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Can we have a Theory of Fallacies?

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Abstract In his *Fallacies*, C.L. Hamblin asserts that «we have no *theory* of fallacy». In light of his discussion of «the nailing problem», regarding the difficulties of pinning an argument on a speaker, of determining the form that such an argument might display, and of evaluating arguments that do not display the forms characteristic of known systems of (deductive) logic, I consider why one means to discredit an argument, which we may call «refutation by analogy», is insecure. Refutation by analogy invites us to construct an argument that is on all fours with the argument to be discredited, but that has true premise(s) and false conclusion. The insecurity of this procedure emerges both from a fatal indeterminacy about the phrase «on all fours» insofar as that invokes the problematic notion of there being such a thing as *the* form to be attributed to any given argument, and from the way that the most obvious form attributable to refutation by analogy appears to be on all fours with at least one argument that has a true premise and a false conclusion. Because refutation by analogy refutes itself, I suggest that the best we can hope for is a folklore of fallacies.

Keywords: Fallacy, Invalidity, Refutation, Analogy, Logical Form

1. Theories of fallacy and research into fallacies

Despite being out of print for long periods, C.L. Hamblin's monograph *Fallacies* (HAMBLIN 1970) has exerted an enormous influence on the study of argumentation over the last forty or so years. While historical research has added to the detailed understanding of some of Hamblin's sources[1], and speculative-normative research has developed some of his suggestions about formal dialectic[2], my concern in the present paper is to offer an explanation of why, as Hamblin notes, we have no theory of fallacies.

When he says on p. 11 of his book «we have no *theory* of fallacy», Hamblin puts the word «theory» in italics but does not there elaborate on quite what he means by a «theory», saying rather that «we have lost the doctrine of the fallacy, and need to rediscover it». In setting out what he describes as the Standard Treatment of fallacy, Hamblin diagnoses the loss of the doctrine in question as a result of an «incoherent» tradition (*ivi*: 50). Likewise, a critic hostile to Hamblin's treatment of the Standard

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[1] E.g. (EBBESEN 1981); (STUMP, SPADE, 1982); (FAIT, 2007).
[2] E.g. (RESCHER, 1977); (HANSEN, PINTO, 1995); (VAN EEMEREN, GROOTENDOORST, 2004).

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Treatment and writing some twenty years after the publication of *Fallacies*, noted
that it was still true that there was «no theory of fallacy», associating that lack with
the lack of a «coherent doctrine of fallacy» (JOHNSON 1990: 164). Some twenty
years further on, I too am prepared to say that we have no theory of fallacy. The
interesting research, both historical and speculative-normative, that has been done in
the wake of Hamblin's book has not, as a matter of fact, produced a theory of fallacy.
I think that this fact calls for some explanation. The thesis of the present paper is that
the explanation of this apparently contingent fact is that it is not contingent at all, but
a matter of necessity: no-one has a theory of fallacies because there cannot be a
theory of fallacies.

Hamblin gives some clues to what would be meant by a theory of fallacy when he
returns to the question most explicitly in chapter 6 («Formal Fallacies»), where he
asks «whether any general and synoptic theory of fallacy can be extracted from
formal studies or stated in formal terms» (HAMBLIN 1970: 193), and he expects a
negative response. The two reasons that Hamblin provides in support of the thesis
that there cannot be a theory of fallacy are, in the first place, that the rules of any
given logical system can do no more than vindicate certain arguments as acceptable
relative to that system, and, second, that ordinary-language arguments have to be
«brought into relation» (*ibidem*) with the canonical expressions of some formal
system in order to be judged in relation to the rules of that system. Hamblin appears
to think that the process of «bringing into relation» may be best thought of not so
much in terms of an operation of translation or of interpretation (HAMBLIN 1970:
219-221), as rather in terms of procedures of supplementation, of regulation and of
adjustment, which cannot themselves be set out in the formalism of choice.

From the reasons that Hamblin provides for his thesis that there cannot be a theory of
fallacy, we can see what it would mean for there to be a theory of that sort. Such a
theory would be some formalism that is both general and synoptic in which the
fallaciousness of fallacious arguments could be determined, and that is based on
some scheme for bringing ordinary-language arguments into relation with the
canonical expressions of a formal system. Even if I can manage to bring out why we
cannot hope for, and hence should not be looking for, a theory of fallacy in the sense
given, I should make it clear that this does not mean that we should give up research
into fallacies, either by way of historical enquiry into what Aristotle and his
successors in the tradition of sophistical refutations were up to, or by way of the
speculative elaboration of rules for regulating debating practices. These are areas
where progress has been made since Hamblin's book was published. But they have
not brought us any closer to a theory of fallacy. I return in the final section of this
paper to look at the status of the results of these sorts of research, and to suggest how
they might be mobilised to help us be on guard against fallacious reasoning.

But first, we should try to get clear about the obstacles in the way of a «general and
synoptic theory» for determining the fallaciousness of certain ordinary-language
arguments.

2. The «nailing» problem

At the beginning of his chapter 7 («The Concept of Argument»), Hamblin adverts to
«the problem of "nailing" a fallacy» (*ivi*: 224), by which he means in the first
instance that a person proffering a consideration to which it is objected that he has
committed a fallacy (in the case imagined, an *argumentum ad hominem*) «cannot be
convicted of fallacy until he can have an *argument* pinned on him» (*ivi*: 224-225).

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And he asks «what are the criteria of that?» (*ivi*: 225), but he leaves the question hanging. When he returns at the end of the chapter to «the problem of "nailing" a fallacy» (*ivi*: 251), he seems to mean something slightly different from the question of the criteria for «pinning» an argument on someone, but rather the question of whether any «argument ever is knock-down» (*ibidem*) in the sense of its being such that anyone exposed to it must – in some strong sense of «must» – be convinced by it. Given this slight instability in Hamblin's use of «nailing» a fallacy or «pinning» a fallacy on someone, or «convicting» someone of having committed a fallacy, we may allow ourselves a little liberty in reformulating what we may call «the "nailing" problem».

One way of presenting the «nailing» problem takes off from the idea that arguments and, more in general, inferences have what is often called a «form» or a «structure». The idea is not a bad one, though it is rarely formulated with much determinateness. Roughly speaking, the idea is that the form or the structure of a given argument or inference is what that argument or inference has in common with other arguments or inferences of the same form or structure. Another way of putting it, equally roughly, is that the form or structure of an argument or inference is what there is to it that does not have to do with the particular subject-matter of the elements that make up the argument or inference. There are slightly fancier ways of expressing the same idea, which employ some terms of the logician's art, but these terms do not make the idea expressed much less rough. For instance, one might say that form or structure of an argument is determined by the interrelations of the categorematic and the syncategorematic terms that it contains. Equally fancy and no less rough is the notion that form or structure is a matter of how the constants and variables are distributed within the argument or inference. Alternatively, we might admit the roughness and content ourselves with talking about «structural elements» as distinct from substantive or subject-specific elements. All these ways of speaking remain rough and indeterminate until we have some account of sameness-of-structure, of subject-matter, of how to distinguish categorematic from syncategorematic terms, of how to distinguish constants from variables or of how to distinguish structural elements from others. For accounts of these notions await determination by the elaboration of formal systems of logic. And different systems will pick up different features of arguments as the forms or structures that are systematised.

Can we, nevertheless, have a general account of the form or structure of any argument or inference whatever? Yes. As follows:

> (F)   Something (therefore) Something

The «therefore» is placed in brackets because it needs not be made explicit or it may be replaced by «hence», «so», «*ergo*», «∴», a horizontal line or some other inference markers (perhaps a picture of a koala bear). (F) does not tell us whether the Something before the inference-marker is the same as or different from the Something after it. Nor does it tell us whether either of the two Somethings themselves have any internal form or structure. Conventionally, we say that the Something before the inference marker represents the (set of) premise(s) and that the Something after represents the conclusion of the argument or inference.

If, therefore (and I use the word advisedly), some linguistic act is to be counted as an argument or an inference, it will, as they say, display the form or structure displayed by (F). Another way of putting this is to say that, for any argument or inference, we can «nail» or «pin» or «convict» it of being of the same form or structure as

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displayed by (F) insofar as it presents a passage from some premise(s) to some conclusion.

With the determination, within the various logical systems, starting with Aristotle's syllogistic, that have been developed of the notions of sameness-of-structure, of subject-matter and the specification of syncategorematic/categorematic or constant/variable distinctions, we have learnt that some inferences that display the form or structure displayed by (F) also display forms or structures such that, given certain Somethings, Something else is of necessity in virtue of their being so (Arist., *An. Pr.*, I, i, 24b 19-22; cf. *Top.*, I, i, 100a 25-27). In the light of these sorts of developments, we can «nail» some inferences as displaying the features that a given system systematises. For instance, some arguments that we encounter in everyday life can be re-managed to exhibit the form or structure of a syllogism in Barbara (or, indeed, Baroco). Others might lend themselves to being re-cast in the form of *modus ponendo ponens*. And, if we wait long enough, we might even overhear someone arguing in line with the Barcan Formula. But none of these displays tells us what «the» form of the inference in question is because every argument or inference can be represented as displaying at least the form (F) and then, perhaps, some other. If one of the other forms is that of Barbara, of *modus ponendo ponens* or of the Barcan Formula then so much the better for the inference (though the last case may not have pleased Quine). For, in such cases, we may (but in the last case, perhaps we need not) say that the Something that comes after the inference-marker is implied by, follows from, or can be deduced from the Something that comes before it.

What I have been describing is, of course, what we do when we try to show that a given argument is, as we moderns say, valid. That is, when an argument is, as Hamblin says, «brought into relation» with a certain formalism and found to display a form or structure that that formalism indicates as valid, then we can «nail» the argument as displaying a form or structure that is valid in that formalism. In those happy cases in which validity in the given formalism is also a guarantee that, given certain Somethings, Something else is of necessity in virtue of their being so (i.e. the formalism is semantically secure), then we can «nail» the argument as valid *tout court*. Though a valid form should not have invalid instances brought into relation with it, an invalid form can have valid instances brought into relation with it, namely those that can be brought into relation with some other valid form.

But the happy cases are relatively rare and the work of bringing an ordinary-language argument into relation with a formalism that provides the relevant guarantee is not itself guaranteed by the formalism. Which I take to be equivalent to Hamblin's second reason for the thesis that a theory of fallacies is impossible, because a theory that is general, synoptic and formal of the «bringing into relation» relation is impossible.

3. Some vagaries of the «bringing into relation» relation

We may offer two observations about the operations of bringing an ordinary-language argument into relation with a formal system.

One is that it is child's play to bring any argument or inference whatsoever into relation with a formalism that does provide a guarantee that, given certain Somethings, Something else is of necessity in virtue of their being so. Take any argument that we may represent as of the form or structure «*p* therefore *q*», a pure instance of (F) so to say. It does not take much to bring this into relation with a system of propositional logic that encompasses the rule *modus ponendo ponens* in

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such a way that, in the process of rendering explicit an allegedly suppressed premise, we supply the formula «if p, then q». We thus have the premise-set made up of «p» and «if p then q», and we can say that one thing that follows from it is «q», which is the conclusion of the argument that we have brought into relation with a formalism that we happen to know has the happy characteristic of semantic security.

The other point, to be further elucidated, is that even though the «nailing» of an argument or inference as valid relative to some guarantee-providing formalism is a rare enough feat, it is still to be seen whether it is possible to «nail» an argument or inference as invalid, perhaps by bringing it into relation with every possible semantically secure formalism and *not* finding that it displays a valid form or structure in any of them. For this would be the only guaranteed way to «nail» the argument as fallacious. Even stated in this preliminary way, it should be obvious that, given that we have no idea what it would be to have at our disposal «every possible semantically secure formalism», still less what it would be to bring any argument into relation with all of them, the prospects for invalidity proofs seem dim indeed.

Not only are the happy cases in which an ordinary-language argument can be «nailed» as valid fairly rare, these cases present strong similarities one to another. The simplest explanation of these similarities is the fact that we have at our disposal only a fairly limited range of logical systems with the happy characteristic of semantic security, that is, of guaranteeing that, given Something, Something else is of necessity in virtue of its being so.

The three systems already alluded to in giving examples of cases in which an argument can display a form that guarantees semantic security for some passages between premises and conclusion (Aristotelian syllogistic, the propositional calculus and quantified modal logic) do not by any means exhaust our resources of formalisms into relation with which ordinary-language arguments can be brought with a view to assessing validity. But at least the first two are central cases of what is uncontroversial in formal logic, and most other formalisms, including quantified modal logics, even when they are outcrops or extensions of these deductive systems, are themselves harder to assess than might be the inferential goodness (or otherwise) of a large range of ordinary-language arguments.

If an ordinary-language argument turns on some form or structure that can be brought into relation with one or other of our core formalisms, then «nailing» it as valid can be relatively secure. Consider then an argument such as the following:

     (D*)   All Hellenes are men (therefore) All Greeks are mortal.

To bring (D*) into relation either with Aristotelian syllogistic or with first-order predicate logic, we have to perform two principal operations on it. One is to supply a suppressed premise rather as we did with «if p then q» to help «p therefore q» along. And the other is to regularise the synonyms «Hellenes» and «Greeks», choosing just one of them to occur in both premise and conclusion. Once we have performed these operations of bringing (D*) into relation with one or other of our core formalisms, we will have «nailed» it as valid, because it displays a form or structure that is valid in those formalisms. The reasonableness of adding the premise «All men are mortal» and of ironing out the difference between «Hellenes» and «Greeks» is such that someone who proposes (D*) may be allowed to have offered an argument that is in only slightly ragged logical order. This is because «all» is salient in (D*), and

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Aristotelian syllogistic and the universal quantifier of first-order predicate logic are happy to have arguments in which «all» is salient brought into relation with them.
The point remains, however, that reasonableness and salience are not themselves features that either syllogistic or predicate logic can contemplate: they are not formal features and we cannot have a theory, in Hamblin's sense, of either of them.

4. «Nailing» invalidity by analogy
Why have I been making such heavy weather of the difficulties and dangers of operations aimed at bringing ordinary-language arguments into relation with formal systems? In part, my aim has been to indicate that there is a potentially fatal indeterminacy about how to identify the forms or structures that such arguments can be represented as displaying, insofar as the features of ordinary use that are picked up by a formal system are not themselves specified formally, but are determined only within the conventions of ordinary use. But, in part, I have also been laying the ground for one potentially disastrous consequence of the conjunction of the fact that every argument or inference is «of the form» (F) with a widely-held notion of what would be involved in finding an argument invalid.
Though I have amassed for my private delight a fairly large collection of expressions of the notion about invalidity that I believe is widely held, I shall present just four instances of it, all of them published after Hamblin's *Fallacies* first appeared. As I am concerned with them only insofar as they are expressions of a notion that I believe to be widely (though not unanimously) held and all four appear in textbooks of elementary logic, and, hence, their authors may be regarded as not on oath in such performances, I think it as well to quote them under the cover of anonymity. In any case, two of the authors themselves go on to raise some doubts concerning the reliability of the technique they describe, and another of them makes reference (in another connection) to Hamblin's book.

> (1) To prove the invalidity of an argument, it suffices to formulate another argument that (1) has exactly the same form as the first and (2) has true premises and a false conclusion.

> (2) We can test for […] invalidity, when in doubt, by observing that it is "on all fours with" an argument patently invalid.

> (3) What one seeks, to show an argument is invalid, is a *structurally similar* argument with true premises and false conclusion.

> (4) The argument form
> $A_1, A_2, \ldots, A_n; \square\square\square$
> Is *invalid* if it is possible to assign truth values to the statement variables occurring in such a way as to make each of $A_1, A_2, \ldots, A_n$ take the value T and to make A take value F. Otherwise the argument form is valid.

Setting to one side the differences in style and vocabulary, it is clear that (1)-(4) share a broad notion of what it would be to find an argument invalid. Indeed, two of the authors descend from the mild technicality of talking about «premises», «conclusions» and «assignments of truth values» to say that the use of the technique is a matter of issuing the challenge «you might as well say...» or «you might as well

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argue…». This, I think, is perfectly proper of them, as we shall see in our concluding section.
Let us call the notion of how to find an argument invalid expressed by (1)-(4) «refutation by analogy» and state briefly what I called the potentially disastrous consequence of conjoining refutation by analogy with the fact that every argument or inference is of the form (F). This is that at least the following argument or inference is of the form (F):

> (Dis)   Paris is in France   (therefore) Moscow is in Spain.

Given that every argument or inference whatever is of the form (F), every argument or inference whatever is of «the same form as», is «"on all fours with"», is «*structurally similar to*» or can be «assigned truth values» in the way that (Dis) can. (Dis) has a true premise and a false conclusion. Hence (Dis) is invalid; indeed, as (2) says, «patently invalid». Therefore, if (1)-(4) are to be believed, every argument or inference whatever that is of «the same form as», is «"on all fours with"», is «*structurally similar to*» to (Dis) or can be «assigned truth values» in the way that (Dis) can, is invalid. Therefore every argument or inference whatever is invalid. Including, of course, the present argument or inference which, starting from refutation by analogy and the invalidity of (Dis) leads us to the conclusion that every argument or inference is invalid. For this argument too is of the same form as, is on all fours with, is structurally similar to (Dis) and can be assigned truth values in the way that (Dis) can.
It will be naturally objected that, in drawing the disastrous consequence of refutation by analogy, I have traduced the spirit, so to speak, of (1)-(4), a spirit that may be rather gnomically expressed in (1) by «exactly», in (2) by the very phrase «is on all fours with» (which appears in the original in quotation marks), in (3) by the use of italics for «structurally similar», and in (4) perhaps by «in such a way». The objection is not ill-founded. But it is rather hard to spell out what this spirit actually demands. For instance, it may be reasonable to demand that the analogue argument appealed to, unlike (Dis), display as much as possible of the form or structure of the argument subject to test. Yet this again leaves an indeterminacy: how much is «as much as possible»?
It may be that the phrase «is on all fours with» gets closest to expressing the demand for the display of as much structure as possible, but it is not entirely clear what the phrase means (it is not recognised by *Shorter OED*). Perhaps it means something like: «for each structural element SE_1 in the argument subject to test, there is an corresponding structural element SE_2 in the analogue argument, and SE_1 plays the same role in the argument in which it appears as SE_2 does in its». This seems to be going in the right direction. But we are not there yet. Although, for instance, we can identify «all» as a structural element of English, as we did in (D*), we do so *because* we have specified «all» or the universal quantifier as a structural element in syllogistic and in first-order predicate logic respectively. This is a deviation, which shows up when we try to say what it is for the English term «all» to be «playing the same role» in its argument as the formalised SE_2 does in its. For, whereas the role that the SE_2 plays is formally specified, what we are doing in trying to bring the argument in which SE_1 appears into relation with the formalism is trying to determine what role SE_1 plays in its argument. To repeat what we have already heard from Hamblin: bringing an argument or inference into relation with a formalism is not an operation determined – still less guaranteed – by the formalism.

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Were we to find an argument with, as (1) and (2) say, «true premises and a false conclusion», one that is, in line with (3), «patently invalid» or one to which, following (4), we can assign truth values «in such a way as to make each of A_1, A_2, …, A_n take the value T and to make A take value F» and that is, in some appropriate way, «on all fours with» an argument we are testing for invalidity, would refutation by analogy be sufficient to «nail» a fallacy?

I think not. Consider the following pair of arguments or inferences (more or less mythologically associated with Wittgenstein's abandonment of the *Tractatus* programme):

     (RG)   The wall is red (therefore)   The wall is not green

and

     (RC)   The wall is red (therefore)   The wall is not crumbling

Suppose we want to test (RG) for invalidity and are looking for an argument that is on all fours with it. We light upon (RC). Perhaps two facts are pretty plain here. One is that (RG) is an inference or argument in good order, while (RC) is not. The other plain fact is that this difference between (RG) and (RC) would not show up by bringing them into the same sort of relation to the formulations of first-order predicate logic that we find in standard textbooks. If the operations of bringing into relation with that system were applied equally to (RG) and to (RC), then either they would both appear to be in good logical – formal or structural – order or (what is actually the case) neither of them would be vindicated.

In particular, if we were to suppose that there is a premise suppressed in (RG), which might be some suitably hedged version of «nothing can be both red and green», then we would have to supply the corresponding premise for (RC), namely «nothing can be both red and crumbling». But the suppressed premise for (RC) is false. Hence (RC) will not be an analogue argument with true premises, which is what is required for refutation by analogy. Thus, even though (RC) looks like a candidate to try to show the invalidity of (RG), insofar as it issues a «you might as well argue…» challenge, that challenge can be met – and (RG)'s goodness defended – by citing the difference in truth value as between the two proposed suppressed premises. That is to say, the operation of bringing (RG) and (RC) into relation with some formal system can bring to light a respect in which the appealed-to argument or inference is not good. And it is still to be shown that that respect is also a respect in which the argument being tested is not good.

On the other hand, we are free to develop an extension of first-order predicate logic to deal with the interrelations among colour predicables, so that no extra premises need be added to (RG). In which case, it would be inappropriate to bring (RC) into relation with the extension, because «crumbling» is not a colour predicable. But we might note that the development of such an extension of our core system would itself be a reflection of, rather than a justification of, what we already know about the fact (suitably hedged) that nothing can be both red and green, or that everything that is scarlet is red. That is to say, we would be bringing our extension of our core system into relation with arguments or inferences that we already recognise as in good order, which would be to reverse the order of what we are trying to do when we try to validate an ordinary-language argument by showing its formal or structural affinity to a valid scheme in some recognised formalism.

An intermediate moral that we can draw from the case of (RG) and (RC) might be expressed as follows. Even when two arguments are on all fours, and the one being

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appealed to as the analogue that would indicate invalidity in some formal configuration, a refutation by analogy may fail to pick up what makes for the goodness of the argument or inference under test. Such a failure may be caused by the specific subject-matter of the argument. Since the specific subject-matter of arguments is not a formal or structural matter, the badness of the one appealed to and the formal or structural similarities between the two arguments are not decisive to establish the badness of the argument under test. As Aristotle recognised, there cannot be an art (*techne*) that treats all sophistical refutations (which we are taking to be counterparts of invalid arguments or fallacies), because of the variety of the sciences (*epistemai*: *SEl.*, ix, 170a 20-4).

5. A refutation of «refutation by analogy» by analogy

Consideration of Hamblin's «nailing» problem for validity sensitises us to the ways that ordinary-language arguments and inferences can be cast in any of many forms, including some that cannot be shown to be on all fours with arguments that are valid and some that can be shown to be on all fours with arguments that are prime instances of invalidity. Let us suppose that we have found some way to specify what we mean by «on all fours with» so as to overcome the disastrous consequence of the fact that every argument or inference is of the form (F) and hence analogous to (Dis), and re-consider how «refutation by analogy» is meant to «nail» invalid arguments.

The fact that (Dis) has a true premise and a false conclusion is sufficient to show that (Dis) is invalid. For, if an argument is valid and has true premises, then the conclusion is true. We may therefore state this principle regarding the relation between invalidity and form:

> (IF)   If some argument *a* of form *F* has true premises and false conclusion, *a* is invalid

And we saw that the technique of «refutation by analogy» calls on us to construct an argument *a* that is on all fours with the argument *b* that is being tested for invalidity, but such that *a* has true premises and false conclusion. Underlying this technique, there is a general principle of analogous form, which we may state as follows

> (GPAF) Some argument *a* of form *F* is invalid, therefore every argument of form *F* is invalid

Unless (GPAF) were a valid inference, the fact that the constructed argument *a* is invalid would say nothing about whether the argument *b* that is being tested is invalid or not. That is, (GPAF) says that it is permitted to generalise from the invalidity of *a* to the invalidity of every argument that is on all fours with *a*, including *b*. Crucial to this idea of generalisability is the notion that two arguments *a* and *b* can share a form *F*. This is a notion that we have already seen in action: for each structural element in *a* there is a structural element in *b* playing the same role in the two arguments. One thing that (GPAF) expresses is that invalidity is a matter of form, which is shared by every argument in which the structural elements are disposed in this or that way on all fours with an argument that is known to be invalid because it has true premise(s) and false conclusion. So let us consider a little more closely what is involved in this generalising and construct an argument that is on all fours with (GPAF), as follows:

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> (ED)   Some Englishman is a drunkard, therefore every Englishman is
> a drunkard

(ED) has, I take it, a true premise and a false conclusion. Hence, by (IF), it follows
that (ED) is invalid, which we may express as

> (IG)   The argument «Some Englishman is a drunkard, therefore every
> Englishman is a drunkard» is invalid

If (ED) is invalid, it follows, by (GPAF), that every argument that is on all fours with
or is of the same form as (ED) is invalid. We may then ask what the form is of (ED)
in virtue of which every argument that is on all fours with it is invalid. And it is not
idle to suggest that what makes (ED) invalid is that it passes from a particular
premise ruled by «some» to a universal conclusion ruled by «every». Here, then, we
have a premise that may be applied to every argument that is on all fours with (ED):

> (FG)   The argument «Some Englishman is a drunkard, therefore every
> Englishman is a drunkard» is of the form «some (therefore) every»

In order for «refutation by analogy» to work, which is to say, in order to show that, if
a given argument is on all fours with (ED), then it is invalid, we need to be able to
generalise invalidity in such a way that, having picked out the cause of invalidity in
(ED), we can «nail» any argument of that very same form as invalid. Thus we may
state the principle of generalising invalidity, as an instance of (GPAF),

> (GI)   Some argument of the form «some (therefore) every» is invalid,
> therefore every argument of the form «some (therefore) every» is invalid

Now it is apparent that (GI) is an argument of the form «some (therefore) every»,
and its conclusion says that every argument of this form is invalid. As a result, we
may infer that, (GI) is invalid. What is the form in virtue of which (GI) is invalid?
The form it displays in virtue of being on all fours with (ED). Now, insofar as (GI) is
an instance of (GPAF), we may ask whether (GPAF) is valid. Yet, if (GPAF) is
valid, then (IG) is not true, either because no Englishman is a drunkard (false
premise) or every Englishman is a drunkard (true conclusion). But some Englishman
is a drunkard and some Englishman is not a drunkard (i.e. not every Englishman is a
drunkard); so (IG) is true. But, if the validity of (GPAF) implies the falsity of (IG),
(GPAF) is invalid.
Perhaps we can clarify matters a little – or merely re-state for emphasis – by
transposing (GPAF) from an argument into a conditional:

> (GPAF*)   If some argument a of form F is invalid, then every
> argument of form F is invalid

And we ask whether (GPAF*) is true in light of

> (GI*)   If some argument of the form «some (therefore) every» is
> invalid, then every argument of the form «some (therefore) every» is
> invalid

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It is apparent that, if (GPAF*) is false, then we cannot use «refutation by analogy» to «nail» any argument as invalid. But, if (GPAF*) is true, either (i) (GI*) is false and (ED) is valid (recall the formulation in (4) above: «if a form is not invalid, it is valid»), so either (ED)'s premise is false or (ED)'s conclusion is true (but neither of these holds because of the variety of drinking habits among Englishmen); or (ii) (GI*) is true, and (GPAF*) is false of itself. Therefore (GPAF*) is either if false, false or, if true, false of itself; and, if false of itself, false. So, in any case, (GPAF*) is false. But, unless (GPAF*) is true, «refutation by analogy» is fatally flawed.

Therefore, «refutation by analogy» is fatally flawed. So we cannot use it to «nail» any argument as invalid.

6. A theory, a doctrine or a folklore of fallacies?

The conclusion of the preceding section may appear apocalyptic, for it undermines what is probably the most widely-used and certainly the most widely-advertised technique for trying to test arguments and inferences for invalidity, fallaciousness or sophisticalness. Given that the principle underlying «refutation by analogy» is self-refuting when applied to arguments like (ED) and, in any case, the technique relies on a suspect inference from «some» to «every», any test carried out in accordance with it is open to serious doubt.

If there were some prospects not only for providing a theoretical solution to Hamblin's «nailing» problem, but also for shoring up «refutation by analogy», there might be some chance of getting closer to a theory of fallacy. I hope to have indicated why I think that such prospects are extremely remote: unless I have made some serious errors in the foregoing discussion, such prospects might even be said to be nil because they require at least two impossibilities to be true. These impossibilities are, to summarise: (i) the impossibility of a formalism that determines a «bringing into relation» relation for non-formal expressions; and (ii) the impossibility of using formal analogy to «nail» a given argument as invalid.

Are there, then, any techniques for establishing that a given argument is invalid? Yes. There is at least one. There is the technique whereby, in line with (IF), we show that the premises of that very argument itself are true and the conclusion false. That, alas, does not get us very far, if what is at issue is whether or not the conclusion is true, given the premises. But it is at least relevant and secure. What is more, it can be emulated by describing a situation or counter-instance in which those very premises were true although the conclusion need not be. Nevertheless, if we can show that the conclusion is or might be false in the hypothetical case, we are apt to lose interest in the question of validity and are likely to settle for the disjunction: either the argument is invalid or one or more of the premises is false. Which reminds us of how we normally go about resisting an unwelcome conclusion: by showing that at least one of the premises, whether explicit or suppressed but called for, is false. This is enough to show that the conclusion is not supported by the argument, and once again our interest in the question of the validity or otherwise of the argument is likely to wane.

The question then arises of what we are to do with all scholarship and speculation that has gone into describing arguments with the unhappy characteristic of having true premises and false conclusion, or possibly false conclusion despite true premises. For, on the face of it, these should be the least interesting sorts of arguments: as Aristotle would have said, in these cases, «there is no syllogism». The point, however, is that, as he might also have said, they look like good arguments but

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are in fact paralogisms (*SEl.*, i, 164a 20-1), where the accent is on «looking like» or «seemings» (*ta phainomena*).

On one line of thought, unless we can have a theory of fallacies, the «informal logician […] is simply out of business' (WOODS 1995: 193). This is, as its proponent candidly admits, a fearful prospect, but at least one that would justify the practice, initiated perhaps by George Boole in 1854, of writing logic books without tacking on an account of what John Stuart Mill called «a theory of bad arguments' (MILL 1843: V, i, 1). Even if we cannot have a «theory», it nevertheless appears that there is still business to be done. So, on a less drastic understanding of the situation, we might remind ourselves of some of the benefits of knowing something about fallacies, and suggest some models for the standing of the doctrine in question.

One general characterisation one might give of Aristotle's *Sophistici Elenchi* is that it offers some practical guidance for the conduct of school exercises in dialectic that could also be applied in the wider world to which its initial recipients were destined, in the law courts and assemblies of fourth-century Athens. The list of thirteen sophistical refutations that we find in the book's fourth and fifth chapters are just examples of some of the tricks that (fourth-century Athenian) sophists got up to and that it was well to be aware of in advance. It was well to be aware of them in advance both so as to prevent the book's reader from being tricked and to advise the reader against using such tricks, given that their sophistical nature was recognised by other readers of the same book. In this sense, they are like professional fouls in football: the initiated might try one on against tyros, because it looks like (*phainetai*) fair play though in fact in contravention of the rules; but, in front of an expert referee (such as Aristotle) or against other players in the know, it is useless to try because the foul will be called and the trick revealed. As Aristotle underscores in the *Topics* (esp. VIII, xiv), it is well to be familiar with this material because it allows us to have ready reflexes.

Even though Aristotle does not give much by way of definition or characterisation of the sophistical refutations he lists and exemplifies, he does supply labels, such as «consequent» (*epomenon*: *SEl.*, v, 167b 1). These labels can be used to group together tricks that, in one way or another, resemble each other. This being so, if two discussants *A* and *B*, both familiar with *Sophistici Elenchi*, are contending with each other in some dialectical exercise and *A* produces an argument that too closely resembles,

(C1)   This is yellow; If this is honey, it is yellow (therefore) This is honey (cf. *SEl.*, v, 167b 5-6)

B can suggest, by way of shorthand, that it looks as if *A* has committed «consequent» or can have «consequent» pinned on him. This is where the «you might as well argue…» challenge comes into play. In more modern vein, *B* might be tempted to say that the form of (C1) is that displayed by the schema «$P \supset Q$, Q (therefore) P», which we can ascertain, by means such as truth-tables, not to be a valid form of the propositional calculus. But, if *A* proffers the argument

(C2)   The ground is wet; When it has rained, the ground is wet; (therefore) It has rained (cf. *SEl.*, v, 167b 6-7)

he is free to say in the face of a reiteration of *B*'s challenge that, for sure, the argument is a case of «consequent», but it is also a good abduction and so is an

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argument that gives good support to its conclusion: it would be simply dim for someone to see water on the ground both in the courtyard and in the street and be unready to suppose that it has rained in the night. Indeed, Aristotle seems not to have noticed that at least three of the five arguments he gestures at as instances of «consequent» are perfectly good inferences.

In these sorts of ways, the literature on fallacies provides at most the rudiments of a vocabulary for describing – and perhaps even starting to evaluate – arguments whose description does not fall within the purview of the theories we have at our disposal for good (deductive) arguments. As Hamblin's review of the Standard Treatment illustrates, the vocabulary available for describing wayward arguments is not fully standardised, neither as regards what to include or to exclude, nor as to the accepted meanings attributable to it. For instance, as Hamblin remarks, «very few modern logicians bother to mention» the trick known as «figure of speech» or better «form of expression» (*schema tes lexeos*) (HAMBLIN 1970: 25), if for no better reason than that the sorts of morphological considerations Aristotle brings into play have few direct correlates in the English of many modern logicians (*SEl.,* iv, 166b 10-19). Likewise, tags like «*petitio principii*» and «*ignoratio elenchi*» seem to lend themselves to redefinitions of greater or lesser fancifulness, according to the set phrases of English that are recruited to give a sense to them: should «*petitio principii*» be used to mean «begging the question» or «arguing in a circle» or both or neither, and what do they mean anyway?

The rather hand-me-down nature of many discussions of fallacies and invalid argumentation in those introductory logic textbooks that still dedicate a chapter or two to the matter, as well as in handbooks of things like «critical thinking», explains the degree of uniformity that we do find among the various treatments and in large measure justifies Hamblin's tag «the Standard Treatment». Such differences as can be found about which arguments to pick on as bad can vice-versa be attributed to the fact, which I hope to have made plain in the foregoing sections, that an invalid argument can be of any form whatever, except a valid one.

Tolstoy famously remarked about families that there is only one way they can be happy, but each of the unhappy ones is unhappy in its own way. While it may be possible to say what ingredients go into making for a happy family, the infinite variety of excesses and defects in human behaviour that make for unhappiness do not lend themselves to much systematisation or theorisation. What we have instead are lots and lots of novels, telling lots of different, particularised stories about how Anna was unhappy, about how Dorothea was unhappy, about how Emma was unhappy, and so on. Likewise, the range of possible fallacious arguments and inferences is limited only by the scope of the ingenuity and perversity of the human mind, and we do not have much of a theory of that.

Nor, again, do we have a theory of «looking like a good argument». In one sense, any inference can look good to someone who is sufficiently desperate to suppose that their proposed conclusion can be supported by the materials to hand. To plumb the depths of this desperation, it is quite enough to listen for three or four minutes to the average party politician on the stump. In another sense, there is a fairly well-documented set of kluges (MARCUS 2008) and mental traps (MOTTERLINI 2008) into which almost everyone is inclined to fall, such as the phenomena of anchoring, of the attribution asymmetry, of escalation in the dollar game, of framing and focusing illusions, of hindsight-foresight swaps, of perceptual biases and of all the varieties of wishful thinking that seem common to all human beings. The confines of this set are indeterminate and, in great measure, unexplained, and the grade of

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inclination to fall into them is highly variable from person to person and, indeed, from moment to moment. Yet if we are aware that, in addition to variants on «consequent», these are errors to which we are ourselves prone, we may be able to catch ourselves making them and correct ourselves. But there is no theory of these sorts of weakness of will and intellect.

A further point of analogy might be with linguistic solecisms. We are familiar enough with what grammar books and dictionaries can tell us about a language. The former tell us about the rules of proper formation and the latter about the meanings of the vocabulary (and about their orthography). Thus also a logic book will tell us both about the syntax and about the semantics of certain formal systems. Grammar books and dictionaries of natural languages have to be updated now and again to keep pace with changes in theoretical understanding of how a given language works, and with the addition or obsolescence of words in the vocabulary. But it is noticeable that books that focus on linguistic solecisms – call them «style guides» or «usage books» – have to be revised much more frequently to keep up with the phenomena they are concerned with: formulations that look good (*phainetai*) to their formulator, but are barbarisms.

We may note four, perhaps interrelated, points about these sorts of publications that offer analogies with the treatment of fallacies. One is that they respond to the protean powers of writers – often enough journalists and bureaucrats – to produce ever-new ways of maltreating the language. In the second place, they often adopt *ad hoc* labels for the structures they are out to alert us to («Out of the frying pan» being one favourite (FOWLER 1926: s.v.). Third, they are called on to supply illustrative quotations of the disparaged forms, even though it is not always clear how «arguments by analogy» would apply to them. And, lastly, they are of their nature incomplete and their contents may be unpredictable by a reader with his own stylistic bees in his bonnet. In these ways, while a grammar and a dictionary give us some theory of the language they describe, a usage book nevertheless offers doctrine, of a rather «take-it-or-leave-it» sort, about linguistic usage. And it is no less useful than a theory for that.

Two instances may be cited of this sort of usefulness that correspond to the usefulness of having labels for fallacies. One is that of what Fowler calls «Superstitions» or «Fetishes», a case in point being the formation known as «split infinitive». However one resolves on any given occasion the tension between a rule of thumb against separating «to» from its verb and the need to avoid ambiguity or unclarity in what one is writing, the presence of this *topos* in the literature is a reminder that care is called for. If one has decidedly to boldly split an infinitive, then one must be prepared to defend it, as the proponent of (C2) must be ready to weather the charge of «consequent».

Another instance of the usefulness of non-theoretical adjuncts to grammars and dictionaries arises from the fact that many users of a language are not native users. In such cases, more or less accidental similarities – sometimes known as «false friends» – between their mother tongue and the language they are learning may interfere with their competence. Thus, there is a recognisable range of errors that, for instance, an Italian will be almost unable to avoid in writing English, but that it would not occur to a Russian to make. A listing of «Italianisms» is in no way an addition to the theory of English, because it will not help, say, a Russian to avoid *her* errors. Yet such a listing may help to bring an Italian-speaker into relation with English.

Given the unsystematic nature of their subject-matter and the occasional nature of their usability, historical and speculative enquiries into fallacy can, in their different

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ways, furnish us with what I want to call a «folklore» of ways that argumentation can go astray. By a «folklore» I mean, in anthropological vein, a body of beliefs that is sedimented in a community, that may or may not include some guiding principles for its applicability, that may or may not have been subjected to rational scrutiny, and that is consulted *ad hoc* for practical purposes. Taken as a folklore rather than as a theory, the doctrine of fallacies certainly is sedimented in the tradition of Western philosophy, as we see from the persisting presence of more or less gestural references to Aristotle's list in *SEl* (iv-v). It does include some rules of thumb and some rough-and-ready pointers; for instance, as we have seen, it enjoins us, rather arbitrarily, to object to arguments that resist being cast as deductions. Yet much of it has not only come away from its original moorings in the practice of *viva voce* dialectic in the Academy and the Lyceum or in the medieval game of obligations, but has acquired accretions and misconceptions – some verbal, others substantial – along the way. And the principal use of the study of the fallacy tradition is to build up a sensitivity to cases where a stretch of argumentation looks suspect and to furnish a vocabulary for expressing such suspicions, whether well-founded or not.

To say, as I want to say, that the study of fallacies is an exercise in folklore seems deflationary – not to say «insulting» – about the efforts of scholars and logicians from Aristotle on down through Hamblin to the present day. For, while an anthropologist might study the beliefs of some community of which she is not a member with no sense that her research is in any way being diminished by being called an exercise in folklore, it seems that the doctrine of fallacy is part of the folklore of the Western tradition in philosophy, of which Aristotle, Hamblin and we are members. It is *our* folklore. Whereas the folklorist has no commitment of her own to the body of beliefs that she describes, Aristotle, Hamblin and we might want to be able to have some confidence in the results we reach when we discuss fallacies. But, if the foregoing account is not entirely erroneous, it might seem that the confidence that is a professional deformation of logicians may not be on offer when it comes to the doctrine of fallacies. If the best we can have by way of a doctrine of fallacies is really of the nature of a folklore, then we might just have to lower our expectations and proceed with a little caution on a case-by-case basis.

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