LEARNING FROM OUR MISTAKES—BUT ESPECIALLY FROM OUR FALLACIES AND HOWLERS

ANDREW ABERDEIN

And you claim to have discovered this . . .
from observations on sick people*?*
(Freud, 1990, p. 13)

Mathematical errors occur in many different forms. In his short treatment of the topic, E. A. Maxwell distinguished the simple MISTAKE, which may be caused by 'a momentary aberration, a slip in writing, or the misreading of earlier work', from the HOWLER, 'an error which leads *innocently* to a *correct* result', and the FALLACY, which 'leads by *guile* to a *wrong* but plausible conclusion' (Maxwell, 1959, p. 9). We might gloss this preliminary taxonomy of mathematical error as correlating the (in)correctness of the result to the (un)soundness of the method, as in Table 1.

	True Result	False Result
Sound Method	Correct	Fallacy
Unsound Method	Howler	Mistake

TABLE 1. A Preliminary Typology of Mathematical Error

However, there are a number of problems with this picture. Most significantly, a truly sound method would never lead to a false conclusion. This paper addresses these problems, and seeks to derive some general lessons from the treatment of the fallacy, the howler, and related sources of confusion.

1. FALLACIES

It might be supposed that mathematical fallacies could be defined very simply. If all mathematical reasoning is formal and deductive, then surely mathematical fallacies are merely invalid arguments? This definition has several shortcomings. Firstly, there are many invalid mathematical arguments which would not normally be described as mathematical fallacies. Secondly, much reasoning in mathematics is conducted informally. So a satisfactory account of mathematical fallacies must explain what is distinctive about formal fallacies, beyond their invalidity, and also address informal fallacies.

Most logic textbooks contain a chapter on informal fallacies. But this is typically no more than a catalogue of (not very) misleading arguments, which endures through successive editions, much as Oliver Wendell Holmes said of legal precedent, like 'the clavicle in the cat [which] only tells of the existence of some earlier creature to which a collar-bone was useful' (Holmes, 1991, p. 35). No attempt is made to provide an underlying theory which might explain their significance, their application, or their continued presence. However, in recent years fallacy theory has been reinvigorated. We shall see whether this new lease of life may be passed on to the study of the mathematical fallacy.

1.1. **The standard treatment.** Most textbooks echo the 'standard treatment' of fallacy, as an argument which '*seems to be valid* but *is not* so' (Hamblin, 1970, p. 12). This definition may be traced back to Aristotle, for whom 'that some reasonings are genuine, while others seem to be so but are not, is evident. This happens with arguments as also elsewhere, through a certain likeness between the genuine and the sham' (Aristotle, 1995, 164a). The problem with the standard treatment, which has brought it into disrepute, is that we have no systematic account of the apparently subjective and psychologistic concept of 'seeming valid'.

This problem is perhaps especially acute for mathematics. As the philosopher and novelist Rebecca Goldstein has her fictitious mathematician Noam Himmel declare,

> [I]n math things are exactly the way they seem. There's no room, no *logical* room, for deception. I don't have to consider the possibility that maybe seven isn't really a prime, that my mind conditions seven to appear a prime. One doesn't—can't—make the distinction between mathematical appearance and reality, as one can—must—make the distinction between physical appearance and reality. The mathematician can penetrate the essence of his objects in a way the physicist never could, no matter how powerful his theory. We're the ones with our fists deep in the guts of reality. (Goldstein, 1983, p. 95)

On this account, Aristotle's 'likeness between the genuine and the sham' could never arise, since there would be no sham. This would rule out the possibility of mathematical fallacies, at least on the standard treatment.

However, the difference between appearance and reality which Himmel believes impossible is not that upon which the standard treatment rests. Himmel is concerned with radical scepticism about mathematical truth: he thinks it just could not happen that, despite our best efforts, we were fundamentally wrong. His position is persuasive, although not indisputable, but it still leaves room for a weaker, error-based distinction between appearance and reality. Students and researchers alike frequently have the experience of some mathematical object appearing to be other than it turns out really to be—because they do not yet understand the matter. Himmel's argument might be paraphrased as the thesis that mathematical understanding guarantees mathematical truth: once one understands the concept of prime number, one realizes that seven must be prime. This is not true in natural science: understanding the concepts of ether, caloric or phlogiston does not make them real. But in both cases, if understanding is absent, truth may well be also. This leaves plenty of room for mathematical fallacies.

1.2. **Other distinctions.** Before considering alternatives to the standard treatment we will examine some further distinctions which may be drawn amongst fallacies. Many theorists distinguish the SOPHISM, which is intended to deceive, from the PARALOGISM, an innocent mistake in reasoning. This distinction is sometimes made part of the definition of specific fallacies. But that seems to miss the point: since a fallacy is a sort of argument, if the same argument is used, then the same fallacy should be committed. The speaker's intent should not make a difference. In fact, every fallacy can occur as either a sophism or a paralogism, since any fallacy can be used deliberately to deceive another, who, if he does not realize that he has been deceived, may guilelessly, if negligently, repeat the fallacy to a third party.

Dual to this distinction is one drawn by Francis Bacon:

> For although in the more gross sort of fallacies it happeneth, as Seneca maketh the comparison well, as in juggling feats, which, though we know not how they are done, yet we know well it is not as it seemeth to be; yet the more subtle sort of them doth not only put a man beside his answer, but doth many times abuse his judgment (Bacon, 1915, p. 131).

We may summarize this as a distinction between the GROSS fallacy, in which something seems wrong (and is) and the SUBTLE fallacy, in which everything seems OK (but is not). Just as the distinction between sophism and paralogism is a distinction confined to the speaker's intent, with no necessary reflection in the form of the fallacy, so that between gross and subtle fallacies is restricted to the auditor's understanding. Again, the same fallacy could fulfill either role: the sharper your judgment, the less likely it is to be abused. However, at least on the standard treatment, which Bacon's account explicitly echoes, the auditor's understanding, unlike the speaker's intent, is a part of the definition of fallacy, since it determines whether the fallacy seems valid to the auditor.

Reflection on Bacon's discussion may draw attention to a third case, that of SURPRISE, in which something seems wrong (but is not). This is a common phenomenon, and perhaps as great a source of error as actual fallacy. A popular supposition is that 'when the results of reasoning and mathematics conflict with experience in the real world, there is probably a fallacy of some sort involved' (Bunch, 1982, p. 2). This is a plausible inference, but it can be profoundly misleading. Once we outstrip the mathematics consonant with our experience of the real world, our everyday intuitions can no longer guide us. Wilfrid Hodges attributes the frequency of attempted refutations of Cantor's diagonal argument to such reasoning. 'Until Cantor first proved his theorem ... nothing like its conclusion was in anybody's mind's eye. And even now we accept it because it is proved, not for any other reason' (Hodges, 1998, p. 3). As this is often the first such proof enthusiastic amateurs encounter, prominent logicians and journal editors such as Hodges keep receiving their attempted refutations.

The distinction between gross and subtle fallacies, and the existence of surprises, demonstrate that it is not only the method of argument which may seem to be one thing while actually being another. The result of the argument is susceptible to similar confusion. Results which conflict with our prior beliefs will seem to be false, irrespective of their actual truth value. Conversely, results which reinforce those beliefs will be much easier to accept, even when they lack sound justification.

1.3. **Argument schemes.** Many alternative theories of fallacy have been suggested. One influential approach employs ARGUMENT SCHEMES: 'forms of argument that model stereotypical patterns of reasoning' (Walton and Reed, 2003, p. 195). The underlying idea has a venerable history. It can be traced back to Aristotle's *Topics* and its successive reappropriations in Cicero, Boethius and mediaeval logic (Macagno and Walton, 2006, p. 48). More recently, the tradition has been revived in informal logic. Several subtly different characterizations of the argument scheme (or argumentation scheme) are in use. These have given rise to independent, but largely overlapping classifications, the most extensive being that of Manfred Kienpointner, who distinguishes 58 different schemes (Kienpointner, 1992). We shall be following the somewhat more modest, but highly influential treatment of Douglas Walton, defended in several books and articles since the early 1990s.

Walton's argument schemes are presented as schematic arguments which are typically accompanied by CRITICAL QUESTIONS. The critical questions itemize known vulnerabilities in the argument, to which its proposer should be prepared to respond. In principle, they can always be incorporated into the schematic argument as additional premises (Walton and Reed, 2003, p. 202). This has advantages in formal implementations of argument schemes, but at the cost of obscuring the characteristic dialectical context in which the schemes are typically employed. Many of the schematic arguments are special cases of *modus ponens* in which the hypothetical premise lacks the force of a deductive implication. Hence most of Walton's argument schemes are presumptive or defeasible. But deductive inferences can also be understood as argument schemes. However, there is a great deal of reasoning in mathematics for which informal argument schemes would be more appropriate.

A crucial question for the argument scheme approach is that of normativity: what makes an instance of an argument scheme good or bad? The answer must in part depend on the nature of the scheme. If the scheme is deductive, then it is good precisely when it is valid, as we would expect. Defeasible schemes obviously have less normative force. There are several possible ways in which this might be captured. Broadly, Walton's approach is to say that a scheme is persuasive if all of its critical questions (or at least those where the burden of proof is on the proposer of the argument) receive satisfactory answers, and otherwise not. Hence restating defeasible schemes to incorporate the critical questions as additional premises will have the effect of converting them into deductive schemes (Walton and Reed, 2003, p. 210).

1.4. **From argument schemes to fallacies.** Fallacies may be understood as argument schemes used inappropriately. This cashes out the troublesome concept of 'seeming valid' in the stereotypical character of the schemes. Essentially, they have been chosen as representative of the sort of arguments generally found convincing.

The use of an argument scheme can be fallacious in two distinct ways. Firstly, some schemes are invariably bad. They are distinguished from other invalid arguments by their tempting character, presumably a consequence of their similarity to a valid scheme. This is typical of formal fallacies, such as the quantifier shift fallacy. Secondly, argument schemes with legitimate instances can be used inappropriately, characteristically when deployed in circumstances that preclude a satisfactory answer to the critical questions. For example, the argument scheme for Appeal to Expert Opinion is associated with a list of questions addressing the expertise of the source of the opinion. The traditional *ad verecundiam* fallacy arises when these questions are not properly answered. One substantial attraction of the argument scheme approach to fallacy theory is that it rehabilitates the conventional lists of fallacies as defective examples of defeasible but sometimes persuasive argument schemes.

Can all fallacies be analyzed in this way? Walton does not claim this. Indeed, he lists several informal fallacies which he says cannot be reduced to the misuse of a specific argument scheme:

(1) Equivocation
(2) Amphiboly
(3) Accent
(4) Begging the Question
(5) Many Questions
(6) *Ad Baculum*
(7) *Ignoratio Elenchi* (Irrelevance)
(8) *Secundum Quid* (Neglecting Qualifications) (Walton, 1995, pp. 200 f.)

Ad baculum seems out of place in this list, since it can be characterized as a defective instance of Walton's scheme for Argument from Threat, which is sometimes admissible in negotiation, but never in persuasion.[1] The other fallacies on this list are of two types. Equivocation, amphiboly, accent, and *secundum quid* are problems that can arise in many different argument schemes, and so cannot be characterized by any one scheme. Begging the question, many questions, and *ignoratio elenchi* need extended sequences of argumentation to become manifest, hence they are not associated with any single argument scheme either.

Nevertheless, all of these fallacies are within the scope of a slightly more broadly drawn argument scheme approach. The former kind, equivocation, amphiboly, accent, and *secundum quid*, can certainly be understood as arising from an inappropriately applied scheme, even if there are several possible schemes which it might be. Moreover, since they mostly seem to turn on definitions or verbal classifications which are ambiguous, insufficiently qualified, or question begging, they might be seen as characteristically besetting the Argument from Verbal Classification. (We shall discuss this scheme further below.) The latter sort, begging the question, many questions, and *ignoratio elenchi*, require us to broaden our scope from the individual scheme. But each fallacy might still be understood as a pathology of inappropriately applied argument schemes, even if we are unable to narrow the blame down to a single scheme.

Hence we have a means of restating the standard treatment without appeal to subjective or psychologistic content: 'seems good' may be analysed as 'employs argument schemes'. We may thereby define a fallacy as a defective instance of argument scheme(s) which may or may not be invariably defective.

Since we now have defensible accounts both of how propositions may seem true and of how arguments may seem sound we are in a position to update the typology offered in Table 1. These qualifications are incorporated in Table 2. In this table the combination of a sound method with a false result is labeled $\emptyset$, since we now have a rich enough classification to explicitly rule such cases out. If we concentrate on cases of mathematical error which arise where the result is false, whether or not it seems so, a simpler picture emerges, as displayed in Table 3.

1.5. **Applicability to mathematics.** There are two approaches to the application of argument schemes to mathematics. Firstly, we could develop specifically mathematical argument schemes which captured patterns of reasoning unlikely to arise in any other context. Secondly, we could explore the context-independent aspects of mathematical reasoning by attempting to apply context-independent argument schemes. We shall concentrate on the second approach, which promises

[1] Walton does ask whether the fallacy should be drawn more broadly, to include 'scaremongering tactics that do not involve a threat' (Walton, 1995, p. 41), but does not answer his own question.

Method *seems*	Method *is*	Result *seems*	Result *is*	
G	G	T	T	Proof
G	G	T	F	∅
G	G	F	T	Surprise
G	G	F	F	∅
G	B	T	T	Howler
G	B	T	F	Fallacy
G	B	F	T	Howler
G	B	F	F	Fallacy
B	G	T	T	Surprise
B	G	T	F	∅
B	G	F	T	Surprise
B	G	F	F	∅
B	B	T	T	Howler
B	B	T	F	(Tempting) Mistake
B	B	F	T	Howler
B	B	F	F	Mistake

Table 2. A Richer Typology of Mathematical Error

	Result seems True	Result seems False
Method seems Sound	Subtle Fallacy	Gross Fallacy
Method seems Unsound	(Tempting) Mistake	Mistake

Table 3. Mathematical Error Where Result is False

to help make explicit the connexions between mathematical practice and ordinary reasoning. However, the first approach is also of considerable interest.

Specifically mathematical argument schemes have many antecedents, both in argumentation theory and in mathematics. Finely tuned argument schemes have been developed by proponents of agent-based reasoning in the implementation of expert systems designed to tackle very specific problems. For example, bespoke argument schemes have been used to model decision making in the area of organ donation and transplantation (Tolchinsky et al., 2006). In mathematics, several independent approaches have led to the study of a variety of structures loosely comparable to argument schemes. In particular, the automated theorem-proving community has developed 'proof plans' and other models of common fragments of mathematical reasoning (Bundy, 1991). In philosophy of mathematics, the 'inference packages' introduced by Jody Azzouni as 'psychologically-bundled ways of phenomenologically exploring the effect of several assumptions at once' might also be construed as specifically mathematical argument schemes (Azzouni, 2005, p. 9).

Many mathematical fallacies exemplify invariably bad argument schemes. This is true whether we look for general schemes, such as quantifier shift, or more specifically mathematical operations, such as dividing by zero. Are there any mathematical fallacies in which the argument scheme is not invariably bad? An historical example of a fallacy manifesting as an inappropriate deployment of a specifically mathematical argument scheme may be found in the work of Isaac Newton, courtesy of George Berkeley's debunking of Newtonian analysis:

> Suppose the product or rectangle AB increased by continual motion: and that the momentaneous increments of the sides A and B are a and b. When the sides A and B were deficient, or lesser by one half of their moments, the rectangle was $\overline{A - \frac{1}{2}a} \times \overline{B - \frac{1}{2}b}$ *i.e.* $AB - \frac{1}{2}aB - \frac{1}{2}bA + \frac{1}{4}ab$. And as soon as the sides A and B are increased by the other two halves of their moments, the rectangle becomes $\overline{A + \frac{1}{2}a} \times \overline{B + \frac{1}{2}b}$ or $AB + \frac{1}{2}aB + \frac{1}{2}bA + \frac{1}{4}ab$. From the latter rectangle subduct the former, and the remaining difference will be $aB + bA$. Therefore the increment of the rectangle generated by the initial increments a and b is $aB + bA$. *Q.E.D.* But it is plain that the direct and true method to obtain the moment or increment of the rectangle AB, is to take the sides as increased by their whole increments, and so multiply them together $A + a$ by $B + b$, the product whereof $AB + aB + bA + ab$ is the augmented rectangle; whence, if we subduct AB the remainder $aB + bA + ab$ will be the true increment of the rectangle, exceeding that which was obtained by the former illegitimate and indirect method by the quantity ab. (Berkeley, 1996, ¶9)

Newton employs an ingenious procedure, for which there may be plausible applications, but as Berkeley correctly observes, this is not one of them.

More generally, it is a familiar phenomenon in mathematics that methods of widespread usefulness can produce paradoxical results in a minority of cases (see, for example, Maxwell, 1959, p. 51, or Barbeau, 2000, pp. 109 f.). We shall return to this issue in the discussion of howlers. But first we will seek to illustrate by example the context-independent approach to the application of argument schemes to mathematics.

2. Examples

There is no consensus on how best to classify argument schemes. Part of the problem is that there are several mutually independent dimensions of similarity which we might hope that a classification should respect. However, schemes may be loosely grouped in terms of the nature of the conclusions they establish. These include particular and general propositions to be accepted or rejected, actions to be performed, assessments of other arguments, causal claims, rules to be followed or ignored, and commitments to be ascribed to agents. Arguments of most or all of these kinds may be found in mathematical reasoning. Practicing mathematicians have long observed with J. J. Sylvester 'how much observation, divination, induction, experimental trial, and verification, causation, too ... have to do with the work of the mathematician' (Sylvester, 1956, p. 1762). Mathematics is not just the derivation of conclusions from axioms.

In this section we shall consider how some specific argument schemes may be applied to mathematics. Inevitably, the selection is narrow and unrepresentative, but should give a flavour of the approach.[2]

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[2]For consideration of the role played by a much wider range of argument schemes in mathematical reasoning, see Aberdein (2007).

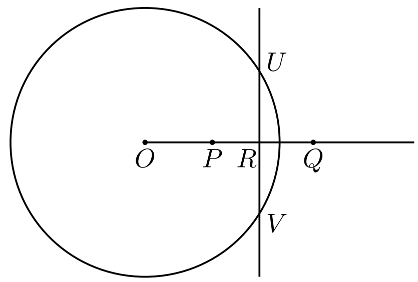

FIGURE 1. The Fallacy of the Empty Circle (Maxwell, 1959, p. 18)

2.1. Argument scheme for Argument from Verbal Classification.

Individual Premise: a has property F.
Classification Premise: For all x, if x has property F, then x can be classified as having property G.
Conclusion: a has property G.

CRITICAL QUESTIONS:

(1) What evidence is there that a definitely has property F, as opposed to evidence indicating room for doubt on whether it should be so classified?
(2) Is the verbal classification in the classification premise based merely on a stipulative or biased definition that is subject to doubt? (Walton, 2006, p. 129.)

In many mathematical cases, the premises will be provably true, and thus the argument deductively sound. But in informal mathematical reasoning this need not be the case. The classification premise might be a hypothesis, well corroborated, but as yet unproven, or a hunch, or even something known to have exceptions, but which seems plausible enough in context. The classification premise could also be a definition. This is harmless if the definition is uncontested, but can give rise to abuse. In Dana Angluin's widely circulated list of spurious proof types, this is neatly characterized as 'proof by semantic shift: some of the standard but inconvenient definitions are changed for the statement of the result' (Angluin, 1983, p. 17). Such stipulative definition clearly precludes a satisfactory answer to the second critical question.

On the other hand, many mathematical fallacies turn on question-begging definitions, which smuggle in improbable conclusions disguised as innocuous classification premises. For example, consider Maxwell's 'Fallacy of the Empty Circle':

> *To prove that every point inside a circle lies on its circumference.*
>
> GIVEN: A circle of centre O and radius r, and an arbitrary point P inside it (Fig. 1).
>
> REQUIRED: To prove that P lies on the circumference.
>
> CONSTRUCTION: Let Q be the point on OP produced beyond P such that $OP.OQ = r^2$ and let the perpendicular bisector of PQ cut the circle at U, V. Denote by R the middle point of PQ.

PROOF:

$$\begin{aligned}
OP &= OR - RP \\
OQ &= OR + RQ \\
&= OR + RP \quad (RQ = RP, \text{construction}) \\
\therefore OP.OQ &= (OR - RP)(OR + RP) \\
&= OR^2 - RP^2 \\
&= (OU^2 - RU^2) - (PU^2 - RU^2) \quad \text{(Pythagoras)} \\
&= OU^2 - PU^2 \\
&= OP.OQ - PU^2 \quad (OP.OQ = r^2 = OU^2) \\
\therefore PU &= 0
\end{aligned}$$

$\therefore P$ is at U, on the circumference (Maxwell, 1959, pp. 18 f.).

This can be translated into the scheme as follows: a is the arbitrary point P; F is the property that there exists a point Q on OP produced beyond P such that $OP.OQ = r^2$; G is the property of being on the circumference. The fallacy may now be clearly understood. The classification premise is true and supported by the proof. But the individual premise is false: F is not a property of arbitrary points, but only of points on the circumference. Supposing otherwise is the critical loss of generality on which the argument turns. The first critical question will clearly receive an unsatisfactory answer.

2.2. **Argument scheme for Argument from Popular Opinion.**

General Acceptance Premise: A is generally accepted as true.
Presumption Premise: If A is generally accepted as true, that gives a reason in favor of A.
Conclusion: There is a reason in favor of A.

CRITICAL QUESTIONS:

(1) What evidence, such as a poll or an appeal to common knowledge, supports the claim that A is generally accepted as true?
(2) Even if A is generally accepted as true, are there any good reasons for doubting it is true? (Walton, 2006, pp. 91 f.)

There is an important distinction to be made between cases where the opinion of the majority is *constitutive* of the truth of A, and all other cases. Thus, when a plebiscite was held to resolve the Schleswig-Holstein question, the opinion of the populace as to whether they were German or Danish was constitutive of the right answer. But a referendum over the moral status of abortion, or indeed the value of π, cannot be defended in this fashion: the popularity of the answer would be independent of its truth, a failure to satisfactorily answer the second critical question.

Are all mathematical questions of the latter sort? No. Exceptions include widely endorsed arbitrary conventions. Many of these, such as axioms, have a claim to self-evidence (whatever that means) that militates against their arbitrariness—they're not true because everyone says so, they're true because they can't but be true. So, not only do we not have reason to doubt them, we have better reasons to believe them than can be provided by this scheme. But there are cases where this scheme may be as good as it gets. For example, the definition of 'straight edge and compass construction'. The impossibility of certain constructions, such as trisecting an arbitrary angle, is a well-established result. But angle trisectors still claim to have produced such constructions, sometimes through sheer incompetence, but in the more adroit cases through equivocation on 'straight edge and compass construction'. They produce a 'construction', using a 'straight edge' and a 'compass', but in a non-standard fashion, hence it is not a 'straight edge and compass construction': a point they may find hard to accept. So, to say that an arbitrary angle cannot be trisected invokes a convention on what counts as a construction. If that's given up, then an arbitrary angle *can* be trisected, but that's not the historically interesting problem. Why not? Because it's not what mathematicians generally accept. So the best argument available in defence of the convention will be an instance of this scheme.

Another example is provided by Hodges's response to analogous attempts to refute the diagonal argument:

> Other authors, less coherently, suggested that Cantor had used the *wrong positive integers*. He should have allowed integers which have infinite decimal expansions to the left, like the p-adic integers. To these people I usually sent the comment that they were quite right, the set of real numbers does have the same cardinality as the set of natural numbers in *their* sense of natural numbers; but the phrase 'natural number' already has a meaning, and that meaning is not theirs (Hodges, 1998, p. 4).

Here Hodges's counter-argument is an instance of Argument from Popular Opinion, whereas the anti-diagonalization argument he is criticizing is an instance of Angluin's 'proof by semantic shift,' that is a defective instance of Argument from Verbal Classification.

2.3. **Argument scheme for Argument from Popular Practice.**

> **Premise:** A is a popular practice among those who are familiar with what is acceptable or not with regard to A.
> **Premise:** If A is a popular practice among those familiar with what is acceptable or not with regard to A, that gives a reason to think that A is acceptable.
> **Conclusion:** Therefore, A is acceptable in this case.
>
> CRITICAL QUESTIONS:
> (1) What actions or other indications show that a large majority accepts A?
> (2) Even if a large majority accepts A as true, what grounds might there be for thinking they are justified in accepting A? (Walton, 2006, pp. 93 f.)

This special case of Argument from Popular Opinion, to which similar considerations apply, arises when it is a practice not a proposition that is at issue. As with that scheme, where majority practice is constitutive of correctness, this scheme may be legitimately invoked against those whose practice deviates from the majority. An important example is the enforcement of contemporary standards of rigour. Consider the dispute over Wu-Yi Hsiang's alleged proof of Kepler's conjecture, that the maximum density of a packing of congruent spheres in three dimensions is $\pi/\sqrt{18}$. In his review of the proof, Gábor Fejes Tóth employs Argument from Popular Practice:

> Hsiang might consider this objection 'a dispute about subjective standards of how much detail a properly written mathematical proof has to contain' ... However, he has to bear in mind that a mathematical proof is a social process: It is only the acceptance by the mathematical community which affirms the legitimacy of a proof (Fejes Tóth, 1995).

The remark of Hsiang's which Fejes Tóth quotes may be understood as an attempt to find a basis for challenging the argument along the lines indicated by the second critical question: if the standards of proof are ultimately subjective, or at least if Hsiang's proof is within the range of subjectively acceptable proof, then the views of the majority are irrelevant.

In other circumstances, Argument from Popular Practice is notably weak, although it can still be useful, especially in resource-limited contexts: following the crowd can be the simplest way to get where you want to go. For example, it might be used in defence of time-honoured 'trade craft': practical heuristics that have been found useful by generations of teachers, students, and professionals, such as the LIATE or ILATE rule for integration by parts. Of course, this scheme can only provide a weak, and potentially misleading justification for such material. Where the heuristic is genuinely useful, there must be a more robust rationale for it. In some cases such practices can degenerate into ritual, precisely because they are warranted *only* by this scheme, the use of which has obscured the absence of proper justification.

3. Howlers

Having addressed some of the questions raised by fallacies, we shall move on to howlers. These are less immediately problematic, since there is no contradiction in an unsound method leading to a correct conclusion. However, we shall see that they lead to deep issues concerning the nature of mathematical knowledge.

3.1. **A shopping list and other howlers.**

Maxwell introduces his discussion of the howler with the following now rather nostalgic example:

> *To make out a bill:*
>
> | | |
> |---|---|
> | $\frac{1}{4}$ lb. butter | @ $2s.\ 10d.$ per lb. |
> | $2\frac{1}{2}$ lb. lard | @ $10d.$ per lb. |
> | 3 lb. sugar | @ $3\frac{1}{4}d.$ per lb. |
> | 6 boxes matches | @ $7d.$ per dozen. |
> | 4 packets soap-flakes | @ $2\frac{1}{2}d.$ per packet. |
>
> The solution is
>
> $$8\tfrac{1}{2}d. + 2s.\ 1d. + 9\tfrac{3}{4}d. + 3\tfrac{1}{2}d. + 10d. = 4s.\ 8\tfrac{3}{4}d.$$
>
> One boy, however, avoided the detailed calculations and simply added all the prices on the right:
>
> $$2s.\ 10d. + 10d. + 3\tfrac{1}{4}d. + 7d. + 2\tfrac{1}{2}d. = 4s.\ 8\tfrac{3}{4}d.$$
>
> (Maxwell, 1959, p. 9)

The ingenious schoolboy has discovered a technique much simpler than his teacher's, and which still gets him the right answer. Unfortunately, it does so because of a peculiarity of the formation of the question. Had the shopping list been slightly different then his technique would have been unsuccessful.

In some cases the implied technique holds greater interest:

To solve the equation

$$(x+3)(2-x)=4.$$

$$\begin{array}{lll} \textbf{\textit{Either}} & x+3=4 & \therefore x=1, \\ \textbf{\textit{or}} & 2-x=4 & \therefore x=-2. \end{array}$$

Correct. (Maxwell, 1959, p. 88)

As Maxwell observes, the perpetrator of this howler has innocently stumbled on something fascinating. Although the technique obviously does not work in general, every quadratic equation can be expressed in a form for which this technique would work: $(1+q-x)(1-p+x)=1-p+q$, for an equation with roots p, q.

Not all of Maxwell's howlers are of this sort. Some of those 'perpetrated innocently in the course of class study or of examination' (Maxwell, 1959, p. 88) defy the reconstruction of any underlying technique. For example:

To solve the equation

$$\begin{array}{rcl} x^2+(x+4)^2 & = & (x+36)^2. \\ x^2+x^2+4^2 & = & x^2+36^2 \\ \therefore x^2+x^2+16 & = & x^2+336 \\ \therefore x^2+x^2-x^2 & = & 336-16 \\ \therefore x^4 & = & 320 \\ \therefore x & = & 80. \end{array}$$

Correct. (Maxwell, 1959, p. 93)

Here one might suspect that the student, having fortuitously struck upon the right answer, has constructed a sequence of *non sequiturs* which superficially resembles proofs he has been taught. Some mathematics educationalists refer to such behaviour as exhibiting a 'ritual proof scheme' (Harel and Sowder, 1998, p. 246).[3] One might also see in the student's surreptitious indifference to the truth values of his intermediate statements an instance of what Harry Frankfurt has more pithily described as 'bullshit' (Frankfurt, 1988, p. 130).

Clearly there are several distinct phenomena that meet our original characterization of the howler, that is that lead by an unsound method to a correct result. Some of these are more epistemologically problematic than others. Firstly, there are cases such as that discussed last where there is no discernible method, or if there is a method, it is inherently spurious. This is a straightforward instance of unjustified true belief, which must fall short of knowledge on any conventional definition. Secondly, there are cases where an explicit procedure is followed, which happens to work in the specific case, but not generally. These pose somewhat more of a problem, since, although the more blatant examples, such as those discussed above, might seem to be unjustified, this becomes harder to maintain the more correct cases there are. We might insist that this falls short of the standards of justification required for rigorous mathematical knowledge, but does that mean that the result might be known in some other way? Moreover, these cases differ only in

[3] If proofs are arguments, are proof schemes argument schemes? No, because 'proof' and 'argument' both exhibit a 'process-product ambiguity' (Van Eemeren et al., 1984, p. 7) which the two sorts of scheme resolve differently. Proof schemes concern proof as process; argument schemes argument as product (*pace* Reed and Walton, 2004).

degree from procedures that work in all *except* a few specific cases. Provided that the case in question was not one of these exceptions the result would be correct, but would it still qualify as knowledge if the exceptions are not ruled out? Applied mathematicians are frequently insouciant about such risks:

> [M]any books dealing with Fourier's series continually repeat the condition that the function must not have an infinite number of maxima and minima. We have generally omitted specifying this condition, since no practical function ever does behave in such a manner. Such behaviour is exclusively confined to functions invented by mathematicians for the sake of causing trouble. (Albert Eagle, 1925, *A Practical Treatise on Fourier's Theorem and Harmonic Analysis for Physicists and Engineers*, quoted in Barbeau, 2000, p. 142).

Lastly, there are some procedures which always work, yet we cannot explain why. This was Berkeley's complaint about the calculus. 'For science it cannot be called, when you proceed blindfold, and arrive at the truth not knowing how or by what means' (Berkeley, 1996, ¶22). By Berkeley's reckoning even these cases would fail to qualify as knowledge, but, despite the manifest shortcomings of Isaac Newton's method, it seems perverse to insist that he did not know that the derivative of x^2 is $2x$. Most failures of rigour are less extreme than that of seventeenth century calculus, but thereby that much more difficult to see as precluding knowledge.

3.2. **Mathematical luck.** One way of posing the questions raised at the end of the last section is in terms of luck. Is all non-rigorous mathematics lucky? This would include not only historical proofs that fall short of modern standards of rigour, but also contemporary informal mathematical reasoning, unless a rigorized version is readily available. Much of both sorts of mathematics would generally be regarded as suasive, despite not exhibiting maximal rigour, so it seems strange to say its results are obtained by luck. There are several possible answers to this question.

We could just accept the characterization. Perhaps, at least in the strictest sense, such results are lucky. If rigour is required for certainty, and the argumentation in question is non-rigorous, its conclusions cannot be certain. Without certainty, there is always the possibility, however slight, of contradiction. That no such contradiction arises might be regarded as luck: the ability of a conjecture to survive repeated testing does not have the force in mathematics that it has in empirical science. Long-standing conjectures have turned out not only to have counterexamples, but to have no actual examples. Gian Francesco Malfatti conjectured in 1802 that to maximize the sum of the areas of three circles inscribed in a triangle, each circle must touch exactly two sides of the triangle and both of the other circles. This was accepted for well over a century, until a counterexample was found in the 1960s, and in 1967 it was shown that an arrangement of greater area than any complying with the Malfatti conjecture could be found for any triangle (Aste and Weaire, 2000, p. 126). Moreover, as we saw above, what counts as rigour is at least in part socially determined and historically contingent. As Roy Sorensen observes, a mathematician developing a novel technique, as Cantor was with his diagonal argument, cannot always anticipate whether it will be accepted by the community (Sorensen, 1998, p. 332). This makes the cogency of his proof partly a matter of luck.

Alternatively, the characterization could be resisted by a thorough-going platonism. The platonist may argue that the proof on the page is not the *real* proof, an abstract object of which it is a more or less imperfect reflection. If the mathematician has genuinely apprehended such an entity, then he can be certain of its results. That the proof he writes down fails to capture every nuance of its platonic counterpart is a comparatively minor, and in principle remediable, detail. However, this degree of metaphysical extravagance poses its own well-known problems.

Another possibility would be to pursue reliability without rigour. If the argumentation is reliable, then its results are not lucky. This smacks of Berkeley's explanation of the success of the calculus in terms of a 'compensation of errors,' whereby errors made at different stages systematically cancel each other out. Berkeley suggested that this approach might yet be formalized to yield a rigorous foundation for the calculus, a proposal taken seriously by some subsequent mathematicians, notably Lazare Carnot, although with little success (Grattan-Guinness, 2000, p. 102). Of course, if such rigorization succeeds, then the problem of luck would no longer arise. But if it does not, then the problem has not been solved. In general, this proposal embodies a fundamental and dangerous confusion. One may advocate the use of informal or defeasible systems of reasoning to capture non-rigorous inference in mathematics without remotely suggesting that such systems might be substituted for deductive logic, or obviate the need for mathematical rigour.

Perhaps the underlying difficulty is that we do not have a clear enough understanding of what epistemic luck means, or why it is inconsistent with knowledge.

3.3. **Epistemic luck.** One recent influential treatment distinguishes between six different varieties of epistemic luck, of which the first four are argued to be consistent with knowledge:

> ***Content Epistemic Luck:*** It is lucky that the proposition is true.
> ***Capacity Epistemic Luck:*** It is lucky that the agent is capable of knowledge.
> ***Evidential Epistemic Luck:*** It is lucky that the agent acquires the evidence she has in favour of her belief.
> ***Doxastic Epistemic Luck:*** It is lucky that the agent believes the proposition.
> ***Veritic Epistemic Luck:*** It is a matter of luck that the agent's belief is true.
> ***Reflective Epistemic Luck:*** Given only what the agent is able to know by reflection alone, it is a matter of luck that her belief is true. (Pritchard, 2005, p. 134; 136; 138; 146; 175).

These definitions require some unpacking, not least to explain how 'lucky' is to be understood:

(L1) If an event is lucky, then it is an event that occurs in the actual world but which does not occur in a wide class of the nearest possible worlds where the relevant initial conditions for that event are the same as in the actual world.

(L2) If an event is lucky, then it is an event that is significant to the agent concerned (or would be significant, were the agent

> to be availed of the relevant facts). (Pritchard, 2005, p. 128; 132).

The second condition is unproblematic, but the first complicates application to mathematics, in which the propositions are necessary. Notice however, that three of the itemized varieties of luck do not attribute luck to propositions directly. We shall address these first.

Capacity Epistemic Luck arises when the agent is only fortuitously in a fit state to acquire knowledge, as for example when he narrowly avoids a fatal accident. Clearly this does not make the knowledge he acquires any less knowledge, whether or not it is mathematical in nature. Such cases are unrelated to those under discussion. Evidential Epistemic Luck turns on the lucky apprehension of the evidence supporting the agent's belief. Again, if the evidence is sufficient justification, the belief is unproblematically knowledge. The precise analogue for evidence in mathematical epistemology is open to dispute, but it is clear that this sort of luck is an issue in the context of discovery, not of justification. There are certainly mathematical discoveries which were lucky in this sense, perhaps because the discoverer combined essential experience of two recondite and unrelated fields, but this is not what we are concerned with either. Doxastic Epistemic Luck arises when the agent might in similar circumstances not have formed the belief, despite having all the same data at his disposal. Again, this is consistent with knowledge. It also describes a very familiar experience in mathematics, which is possibly that of the cruder sort of howler. But it does not seem to describe the cases of insufficient rigour, where the agent is following an entirely familiar procedure, such that his belief formation process is routine and predictable.

In the other three varieties, the luck is attached to the proposition, making the application of (L1) problematic. As it stands this constraint is trivial for necessary propositions, since these are by definition true in all possible worlds, and thus in all nearby ones. One avenue would be to restate the condition in terms of impossible worlds, in which necessary propositions might be false. This would avoid triviality, but generates its own difficulties, including the characterization of 'nearby' in this context. A more attractive approach might be to rewrite the condition without appeal to worlds, possible or otherwise. We might focus instead on variation in the statement of the situation.

However this is achieved, Content Epistemic Luck does not seem to have anything to do with howlers. The scenario envisaged here is that of a highly unlikely truth, such as a far-fetched coincidence. Clearly if it is true, its unlikelihood is no obstacle to its truth. Propositions of this character arise in empirical science—they are the basis of the so-called Anthropic Principle. One might dispute whether they have a place in mathematics, or whether apparent coincidences are merely indications of our limited understanding. But they are not howlers. Reflective Epistemic Luck is concerned with 'knowledge' obtained without reflective awareness of the process underlying its acquisition. This is closer to the howler, and could describe some unusual howler-like propositions, for example beliefs obtained by an infallible but inscrutable intuition. Some worries about computer-assisted proof might be related to this form of luck as well. But in most of the more serious cases of anxiety over insufficient rigour, the risk would seem best characterized as Veritic Epistemic Luck. The agent is concerned that, despite his best efforts, his beliefs may not be true.

If the hope was that mathematical luck would prove to belong exclusively to benign varieties of epistemic luck, then it has been dashed. We have established that epistemic luck represents a genuine problem for mathematical knowledge.

4. Summary

In conclusion, these observations on the pathologies of mathematical reasoning have led to several significant theses:

- Sensitive treatment of fallacies and howlers brings to light a richer typology of mathematical error.
- Mathematical fallacies may be better understood in terms of argument schemes. This is potentially an extremely powerful device for the understanding of mathematical reasoning, whether formal or informal.
- The howler is an instance of epistemic luck. As such it raises hard questions for the epistemology of mathematics.

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Humanities and Communication, Florida Institute of Technology, 150 West University Blvd, Melbourne, Florida 32901-6975, U.S.A.

E-mail address: aberdein@fit.edu