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DISCUSSION

Pictures, Proofs, and 'Mathematical Practice': Reply to James Robert Brown

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In a recent article, James Robert Brown ([1997]) has argued that pictures and diagrams are sometimes genuine mathematical proofs. Though Brown raises some interesting considerations about the utility and importance of pictures, he does not show that pictures are, or can be, proofs. I discuss here two specific weaknesses and one more general problem with Brown's paper. First, the specifics.

One problem with Brown's argument is that although he wishes to supplant the traditional or common conception of proof,[1] he nowhere provides an alternative account. It is therefore difficult to see why a picture should be able to count as a proof at all. Among features in favor of picture-proofs he cites the ability of pictures to convince us of the truth of some mathematical claim or other. But the ability to convince is neither necessary nor sufficient for being a proof.[2] Claims made by authorities and inductive evidence can both be convincing, but they do not add up to proofs. And many proofs are not psychologically convincing, especially if they are too long or too difficult to follow. The same goes for the criterion of psychological certainty, which Brown claims pictures can also provide.

Another problem is that Brown undermines his own view about pictures and diagrams by appearing to contradict it. On the one hand, he claims that pictures *are* proofs, at least when they provide evidence and justification. That is, pictures play more than just a heuristic role: 'In short, pictures can prove things' (p. 161). On the other hand, when explaining why certain specific pictures are proofs, he compares them to windows and telescopes. Now telescopes are instruments which enable us to see certain things otherwise not visible. Similarly, Brown claims, diagrams can function, not as

[1] Where 'proof' means a symbolic or verbal derivation; i.e. a deductive argument, usually in some axiomatic system (*cf.* Brown [1997], p. 162).
[2] As I have argued in Folina ([1998], section 3).

representations, but as instruments which enable us to see the general mathematical fact in question (which lies, according to Brown, in 'Plato's heaven') (p. 174). In this way, although a diagram can only instantiate a specific case, it enables us to 'see' in our 'mind's eye' the general fact which corresponds to the theorem.

The difficulty, of course, is that telescopes are not *themselves* justificatory: it is not the telescope which is cited as the primary justification for an astronomical claim. Similarly for windows. Consequently Brown has here drifted into the position that the physical picture, or even diagram type, is not what provides the primary evidence for a general claim. Rather it is merely a tool which enables us to 'see' the evidence, or the abstract 'picture'. The abstract picture, according to Brown, 'is the one seen by the mind's eye—and it has no particular number of elements in it. That is the one we grasp, the one that provides the evidence for the theorem' (p. 176). Hence, on Brown's own account, it seems that although he wants to call them 'proofs', visual pictures do not justify general mathematical claims; only mental, or abstract, 'pictures' can provide such appropriate evidence.

Plato's theory of forms notwithstanding, this is a non-starter. Brown seems unable give a positive argument for his extreme realist view against, for example, a Kantian interpretation of why pictures might be revealing, or convincing.[3] He only argues that platonist 'picture windows' are a possibility. Possible or not, this is a different view from that which he sets out to defend, namely, that actual visual pictures and diagrams can provide the right kind of evidence to count as a proof. What I may see in my 'mind's eye' is not what is in question here.

Aside from these specific problems, I want now to argue that there is a more general problem with the structure of Brown's argument. It is instructive to understand this, as his argument goes wrong in an interesting way, and he has some important company on this path.

Brown's argument begins with what he considers to be the common attitude about mathematical knowledge and mathematical proofs:

> The common attitude towards Bolzano reflects the generally accepted attitude towards proofs and pictures. On this view only proofs give us mathematical knowledge; moreover, proofs are derivations; they are verbal/symbolic entities. Pictures, on the other hand, are psychologically useful, often suggestive, and sometimes charming—but they do not provide evidence (p. 162).

[3] While in many ways the Kantian explanation has more immediate appeal. Via some Kantian-like intuition we may be able to see the possibility of indefinitely iterating the specific drawings to gain insight into the general case. This would connect the justificatory nature of the pictures with the inductive nature of the rigorous proofs in some of Brown's examples.

This attitude, which Brown wishes to oppose, has two components. The first is that only proofs give us mathematical knowledge. The second is that mathematical proofs are symbolic/verbal derivations. According to this attitude, pictures play at most a heuristic role; they do not provide independent justification for a mathematical claim. Other less-than-traditional mathematical arguments get thrown into the same pile as pictures: inductive evidence, reasoning by analogy, broad experience, informal argument, etc. (p. 167). This 'common attitude' is offensive to Brown and others[4] who seem to think that it demeans any kind of evidence which falls short of a traditional, deductive mathematical proof. The worry appears to be that this common, or traditional, attitude leaves *no role at all* for the less-than-formal kinds of evidence.

Of course, people wish to argue that there is a role for less-than-formal kinds of mathematical evidence. It is patently obvious, they claim, that other forms of reasoning take place within mathematics. Informal arguments, new axioms and new definitions show this to be so. Unless we regard such things as arbitrary, and not mathematically justifiable, there must be more to mathematics than axiomatic proof (Mendelson [1990], p. 233; Brown [1997], p. 166). So, they conclude, there must be more to proof than the common conception of proof in some axiomatic system. Take your pick, this 'more' can include: informal arguments (Mendelson), picture-proofs (Brown, Devlin), computer-aided testing (Horgan) . . . The general conflation is often between what is mathematical and what is mathematically provable. These are clearly not the same, but the arguments I want to critique tend to treat them as such.

To return to Brown's version of this argument, let us remember that he wants to argue against a conjunction which he thinks represents the 'common attitude'. And a conjunction can be shown false by showing that either conjunct is false. Our conjunction is: *Only proofs give us mathematical knowledge* AND *Proofs are traditional derivations*. Brown thinks the problem with the conjunction is the second conjunct; and he thinks pictures ought to be included as proofs along with traditional derivations. But he does not argue against the second conjunct. To do so would require setting out an alternative conception of mathematical proof, and arguing that it is better, or more accurate, than the traditional or common conception. Instead of focusing on the nature of mathematical proof, however, he attends to the variety of functions which pictures can play in mathematics. Of the two conjuncts above, these arguments are more easily seen as directed against the first.

Here are some of his major claims about pictures which he believes support the thesis that pictures can be proofs. Pictures provide evidence, and can justify

[4] For example, Elliot Mendelson in his ([1990]).

a mathematical claim (p. 161). A picture can show that something is obviously, or trivially, true (pp. 163–4). Pictures can give powerful reasons for believing a theorem; they can even provide certainty (p. 164). Pictures provide data against which a formal theory can be 'tested' (p. 165). There is more to mathematical reasoning than proving theorems (p. 166). Mathematical practice is inductive; we rely on 'intuitions' and data to come up with new ideas and new formalisms (pp. 167–8). Pictures are singular, like intuitions (pp. 169, 173); yet they can convince us of general claims (p. 169), and they sometimes do this more easily than a traditional proof (pp. 169–73). Pictures are legitimate tools for helping us to see and understand general mathematical facts (p. 174).

My point is that this can all be true (and I think it mostly is true) without it being true that pictures can be mathematical proofs. What has Brown shown? That there are forms of evidence and reasoning in mathematics—forms which are critical to mathematics—other than traditional proofs. Pictures, intuitions, inductive evidence and informal argument all play important roles in mathematics.[5] Some of these even provide evidence and justification. What does this show? That our first conjunct is probably wrong. Especially if we are not formalists, the case of axioms is a particularly compelling argument that not ALL mathematical knowledge can be justified by a traditional proof (i.e. a proof *in* some axiomatic system).

Brown notes cases such as the Banach Tarski 'paradox' and the result that there are continuous functions with no derivatives. These are cases where the intuitive, or picture, evidence is overridden by a verbal/symbolic derivation. Brown considers these as mere exceptions to his view that pictures can function in the same sort of way as derivations. In contrast, I think they show something very significant and very different. They show that pictures, and other sorts of intuitive-based evidence, are *dis*analogous to proofs. When a picture conflicts with a derivation in an accepted system, it is the picture which is rejected as misleading, not the proof.[6] True, pictures or intuitive concepts may be needed to shape a theory, but once shaped it is proofs which trump all other kinds of evidence. Proofs thus yield a higher standard of evidence than pictures. These are different sorts of justification; pictures are not on a par with deductive proofs.

Now, if we take for granted that *only proofs give us mathematical*

[5] Jaffe and Quinn ([1993]) have a nice paper which shows the role of analogy to physics in mathematics. (They do not suggest that making a physical analogy can constitute a mathematical proof however!)

[6] Unless there is an error in the proof-attempt, i.e. unless it is not a proof. These examples also generally undermine the trustworthiness of pictures. They show us that how things look, or seem, are not always how they are. For me this weakens the idea that, on their own, pictures can even show, or depict, the mathematical facts (no less justify them). Yet again, there is a gap between what convinces and what justifies.

knowledge, then Brown's argument might persuade us that other convincing forms of mathematical argument are proofs. But why should we take that for granted? Is it even the 'common attitude'? Not if the common attitude includes a traditional conception of mathematical proof, and is not presumed to be formalistic.

Brown offers us reasons to think that there is more to mathematics than traditional proofs, that pictures are important tools, that it is often legitimate to be convinced by a picture, etc. But I think we already knew this. And these reasons only tell directly against the first conjunct, that *only* proofs offer appropriate mathematical evidence, not the second.[7] The attitude identified by Brown as 'common' does seem to be wrong, but it is not wrong for the reason Brown claims. It is wrong because the first conjunct is false, not the second. Mathematical evidence *can* take a variety of forms, but not every kind of convincing evidence for a mathematical claim counts as a proof. In particular, Brown does not show that a picture, or anything 'picture-like', can be a proof. In my view, he does not really argue for this.

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[7] Again, they are only telling against the second—that proofs are traditional derivations—if we presume that the first is true.

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