

Dogmatism and Dialethism: Reflections on Remarks of Sorenson and Armour-Garb

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My principal purpose in writing *Paradox and Paraconsistency* (Woods [2003]) was to draw attention to what I took to be a regrettable omission in the methodology of the abstract sciences. There was not, I thought, sufficient attention paid to ways and means of settling disagreements in disciplines such as logic and mathematics. This deficiency resonated with me with a particular emphasis. I had spent a good part of thirty years in trying to say something intelligent about the structure and evaluation of argument; and yet nowhere in my own work, or anyone else's so far as I could tell, was this issue so much as addressed, never mind settled. And I must say that I was drawn to the view – and still am – that a theory of argument is seriously incomplete if it does not achieve some success with this matter. It is a vexing issue, made so by the fact that logic and mathematics and the other abstract sciences are not white-coat disciplines, subject to experimental adjudication. My hope was that the relevant research communities would see this omission as important and needful of repair. Judging from the extremely interesting remarks of Roy Sorensen [2003] and Brad Armour-Garb [2003], I think I may derive some measure of assurance that that objective is now in process of being met. Even so, the devil really is in the details. Sorensen, for example, is right in saying that the inaccessibility of such disciplines to empirical checks is not as clear a matter as it may appear to be in practice. I agree with this, and said as much in the book. What I didn't say is that the method of costs and benefits, of which in due course I would try to make something, itself requires that the doctrine of empirical inaccessibility be handled with particular care. After all, the economic approach would go nowhere unless costs and benefits were at least sometimes discernible, and this, it could be argued, makes *them* a kind of empirical check. All the same, I stick to my guns in thinking that there is an important sense in which a dispute over a principle of logic cannot be handled in the way in which a dispute in physics can be, and that this is difference enough to give to conflict resolution in the abstract sciences a particular, and demanding, character.

Paradox and Paraconsistency had a second objective. It was to undermine the epistemological presumptions of analytic philosophy, if I might be forgiven for saying so with such pomposity. I mean “analytical philosophy” in something like Moore's original sense, which is the form in which its deficiencies are particularly egregious. Analytic philosophy is a response to the non-white-coat character of philosophy itself. Philosophy would be done not on the basis of our sensory observations but rather on the basis of “our” intuitions, intuitions that would “unpack” the content of relevantly situated concepts. This is a matter of some awkwardness, of course. Philosophical theories can't be thought up at all, except on the basis of some originating beliefs. We may call these beliefs “intuitions” if we please, but it is a mistake to privilege them epistemically, which is a confusion of confidence with objectivity. While causally indispensable, intuitions

often prove wanting. My particular complaint against the method of analytic intuitions, or, what comes to the same thing, conceptual analysis, was a twofold one. One is that citing intuitions is a dialectically impotent manoeuvre under conditions of attack. The other has to do with the analytical diagnostics of the paradoxes. Here too Sorensen is right. It is a nontrivial mistake to confuse the emptiness of a concept with its non-existence. The confusion, if I may say so, is not mine, however. The culprits are, in varying degrees, the early analytical philosophers or those of like mind. Frege certainly thought that it flowed from the very idea of what Russell would call a class that there are no classes. It was likewise Tarski's view that it flows from the very idea of true sentence that there are no true sentences. Count Korye' went so far as to proclaim the downfall of reason itself. Perhaps this was not as excessive as it may strike the present-day ear, in light of Frege's similar despair over arithmetic. (Even so, the vulgar alarm that Sorensen chastens against is theirs, not mine.) They got it all wrong, of course. These thinkers misdiagnosed the paradoxes of sets and the liar, and they misjudged their triage. In this they were hugely abetted by their fidelity, express or tacit, to the method of analytical intuitions. For, on this view, if an inconsistency flows from the very idea of a thing, the idea indeed is empty; nothing instantiates it. And if the very idea of what it is to be, e.g., a set is such that there can't *be* any, how much of a stretch is it to say that there is no concept of set?

The bungling of the diagnosis and triage of the paradoxes by the early analytic philosophers leaves the question of their correct management to be sorted out. It is a complicated question made considerably more so when attended by fundamental disagreement. As a matter of intellectual history, the paradoxes have landed us in what the book calls the Great Delta Debate, which instantiates Philosophy's Most Difficult Problem. This is the problem of determining in a suitably general and principled way whether a valid argument is a *reductio* of something in its premiss set, or a sound demonstration of something surprising or even highly counterintuitive. The Great Delta Debate is thus the debate between classical and paraconsistent logicians about how this question should be answered in the case of the proofs of the paradoxes. Given that such disagreements themselves turn on differences of intuition, an appeal to intuitions is a risky resolution device. Enter the method of costs and benefits. A classical logician sees the paradoxical proofs as *reductios*, and pins the blame (less dramatically than the analysts of yore) on something in the premiss sets. Paraconsistentists of dialethic bent see the proofs as sound demonstrations of something approaching Heraclitean import (if not Heraclitean scope). They are proofs of the existence of true contradictions. In my hands, the method of costs and benefits gives the nod to the classical logician. Dialethic logics are vastly more difficult and more complex than the garden variety first-order. Quine reminded us a long time ago (and it was known before Quine told it) that classical logic cannot brook even slight changes to its logical particles. Change one connective, says Quine, and you lose them all. And so you do, as the functional completeness metatheorem attests. Bearing on this are the two most common resolution devices currently on view, Surrender and Reconciliation. To see how they work, let us change the example slightly.

In the early days of relevant logic there was a considerable row about *ex falso quodlibet*, a theorem that is derivable in classical logic and underivable in relevant logic (indeed in any paraconsistent logic). In the standard relevantist approaches the underivability of *ex falso* is purchased by downgrading disjunctive syllogism from a valid to a merely admissible rule. In the actual history of this demotion, this was effected without the slightest antecedent suspicion that disjunctive syllogism was indeed a defective rule. And it was done, moreover, in utter innocence of what the dismissal of that rule would ultimately cost. In time, it became clear that the resultant new logic would lose its truth functional character entirely; its decision problem would become either unsolvable or computationally intractable; and its semantics would become a mess. This, I said, was evidence of dogmatism. Dogmatism, here, is a dominantly economic matter. Someone is a dogmatist to the extent that he pays a price for a proposition, principle or theory that, by his own antecedent lights, would have been judged at least unattractive, if not unbearable. Costs being in the eye of the beholder, perhaps it is not obvious that dogmatism is intrinsically something to condemn. But it does harbour an oddity. In the case of *ex falso* it provides for the avoidance of something counterintuitive only at the cost of embracing consequences whose own cumulative counterintuitiveness is at least a rival of the original. No one who thought that *ex falso* should be expunged from classical logic had the foggiest idea, in the beginning, that the cost of this apparently key-hole surgery would be the loss of classical logic itself. In a way, then, the dogmatist is a sunk-costs reasoner *in extremis*^[1]. Perhaps, in the end, this is what makes dogmatism a bit dodgy.

Sorensen queries my suggestion that in disagreements of the kind we are here discussing, the burden of proof rests with those who challenge a received position. He tells us that in their *Notre Dame Philosophical Reviews* piece, Beall and Ripley accept the principle, but insist that it gives the nod to the dialethist, this on the strength of the historical priority of Heraclitus and his ilk. But this is a confusion. A theory's status as the standard or received view is wholly a matter of its current standing in the research communities of the protagonists. Sometimes this is a standing achieved by historical priority, but it need not always be so, and frequently isn't. Modern-day dialethists can claim all the kinship they desire with Heraclitus (or Lenin, if they prefer); but this cannot change the fact that in 1960 classical logic was the received view and relevant logic was the new kid on the block.

Brad Armour-Garb pays close attention to the details of the Great Delta Debate. It is, he agrees, a context in which putative damage is done to the dialethic option. He is right to see that if damage there be, it is occasioned by economic considerations. But, Armour-Garb thinks that considerations of cost and benefit underdetermine the question of the tenability of dialethism. He thinks that the dialethic option does not exact higher costs than the classical option, and even if it did, that would not settle the hash of dialethism. If Armour-Garb is right about this, he will have

^[1] In the book I observed that dogmatists tend not to respect the *reductio*-of-a-premiss versus proof-of-something-surprising distinction. It is certainly true that this is how dogmatists frequently behave. But I would not want to make such behaviour intrinsic to dogmatism. Sorenson's remarks are helpful on this point.

demonstrated a significant limitation on the method of costs and benefits. I will come back to this point later.

My view is that, if valid, the proofs of the paradoxes are *reductios* of something in their premises-sets, and are so in a way that instantiates the standard diagnosis of the Barber. This makes for a modest triage across the board: There is no such set; there is no such statement; there is no such barber; and there is no such secretary. Armour-Garb questions this. He wants something like the Barber-description to pick out a real barber and something like the Secretary description to pick out a real secretary. And he rightly observes that this can easily be achieved with a suitable re-jigging of the descriptions in question. However, he wants nothing to with a like latitude for sets and liars, each salvageable with like re-jigging. This prompts an obvious question. Do we have a general and principled way of determining when to paraphrase a paradox away and when to leave it be and bravely take our medicine. (I'll return to the issue of principled generality in a moment.) Sorensen is interested to know whether a refusal to countenance the dialethic option counts as dogmatic in my sense. He asks us to consider the case of David Lewis, who, when invited by Richard Routley to prove the Law of Non-Contradiction, refused flatly to rise to the challenge. In refusing to be thus drawn, Lewis avoids Aristotle's fate, who did allow himself to be drawn, and with such unedifying effect, in Book Gamma of the *Metaphysics*. By dialethic lights, Lewis' refusal might be pig-headedness, but it is not dogmatism in my sense. For it to qualify as dogmatism, Routley would have to have got Lewis to concede that his fidelity to this law commits him to implications of a kind that he would antecedently have considered too counterintuitive for serious consideration. My position in the book was, in effect, that by the very nature of his subscription to true contradictions, the dialethic logician could not put Lewis to that disadvantage, could not drive him into the embrace of what I call Converse Begging the Question. It is that Armour-Garb remains dubious about. He thinks that there are considerations that, fairly estimated, would subdue this Lewisian passivity.

The two great strategies of conflict resolution in the abstract sciences are, as I say, Surrender and Reconciliation. Surrender is a form of Aristotle's (and later Locke's) *ad hominem*, in which you get your opponents to see that their positions carry costs that they themselves are not prepared to pay. Reconciliation seeks to find different things for the conflicted intuitions to be true *of*. So, with *ex falso* again our example, one proposes that what *ex falso* is true of is the propositional relation of logical consequence, and what the relevantist's contrary insight is true of is belief modification, or inference. Seen this way, it is agreed that it would never be rational to believe everything whatever in the face of an inconsistency. But this leaves it wholly unaffected as to what the logical consequences of what you believe actually are. This turns out to matter for the method of costs and benefits in ways that I did not adequately foresee in the book. The economic advantage that the classicist claims over the relevantist is that *ex falso* doesn't have to be false for a theory of inference to be true. The disadvantage for the relevantist is that he relevantizes the consequence relation without the need to do so. Fine as far as it goes, it is also true that if the story that you tell about consequence isn't the same as the story you tell about inference, and moreover, if the story that you tell about the latter cannot allow *ex falso* to apply to inference, then you are going to have to relevantize inference anyway, never

mind whether you also relevantize consequence. You are going to have to do the work of building an *ex falso*-free theory irrespective of whether you build it on the consequence relation or the inference operation. Doesn't this dissipate the economic advantages of staying with a classical consequence relation?

It bears on this point that, from its very beginning, logic saw the task of building a realistic inference relation as a coterminous with that of building an appropriate consequence relation. The reason for this was that the reasoning that logic was interested in is a matter of drawing consequences from sets of premisses in a truth preserving way. This, we might say, is archaeological reasoning. It is the extraction of information already present in the premisses. But to recognize what the consequences of a bunch of propositions might be, one surely needs to know by what consequence relation those propositions are generated. No consequences without a consequence relation. Right at the beginning, Aristotle recognized that classical consequence could not deliver the goods for reasoning of this kind. So he imposed conditions that converted classical consequence to syllogistic consequence, and in so doing produced the first intuitionistic, paraconsistent, non-monotonic logic ever conceived of. (Woods [2001]) Aristotle's hope was that drawing *sylogistic* consequences was indeed what archeological reasoning actually came down to. It would be reasoning that is truth-preserving, psychologically real and computationally tractable for the thinker on the ground. It was a remarkable achievement. It permitted us to characterize any given instance of the thinking in question as the drawing of *all* the syllogistic consequences from a low-finite set of premisses. It would be total evacuation of syllogistic information re-packaged in a single proposition. The relevant logician seeks to relevantize his consequence relation to like effect. So what is all the fuss about? Had he understood what he was doing, the relevant logician would have recognized right at the outset that the necessity to produce a relevantized consequence relation in no way obliges him to say that classical consequence isn't real consequence or that there is no real consequence relation of which *ex falso* is true. He need only say that when someone draws truth-preserving consequences from a set of premisses he is not drawing all of what those premisses classically yield. This answers our more general question. Why not declassify consequence in the spirit of Aristotle's accommodation of human thinking? The answer is, do it if you like when human reasoning is your objective; but leave it alone when other ends are in view (say, the reduction of number theory to logic).

Amour-Garb exposes to a probing and helpful scrutiny my position against the Liar. As developed in the book, it is a rather complex case; and both Beall and Ripley, as well as Bromand in his yet to be published review for *Mind*, echo Armour-Garb's observation that in places I am not as clear as one would wish. I fear that this is so. If I had had the chance, I'd have willingly tried to clean up my act. In so doing, no doubt I would have been guided to advantage by Sorenson's *A Brief History of the Paradox* (had it been available). But not today. We simply lack the time. Let me, then, *faute de mieux* concentrate on the dialethist's main case against consistentists. It is what Armour-Garb calls *The Dialethist's Conjecture*. As Armour-Garb describes it, the consistentist is doomed to fall into one or more of three "pitfalls". (p. 12). First, the consistentist may have a plausible diagnosis of a given version of the Liar, which fails to generalize to the

other versions. Secondly, the consistentist may have a plausible and principled diagnosis of all versions of the Liar, which fails to generalize to the whole class of semantic paradoxes. Thirdly, the consistentist may have a diagnosis of all versions of the Liar, but it is *ad hoc* and therefore “unprincipled”. Let me deal with the last first by saying simply that I fail to see why Armour-Garb puts it in the list. He claims that for realists *ad hoc* manoeuvres are serious trouble, but for anti-realists they are scarcely more than nuisances, making for jerky theories when smoothly integrated ones are so much nicer. I think that he is telling us on page three that he himself is an anti-realist; so he worries about *ad hocery* for my sake. But I am no realist either; the proffered accommodation, while appreciated, is not necessary. The other two pitfalls are of a piece. Of course, we want our answers to be as general as we can get them to be. Perhaps we should concede that generalizable solutions are on the whole better than results achieved piece-meal. But if answers were obliged to be given up whenever a better might be conceived of or wished for, there would be precious little to say in most disciplines. *The Dialethist's Conjecture* goes too far in my view. It elevates a desideratum to the status of a necessary condition. It is helpfulness run amok.

I do grant the desirability of principled generality. With generality depth goes hand in hand. Consistentists and dialethists are both involved in the Great Delta Debate. Each has the task of determining when a Δ is a *reductio* or a dialethic proof. I trust that it will not offend against the spirit of the season if I suggest that the dialethists themselves have not yet achieved this determination with the desired generality or principledness. Small wonder, since the dialethist's task is more onerous than that of his classical counterpart. The dialethist must show us why *this* Δ is a *reductio* and this *other* Δ is not. He must find a basis for allowing the Liar and a different basis for disallowing Curry, which is the Hereclitean menace - and then some - of the modern era. By comparison, the consistentist's lot is a lark. I say this in the spirit of Sorensen's classical achievements in *Contradiction and Vagueness*. The consistentist has an answer to this question which is both general and principled. The answer is that a Δ is *always* a *reductio*; and that what they always show is the nonexistence of something claimed or presupposed in their premiss-sets. There is no such set, no such statement, no such barber, no such secretary and, yes, no such property (e.g. heterologicality). The desired generality is got by the universality of the identification of Δ s with *reductios*. It owes its principledness to the recognition that contradictions can't be true.

This would be a good point to revisit Armour-Garb's claim that if the economic approach suffices for the settlement of the consistentist-dialethist standoff, then the nod goes to dialethism for reasons of lower cost; yet even if this were not the case, the nod would still go to the dialethist on non-economic grounds. If this were so, it would be a telling objection against one of the pillars of the book. But there are reasons to think that it is not so. In the time remaining, I shall cite two of them. One is that, in light of considerations canvassed in the preceding paragraph, dialethism fails Armour-Garb's own adequacy condition, *The Dialethist's Conjecture*. If so, that is certainly a cost; and if so, it is a cost no lower than that which falls on the consistentist if, as Armour-Garb avers, he too fails *The Dialethist's Conjecture*. On the other hand, if dialethism were to lose the cost-benefit contest with consistentists, there must be some basis, other than economic advantage, on

which this victory would rest. Realists may think that they have some coherent idea of what such a basis would be, but for anti-realists costs and benefits is all there is to go on. And, again, about these matters Armour-Garb is himself an anti-realist.

Even with these things said, I do not for a moment suppose that in this book or these remarks this morning I have settled the hash of anything of importance, except perhaps of the importance of the project of conflict resolution in the abstract sciences. It inheres in the cost-benefit strategies to which I am so drawn that rarely are they permanently decisive. In conflict resolution, it takes two to tango and two to reach accord, but only one to break an accord. With this in mind, I greatly look forward to the instruction on offer in Armour-Garb's forthcoming *New Essays on Non-Contradiction*. I am, however, greatly heartened to think that this morning's critics find something of value in the project, and I thank most warmly Roy Sorensen and Brad Armour-Garb for the pains they have taken with this book. My thanks, too, to Dan Boone for having organized and chaired the session so ably, and to Jonathan Adler for having suggested it in the first place. Finally I extend my gratitude for their interest to the members of the audience.

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