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CAN PICTURES PROVE?

IAN DOVE

Abstract
Although historically diagrams have played an influential role in the development of mathematics, for the last hundred years or so, diagrams have been knocked out of their previously important position in favor of verbal/symbolic elements. This is an untoward trend. The arguments against the deployment of diagrams as essential elements in proofs depend on mistaken views regarding the use of diagrams. There are essentially two difficulties for the use of diagrams within proofs. First, there is a problem in drawing a general conclusion from reasoning based on individual instances. Second, the use of diagrams seems to invoke perception in a way that counts against necessity. Answering these two difficulties will further the cause of diagrams.

1. *Introduction*

Late in the *Critique of Pure Reason* Immanuel Kant distinguishes two varieties of mathematical constructions. As examples of one variety, the ostensive constructions, Kant considers the constructions of geometry (and arithmetic). For Kant these constructions are basic.[1] According to Kant the other variety of construction, the symbolic or characteristic construction, is merely a shorthand symbolization of the basic ostensive construction. A symbolic construction depends on the possibility of an ostensive construction for its objective reality. This is the opposite of the current situation. The standard view of the history of mathematics sees the use of figures as a hindrance to

[1] Cf. Lisa Shabel's interesting account of the second-class nature of symbolic construction when compared to the ostensive constructions of geometry. (Shabel *1998*, 609ff.)

progress in proofs. For some, this difference should be understood as a conflict between intuition and rigor. Since diagrams are intuitive[2], they fail to be rigorous. And proofs, in order to be proofs, must be rigorous. The great leap forward in mathematics comes when figures and diagrams are left behind in favor of purely symbolic derivations. What accounts for this change since Kant? The current view of diagrams is that they are wholly inessential elements of proofs. Diagrams may aid in understanding a particular proof, but in this regard they are merely illustrative. It is indisputable that diagrams can be put to this use. And we don't want to imply that diagrams never are used as merely heuristic devices or intuition pumps. They surely are. However, in this paper the argument focuses on the possibility of employing diagrams as part of the actual inferential machinery. That is, diagrams can play a role analogous to (perhaps even equivalent to) the purely symbolic elements of modern mathematics.

Part of the change since Kant's time is the foundational role that arithmetic (really algebraic manipulation) seems to have played in the development of real analysis. Prior to the arithematization of the calculus, begun, perhaps by Cauchy, the inferences within calculus depended upon intuitive interpretation of mathematical concepts by figures or diagrams. But as the discipline evolved, diagrams and figures and figurative geometry generally played a less significant role. Giovanni Ferraro, e.g., makes the following claim:

> During the eighteenth century a process of degeometrization of calculus took place, which consisted in the rejection of the use of diagrams and in considering calculus an 'intellectual' system where deduction was merely linguistic and mediated. (Ferraro *2001*, 535)

Ferraro is keen to show that the arithematization did not take place all at once and that instead vestiges of figural geometry remained and informed the move to analysis. However, the history of analysis suggests that the process was completed and that figures and diagrams are no longer needed in proofs. Figures and diagrams survive as heuristic devices that aid in understanding proofs. They do not constitute any of the actual inferential machinery of proofs, or so the standard view seems to be.

Against this view several philosophers have sought to reinstate diagrams, figures and even pictures into their formerly vaunted position (Cf. (Brown *1997*), (Giaquinto *1994*), (Giaquinto *1993a*), (Giaquinto *1993b*) and (Giaquinto *1992*)). James R. Brown, for example, thinks that diagrams allow humans to see, if only fallibly, into a realm of mathematical objects. Brown puts it more poetically by calling the diagrams "windows to Plato's heaven."

[2] We attach no special Kantian significance to the word "intuition," as we don't think this view is aimed at Kant in particular, though surely Kant is sometimes the target of this critique.

(Brown *1997*, 174) For Brown, though, the criticisms of the use of diagrams are correct; however diagrams facilitate a kind of understanding of the mathematical realm. Thus, mathematicians are correct in deploying diagrams in *proofs*, as diagrams reliably lead to correct results (though they never seem to get beyond the realm of mere evidence for Brown). Marcus Giaquinto, on the other hand, sees diagrams and figures as having unlimited employment in geometry and arithmetic while they have only limited application in calculus or analysis. The view of the present paper is that neither of these positions is quite right, though both go a long way towards reestablishing diagrammatic reasoning as legitimate within mathematics.

These views do not go far enough, however. On the one hand Brown concedes too much to the critic of diagrammatic reasoning, though he does this not out of a failure to appreciate the arguments. Instead, Brown has a fallibilist view of proofs generally, i.e., he is willing to see even verbal/symbolic proofs as providing evidence. Thus, pictures, diagrams and figures fit well within this view of proof. Unfortunately, it is unlikely that Brown's view of proofs will convince the motivated skeptic. Thus, Brown's gains come at the cost of great revision to what we may take as the received view of mathematical practice. Normally a proof provides more than mere evidence of the truth of a theorem. Rather, a proof guarantees the truth or shows the necessity of the theorem. Mere evidence is too weak of a notion to capture the standard view of proof. Furthermore, Brown's examples of diagrammatic failure are unconvincing, even to the mathematically naïve.

Giaquinto, on the other hand, is very careful to distinguish what he calls "evidential" uses of diagrams from their appropriate deployment in the discovery of significant theorems. Against the standard pitfalls involved in the use of diagrams in mathematical reasoning, Giaquinto shows how what he calls "visual methods" are appropriately deployed in both arithmetic and geometry. Furthermore he shows that these visual methods can be used with limited success in elementary real analysis (or calculus). The reason that visual methods fail to have unlimited application in analysis is that analysis requires the concept of infinity, e.g., in the concept of continuity. Visual methods cannot model infinite processes adequately. Thus, for Giaquinto at least, visual methods can be used so long as one is careful to avoid drawing conclusions regarding infinite processes. Of this result Giaquinto seems correct. However, his analysis of the possible hazards of invoking visual methods in analysis is not entirely correct. We show that there are ways of avoiding some of the risks Giaquinto finds lurking in visual methods.

2. *The Formalist Objection*

The standard complaint against using diagrams in proofs is that if the proof depends on the diagram, the resulting inference will lack the necessity usually associated with proofs. One finds, for example, the following criticism given by Niel Tennant (Tennant *1986*).

> [The diagram] is only an heuristic to prompt certain trains of inference; ... it is dispensable as a proof-theoretic device; ... indeed, ... it has no proper place in the proof as such. For the proof is a syntactic object consisting only of sentences arranged in a finite, inspectable array. (Tennant *1986*, 304, quoted in Shin *1994*, 2, emphasis added)

This criticism seems to have three related components. First, diagrams are merely heuristic, i.e., the diagram can serve no greater purpose than that of intuition pump. A diagram may help one to understand a given inference, but it doesn't help to actually draw the inference. Put another way, the diagram isn't really a part of the proof as much as it is part of an explanation of the proof. Secondly, the diagrams are formally dispensable. The proofs work without any associated diagrams. Take away an associated diagram from a modern formal proof and the proof still works. Lastly, since proofs are (wholly) syntactic objects and diagrams aren't, diagrams can't be elements of real proofs. These three elements are part of what we will call the "formal objection." Although we shall spell out the components of this objection in more detail below, the basic problem for diagrams is that they can't impart necessity or guarantee truth in the way that purely formal or wholly symbolic reasoning can. We consider two components to the formalist objection. First, there is a problem of generalization. Since diagrams are individual instances of mathematical concepts it is unclear how we could draw general or universal conclusions from reasoning based on these. A second problem comes from the way that diagrams contribute to the understanding. The standard view is that the properties of diagrams are read off the diagram in an empirical way. That is, if you want to know something about a diagram you simply look at it. The employment of an empirical process counts against the necessity of the mathematical claim. We shall deal with these problems separately.

2.1. *The Problem of Generalization and Individual Instances*

In logic, for example, one can draw a general conclusion from reasoning based upon individual instances just in case the individuals are properly restricted. Depending on the system under consideration the restrictions are spelled out in terms of new constants or flagged individuals. If one can derive a formula, "Pa," containing a constant "a," where "a" doesn't appear in any open assumptions[3], then one can conclude $(\forall x)Px$ where every instance of "a" is replaced by "x." Without these restrictions in place, the worry is that one would make hasty generalizations. For example, without this restriction one could infer, "everyone is a baseball player," from the fact that "Pete Rose is a baseball player." The problem for this inference is that Pete Rose isn't a properly restricted individual. In fact, Pete Rose has properties that make him different from the population at large. As such, we shouldn't be able to generalize from the case of Pete Rose.

How can we stop hasty generalization in cases that involve diagrams? Put another way this is the worry that we can never tell when a given inference is justified in cases that involve diagrams because there is no way to distinguish the essential from the accidental qualities of a diagram. Jody Azzouni (forthcoming), e.g., points out that not all properties of figures are relevant to a given proof. Hence, we need a way of distinguishing the relevant from irrelevant elements.

A set of related developments require comment here. Logic diagrams, in the form of Venn Diagrams, Euler Circles and Line Diagrams, have held a recent vogue. For example, Eric Hammer (Hammer *1994*) has spelled out a formal system that includes both verbal/symbolic elements as well as diagrammatic elements. His formalism employs Venn-like Diagrams as elements and gives rules for inferring from both sentences (in appropriate form) and diagrams. Thus, he is able to do precisely what is claimed not to be possible if the formal objection is correct. Unfortunately, we cannot appeal to Hammer's results. This is so because the system he develops cannot be generalized to mathematical diagrams, though his system does represent a counterexample to the formal objection to logic diagrams.

Hammer's system begins with what he calls "well-formed representations." The verbal/symbolic elements of representation are the usual logical elements that make up well-formed formulas. In addition to these representations Hammer includes the notion of a "well-formed diagram." The primitive elements of a diagrammatic representation are a "rectangle," "closed curve," "shading," "line" and "x" as shown below in Figure 1 (Hammer *1994*, 75).

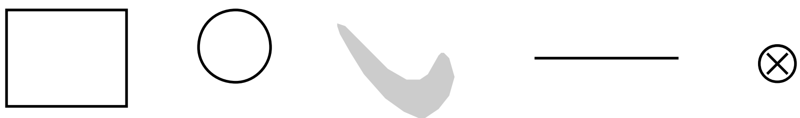


Figure 1

[3] An open assumption is one that hasn't been discharged by a discharging rule like *Conditional Proof* or *reductio ad absurdum*.

A well-formed representation is any well-formed formula (wff) defined in the usual way or any well-formed diagram. A diagram is well formed if it conforms to the following conditions.

Definition 2.1: *The set of "well-formed diagrams" (wfds) is the smallest class satisfying the following four conditions:*

1. *Any rectangle is a well-formed diagram (wfd).*
2. *If D is a wfd and C is a closed curve labeled by exactly one set term not occurring in D, the diagram obtained by adding C to D such that C intersects each enclosed region of D exactly once, and that it overlaps only part of each enclosed region, is a wfd.*
3. *If D is a wfd and b is any constant symbol, then the diagram obtained by adding either a b-sequence or an x-sequence to D is a wfd, provided that every link of the sequence falls entirely within the rectangle and does not contact any border of a closed curve of D.*
4. *If D is a wfd, then the diagram obtained by shading some enclosed area of D is a wfd, provided that the shading is entirely bounded by parts of closed curves and the rectangle. (Hammer 1994, 75–76)*

An x-sequence is a finite chain of $\otimes$'s connected by lines and a b-sequence is a finite chain of b's connected by lines (Hammer *1994*, 75). From these definitions we can relate the usual semantic notions of a formal language to this system. The rectangle represents a Domain.[4] The closed curves represent predicates (the extensions of sets). The $\otimes$'s represent variables. In a diagram the region of overlap of two closed curves represents the intersection of those predicates (the intersection of the extensions of the given sets). If a region is shaded it is empty. If a region is not shaded it is possibly non-empty. An $\otimes$ in a region represents that region's non-emptiness. And a b in a region represents that b is an element of the predicate(s) of the region.

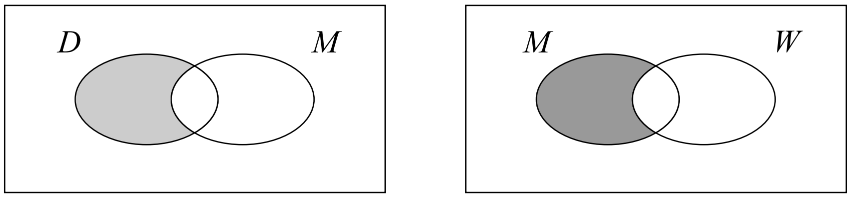
From this Hammer is able to give a system within which one can infer a well-formed diagram from another well-formed diagram, a well-formed formula from another well-formed formula, a well-formed formula from a well-formed diagram and a well-formed diagram from a well-formed formula. Each of these inferences is completely syntactically specified so it answers the formal objection. Furthermore, the relations between wff's and wfd's can be made explicit. One can represent the same *propositions* in two different modes: one as a formula the other as a diagram. For instance one

[4] Here we diverge from Hammer's actual presentation. For Hammer, it seems, the rectangle can stand for a set, property or predicate. We take the rectangle to represent the extension of the domain.

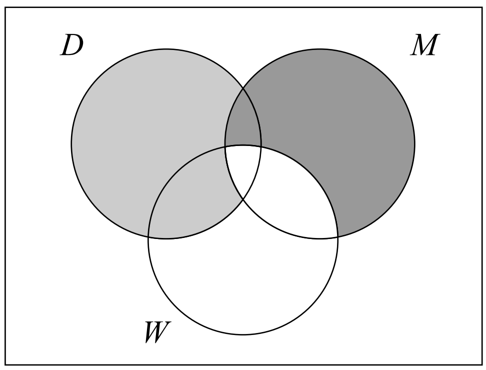
could symbolize the proposition, "all dogs are mammals," and the proposition, "all mammals are warm-blooded," as follows.

(1) $\forall x(Dx \supset Mx)$
(2) $\forall y(My \supset Wy)$

To represent the same propositions in diagrams we simply use a rectangle for the domain and closed curves for each of the predicates.



To show that all dogs are mammals one must eliminate all dogs from the diagram that fall outside of the extension of the set of mammals. This is accomplished by shading. We shade the region where non-mammalian dogs would be in order to show that the region is empty (See Figure 2a). In these wff's the domain is the universe and the predicates are the obvious ones as is the case with the wfd's.



From figure 2c one can infer the wff:

(3) $\forall z(Dz \supset Wz)$

This is possible because although it is a diagram, it represents the content of proposition (3). Thus, if propositions (1–2) are true, then, (3) has to be true. In this case the diagrammatic reasoning preserves truth and validity.

This system, however, doesn't help in the case of mathematical proofs generally. The problem is that within this Venn-like logical system, the diagrams have clearly stated syntactical rules for their construction (the well-formed diagram rules). Moreover there are explicit rules that allow inferences between diagrams and formulas (and vice versa).[5] The syntactic units of the diagrams are regions and these regions behave like digital representations as opposed to analog representations.[6] The main difference between an analog and a digital representation is that for analog representations and not for digital representations every difference can make a difference[7] (Cf. Goodman *1976*, 164ff.). The diagrams are finite-state representations where the basic element is a region. Within a region there are only a finite number of different states it can be in. And, depending on the state of the region, different relations to other regions obtain. So, for example, it doesn't matter where in a given region an $\otimes$ occurs. The difference between the upper-right of a region and the lower-left of a region make no difference to the state of the diagram. What's more, the state of the diagram is "read off" from the diagram.

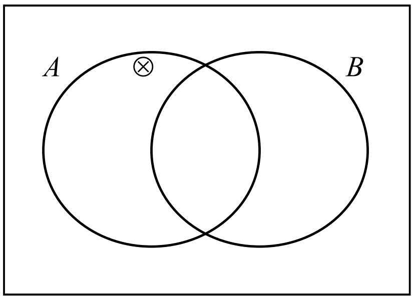


Figure 3a

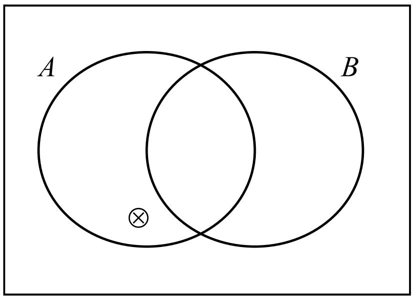


Figure 3b

The diagrams in figures 3a and 3b are equivalent as both represent the content of the symbolic sentence $(\exists x)Ax$, i.e., "something is an A." This is known because their regions are equivalent. And this equivalence isn't affected by the location of an $\otimes$ within a region. In both of these cases the regions corresponding to the A's that aren't B's aren't empty, regardless of where the $\otimes$'s are.

This simply isn't the case with typical mathematical diagrams. Nevertheless, in what follows we will make a case for employing diagrams within mathematical proofs that avoids the formalist objection. Though we can't

[5] It is the existence of these rules that makes the system "heterogeneous," in Hammer's terminology. (Cf. Hammer *1994*, 74)

[6] This terminology is from Nelson Goodman (Goodman *1976*, 159–164).

[7] This explanation is due to Rachel Zuckert (private communication).

appeal to the particular system developed by Hammer[8] we can use the insight into what makes these diagrams acceptable to inform the account of diagrams we will pursue. The basic idea will be that though the employment of diagrams in mathematics does (superficially) appeal to individuals, this reasoning is properly general because the individuals are arbitrary. This means that the conclusions based on the diagrams will be appropriately necessary. Consider, for example, the proof that the perpendicular bisector of the base of an isosceles triangle will also bisect the summit angle. Start with an isosceles triangle, $\triangle ABC$.

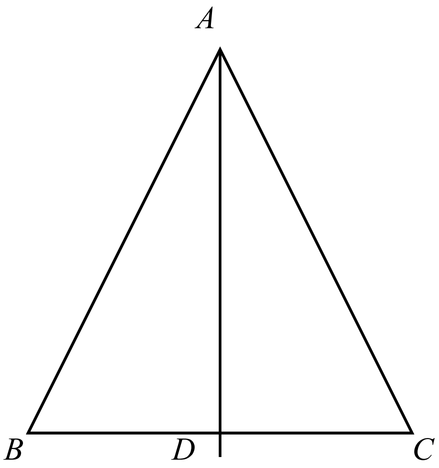


Figure 4

Construct the perpendicular bisector to the base BC from point D to the summit angle A. Since $\triangle ABC$ is isosceles, $\angle B = \angle C$. Moreover, $\angle CDA = \angle BDA$ since they are both $\perp$. And, $CD = BD$ because D is the bisector of CB. Then triangle $ADC = ADB$ by angle-side-angle (ASA). Since the triangles are congruent, DA bisects $\angle A$.

How can we be sure that there isn't something about this particular figure that acts like Pete Rose did in the fallacious inference from the fact that Pete is a baseball player to the false conclusion that everyone is? We answer this question in section 3 below.

2.2. *The Problem of "Reading" a Diagram*

Besides the problem of generalization, there is another problem that warns against using diagrams in proofs. The problem is that the use of diagrams

[8] Or any of the related systems developed by, e.g., Shin (Shin *1994*).

seems to invoke perception in a way that counts against necessity. Since proofs are meant to establish necessary results, the use of an empirical method would count against the supposed necessity of the conclusions. The problem can be stated rather simply. Although a diagram is meant to represent mathematical relations, our knowledge of these representations is perceptual and the information we glean from the diagram is perceptual knowledge. Perception is fallible. Thus, no proofs can depend on diagrams.

We see this view espoused even by friends of diagrams. For example, James R. Brown, in discussion the virtues of a diagrammatic proof of the Intermediate Zero Theorem thinks that the picture plays a perceptual role in giving evidence for the truth of the theorem.

> I should add that the way the picture works is much like a direct perception: it is not some sort of encoded argument. (Brown *1997*, 166 *emphasis added*).

What is the key about Brown’s statement is that one doesn’t really reason from the picture or diagram. Instead, one perceives some feature that is represented in the picture or diagram. One may “interpret” various elements of the picture in the process of “seeing” the conclusion, but this is a very different method, or so it seems to Brown, from reasoning with purely verbal/symbolic elements.

David Tall, although a fan of using “visualization” in teaching mathematics, considers this method substandard as regards proof.

> Visualization has its distinctive downside. The problem is that pictures can often suggest false theorems. (Tall *1991*, 106)

The problem it seems that any appeal to a “visual technique” or a “visual object” will open the door to fallibility.

To see that this view of diagrams can lead to error, consider the following example (given by (Brown *1997*, 162ff), (Tall *1991*, 106) and (Giaquinto *1994*, 798ff.). The Intermediate Zero Theorem states that if a continuous function defined on a closed interval $[a, b]$ has a value of $f(a) > 0$ and $f(b) < 0$, then there is a point, c, between points a and b such that $f(c) = 0$.

> For instance, it was considered satisfactory to give a visual proof of the intermediate value theorem[.] The curve was considered as a ‘continuous thread’ so that if it is negative somewhere and positive somewhere else it must pass through zero somewhere in between. Yet we know that the function $f(x) = x^3 - 2$ defined only on the rational numbers is negative for $x = 1$, and positive for $x = 2$, but there is no *rational* number a for which $f(a) = 0$. Thus visualization skills seem to fail us. Life is hard. (Tall *1991*, 106)

The problem is that we cannot visually distinguish a curve that is continuous from one that is merely dense. Both curves have the same kind of representation in a diagram.

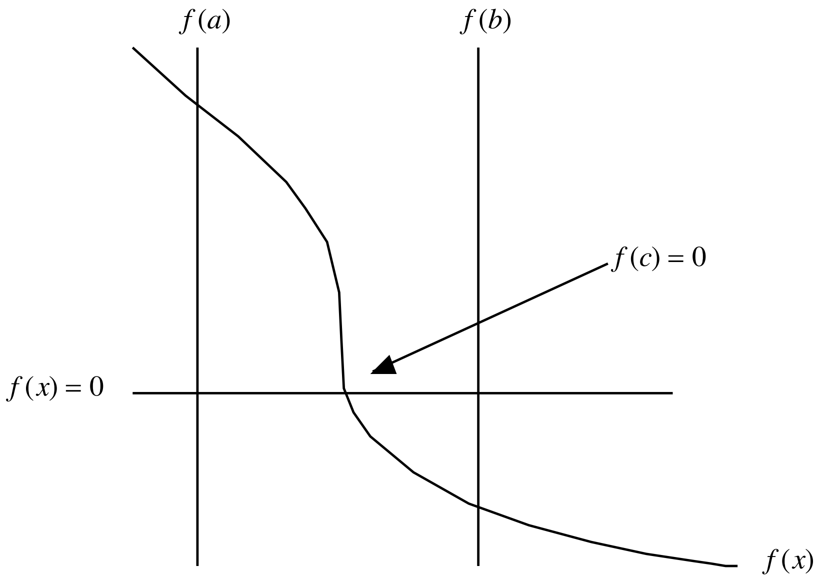


Figure 5

The visual proof of this theorem, is criticized by Giaquinto as well (Cf. Giaquinto *1994*, 799). And even Brown, whose stated goal is to "make a case for pictures having a legitimate role to play as evidence and justification, well beyond a heuristic role" (Brown *1997*, 161), explains the use of this diagram in terms that seem to count against it being a mathematical process.

> Even if the picture merely does psychological work, that in itself could only be explainable by assuming that $\delta - \varepsilon$ continuity and pencil continuity are somehow deeply related. (Brown *1997*, 166)

Brown concedes that the picture, at best, captures a quasi-mathematical concept that falls somewhere short of the rigorous concept of $\delta - \varepsilon$ continuity. He goes on to compare the use of pictures in proofs to tests or experiments in natural science (Brown *1997*, 166). This indicates that for Brown, the use of diagrams, if not wholly empirical, is at least analogous to empirical processes. And this use is therefore subject to the same pitfalls as empirical methodology. We will return to this problem in section 4.

3. *The Arbitrary Instance*

The first step towards making diagrams legitimate in proofs is to make it clear how it is possible to generalize from reasoning based on individual instances. This problem has a long history. For example, Ian Mueller suggests that Greek mathematicians had a sense of the possible difficulties, though he doesn't think that they ever gave an adequate solution.

> It is natural to ask about the legitimacy of such a proof. How can one move from an argument based on a particular example to a general conclusion, from an argument about the straight line AB to a conclusion about any straight line? I do not believe that Greeks ever answered this question satisfactorily, but I suspect that the threefold repetition of what is to be proved reflects a sense of the complexity of the question. (Mueller *1981*, 13)

The "threefold repetition" that Mueller mentions refers to the standard presentation of a geometrical proof, i.e., the proposition to be proved is stated three times. The conclusion to be proved is stated, initially, in general terms. Then, on the basis of an initial (particular) figure the conclusion is drawn in terms of the individual figure. Finally, in the last step of the proof the conclusion is stated in the same general terms as its initial formulation at the beginning of the proof. Mueller sees the Greeks as prescribing a rule or principle.

> Of course, insisting that the particular argument is sufficient to establish the general *protasis* is not a justification, but it does amount to laying down a rule of mathematical proof: to prove a particular case is to count as proving a general proposition. (Mueller *1981*, 13)

By itself this rule is insufficient. The problem is that it doesn't seem to rule out hasty generalizations of the kind mentioned above regarding Pete Rose. To understand the problem let us revisit the logical situation.

Consider the following inference from (4) to (5).

(4) Steve is a human with a heart and two lungs.
(5) So every human has a heart and two lungs.

The problem with the inference is that it is possible, though unlikely, that there are (living) humans with no hearts or less than two lungs. Having a heart and two lungs is inessential for being a human. And simply restating the conclusion three times would not make it true. Since the generalization is meant to cover humans universally, the characteristics that we attribute to them from an inference must not depend on any accidental features of the particular human we investigate.

Formally this possible difficulty is overcome by restricting the individuals within the context of a generalization. But the restrictions are based on an informal principle.

> As is well known, there exist certain informal procedures for arguing to a universal conclusion from an existential premise. We may establish that all objects of a certain kind have a given property by showing that an arbitrary object of that kind has that property[.] (Fine *1985*, 1)

Kit Fine relates the informal procedure to the formal quantification rule of universal generalization. But he is not alone in this.

> The introduction rule for $\forall$ will model the principle that whenever we have deduced an instance of a universal proposition from a set of premises, we can deduce from these premises the universal proposition itself, provided that the instance is arbitrary. This proviso can be spelled out as the demand that the individual with which the universal proposition is instantiated doesn't figure in any of the premises or in the universal proposition itself. (Zalabardo *2000*, 132–133)

This leads to the question of whether the informal or pre-formal principle depends on its formal statement for its truth. If the pre-formal principle depends on the formal rule for its correctness, then unless we can give a formalization of the rules of diagrammatic reasoning in a manner similar to Hammer above, no general results will be justified on the basis of particular figures. And we think that it is unlikely that a formal theory of diagrams is forthcoming. However, it seems that the formal theory is informed by informal or pre-formal principles. For example, suppose that Hilbert's formalization of (Euclidean) geometry lead to the proof of theorems that contradicted the known results of Euclidean geometry. Which theory would yield? It is our assertion that in this case the formal theory would yield. And in this we follow Imre Lakatos (Cf. Lakatos *1976*) in supposing that formal mathematics gets its sense from pre-formal mathematical theories.

Still, we need an account of arbitrary instances so that we can tell the correct generalizations from their hasty and fallacious cousins. In this regards we will follow Kit Fine's presentation in spirit if not in detail. The basic idea, initially, is that an arbitrary instance is one that has all and only the properties of the class it is meant to represent.

> (AI) An instance of a concept is arbitrary if and only if it has all and only those properties common to the class it is meant to represent.

However, this view faces well-known and long-standing difficulties. For example, consider the case of an arbitrary triangle. What are its properties if it has all and only those properties of the class it is meant to represent? Let us suppose that the concept of a triangle is well defined and that it is a plane figure composed of exactly three sides and three angles. Is this arbitrary triangle equilateral? Is it isosceles? The answer must be no, as these are properties that aren't shared by all triangles. But, as soon as we have a given triangle, it will be either equilateral or not. Thus, there can't be an arbitrary triangle, as any given triangle will have *special* properties.[9] The special properties are those that aren't shared by all of the members of the class of triangles.

Instead of attributing all the properties of the intended representative class to the arbitrary objects, we should instead let them represent their class by not appealing to any of their special properties when we deploy them in reasoning.

> (AO) An individual representation is arbitrary if none of its special features are used in drawing an inference from the object.

From this we will make a principle for drawing general conclusions.

> (PP) If a conclusion is reached on the basis of an arbitrary representation, that result obtains for all individuals in the representative class.

This will be easier to see by comparing two examples. In the first example, a figure will be used that seems to be arbitrary, but upon scrutiny it is seen not to be arbitrary. Against this another example is given in which the figure is used as an arbitrary representation. First, consider the following procedure for *demonstrating* that all triangles have interior angles that sum to 180°. First, take a triangle, $\triangle ABC$. Cut off each of its vertices. Arrange the vertices on a plane so that each vertex coincides with a given point P, and the adjoining sides overlap.

This isn't a proof, however. Nor is it a correct generalization, though what it purports to show is a geometrical fact. However, it is difficult to see at which step the procedure fails. On the one hand $\triangle ABC$ is meant to be an arbitrary triangle. But, $\triangle ABC$ does have some special properties.

And we don't want to appeal to any of these properties in drawing our conclusion. To see that we have appealed to some special features of $\triangle ABC$ we must consider precisely what the procedure requires. In this case the procedure is equivalent to measuring the angles and adding them together. The fact that we don't determine the particular measurements isn't important. What is important is that when the angles are summed (or arranged on a plane), they total 180°(i.e., they fit together nicely by forming a straight

[9] A similar argument is given by Fine (Fine *1985*, 9) who attributes it to Berkeley. Fine's reply to the argument is to reject the notion that arbitrary objects must have all and only the properties of their intended representative class. He claims that this argument depends on a false principle of generic attribution.

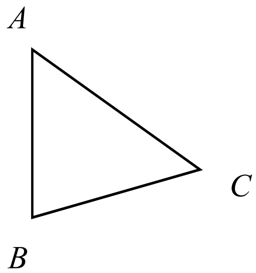


Figure 6a

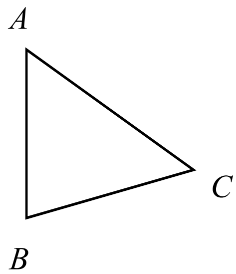


Figure 6b

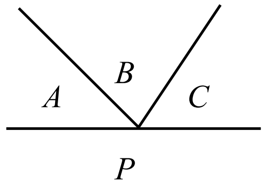


Figure 6c

line). And measuring, regardless of how it is accomplished, depends on the particular (viz. special) features of the given triangle. Thus, the procedure fails the intuitive or pre-formal generalization principle.

The usual Euclidean proof, on the other hand, makes essential use of an arbitrary triangle. This use doesn't appeal to the actual measures of the given angles in the representative figure. Take an arbitrary triangle (Figure 7a). Then extend the base. Construct a line parallel to one side adjacent to the extended base (Figure 7b). In this case we have labeled the angles to make the relations clear. To arrive at these angles one appeals to the rules for alternate interior angles for the summit angle and its mirror, and to adjacent angles to for the left-hand base angle. Clearly, the right-hand base angle is equal to itself. The conclusion is drawn when one notices that the sum of the interior angles is equivalent to the sum of the newly constructed angles. Furthermore, the sum of the newly created angles (plus the right-hand base angle) is the angle of a straight line, i.e. 180°. Since the proof doesn't depend on the angles having any particular measurement, one is correct is generalizing this procedure to any triangle whatsoever. Thus, the sum of the interior angles of any (Euclidean) triangle is 180°.

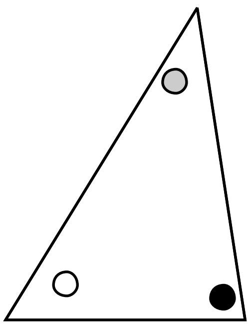


Figure 7a

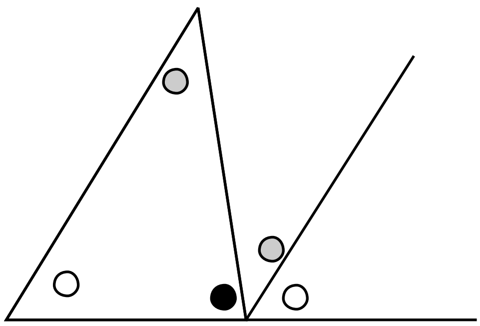


Figure 7b

The proof is better than the measurement procedure because the proof isn't inductive.[10] So, though by cutting and arranging a variety of differently angled triangles one may improve one's subjective confidence in the proposition that all (Euclidean) triangles have interior angles that sum to 180°, this is simply subjective confidence not proof. The proof on the other hand doesn't depend on the particular measure of any of the angles. Instead, it made use of the fact that the triangle had three angles as well as some auxiliary geometrical notions. The triangle had special characteristics, but none of these played a role in the subsequent proof. Thus we are justified in drawing the general conclusion because we could have applied the same procedure to any such triangle.

It is tempting to think that the pre-formal principle regarding generalization depends on its formal counterpart for its validity. However, it is clear that as far back as Proclus, who died in 485, that the principle was explicitly used to justify general claims.

> [M]athematicians are accustomed to draw what is in a way a double conclusion. For when they have shown something to be true of the given figure, they infer that it is true in general, going from the particular to the universal conclusion. Because they do not make use of the particular qualities of the subjects ... they consider that what they infer about the given angle or straight line can be identically asserted for every similar case. (Proclus *1970*, 162)

He goes on to explain that the figures set out in the proof aren't really individuals. Instead, they are representatives of the class to which they belong (Loc. Cit.). As representatives we must ignore any of their special properties in drawing a conclusion from them, as this would negate their status as arbitrary or representative figures. The fact that Proclus explicitly gives this principle suggests that such conclusions are not as mysterious as Mueller believed (see above). That formal presentations of geometry (and logic for that matter) incorporate formal analogs of the principle (PP) suggests that the principle is both plausible and true. Thus, it seems that we have answered the first problem regarding the employment of diagrams in proofs, viz. we have shown how to draw general conclusions from the use of specific of individual diagrams.

On the other hand, there seem to be cases where the reasoning is correctly general at least in terms of our pre-formal principle (PP), but fallacious nonetheless. E. A. Maxwell (Maxwell *1959*, 13–14) gives a standard example purporting to show this is more than a mere possibility. Maxwell gives a proof that every triangle is isosceles. Furthermore, his reasoning seems to

[10] Nor is the proof an empirical method, though we will have more to say about this below.

accord with the canons of rigor established in this section. Start with a triangle, $\triangle ABC$ (see Figure 8). This is supposed to be an arbitrary triangle. To show that this triangle is isosceles we must show that the sides $AB = AC$. To do this we will make some auxiliary constructions as well as appealing to auxiliary geometrical notions. Construct the bisector of $\angle A$ so that it meets the perpendicular bisector of BC at a point O. Construct the perpendicular bisectors of AB and AC so that they meet at point O. Construct the bisector of $\angle B$ to point O and the bisector of $\angle C$ to O.

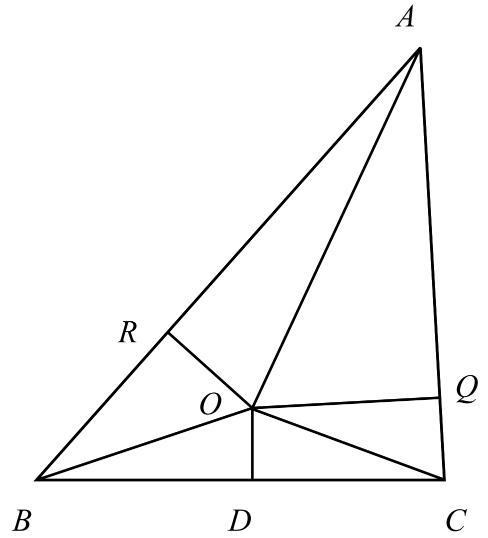


Figure 8

The proof proceeds by showing that the internal constitutive triangles are congruent. From this we can conclude that the sides $AB = AC$. Start with the base triangles BDO and CDO. We can show that they are congruent. First, $OD = OD$. This is a side of both triangles. Secondly, $\angle ODC = \angle ODB$ because OD is the perpendicular bisector of BC. Thirdly, the sides $BD = DC$ because point D bisects BC. Thus, from the side-angle-side property (SAS), $\triangle ODB = \triangle ODC$. Next we need to show that summit triangles are congruent, $\triangle AOR = \triangle AOQ$. First, $OA = OA$ and the triangles share that side. Secondly, $\angle OAR = \angle OAQ$ because AO bisects $\angle A$. Next, $\angle ORA = \angle OQA$ because both of these angles are the result of constructing the perpendicular bisectors of the given sides. So, by the angle-angle-side property (AAS), $\triangle AOR = \triangle AOQ$. Next we need to show that the interior triangles are congruent, $\triangle BRO = \triangle CQO$. We know that $BO = CO$ from the congruence of the base triangles. And we know that $RO = QO$ by the congruence of the summit triangles. And we know that $\angle OQC = \angle ORB$

because both are right angles (by construction). So, the interior triangles are congruent. So, $AC = AB$. So $\triangle ABC$ is isosceles. We know that the conclusion is false, so we know that something is wrong with the proof. The question is whether what is wrong with the proof is the geometrical method we are using or something else.

Maxwell's own analysis of what has gone wrong in this proof is illuminating though complex. The first thing to note about the proof regards the accuracy of the figure. This is precisely why Felix Klein (Quoted in Mueller *1981*, 5) thought that such "geometrical sophisms" were real dangers to learning geometry. We shall have more to say about how figures represent mathematical concepts below. For now we should focus on the notion of an internal bisector to $\angle A$ and the perpendicular bisector to BC. Where will these two lines meet? In the proof above these lines met *inside* $\triangle ABC$ at point O. This fact is essential for the proof to work. For, if point O were to lie outside of $\triangle ABC$, we wouldn't be able to construct the component triangles. And, if we can't construct the component triangles, we won't be able to establish their congruence. Furthermore, without the congruence relations between the components, we won't be able to establish the equality of the sides AB and AC. To see that point O need not occur inside of the triangle let us construct a different triangle (Figure 9) to demonstrate this. This is sufficient to show that the previous proof depended upon a special property of $\triangle ABC$. Thus, $\triangle ABC$ isn't arbitrary (at least it wasn't deployed in an arbitrary or representative way). Hence, we cannot draw a general conclusion on the basis of this reasoning.

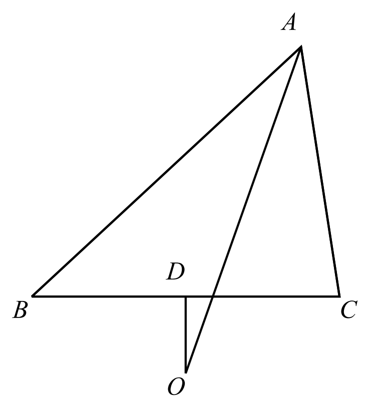


Figure 9

4. *Taking Vision out of Visual Methods*

Maxwell, on the other hand, thinks that the flaw could not be discovered by purely geometrical considerations. This is so, according to Maxwell, because the concept, "inside figure F," isn't defined in geometry. To discover the position of a given point relative to a given figure, one simply looks. Such empirical considerations are too unreliable to be mathematical, or so Maxwell complains. Against this view, let us review how we determined that the former proof depended on a special property of a given figure. We didn't just look at the figure. Instead, we considered possible permutations of the figure to see whether the necessary auxiliary constructions were in fact possible. They weren't, so the proof failed.

This isn't enough for Maxwell, however, as he thinks other proofs rely on the kinds of empirical considerations he questions regarding inside/outside figures. If we can't rely on purely geometrical considerations in drawing our conclusions, then the results will not be necessary. Consider the proof of the theorem that the exterior angle of a triangle is greater than the interior opposite angle (Maxwell *1959*, 20ff.). Start with an arbitrary triangle, $\triangle ABC$. Extend the base, BC to a point P (see Figure 10). To prove the theorem we must show that $\angle PCA > \angle BAC$. Let O be the midpoint of AC and construct BD through O so that $OD = BO$. By stipulation, $AO = OC$ and $OB = OD$. Thus, $\angle AOB = \angle COD$ because they are vertically opposite. Hence, $\triangle AOB \equiv \triangle COD$ (SAS). Thus, $\angle BAO = \angle DCO$ because they are relevant angles of congruent triangles. So, $\angle BAC = \angle DCA$. But, since $\angle DCA$ is a component of $\angle PCA$, $\angle PCA > \angle DCA$. But, if $\angle DCA = \angle BAC$ and $\angle PCA > \angle DCA$, then $\angle PCA > \angle BAC$. Thus, $\angle PCA > \angle BAC$.

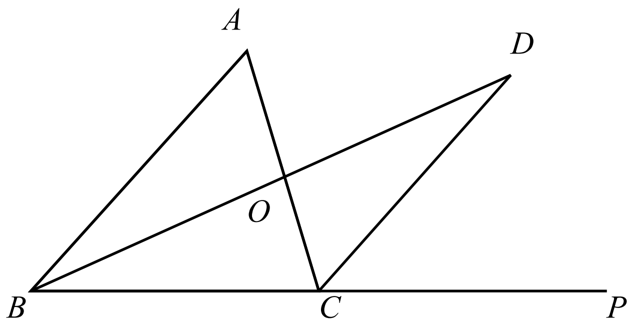


Figure 10

Maxwell finds fault in the proof because there is no geometrical way of determining which of $\angle PCA$ and $\angle DCA$ is the component and which is the whole.

> The only reason for selecting $\angle PCA$ as the whole is that it 'looks like it'; but whether it would continue to do so for a triangle of atomic or astronomic dimensions is a very different matter. (Maxwell *1959*, 22)

This calls into question the arbitrariness of $\triangle ABC$. And since Maxwell understands the phrase "it looks like it," as an empirical method, he thinks that the proof fails. But this is simply to misunderstand what it means for the triangle to be arbitrary. That is, an arbitrary figure will represent the structural relations generically. And as a generic representation, the relations are necessary and could not be determined empirically. One doesn't inspect the figure empirically to determine its properties.

We have already seen that Brown, Hall, and Giaquinto all find the visual proof of the Intermediate Zero Theorem flawed. The problem, recall, was that when the mathematical concept that is represented in the figure deals explicitly or implicitly with the concept of infinity, our human perceptual abilities are not up to the task of visual discrimination. Thus, the concept of continuity is beyond our abilities to discriminate. As such, this objection is unobjectionable. That is, it is true that if a person is given two curves, one which is really continuous (as defined on the real numbers), and another which is merely dense (as defined of just the rational numbers), such a person would not be able to discover which curve was which.

The problem for the criticism is that it doesn't seem to make sense of the actual practice of proof. It is not as if one has, as it were, a box full of curves from which a particular curve is picked out and deployed in a proof. For, if this were the case, then there would be real danger that the proof relied on the special, but indiscernible, features of the curve. But this is not how representations are given in proofs. If a figure is given, its properties are initially drawn from the concept of which it is meant to be an instance. So, e.g., if the figure under consideration is a triangle, we give a triangle by giving a figure within which the structural features of a triangle are represented. This is a process that is very similar to the logical process of Universal Instantiation (UI). In a standard employment of the UI rule, one goes from a universal proposition to a particular proposition. For example, from the proposition, "All swans descended from dinosaurs," one could draw the conclusion, "Bill's swan 'Daffy' descended from dinosaurs." Yet, the rule UI is not a perfect analog of the initial representation of a concept in a figure. The problem is that the figure is in many cases not really an instance of the concept it represents. Take the simple instance of a straight line. If Euclid's definition is considered the concept of a straight line, then any line drawn on paper, or even imagined in thought, will fail to have some of the properties of a straight line, e.g., the property of being a breadthless width. But this doesn't matter for the representation. For, in cases where the representation isn't really an instance of the concept, we can stipulate the properties of the figure. And in the process of stipulation, we put the properties into the figures. So there are two ways that the figures are given their properties. First, they instantiate or represent a given mathematical concept. Secondly, the properties are stipulated. This is reminiscent of Kant's view of mathematical method.

> The true method, so he found, was not to inspect what he discerned either in the figures or in the bare concept of it, and from this, as it were, to read off its properties; but to bring out what was necessarily implied in the concepts that he had himself formed *a priori*, and had put into the figure in the construction by which he presented it to himself. (Kant, bxii)

For Kant, of course, the figures amplified the concepts as they facilitated inferences that could not be made without the aid of the figures. We won't pursue this use of figures here. Instead, insofar as the figures are constructed in accordance with mathematical concepts, we will assert that they play a role similar to verbal/symbolic elements.

Even if we are correct about the use of diagrams, it isn't clear how this view will make sense of the use of diagrams in proofs. In a well-received book on Kant and the philosophy of mathematics, Michael Friedman argues that Kant's figurative geometrical methods were open to a variety of modern objections. One of the more curious objections concerns the very first proposition of the first book of Euclid's *Elements*. And this objection is relevant to, and indeed is similar to a number of objections raised against the use of diagrams in proofs. Euclid's first proposition states that from any given line it is possible to construct an equilateral triangle that has that line as its base. The proof is fairly easy. Start with a line, AB. Then, construct a circle with radius AB centered at A, call this circle A. Then construct a circle with radius BA centered at B, call this circle B. Where circle A intersects with circle B, label that point C. Finally construct lines AC and BC.

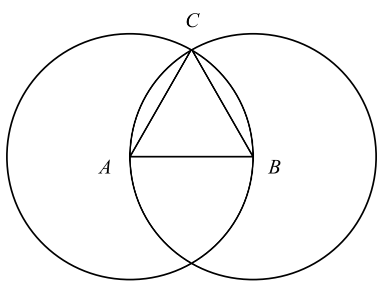


Figure 11

This figure is an equilateral triangle because line AC is the same length as line AB because they are both radii of circle A. Likewise, line BC is the same length as line AB because both are radii of circle B. The problem, according to Friedman, is that it is possible to interpret this proof along with its associated diagram on a plane constituted solely of rational numbers. Why would this be a problem? If AB is a unit length, then point C could be irrational. This would mean that circles A and B wouldn't really intersect even though they look as if they do. And, what else do we have to go on?

The problem is analogous to an earlier criticism of the visual proof of the Intermediate Zero Theorem (See Figure 5 above). Recall that Tall and Giaquinto argued against the visual route to the theorem because of the possibility that a given curve may be dense, but not continuous. Furthermore, the difference between these possibilities cannot be determined visually. But, as was stated above, the curves aren't entities in their own right. Instead, they are mediated objects whose properties are stipulated or constructed in accordance with mathematical concepts. And this negates the worry that something is wrong with the visual proof of IZT. The very statement of the theorem tells us that the only functions we are going to consider in the proof are continuous ones. Thus, a merely dense curve isn't even in the purview of the theorem, and we simply stipulate that the curve we use is continuous. This is roughly the same answer that should be given to Friedman's objection, though in the case of Euclidean geometry the assumption of continuity was tacit for much of its history. But, once the assumption is made clear, then the counterexample fails.

Giaquinto has another example that he finds troubling. Consider a figurative proof of Rolle's Theorem.

(RT) If $f(x)$ is continuous on $[a, b]$ and differentiable on (a, b) and $f(a) = f(b)$, then for some c between a and b, $f'(c) = 0$.

The proof of the theorem begins with a representation of a curve.

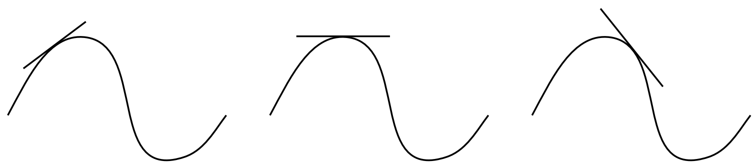


Figure 12a Figure 12b Figure 12c

There are two cases that may be distinguished; though they make no difference for the proof. The function $f(x)$ is either increasing or decreasing at a. Since this makes no difference to the proof because the cases are relevantly similar, we will only consider the case where $f(x)$ is increasing at a. We can represent this by a curve that has the shape of a hill. The derivative of a function at a point is the slope of the tangent line at that point. Thus, the theorem states that there must be a point, c, at which the slope of the tangent line is zero, i.e., it is a horizontal line at c. The proof works by considering the tangent line at a and the tangent line at b (where the function is decreasing), and seeing that there must be a point where the line becomes horizontal as it "rolls" over the top of the hill.

According to Giaquinto this proof falls victim to a number of telling objections. The first is that the hilltop image may be seriously misleading.

> From the supposition that the curve rises and smoothly falls again over $[a, b]$ it was inferred that the gradient of the tangent to the curve must change gradually from positive to negative at least once, as would happen if a segment of the curve had the shape of a path over a smooth hilltop. The hilltop image is then relied on in the ensuing instructions to visualize. But this overlooks the serious question. How do we know that the hilltop image is not misleading? How do we know that the curve has an upper bound? (Giaquinto *1994*, 795–796)

The problem is that even if we can't think of what a curve would look like that was locally unbounded but continuous, that doesn't guarantee that such a curve is impossible. Giaquinto switches between talk of functions and talk of curves without much bother, but this is where the crux of the argument lies. For, in view of the fact that the theorem regards functions and not curves, the only reason we consider a curve is as a (figurative) representation of a function. Now, can there be a continuous function that is locally unbounded? If this is possible, it isn't for the cases that are relevant to Rolle's Theorem, as the statement of Rolle's Theorem tells us that the function is differentiable over the interval. Since it is differentiable and the derivative on one side of c is positive and on the other is negative, the only way to get from positive to negative is to pass through zero. Therefore, the hilltop image is not misleading as regards local maximum (or minimum if $f'(a) < 0$).

A second worry raised by Giaquinto is that the curve is misleading in another way. To see what may be wrong consider a different curve.

> The curve of a function is smooth and appears to have a single peak; closer examination of the peak reveals a small shallow dip, so that now this top segment of the curve appears to have two mounds separated by the little dip; yet closer examination of the tops of the two mounds and the bottom of the dip reveals further fluctuation, each mound again having a dip but shallower than before, and the first dip having a gentle mound separating to very shallow dips; let this be repeated unendingly, ever closer examination revealing ever shallower dips in the new mounds, ever gentler mounds in the new dips. (Giaquinto *1994*, 796 *emphasis added*)

Such a curve will fluctuate infinitely between positive an negative values, but, since it is continuous and smooth it seems to accord with the conditions of Rolle's Theorem. But, we can't visualize the infinite fluctuation. Note that Giaquinto talks about "closer examination" of the figure. So, we can't be sure whether Rolle's Theorem holds of such a function.

This objection is easily met. There are two possibilities. First, the infinite fluctuation may mean that the function is not differentiable at points within the range. If this is the case, then the function doesn't meet the criteria set for Rolle's Theorem. On the other hand, if the function is differentiable over the range, even though it fluctuates infinitely, then we can represent the curve with the hilltop image. This is so because, although it fluctuates infinitely, there is a derivative at every point, so there is a tangent line at every point. Thus, though we can't see the tiny regions of fluctuations, we don't need to. Again, the point of the figure isn't to give us an empirical object to manipulate. Instead, we get a kind of conceptual object to manipulate, i.e., an arbitrary representative of the class.

Giaquinto's third worry regards how we understand the visual proof in cases where the hilltop image is appropriate. He worries that in seeing the rod assume the horizontal position, we might not be able to discern the difference between a slope of zero and a slope that differs from zero by a tiny amount. Again Giaquinto's worry in unfounded as we don't rely on visual inspection to discover the properties of the figures involved. Instead, we have already stipulated that the curves are both continuous and differentiable everywhere along a given interval. Since the curve is continuous on the interval, there are no gaps. Since the curve is everywhere differentiable along the interval, it has a tangent line. And, since the tangent is positive on one side of a point c and negative on the other, the tangent has to have a zero slope at some point. The only way for this to fail is for the curve to be discontinuous. But this possibility is ruled out by the conditions of the theorem.

All of the worries are satisfied when we realize that vision doesn't play a role in the determination of the properties of the figures. At least vision plays no more of a role than it does with wholly symbolic elements.

5. *A Spurious Counterexample*

Before the conclusion we must consider a purported example of how diagrams can lead to false theorems. Brown says that this worry is real.

> Philosophers and mathematicians have long worried about diagrams in mathematical reasoning — and rightly so, they can indeed be highly misleading. Anyone who has studied mathematics in the

> usual way has seen lots of examples that fly in the face of reasonable expectations. (Brown *1997*, 178)

As an example of a "highly misleading" use of diagrams Brown gives the following. Take four unit circles centered at $(\pm 1, \pm 1)$. Construct a circle centered at $(0, 0)$ so that it touches each of the other circles just once. Now construct a square around the unit circles. From this diagram we can see that the interior circle is wholly contained by the square.

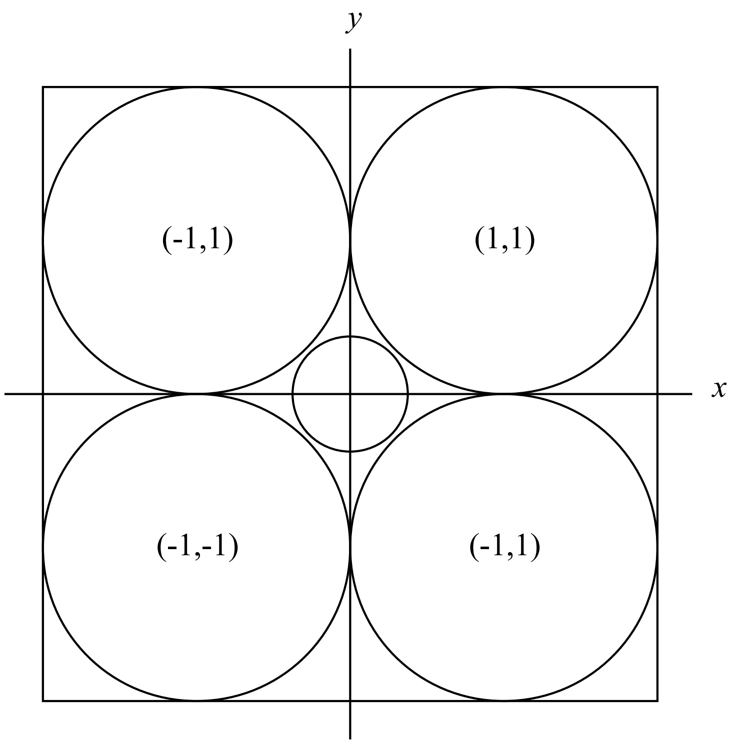


Figure 13a

Since the result holds in two dimensions, try it in three dimensions (see Figure 13b). Take eight unit spheres centered at $(\pm 1, \pm 1, \pm 1)$. Construct inner sphere centered at the origin. Draw a three dimensional box around the eight unit spheres. You will see that the interior sphere is wholly contained in the three-dimensional box. Brown continues:

> Reflecting on these pictures, it would be perfectly reasonable to jump to the 'obvious' conclusion that the result holds in higher dimensions. Amazingly, this is not so. At ten dimensions and higher,

the central sphere breaks through the n-dimensional box. (Brown *1997*, 178)

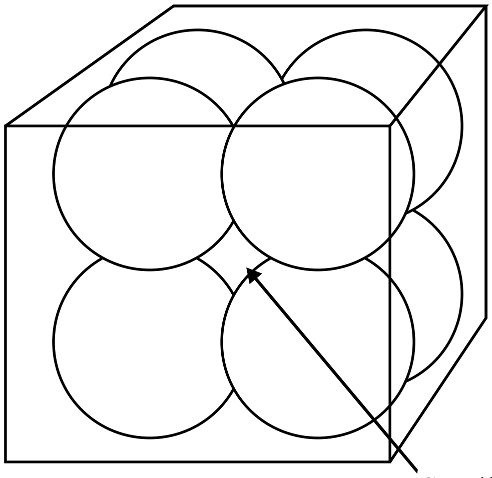


Figure 13b

This is hardly a failure of diagrammatic reasoning. For, in order to make the leap to the false conclusion one must assume that what holds in two and three dimensions will hold at all higher dimensions. This assumption is clearly false. And that accounts for the error in this *proof*. Moreover, it isn’t as if the figure was misleading at all. In each case where the figure was given, the result did hold. The mistake was in thinking that we could extend the figures to higher dimensions. The moral of the story is that we ought to make our assumptions explicit, regardless of whether our proof techniques are figurative or verbal/symbolic.

Hidden assumptions are important. We should make them explicit as a false assumption can infect the reasoning making it unsound. Consider the following figurative proof so that we can make tacit assumptions explicit (Figure 14). We find this proof very convincing. But it depends on a number of assumptions. One is that the figure is infinitely divisible. One may object to this assumption, but that does not show a flaw in the reasoning.

Another assumption is what we might term a pre-formal limit axiom, perhaps in the form of the Principle of Continuity. The idea is that if an error measure can be made arbitrarily small, then the process will reach the limit. This assumption depends on the process converging to a limit in the first place. But, in this case it is clear that the smaller and smaller squares are

$$\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\cdots=1$$

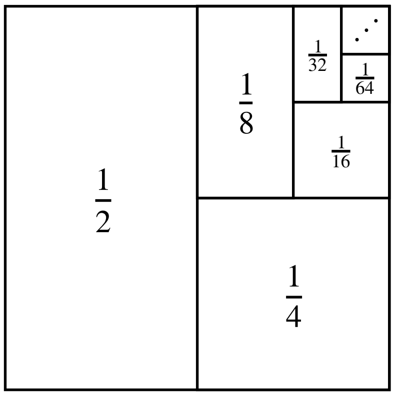


Figure 14

getting closer and closer to filling in the unit square. A case where the convergence is not as easy to discern, but can by discovered in the proof is the following.

$$\frac{1}{4}+\frac{1}{16}+\frac{1}{64}+\frac{1}{256}+\cdots=\frac{1}{3}$$

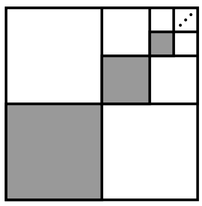


Figure 15

To grasp that the sequence is converging in this case is a bit more difficult, but one can see that each "L" of the unit square has 1/3 of its area shaded. And we know that the "L's" converge to 1. So, the shaded regions converge to 1/3. An analogous process occurs in the traditional verbal symbolic proof (Cf. Brown *1997*, 171–172).

Theorem: $\frac{1}{1}+\frac{1}{4}+\frac{1}{8}+\ldots=1$

Proof (Traditional):

$$
\begin{aligned}
s_1 &= \frac{1}{2} \\
s_2 &= \frac{1}{2}+\frac{1}{4}=\frac{3}{4} \\
s_3 &= \frac{1}{2}+\frac{1}{4}+\frac{1}{8}=\frac{7}{8} \\
s_4 &= \frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}=\frac{15}{16}
\end{aligned}
$$

The Value of the Partial Sums (s_n):

$$\frac{2^n-1}{2^n}$$

The infinite sequence has limit 1, provided that for any number ε, no matter how small, there is a number $N(\varepsilon)$, such that $n > N$, the difference between the general term of the sequence $\frac{2^{n-1}}{2^n}$ and 1 is less than ε.
Symbolically:

$$\lim_{n\to\infty}\frac{2^n-1}{2^n} = 1 \text{ iff}$$

$$(\forall\varepsilon)(\exists N)n > N \rightarrow \left|2^n-\frac{1}{2^n}-1\right| < \varepsilon$$

$$\left|\frac{2^n-1}{2^n}-1\right| \varepsilon \rightarrow \left|\frac{-1}{2^n}\right| < \varepsilon$$

$$\rightarrow 2^n \geq \frac{1}{\varepsilon}$$

$$\rightarrow log_2\frac{1}{\varepsilon} \leq n$$

Let $N(\varepsilon) = log_2\frac{1}{\varepsilon}$

Hence,

$$n > log_2\frac{1}{\varepsilon} \rightarrow \left|\frac{2^n-1}{2^n}-1\right| < \varepsilon$$

So, the sum of the series is 1.

Notice that the pre-formal limit concept and the idea of convergence are both included in a formal presentation of the proof of the theorem. Thus, insofar as those were the assumptions that allowed us to draw the conclusion in the case of the diagrammatic proof, those assumptions were true. Ian Mueller discusses a similar phenomenon as regards the assumptions Euclid made in his proofs.

> There are indeed many instances of tacit assumptions being made, but these assumptions were always true. (Mueller *1981*, 5)

This is clearly not true in all cases where reasoning involving figures relies on tacit assumptions. But, if the assumptions are made explicit and they are seen to be true, then the conclusion of an argument employing this kind of reasoning is no less a guarantee of truth than a verbal/symbolic argument.

Finally, consider the following proof of the Pythagorean Theorem.

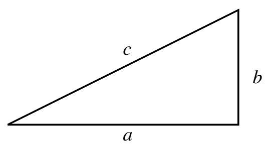


Figure 16a

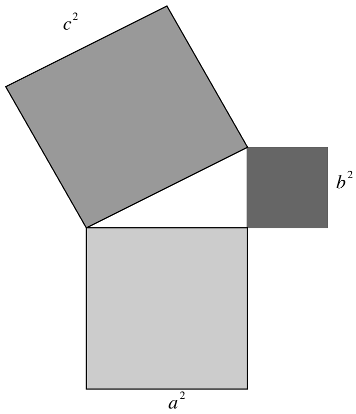


Figure 16b

The proof works by showing that the squares associated with the sides of a right triangle have the relation that the area of the square on the hypotenuse is equal to the sum of the squares on the other two sides.

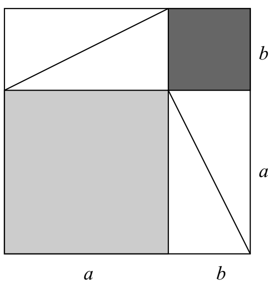


Figure 16c

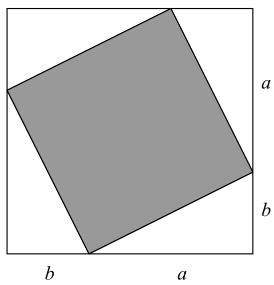


Figure 16d

The proof is usually given with the command: Behold! If you can't draw the conclusion, notice that the large squares have the same area, i.e. $(a + b)$ by $(a + b)$. And, each of the larger squares contains four triangles of the same size as well as either two squares in the case of Figure 16c or one square in the case of 16d. Thus, subtracting the four triangles from each of the squares will leave figures with the same areas. There are a couple of things to notice about the proof. Is the figure arbitrary? Yes. We can thus generalize the results. Did we rely on our perceptual faculties to determine this conclusion? No more than if we had read a verbal/symbolic proof of the same theorem. The conclusion is therefore necessary insofar as it can be generalized to any such figure.

We conclude that implicit assumptions ought to be made explicit. And, though we don't share Mueller's optimism for the correctness of the implicit assumptions of figurative reasoning generally, this is not a problem specific to using figures. Thus, insofar as the objects of mathematical figures are arbitrary and that their properties are not discerned perceptually, then if the assumptions can be made explicit, the diagrams have no pitfalls beyond those inherent in their verbal/symbolic cousins. Indeed, besides being as well behaved as traditional verbal/symbolic elements of proofs, diagrams and figures often have the added benefit of being intuitive. Thus, we should feel no guilt in deploying diagrams in our mathematical proofs. Pictures can prove!

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