation is used correctly; which is to say, provided our inferences conform to the proper mathematical norms for reasoning about squares. Mathematical logic and conventional approaches to the philosophy of mathematics do a pretty good job of clarifying the latter. From a psychological standpoint, it is also important to understand how we are able to reason about squares correctly, and how we represent them. But these are empirical questions, and well addressed by the methods of experimental psychology.

What, then, is the epistemological story supposed to do? It doesn't tell us whether we should be comfortable acting on the results of visualization, whether we should accept visual arguments as sufficient warrants to truth, or whether we should encourage visualization in our teaching practices. In what sense, then, are we to understand the term "knowledge"? The opening chapters of the book read better as speculative psychology, suggesting cognitive models that may help explain our mathematical behaviors. But then it is not a matter of providing an epistemological account of the way visualization *can* provide knowledge; it is a matter of providing an adequate empirical explanation of how we are successful in acquiring beliefs that our normative accounts sanction as true.

And when it comes to understanding our mathematical behavior, the body of psychological data is not nearly as clean as Giaquinto's presentation suggests. In recent years, there has been remarkable progress in charting our core systems of cognition, including cognition of numeric and geometric relationships.[7] The results are fascinating, but often wild and unruly. Consider, for example, disorientation experiments. Take a rat, put it in a rectangular box, and let it discover food in one corner, say, with the long edge to the left and the short edge to the right. Take it out, spin it around, and put it back; the rat will look for food in the two symmetric corners of the box. What works for rats works for young children: show a young child a toy in the corner of a rectangular room, spin her around, and return her to the room, and she, too, will utilize the same geometric cues. So far, as is well and good: we seem to have a fundamental perceptual ability to recognize differences in lengths and orient ourselves accordingly. But now put the same child in a room shaped like a *rhombus*, where all the *sides* have the same length, but two *angles* are larger than the others. Remarkably, the

[7] For overviews, see [10, 24], as well as [3] for a popular exploration of contemporary research on core systems of numeric cognition. Details on the disorientation experiments described below can be found in [2, 12, 13, 25].

ability to distinguish the corners vanishes, and the child will look for the toy in all four corners. The child also looks in all four corners if, instead of using a rectangular room, experimenters use masking tape to mark off a rectangular region on the floor. But here is the kicker: instead of tape, build a small rectangular barrier, just a few inches high, and the geometrically normative behavior returns: children will once again search for the toy in the two geometrically identical corners.

What are we to make of this? Are we to infer that visualization can provide us with mathematical knowledge of lengths, but not angles? Or that visualization can provide us with mathematical knowledge of barrier-lengths, but not line-lengths? Again, it is the lack of a clear correspondence between the cognitive and mathematical notions that leaves us hanging. One notion of "perceiving a square" may be useful for explaining what happens when we try to orient ourselves in a square room; another may be useful in explaining our responses to optical illusions; yet another may be useful in understanding the mistakes we tend to make on geometry tests. The psychological challenge is to weave the data together to provide a compelling account of our fundamental cognitive faculties and the role they play is producing the various observed behaviors. In this regard, Giaquinto's account of perceptual concepts and belief-forming dispositions provides interesting and potentially useful ideas. But then the goal is to develop cognitive models that are carefully supported and tested by experimental data, rather than to determine the proper ways of obtaining mathematical knowledge.

Moreover, perceptual models alone are not likely to get us very far. Mathematics is something that we *learn* to do, and even Giaquinto's most elementary examples of geometric knowledge involve distinctly mathematical concepts like "square" and "area" that are governed by mathematical, and not behavioral, norms. If *that's* the mathematics we are trying to understand, cognitive science seems to support the claim that Frege had it right all along: the data suggest that our core cognitive systems only begin to provide what we take to be bona-fide mathematical behavior when language sets in, and that language provides mechanisms that are essential to mathematics.[8] This makes it unlikely that satisfying explanations of our properly mathematical abilities will be grounded in our core systems of perception.[9]

[8] For example, it is possible to prevent geometrically normative behavior in adult disorientation tasks by "turning off" the language faculty, that is, by distracting the subject with other verbal stimuli or tasks. See also [3], and [23] for neurobiological evidence that some mathematical tasks are linked to language ability.

[9] Maddy [14] reaches a similar conclusion.

All this is not to deny that the philosophy of mathematics can play an important role in supporting efforts in experimental psychology to determine how we understand mathematics, or, say, how it can be taught effectively. Philosophical considerations are needed in formulating the basic questions, which implicitly rely on a conception of what it means to "understand mathematics," and what the goals of a mathematical education are. But such an interaction does not mean that we need to overhaul the methodological apparatus that has served both disciplines well for the last century or so. By conflating normative and empirical issues, Giaquinto's presentation muddies the waters in a way that is not helpful.

Chapter 5 returns to more familiar ground with a discussion of geometric proof, and Giaquinto argues that diagrammatic inferences can play a legitimate role in such proofs. He addresses two objections to such a claim. The first objection is that diagrammatic inferences do not always yield general conclusions. In response, Giaquinto simply argues that, if performed correctly, they can, so long as the inferences do not depend on any features of the diagram that are not guaranteed to hold by the assumptions at play at the relevant stage of the proof. The second objection is that diagrammatic inferences lack the transparency of verbal inferences. Here Giaquinto simply responds that the notion of transparency is context dependent; in ordinary life, diagrammatic inferences can be just as transparent as verbal inferences, and the latter are no less fallible.

It is telling that, in this chapter, models of perception slip into the background. Giaquinto does not make it clear whether he thinks that diagrammatic inferences in proofs have to be justified on the basis of the perceptual models that he describes in his first four chapters, but these issues seem to be orthogonal to the discussion. The more interesting claim is that, in a mathematical proof, proper diagram use can support inferences that are both valid and transparent. Giaquinto had earlier used the perceptual models to make the case that we may be justified in drawing certain immediate inferences even without being able to explain the reasoning to someone else; but there is a better route to this conclusion. The very notion of a proof presupposes that there are some inferences that are fundamental in the sense that they can be applied without any further need of justification. Asked to explain why "A" follows from "A and B," we can do little more than say it is obvious, or appeal to the meaning of the word "and." Understanding the word "and" means, in part, accepting inferences of that sort.

But why should diagrammatic inferences be any different? Doing Euclidean geometry correctly means, in part, being able to recognize the correctness of certain conclusions that are "clear" from the diagram. Understanding the use of Venn diagrams means accepting that regional inclusions can be used to license an implication. Knowing how to read the category-theoretic literature means, in part, knowing how to chase diagrams and infer appropriate equalities. That is not to say that diagrammatic inferences cannot be explained in verbal terms. Indeed, a substantial portion of Giaquinto's prose is devoted to doing just that. Similarly, inferences in Euclidean geometry can be explained in terms of the Cartesian interpretation of points, lines, and circles; and diagram chases in category theory can be replaced by lengthy equational derivations. But the story goes both ways: diagrams can just as well be used to explain verbal inferences, as when we try to convince someone of the validity of an Aristotelian syllogism by drawing a diagram that renders it obvious. The point is simply that in ordinary mathematical practice, certain inferential steps are taken to be immediate and in need of no further explanation. Understanding the mathematics involves accepting these inferences.

The fact that diagrams are often invoked in such settings raises a number of important questions. What roles do diagrams play in proofs? What kinds of information can be recorded in diagrams, and what are the consequences that can validly and transparently be inferred? How do we distinguish between features of the diagram that are essential from the ones that are incidental? These questions provide a basis for rich and rewarding epistemological inquiry. Edward Dean, John Mumma, and I, for example, have undertaken a study [1] of the nature of diagrammatic inference in the *Elements*, building on Mumma's PhD thesis [20] and work by Ken Manders [16]. (Giaquinto cites another analysis of diagram use in Euclidean geometry due to Nathaniel Miller [17]; see also [19, 21].) Our analysis makes it clear that whatever role diagrams are playing in a Euclidean proof, using them correctly is not just a matter of reporting on features observed in a particular diagram. Rather, there are subtle protocols that govern the proper use of diagrams, and making these norms explicit can tell us a lot about the nature of Euclidean geometry.

But characterizing the norms that govern a mathematical practice is simply a form of logical analysis, in a sense that is perhaps broader than Frege intended, but which still renders the analysis distinct from psychology. This enables us to separate the task of characterizing mathematical norms from the more difficult task of explaining why the norms are the way they are. The reason the latter task is more difficult is that it is often not clear what kind of "explanation" we are after; one may reasonably invoke psychological, historical, pedagogical, social, or computational data to explain why we do the mathematics we do. We may, instead, try to understand our mathematical efforts in terms of the role they play in the broader scientific process, or invoke general theoretical or broadly epistemic virtues to justify the practice. But the most important prerequisite to making philosophical progress is to ask questions in such a way that one has at least some sense of what might constitute an answer, and what methods should have bearing on the question. One should therefore be wary of blurring methodological boundaries in the absence of compelling reasons to do so.

By now (perhaps too late) it should be clear that I should by no means be considered an impartial reviewer, having come to Giaquinto's work with a substantial commitment to at least one method of analyzing the role of diagrams in mathematics, as well as an understanding of mathematical practice that cordons off psychology. But this does not mean that these are the only reasonable approaches. I have argued that some of the basic presuppositions of Giaquinto's project are problematic, but that does not rule out the possibility that other ways of incorporating psychological data into the philosophy of mathematics will be more fruitful.

And even if Giaquinto's book does not succeed in attaining its stated goals, it succeeds in a more important way. Giaquinto's explorations of various types of visual reasoning in mathematics, and his exploration of geometric, arithmetic, analytic, symbolic, and structural reasoning, show that our capacities for mathematical thought are structured in deep and subtle ways. A variety of disciplinary approaches will be needed if we are to appreciate the subject in all its complexity and grandeur. By challenging us to embrace the complexity and think about how different disciplinary perspectives fit together, Giaquinto has done us a great service, and provided us with a book that is lively, thought-provoking, and enjoyable.

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