Towards a theory of mathematical argument

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Abstract. In this paper, I assume, perhaps controversially, that translation into a language of formal logic is not the method by which mathematicians assess mathematical reasoning. Instead, I argue that the actual practice of analyzing, evaluating and critiquing mathematical reasoning resembles, and perhaps equates with, the practice of informal logic or argumentation theory. It doesn't matter whether the reasoning is a full-fledged mathematical proof or merely some non-deductive mathematical justification: in either case, the methodology of assessment overlaps to a large extent with argument assessment in non-mathematical contexts. I demonstrate this claim by considering the assessment of proofs, probabilistic evidence, computer-aided proofs, and the acceptance of axioms. I also consider Jody Azzouni's 'derivation indicator' view of proofs because it places derivations – which may be thought to invoke formal logic – at the center of mathematical justificatory practice. However, when the notion of 'derivation' at work in Azzouni's view is clarified, it is seen to accord with, rather than to count against, the informal logical view I support. Finally, I pose several open questions for the development of a theory of mathematical argument.

Keywords: argumentation, proof, mathematics, argument schemes, dialectic, informal logic, Azzouni, Rav.

1. Introduction

One cannot hope to recapitulate all of informal logic in a single paper. Still, to claim that there is a role for informal logic in mathematics requires some stage setting. As a gloss on the practice, consider Maurice Finocchiaro's definition of informal logic.

> [Define] informal logic as the formulation, testing, systematization, and application of concepts and principles for the interpretation, evaluation, and practice of argumentation or reasoning. (Finocchiaro 1996: 93)[1]

For present purposes, focus on the application of concepts and principles for the interpretation[2] and evaluation of argumentation and reasoning. There was, perhaps, a time when one may have thought that the only principle needed for the analysis of arguments was (formal) logical paraphrase and that the only concept needed for argument evaluation was (deductive) validity. But it is against this mythical hegemony of (formal) logic as *the* tool of argument appraisal that informal logic becomes theoretically (and pedagog-

[1] Page numbers are to the anthologized version (Finocchiaro 2005).
[2] What Finocchiaro calls 'interpretation' is often called 'analysis'. I prefer the latter term and generally use it instead.

ically) important. The early history of informal logic[3] should be traced at least to Stephen Toulmin's *The Uses of Argument* (1958). In the preface to the updated edition (2003), Toulmin clearly targets formal logic.

> When I wrote it, my aim was strictly philosophical: to criticize the assumption, made by most Anglo-American academic philosophers, that any significant argument can be put in formal terms. (Toulmin 2003: vii)

What is important to take away from this early history isn't a particular set of techniques; instead, informal logic best understood as a reaction against formal logic[4] as *the* theory of argument assessment. Much of the work that comprises the first part of Finocchiaro's definition – formulation, testing and systematization of concepts and principles of argument appraisal– is an ongoing theoretical enterprise, to which Finnochiaro and others are actively engaged.

Regarding the application of the concepts and principles of this enterprise to arguments or reasoning *in the wild*, there is only a small literature regarding informal approaches to reasoning in mathematics.[5] However, in the next two sections I make room for informal logic in two mathematical settings. Before turning to those, I must briefly consider the competing conceptions of *argument* within informal logic.

It may be surprising to anyone outside of informal logic that there is any debate about the proper definition of argument. Those of us weaned on formal logic may think this debate is a non-starter because there is a perfectly acceptable definition of argument which is synonymous with the definition of *derivation* available in any textbook on formal logic: an argument is a sequence of statements/sentences/propositions/formulas such that each is either a premise or the consequence of (some set of) previous lines and the last of which is the conclusion.[6] Such a definition is required by and antecedent to the techniques of formal paraphrase that constitute the analytic enterprise. However, a close look at *arguments* as they occur in philosophy journals, say, reveals that there may be more to the argumentative estate. For example, in

[3] I unceremoniously identify informal logic and argumentation theory and argumentation studies as a single entity – really a single kind of response to practical difficulties in theorizing about and teaching principles of argument appraisal/assessment. That the phrase *informal logic* is more often used by philosophers or that the phrase *argumentation theory* is more often used by researchers in communication studies is beside the point. The goal is the same: to produce a workable theory of argument appraisal.

[4] Where 'formal logic' is understood as a theory of entailment which may include both formal semantics and formal proof theory.

[5] Included in this literature are: Andrew Aberdein (2007) (2005), Maurice Finocchiaro (2003) and Erik Krabbe (1991/1997), for example. Poincarè may be an interesting forerunner to this approach, cf. (Detelfsen 1992 and 1993).

[6] Some such definition is often prefatory to formal logic textbooks. The openness of 'statements/sentences/. . . ' is simply to cover the expected variations in such definitions. See for example, (Mates: 5) versus (Allen and Hand: 1).

most good papers, an author considers possible objections to his/her arguments as part of the usual manner of defense. It may be possible to recast objections and replies within the strict premise/conclusion structure – one may grant this provisionally. Yet, whether an argument, properly so-called, ought to contain *dialectical* elements could be a matter of definition. Even granting the strict premise/conclusion conception, there is room for debate about what counts as a premise.

Besides questions of whether all arguments must contain dialectical elements, other considerations include: whether purpose defines arguments; whether rhetorical elements must be considered in the definition of argument; whether an argument ought to be defined in terms of its structure; etc. The following list provides a sample of attempts to include/exclude these elements.

> An argument, in the logician's sense, is any group of propositions of which one is claimed to follow from the others, which are regarded as providing support or grounds for the truth of that one. (Copi and Cohen: 5 quoted in Finocchiaro 2003a: 295)

> The simplest possible argument consists of a single premise, which is asserted as true, and a single conclusion, which is asserted as following from the premises, and hence also to be true. The function of the argument is to persuade you that since the premise is true, you must also accept the conclusion. (Scriven: 55-6 quoted in Finocchiaro 2003a: 297)

> An argument is a type of discourse or text–the distillate of the practice of argumentation–in which the arguer seeks to persuade the Other(s) of the truth of a thesis by producing reasons that support it. In addition to this illative core, an argument possesses a dialectical tier in which the arguer discharges his dialectical obligations. (Johnson: 168 quoted in Finocchiaro 2003a: 297)

> *Argumentation* is a verbal, social and rational activity aimed at convincing a reasonable critic of the acceptability of a standpoint [read: conclusion] by putting forward a constellation of propositions justifying or refuting the proposition expressed in the standpoint. (Grootendorst and van Eemeren: 1)

> Now arguments are produced for a variety of purposes. Not every argument is set out in formal defense of an outright assertion. But this particular function of arguments will claim most of our attention [. . .]: we shall be interested in justificatory arguments brought forward in support of assertions, in the structures they may be expected to have, the merits they can claim and the ways in which we set about grading assessing and criticizing them. It could, I think, be argued that this was in fact the primary function of arguments, and that the other uses, the other functions

> which arguments have for us, are in a sense secondary and parasitic upon this primary justificatory use. (Toulmin: 12)

As I am here interested in making room for informal logic in mathematical justification I will let the last conception do most of the work. This paper will be interested in 'justifactory arguments brought forward in support of assertions.' I will construe *support* broadly to include both deductive and non-deductive justifications.

2. The Role of (Informal) Logic in Assessing Proofs

Ralph Johnson (2000) has a widely read and rightly respected theory of argument appraisal. Against its many virtues, however, there is one vice: he unnecessarily eschews, except in one case as a negative illustration, mathematical examples. This decision, I think, can be traced to the conception of proof he accepts.

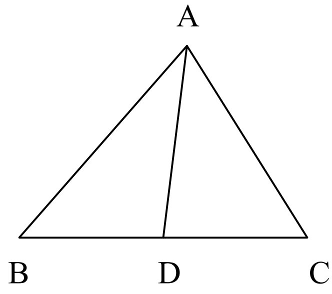
> If we are looking for conclusive arguments, we can do no better than take the paradigm of mathematical proofs; for example, the proof that there is no greatest prime number. Here, a proof is a sequence of steps, each of which is either an axiom (or an otherwise incontestable step) or a valid derivation from previously accepted lines. (Johnson: 231)

Suppose Johnson is correct that a proof really is a (finite) sequence of statements such that each statement is either an axiom (or accepted claim) or the logical consequence of an earlier statement. Would this preclude the use of informal logic in mathematics?[7] One difficulty with this notion of proof is that it contains an unstated qualifier – any proof on this account is a *successful* proof. There is a treasure trove of failed proofs in mathematics. And, I assert, the methods by which these failures were discovered employed the techniques of informal logic.[8] This is not to say that the techniques of informal logical appraisal are only applicable in finding errors. It is a *general* technique of appraising reasoning. Consider the following *proof* (Maxwell: 10-12).

> *To prove that every triangle is isosceles.*
> Let *ABC* be a given triangle.

[7] Johnson nowhere, as far as I can tell, dismisses the use of informal logic in mathematics. Instead, his concern is to distinguish proof from argument-proper.

[8] If this assertion is too strong or too hasty, qualify it to read: The methods for assessing mathematical proofs resemble the methods for assessing non-mathematical arguments.



It is required to prove that AB is necessarily equal to AC.
If the internal bisector of angle A[9] meets BC in D, then by the angle bisection theorem, $\frac{DB}{AB} = \frac{DC}{AC}$.
Now, $\angle ADB = \angle ACD + \angle CAD = C + \frac{1}{2}A$,
so that, by the sine rule applied to the triangle ADB,
$\frac{DB}{AB} = \frac{\sin BAD}{\sin ADB} = \frac{\sin \frac{1}{2}A}{\sin(C+\frac{1}{2}A)}$.
Further, $\angle ADC = \angle ABD + \angle BAD = B + \frac{1}{2}A$
so that $\frac{DC}{AC} = \frac{\sin \frac{1}{2}A}{\sin(B+\frac{1}{2}A)}$.
Hence, $\frac{\sin \frac{1}{2}A}{\sin(C+\frac{1}{2}A)} = \frac{\sin \frac{1}{2}A}{\sin(B+\frac{1}{2}A)}$.
Moreover, $\sin \frac{1}{2}A$ is not zero, since the angle A is not zero,
and so $\sin(C + \frac{1}{2}A) = \sin(B + \frac{1}{2}A)$,
which means that $C + \frac{1}{2}A = B + \frac{1}{2}A$, which means $C = B$.
The triangle, as are all others by generalization, is therefore isosceles. ■

The conclusion of this reasoning is absurd. But what has gone wrong? Leave aside any problems one thinks accrue when reasoning contains diagrams, for, in this case, the problem isn't the diagram at all. Instead, suppose this *proof* occurs as an example in a paper on mathematical method, as it does here. There is, of course, no imperative to find the mistake. One could, I suppose, continue living as if nothing terrible had happened. But I would hope that a rational being confronted by this proof would want to discover the error – even if that being weren't mathematically inclined. Moreover, I like to think that if one attempts to discover the error in the proof and fails, this would cause some tension in one's web of belief. Leaving these hopes and wishes to one side: the important aspect of this thought experiment is to speculate about how someone would go about appraising this argument.

On the one hand, if you know that the conclusion is false, then there is a sense in which you know there must be a mistake before you begin your inves-

[9] The angles in the original triangle, ABC, are labeled simply A, B or C respectively. The other angles are labeled more fully, e.g. $\angle ADC$, to avoid ambiguity.

tigation of the reasoning. On the other hand, perhaps, you don't know where the reasoning fails, or, if the conclusion weren't obviously absurd, you might feel less compelled to critique the reasoning. However, believing that a conclusion is false isn't usually enough to warrant rejection. There are many odd results, both inside and outside of mathematics. Anecdotal evidence, gathered by the author, suggests that mathematically inclined critics initially point to the diagram as the faulty source. One explanation for this may be that there is a well known faulty argument for the same conclusion that is often claimed to rest on a mistaken construction. In the present case, however, the problem isn't with the diagram or the associated auxiliary constructions. Those critics without (recent) mathematical inclinations suppose that the fault lies with either the appeal to the angle bisection theorem or the sine rule applied to the triangle. Again, neither of these is a mistake here. The error, as Maxwell points out (Maxwell: 12) is that equality of sine does not mean equality of angle. For example, $\sin\pi = \sin 2\pi$ but $\pi \neq 2\pi$. So one cannot/shouldn't infer from $\sin(C + \frac{1}{2}A) = \sin(B + \frac{1}{2}A)$ that $(C + \frac{1}{2}A) = (B + \frac{1}{2}A)$.

The mistake in this case is, in some respects, uninteresting. One treats a function symbol as if it were manipulable in the same way as variables or constants.[10] Still, the identification of the mistake, I trust, is interesting insofar as the process one uses to discover the mistake is analogous to the process one might use to criticize an unpalatable argument in non-mathematical settings. Thus, even for cases of proofs, there could be a role for the informal logician – notice that the mistake wasn't discovered by appealing to some formalization of the statements in a suitable language. Rather, this appraisal takes place in natural language.

3. Assessing Mathematical Evidence

Mathematicians regularly use *evidentiary* or non-deductive methods such as computer-assisted proofs, probabilistic sieves, partial proofs and abduction.[11] And mathematicians seem to be pretty good at assessing these uses. Although some mathematical mistakes may remain temporarily unnoticed, claims that are supported by less than deductive means are often ripe for reconsideration – they may even receive closer scrutiny than their deductively justified cousins. In this section I consider how some of these means of support are assessed by mathematicians. I compare the mathematical method with tech-

[10] One way to understand the mistake is that the argument treates the 'f' of $f(x)$ to be capable of manipulation. Suppose $f(x) = x^2$. Then for any n $f(n) = f(-n)$. Then, dividing both sides by f, one gets $n = -n$. Since sin is a function symbol, it isn't itself manipulable. Instead, the function is manipulable only when it is is given with an attending argument.

[11] James Franklin (1987) gives a brief sample of some of the acceptable non-deductive methods.

niques from informal logic to show they are similar if not identical. This leads me to conclude that the logic of assessment underwriting this practice is informal logic.

3.1. Computers and Probabilistic DNA Evidence

Don Fallis (1996, 1997, and 2003) argues that probabilistic evidence ought to be given more credence in mathematics. A surprising use of probabilistic evidence in graph theory concerns the discovery of Hamiltonian Paths among sets of points by encoding the points with unique DNA *addresses* such that a Hamiltonian Path exists only if the sequences of DNA addresses, when mixed in an appropriate solution, combine to form a double helix. The method is reliable and repeatable. That is, the method could result in false-rejections – cases where no double helix is found though there does exist a Hamiltonian Path – though not false positives (cf. Fallis 1996 and 1997). Moreover, even if there were a false positive, there is an ancillary method by which these results can be checked to determine whether the DNA constitutes a real Hamiltonian Path. Fallis argues that there aren't good epistemological reasons to reject such evidence as proofs. As that argument is outside the scope of this paper, bracket it. Instead, I'm interested in how a mathematician would appraise this evidence.

Informal logic, as a theory of argument appraisal, distinguishes two endeavors: analyzing and evaluating arguments. To see this process at work in a non-mathematical, evidentiary case, consider the following.

> During excavations of the Bronze Age levels at El Mirador Cave, a hole containing human remains was found. Tapaphonic analysis revealed the existence of cutmarks, human toothmarks, cooking damage, and deliberate breakage in most of the remains recovered, suggesting a clear case of gastronomic cannibalism. (Cáceres et. al: 899)[12]

There are many different informal techniques one could use to analyze this bit of reasoning: as a Toulmin Diagram (Toulmin: 87ff.), as a numbered step argument or as an argumentation scheme. Let's use a scheme. Schemes present a standard format or template for argument types. To assess an argument's structure one reconstructs the argument using the structure of the chosen scheme. This makes it easier to identify the explicit argumentative elements as well as any material that is left tacit in the reasoning. Along with the canonical form, the scheme also provides evaluative critical questions. In this case, the scheme known as *Argument from Sign*[13] seems appropriate. It

[12] In what follows I've left out the qualifier 'gastronomic' because, for the purposes of providing an example, it is unnecessary. Gastronomic Cannibalism is distinguished by physical anthropologists (cf. Càceres et al.: 899) from Ritualistic Cannibalism solely in terms of purpose.

[13] See Douglas Walton (2006: 112 - 114) for a textbook treatment of this scheme.

has two kinds of premises. One is called the *specific premise*: Some finding, A, is true in this situation. Next there is a *general premise*: Such a finding is indicative of some object, event or action, B. This leads to a conclusion: B obtains, occurs or happens (in this situation). Reconstructing the above argument as an Argument from Sign, one gets as a specific premise: '[The human remains recovered at El Mirador Cave contained] cutmarks, human toothmarks, cooking damage and deliberate breakage in most of the remains recovered.[14] The general premise is: '[the existence of cutmarks, toothmarks, cooking damage and deliberate breakage] suggests cannibalism.' The conclusion, then, is, '[The Bronze Age inhabitants of El Mirador Cave practiced (in at least one instance) cannibalism.]'[15]

To evaluate an argument using a scheme, one answers the critical questions associated with the scheme. The critical questions typically fall under two broad categories: (a) acceptability of the premises, and (b) amount of support the premises, if true, would confer upon the conclusion. In the case of Argument from Sign, the two critical questions are: (1) How strongly is the sign correlated with the result? and (2) Are there counter-signs that indicate a different result or which undermine the acceptability of the sign in this case? The answers to these questions are not always readily available. However, in this case, later in the article the authors answer question (1) affirmatively (Cáceres et al.: 905) and question (2) in the negative (Cáceres et al.: 912-3). This means that the argument, though defeasible, strongly supports its conclusion.

I'll apply this same appraisal technique to the DNA arguments considered by Fallis (1996 and 1997). Take a graph, G, with directed edges and for which the starting node, v_{in}, and ending node, v_{out}, are specified. A *Hamiltonian Path* starts from v_{in}, ends at v_{out}, and travels across each edge of G exactly once. The *Hamiltonian Path Problem* is to determine for an arbitrary directed graph whether there exists a Hamiltonian Path through the graph. This problem has been proven to be NP-complete (cf. Karp 1972: 85ff.), which means that non-deterministic algorithms are likely to be the only kinds of solutions available. Leonard Adleman (1994) provides such an algorithm whose implementation involves the use of DNA.

Step 1: Generate random paths through the graph.
Step 2: Keep only those paths that begin with v_{in} and end with v_{out}.
Step 3: If the graph has n vertices, then keep only those paths that enter exactly n vertices.
Step 4: Keep only those paths that enter all of the vertices of the graph at least once.

[14] The use of square brackets, '[' and ']', indicates that this material is either paraphrased from the original material or added to the material.

[15] The conclusion is clearly indicated by the phrase 'suggests a clear case of gastronomic cannibalism'.

Step 5: If any paths remain, say 'Yes'; otherwise say 'No.' (Adleman: 1021-2)

Adleman implemented this algorithm by encoding a graph's vertices as DNA *addresses*. Then, given DNA ligation, the process by which single strands attach to form double strands, mixing a sufficient quantity of the addresses will produce random paths through the graph (Step 1). Moreover, given accepted techniques for distinguishing DNA sequences, Adleman could complete steps 2-4. Step 5 depends upon whether there are any double helices in the mix.

Fallis, recall, wants to argue that from the standpoint of epistemology, the DNA implementation of this algorithm, with its explicit error measure, entails as much certainty as that of any sufficiently long mathematical proof (Fallis 1997: 166). The details of this fascinating argument are tangential to the present project. Instead, consider a particular (hypothetical) case of Adleman's DNA implementation of the algorithm analyzed and evaluated as an Argument from Sign. That is, suppose that Adelman implements the algorithm for a particular graph *G* such that the result in step 5 was 'No.' In such a case the specific premise would be: 'The DNA implementation of the algorithm gave 'No' as the answer regarding graph *G*.' The general premise would be: 'A 'No' answer for graph *G* is a sign/indication that there does not exist a Hamiltonian Path for *G*.' And the conclusion would be: 'Graph *G* doesn't have a Hamiltonian Path.' The evaluation would follow precisely the evaluation of the argument regarding cannibalism at El Mirador Cave above.

A related mathematical method involves the use of computers in proofs. The most famous and oft discussed example was the use of a computer in proving the Four-Color Theorem.[16] Again, whether the appeal to computers ought to be considered a legitimate case of *proving* is tangential to this discussion. Instead, I'm interested in how mathematicians would assess any particular case of appealing to computers. In the case of the Four-Color Theorem, the proof is simple to explain. For the most part, it is a proof by mathematical induction. There are three cases, one of which contains thousands of subcases. To handle the sheer number of subcases would be outside of the ability of any human. But, a computer could check each of these subcases. Moreover, the structure of the argument would resemble the DNA implementation algorithm in its gross structure. That is, the mathematician would prepare the computer in some way; then, by running the program, the mathematician would get an answer. If the answer were 'yes,' that would be a sign that subcase is four-colorable. If the answer were 'no,' that would be a sign that the subcase is not four-colorable. The program was executed and the answer was 'yes.' Hence, four colors suffice.

[16] For an illuminating philosophical discussion of the use of computers in the proof of the Four-Color Theorem see (Detlefsen 1980).

At least in these two cases,[17] the methodology of appraisal or assessment cannot be formal in the sense of formal logic – mathematicians didn't try to recast these arguments as formulas in a first-order language to determine whether the consequences follow as a matter of derivation or formal semantics. As I have shown, one can assess the reasoning using principles and techniques from informal logic.[18] Moreover, the techniques actually used by mathematicians to assess these arguments are precisely these informal logical techniques. One caveat is that no mathematicians seems to have used an argumentation scheme like Argument from Sign explicitly.[19] One should focus on whether the reconstruction of mathematical reasoning using explicit argument schemes would shed any light on the practice. I think it will and does. That is, by reconstructing the DNA proofs as explicit Arguments from Sign, one simplifies the gross structure of the argument – the analysis – and one has recourse to general critical questions to guide the evaluation of the arguments.

3.2. Abduction in Mathematics

The previous subsection takes as its point of departure those mathematical practices under the purview of no axioms. Hence, those practices may be judged atypical. Yet, turning to axiomatic systems – the very heart of what some may call the *formal approach* – there is, even there, room for informal logic. Penelope Maddy (1988) argues that the acceptability of axioms is itself a matter of reason though not of direct or deductive proof. She takes the development of set theory as a case study in this practice. For simplicity's sake, consider some arbitrary mathematical claim, C. Maddy distinguishes two ways of coming to believe in C. On the one hand, C may be the consequence of some well established first principles, A, such that $A \vdash C$. This is usually the way mathematical claims are thought to garner acceptance. On the other hand, C may garner acceptability in terms of its 'fruitfulness.' Suppose $C \vdash P$ where P is some widely accepted mathematical claim. Does this implication make C any more acceptable? Maddy(1992), quoting Gödel, thinks so.

[17] Whereas DNA proofs will remain defeasible, computer proofs can have *formal verifications*.

[18] For a different assessment using the tools of Toulmin diagrams, see (Aberdein 2007). There, Aberdein reconstructs (a part of) the proof of the four-color theorem as an explicit Toulmin diagram. The use of Argument Schemes vs. Toulmin Diagrams shouldn't be thought of as necessarily opposed. One may be able to capture all of the elements of a scheme in a diagram and vice versa.

[19] Below I suggest that proofs by Mathematical Induction have much in common with Argumentation Schemes. Perhaps, Mathematical Induction is, simply put, a mathematical argumentation scheme. Again, Poincarè's views on mathematical induction could importantly prefigure this idea (cf. Detlefsen 1992 and 1993).

> . . . besides mathematical intuition, there exists another (though only probable) criterion of the truth of mathematical axioms, namely their fruitfulness in mathematics and, one may add, possibly also in physics. (Gödel 1964/1983: 485) Quoted in (Maddy 1992: 77)

Maddy runs through a considerable number of examples of what she terms *extrinsic* support. The important features of this practice, for my purposes, are that they are widespread and not themselves reducible to deductive inferences. The most widely discussed examples involve the acceptance of the axiom of choice.

> But the question that can be objectively decided, whether the principle is necessary for science, I should now like to submit to judgment by presenting a number of elementary and fundamental theorems and problems that, in my opinion, could not be dealt with at all without the principle of choice. (Zermelo: 189-90, quoted in Maddy 1988: 488)

The point, obviously, is that the utility of the axiom of choice in dealing with open problems in mathematics provides reason to accept it, even if only hypothetically.

Let us now distinguish two questions about this practice. First, does the existence of fruitful consequences provide *any* support for a mathematical claim? Second, if so, how much? I take it that the widespread use of this practice suggests that practitioners would answer the first question affirmatively. Regarding the second question, I am much less sure what the correct answer is. But, I'm sure what it is not. Abduction provides some measure of support, I just don't know how much. This means that, at best, the conclusion of an abductive inference ought to be accepted *with reservation*. As I am not here arguing that abductive inferences are proofs, the (perhaps vague) reservations attached to conclusions supported abductively do not thereby undermine the use of informal logic.

Finally, to see that this practice both is even more widespread and has a long history, consider the oft discussed application of summation techniques by Leonhard Euler to divergent series (cf. Polya 1968, Putnam 1975, and Sandifer 2006). Put roughly, Euler applies techniques to divergent series that are only categorically valid for convergent series. In this way, Euler finds a value for the series $A = 1-1+2-6+24-120+720-5040+\cdots$

> This is the key result of this paper, but Euler understands that some readers might not be convinced that he hasn't made any mistakes. So, he solves the same problem several other ways. For example, he finds diverging series for $\frac{1}{A}$ and $\log A$, and finds that similar methods also lead to a value of A near 0.59. He finds a way to write A and $\frac{1}{A}$ as continued fractions and evaluates those continued fractions to get still more estimates consistent with the ones before. (Sandifer 2006: 7)

Sandifer finds neither Euler's conclusion nor his method irrational. Still, Euler was obviously aware of the shaky footing of both result and method. So, to buoy the method, he shows that it is consistent with other results and accepted techniques.

> By the end of the article, Euler has estimated A at least six different ways, and every time he gets the same estimate. *When such different analyses all lead to the same conclusion, it is easy to understand why mathematicians of Euler's time believed in the utility of [divergent series].* (Sandifer 2006: 8 *emphasis added*)

Sandifer's assessment of Euler's reasoning is simply an application of the techniques and principles of informal logic (though perhaps tacitly). Indeed, Sandifer's claims in the above quote suggest that one understand Euler's justification of the method abductively; and at the same time to understand the justification of the result as an accumulation of evidence. In both cases, there are argument schemes that accord with (or predict) Sandifer's assessment. This suggests a rather strong conclusion. Informal logic *is* the logic of mathematical reasoning. In a later section I consider this claim in the light of a debate between Jody Azzouni and Yehuda Rav concerning derivations and their relation to mathematical proofs.

3.3. SUB-CONCLUSION

Mathematicians already, though perhaps tacitly, use the techniques of informal logic. They use them when they appraise proofs, and they use them when they assess mathematical reasoning that isn't proof. This is not to say that mathematicians ought to pay more attention to informal logic or argumentation theory. Rather, this suggests that an accurate philosophy of mathematics ought to recognize this use. Hence, inasmuch as informal logic is already a part of mathematical practice, it makes sense to make the use explicit as part of a larger project to construct a philosophy of mathematics that takes practice seriously.

4. Proofs, Derivations and Algorithmic Systems

In this section, I turn to an account of mathematical proofs that has some affinities with the informal logical account I suggested above. However, whereas I've argued that mathematicians tacitly use informal logic as a theory of appraisal for mathematical reasoning, this account would seem to make much more use of formal technique. I argue that the appeal to formal techniques is unnecessary or illusory.

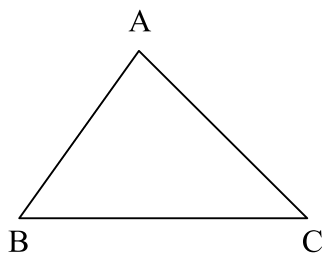
Jody Azzouni (2004 and 2006) defends an account of proofs he calls the 'derivation indicator' view. Put roughly, this means that a mathematical 'proof' is a promissory note or an advertisement: either it promises that one can find a strict derivation of the result of the proof from first principles, or it advertises the existence of such a derivation. The proof is, in Azzouni's words, 'in the vernacular,' whereas the associated or promised derivation needn't be.

This account could make room for informal logic within mathematical practice. For, if proofs take place 'in the vernacular,' then, perhaps they ought to be assessed in the vernacular as well. But this doesn't seem to be what Azzouni intends.

> [A]lthough it's true (on [Azzouni's] view) that *proofs* in Rav's sense are indispensable to mathematical practice, this is compatible with the claim (which is also true on [Azzouni's] view) that it's *derivations*, derivations in one or another *algorithmic system*, which *underlie* what's characteristic of mathematical practice: in particular, the *social conformity* of mathematicians with respect to whether one or another proof is or isn't (should be, or shouldn't be) convincing. (Azzouni 2004: 83)

Azzouni defines *algorithmic system* as: 'one where the recognition procedure for proofs is mechanically implementable.' (Op cit.) He allows for quite a broad understanding of what would count as such a system. For example, he thinks that Euclidean geometry would count on this view. But this complicates rather than clarifies the connection between proofs and derivations. Consider the proof of Euclidean Proposition I.19.

> Proposition 19: In any triangle the greater angle is subtended by the greater side.
> *Proof*: Let *ABC* be a triangle having angle *ABC* greater than angle *BCA*.; I say that the side *AC* is also greater than the side *AB*.



For, if not, *AC* is either equal to *AB* or less.
Now *AC* is not equal to *AB*; for then the angle *ABC* would also have been equal to angle *BCA* [Prop. I.5]; but it is not [by stipulation]; therefore *AC* is not equal to *AB*.
Neither is *AC* less than *AB*, for then the angle *ABC* would also have been less than angle *BCA* [by Prop. I.18], but it is not [by stipulation]; therefore *AC* is not less than *AB*.
And it was proved that it was not equal either.
Therefore *AC* is greater than *AB*.
Therefore [in any triangle the greater angle subtends the greater side.] (Euclid: 284)

The reason to choose this proposition and its *proof* is that it could be derived without recourse to any geometrical reasoning at all. Instead, it is a *logical* consequence of Propositions I.5, I.6 and I.18 by the operation called *conversion* (cf. Euclid: 284-5, Heath's footnote). What is the proof and what is the derivation? Perhaps Azzouni would have the two identified in this case. He doesn't rule out this possibility, so it is open to him to take this option. Still, this would mean that all of Euclid's proofs are also derivations.[20] To

[20] A distressing caveat: Euclid abbreviates his proofs, especially in the last step where the reasoning from a particular figure is generalized. This means that, strictly speaking, each

identify these as such would, I think, take away some of the rhetorical force of Azzouni's otherwise tempting view.

What about proofs from other mathematical fields? Consider another example from graph theory (Aigner and Ziegler: 47) – this one from *The Book*!

> *Theorem 1*. In any configuration of n points in the plane, not all on a line, there is a line which contains exactly two of the points.
>
> *Proof*. Let $\mathcal{P}$ be the given set of points and consider the set $\mathcal{L}$ of all lines which pass through at least two points of $\mathcal{P}$. Among all pairs (P,l) with P not on l, choose a pair (P_0,l_0) such that P_0 has the smallest distance to l_0, with Q being the point on l_0 closest to P_0 (that is, on the line through P_0 vertical to l_0.
>
> *Claim*. The line l_0 does it!
>
> If not, then l_0 contains at least three points of $\mathcal{P}$, and thus two of them, say P_1 and P_2, lie on the same side of Q. Let us assume that P_1 lies between Q and P_2, where P_1 possibly coincides with Q. It follows that the distance of P_1 to the line l_1 determined by P_0 and P_2 is smaller than the distance of P_0 to l_0, and this contradicts our choice for l_0 and P_0. ■

Is this a proof or a derivation? It seems a prototypical proof. Yet, if Azzouni is correct, then one way to critique a proof is to undermine the underlying derivation. So, it matters whether this example is a proof, a derivation, or both. He asserts that proofs take place in natural language, but in what language do derivations occur? Here's a conjecture: Derivations occur in natural language too. There isn't a formal language in which proofs are recast as derivations. Instead, if the conjecture is correct, then the only proofs that aren't also derivations are proofs with gaps – intentional or not. Intentional gaps occur in 'proof sketches.' This raises two related questions, why isn't Azzouni's distinction simply between proofs and incomplete proofs? What gaps are sufficient to keep a proof sketch or an incomplete proof from being a proof-proper or a derivation?

To answer these questions, one must get a better sense for the prerequisites of Azzouni's algorithmic systems. Notice that they aren't called *logical systems* or *deductive systems*. Instead of being deductively or logically perfect, an algorithmic system's implementation must be 'mechanically recognizable.' This is one instance where Azzouni's account directly competes with Yehuda Rav's (1999) account. Rav asserts that proofs contain irreducibly semantic or intensional content that isn't captured by derivations. To see a non-mathematical example of what I think is at issue, consider the reasoning: Andrew is a bachelor; therefore he's unmarried. One might think that the 'therefore' indicates that the arguer has a (logical) derivation although the

of Euclid's proofs is a proof sketch. This is different from the usual complaint about the incompleteness of Euclidean proofs. The usual complaint is that the diagram fills in tacit assumptions, like continuity, that should be made explicit, as in Pasch's Axiom. I discuss proof sketches in more detail below.

reasoning is, as stated, incomplete. On the other hand, one might think that the reasoning is complete – that it is complete because the meanings of 'bachelor' and 'unmarried' overlap in a way that allows one to infer unmarried-ness immediately from bachelorhood. On this view there is no intermediate step that connects bachelorhood and unmarried-ness. Compare with what Rav says about moves in proofs.

> [typical moves in a proof] bring to light the intensional components in a proof: they have no independent logical justification other than serving the purpose of constructing bridges between the initially given data, or between some intermediate steps, and subsequent parts of the argument. But the bridges are conceptual, not deductive in the sense of logic. (Rav: 13, quoted in Azzouni 2004: 101)

My guess is that Azzouni's profligate view on algorithmic systems would simply expand to countenance any 'proof' that Rav thinks is irreducibly intensional. That is, any proof that Rav would classify as containing intensional components would most likely be classified as algorithmically acceptable on Azzouni's account. Moreover, the assessments of either *proofs* for Rav or *derivations* for Azzouni will take place within informal logic. Again, this suggests a rather strong conclusion about which logic is the logic of appraisal in mathematics: it's informal logic.[21]

5. Mathematical Discourse: Problems, Questions and Conclusions

When one applies informal logic to non-mathematical arguments in real contexts, there are many practical problems that don't occur in the mathematical cases. For example, it is quite difficult in practice to determine whether some (non-mathematical) discourse contains an argument. Indeed, in teaching the techniques of argument assessment, argument identification, although it is glossed in most textbook treatments, is where students have great difficulties.[22] Contrast this difficulty with one context in which proofs occur: mathematics texts including journals. In these contexts, mathematicians offer their proofs in a highly stylized manner. This includes clearly indicating the conclusion of the reasoning. Also, the starting point of the reasoning is, more often than not, indicated by the word, 'Proof.' Teaching argument assessment would be greatly simplified if authors generally indicated their conclusions and the start of their reasoning explicitly.

[21] There is nothing in the conception of informal logic I accept would preclude the informal logician from partaking in the fruits of the formal logician's labor. The informal logician simply denies that the method by which mathematical (or really any) reasoning gets appraised is translation into formal language and comparison with accepted formal results.

[22] See Michael Malone's (2005) argument that this is a theoretical as well as practical problem.

There are, of course, contexts in which even mathematicians are less explicit about indicating conclusions and reasons. But this serves to connect mathematical discourse more closely with other contexts rather than separating them. Even in the contexts in which both the conclusion and the reasoning are clearly indicated, this doesn't preclude the use of informal logical techniques and principles. Rather, it looks as if mathematicians use precisely the tools of informal logic, whether they realize and recognize this or not.

Insofar as the forgoing claim is correct and mathematicians are already informal logicians, is there anything to be gained by making this explicit – that is, are there mathematically important results that will accrue from making the practice explicit? I think not. Naming the practice of mathematicians 'informal logic' won't change the practice. But this isn't as negative a result as one might first imagine. Instead, if the focus is on philosophy of mathematics rather than mathematics proper, one can see that giving an accurate account of mathematics as it is practiced, as opposed to its mythic formal practice, is a gain over previous philosophies of mathematics.

This also means that there are a number of open research questions. For example, are there any paradigmatically mathematical argumentation schemes, fallacies, etc.? Above, I used the scheme *Argument from Sign* to assess a mathematical argument. But this scheme isn't mathematical, it is general. Perhaps mathematical induction is a quintessentially mathematical scheme.[23] As a scheme it is no more formal than *Argument from Sign*. Moreover, it is taught in much the same way that argument schemes are taught: the neophyte is presented with simple examples for illustration purposes. The format of the argument is presented, though the actual format for any particular proof by mathematical induction may only approximate the canonical form – for example, how many cases, if any, to consider. Next, one tries out the technique on more complicated cases. Finally, after long practice, one learns when the base case is trivial, when there are multiple cases, when cases are treated *symmetrically*, etc. Mathematicians may make the analytic element of argument assessment much easier by labeling their reasoning explicitly. Indeed, one could take the practice of using mathematical induction to be one in which the arguer does all of the steps of argument analysis for the audience. An argument's critic's only job is to assess the quality of the argument as presented.

Another open question regards whether, or to what extent, mathematics is dialectical. This issue is raised explicitly by Imre Lakatos (1976). But there are hints at dialectical considerations much earlier in the history of mathematics.

[23] Other possible mathematical argumentation schemes include: mathematical symmetry arguments and mathematical analogies (cf. Steiner 48ff).

> Let us first inquire why [Euclid] even includes in the theorem the equality of the angles under the base. He is never going to use this result for the construction or demonstration of any other problem or theorem. Since it will not be used later, why was it necessary to bring it into this theorem. To this question we must reply that, even if he was never intending to use 'and the angles under the base of an isosceles triangle are equal,' nevertheless it will be useful in *meeting objections* to them and *refuting* their adversaries. It is a mark of scientific and technical skill to arrange in advance for the undoing of those who attack what is going to be said and to *prepare the positions from which one can reply*, so that these previously demonstrated matters may later serve not only for establishing the truth, but also for *refuting error*. (Proclus: 192-3)

The idea that Euclid presents the propositions in an order that allows for better strategic, rhetorical or dialectical positioning suggests that dialectics have long been essential in mathematics – perhaps going all the way to the very beginnings of mathematics. The question for informal logicians is how best to account for the dialectic elements of mathematics. For example, Erik Krabbe (1991 and 1997) argues that proofs can be or ought to be recast as dialogues. The strategy of recasting these arguments as dialogue, i.e. of considering proofs to be implicit dialogues falls out of a general theory of reasoning championed by Krabbe (Walton and Krabbe 1995). The implicit dialogue strategy would account for the dialectical elements of proofs.

Whatever the answers to these questions are, it should be clear that mathematical reasoning is already in accord with principles and techniques from informal logic – even if this is unnoticed by the practitioners. Thus, it will be important, if one wants one's philosophy of mathematics to have an accurate account of mathematical practice, to include informal logic in the methodology of mathematics.

Author's Vitae

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