

Eight Theses Reflecting on Stephen Toulmin

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My title embodies an ambiguity that I hope to make something of. In one sense, it suggests a thesis that Stephen Toulmin himself espouses or is committed to. In another, it suggests a thesis held by me, but occasioned, in whole or in part, by reflecting on Toulmin's writings. Of course, the two senses are not robustly disjoint. My principal purpose is to lend these theses some degree of favour, if not in every case theses of Toulmin's own *making*, then perhaps of Toulmin's *example*.

Thesis one. The validity standard is nearly always the wrong standard for real-life reasoning. It is widely assumed that valid argument is nearly the best there is, improved upon only by argument that is sound. When made to note that actual reasoners hardly ever attain the validity standard, the received response is to make the best of a bad thing, insisting that, for beings like us, reasoning is best when it most closely *approximates* to the strict canons of deduction. Against this, cooler heads counsel that the validity standard is best only when a reasoner's target is such as to call for it, as when, for example, one seeks a proof of a proposition of set theory. But even this is wrong. It is wrong in the sense that it fails to make clear how deeply the validity standard is embedded in a network of constraints. When a mathematician wants a proof, it is always as a proof of *some proposition P*. Further, it is nearly always wanted as a proof that draws its premisses from the settled lore of mathematics. Taken alone, validity is useless. Its value rests entirely on its indissoluble link with the other components of proof.

Even when an argument attains it, validity is a rather passive standard. It can be rendered impotent with a single disruption of the reasoner's knowledge-set. On receipt of new information *K* that contradicts the desired-to-be-proved *P*, the reasoner's present valid argument remains valid, but the proof is lost. Axiomatic approaches to mathematics owe a good deal of their motivation to an interest in dealing with the passivity of validity. For if the inputs to the theory's deductive apparatus could carry assurances of their truth, no *K* would emerge to contradict any *P* for which a valid argument for it exists. But, long since, such optimism about axiomatics has been driven by well-known paradoxes into the preferred harbours of system-relativity or other forms of *ad hoc* sanctuary. So validity remains an oddly inert standard.

Validity's station ensures that proof is a *brittle* accomplishment. Validity is wholly indifferent to new information. The premissory successor of a valid argument is a valid argument. This means that if ever we were mad enough to set validity as the *target* of our reasoning, achieving it would constitute wholly adequate grounds for shutting the enquiry down. This makes us see how wrong it is to think of validity as the goal of good argument. The truth is that a proof-*of-P* is (sometimes) the goal of good argument. But it is an expensive goal. It cannot tolerate any case in which, though a *K* exists that contradicts *P*, the argument for *P* retains some positive force. Most reasoning is unlike this. Arguments for are met with arguments against, often in circumstances that leave in contention both the one reasoner's *P* and the other reasoner's contrary of it. But in a proof-context for which a *K* exists, not only must *P* be abandoned, but the proof-principles that abetted its derivation have to be revised, or precious theorems surrendered. This is expensive work in the cognitive economy. Very often, it is work of a kind that we haven't time for.

If the argument here displayed is taken deductively:

- 1) Ocelots are four-legged
- 2) Ozzie is an ocelot
- 3) Therefore, Ozzie is four-legged

then (3) is defeated if Ozzie is three-legged, and *so is (1)*. But if, instead of being seen as a universally quantified conditional, (1) is taken as a *generic* claim ([Carlson and Pelletier, 1995]), then Ozzie's three-leggedness defeats (3) but leaves (1) standing. Re-writing (1) as a universally quantified conditional that is impervious to Ozzie's discouragement is notoriously difficult business ([Gabbay and Woods, 2005b]). But take it generically (and re-write "therefore" as a default operator), we achieve two advantages. We avoid the cost of exceptionalizing (1), and we conform to how we reason about such things anyway. This is not to say that a proof-of-*P* target is always wrong for us. Far from it. But it is brittle, that is, easy to wreck, and expensive, that is, difficult to fix.

Although a part of the proof standard in mathematics, validity has no purchase in any reasoning that aims (however tentatively) at conclusions carrying information not fully conveyed by the premisses. One of the first tasks of deductivism is to establish that the divide between truth-preservation and ampliation is not as chasmatic as usually supposed. I lack time for deductivists here. So I will say only that I wish them luck. They will need it.

Perhaps this would be a good place to make a certain rather general point. It is that in the situations in which real-life participants actually find themselves, targets are usually contextually-cued. Rather than announcing themselves, they have to be attributed. Very often such attributions are made in conditions of uncertainty. When this fact is laid beside a second general fact, interesting consequences ensue. This second fact is that, by and large, beings like us are cognitive adepts. We tend to get most of the right things right enough to matter. So we survive, we prosper, and occasionally build great civilizations. Taken together this pair of facts suggests that in most situations we should hesitate to attribute to other parties cognitive targets that their behaviour then and there runs foul of. If this is right, then it is a kind of default position that the invalidity of a person's argument is reason *not* to attribute to him a validity-demanding target. Another way of saying much the same thing is that an argument's invalidity is typically no grounds for judging it negatively. (This is the essential import of the Charity Principle. Of course, it's got nothing to do with charity.)

But then why is it, we might ask, that since its inception logic has so steadfastly thrown in its lot with deduction? Here, too, there is more to be said than can be said in this note. Even so, it cannot seriously be doubted that the logic's favouritism towards deduction is explained in large part by its impressive success at getting deduction to surrender its secrets. Deductive logic flourishes because logicians have long since figured out how to do it. Nothing succeeds like success. (But see just below.)

Thesis two. Little in good reasoning is topic-neutral. Aristotle was a master reductionist, and a strategically adroit compacter of complexity. In *On Interpretation* Aristotle asserted that everything stateable in Greek is stateable without relevant loss in the language of (what we know as) categorical propositions. In the *Analytics*, Aristotle said,

and repeated, that all correct reasoning reduces to syllogistic. Had Aristotle been right in the first instance, he would have achieved a striking economy in logical grammar. Had he been right in the second instance, he would have had (thanks to the perfectability proof) something approaching effective recognizability for all reasoning. But not even Aristotle thought that correct reasoning could be detached from the protocols of premiss-selection, concerning which he would insist on what he called premissory “appropriateness”. In one strain of universalism espoused by more recent logicians, an argument’s soundness owes nothing to premiss-content. This topic-neutrality of modern formal logic is strictly a matter of the uninterpretedness of the atomic elements of the language. This is, by the way, is a huge encumbrance for any logic that seeks a regulatory role in reasonings transacted in human languages. (The principal reason is that the atomic components of any natural language often stand in logical relations to one another). Logicians seek to minimize the gap between its uninterpreted derivations and natural language reasoning by supplementation of logical particles, which do admit of interpretation after a fashion. The upshot is a proliferation of systems, concomitant with the growth of logical particles; and although valid arguments remain valid under any valuation of atomic components, the validity is now system-relative, not universal.

Before and after Stephen Toulmin’s heretical insistence that logically correct reasoning be made sensitive to disciplinary peculiarities, post-Fregean logicians could hardly have been unaware that the purported universality of pure logic would be of scant use to the deductive sciences.¹ Arising therefrom was the idea that set theory was part of logic. My view is that either it is or it isn’t. If it is, pure logic is not universal even in the sense of system-relative topic-neutrality. If it isn’t, pure logic can’t capture all the deductions even of mathematics, and so must abandon any pretence of being the *übertheorie* of the deductive sciences.

One might think that there is an attenuated sense in which something like first-order logic does achieve a kind of universality. This is the sense in which the analyses it makes of its target properties – validity, entailment, logical truth, consistency – are correct for any context in which these properties are either invoked or attributed. On this view, entailment is entailment, whether in macroeconomics or biochemistry or politics. But no one who has even a nodding acquaintance with the sheer scope of today’s rivalrous pluralism in logic can make this claim for universality with any prospect of serenity. Every one of these properties is the subject of unsettled land claims; and even on those few occasions when treaties have been signed, they all take the low road (by universalists’ lights) of domain-relativity.

Weeks ago there occurred in Montreux the first international conference on Universal Logic. Part of what the organizers sought to achieve was some indication of the kind of all-embracing logical architecture that might offer, however guardedly, promise of universalizing logic’s pluralistic sprawl. Judging from the papers read there, including my own, the organizers will have been left with no alternative but to trudge home and kick the cat.

Thesis three. The probability calculus distorts much of probabilistic reasoning. In 1953, Stephen Toulmin wrote as follows: “Starting with a study of the syllogism, the

¹ “No more than any other science can mathematics be founded by logic alone; rather . . . something must already be given to us in our faculty of representation . . .” ([Hilbert, 1927, p. 464.]

probability calculus and the calculus of classes, and then coming to the physical sciences, logicians have been misled by their earlier preoccupations and interests, vested as they are in formal systems of considerable refinement and elaboration, into looking for the wrong things.” ([Toulmin, 1953, p. 49]) Later in the same work he expands upon this point.

The mathematical theory of probability has some place in the process of theory-establishing, certainly; but is a more restricted one than logicians have thought. It has a central place only in limited branches of theory, such as statistical mechanics and parts of quantum mechanics The application of the calculus of probability in this sort of way raises no general questions of a philosophical kind, but only particular questions of statistical technique: questions to be answered in terms of the theory of curve-fitting, significant deviations and so on ([Toulmin, 1953, pp. 112-113]).

If the behaviour of individual agents is anything to go on, the standard accounts of inductive inference constitute a significant distortion of the actual record. Can the same be said for the linked issue of probabilistic reasoning in the here-and-now? James Franklin sees in probability an interesting parallel with the concepts of continuity and perspective ([Franklin, 2001]). All three of these things took a long time before yielding to mathematical formulation, and before that happened, judgements of them tended to be unconscious and mistaken.

I have a somewhat different version of this story. Sometimes a conceptually inchoate idea is cleaned up by a subsequent explication of it. Sometimes these clarifications are achieved by modelling the target notion mathematically. Sometimes the clarification could not have been achieved save for the mathematics. We may suppose that something like this proved to be the case with perspective and continuity. To the extent that this is so, anything we used to think of these things which didn't make its way into the mathematical model could be considered inessential if not just mistaken. It is interesting to reflect on how well this line of thought fits the case of probability.

In raising the matter, we are calling attention to two questions. (1) What was probability like before Pascal? (2) How do we now find it to be? Concerning the first of this pair of questions, I think that we may say that, in their judgements under conditions of uncertainty, people routinely smudged such distinctions as may have obtained between 'it is probable that,' 'it is plausible that,' and 'it is possible that.' If we run a strict version of the present line over this trio, then not making it into the calculus of probability leaves all that is left of these blurred idioms in a probabilistically defective state. There is a sense in which this is not the wrong thing to conclude, but it is a trivial one. For if what we sometimes intend by 'probability' fails to find a welcome in the probability calculus, then it is not a fact about probability that the probability calculus honours. But unlike what may have been the case with perspective and continuity, we must take care not to say without further ado that those inferences that don't make the Pascalian cut are mistakes of reason.

Let us take it that, unlike perspective and continuity, idioms of probability (or probability/plausibility/possibility) that don't cut the Pascalian mustard leave residues of philosophically interesting usage. If this were so, there might well be philosophically

important issues, the successful handling of which requires the wherewithal of this conceptual residue. Again, standard answers to Kahneman-Tversky questions don't make the grade of aleatory probability, but they might well comport with conditions on plausible reasoning. What, then, are we to say? That these bright, well-educated subjects are Pascalian misfits or that they are more comfortably at home (though unconsciously) with a plausibility construal of their proffered tasks? If we say the second, we take on an onus we might not quite know how to discharge. It is the task of certifying the conditions under which these non-Pascalian manoeuvres are well-justified. In lots of cases, we won't have much of a clue as to how to achieve these elucidations. Small wonder, then, that what I call the *Can Do Principle* beckons so attractively ([Woods, 2003], [Gabbay and Woods, 2003, 2005a]). This is the principle that bids the theorist who is trying to solve a problem *P* to stick with what he knows and, if possible, to adapt what he knows to the requirements of *P*. One of the great attractions of Pascalian probability is that we know how to axiomatize it. *Can Do* is right to emphasize the advantage to be gained if we could somehow bend the probability calculus to the task to hand. But sometimes, the connection just can't be made.

It is worth repeating in any event that targets usually have to be attributed. When a reasoner's behaviour is flect with the idioms of probability, it is safe to assume that his reasoning embeds a given concept, *K*, of probability. But in general, this alone leaves the *identity* of the embedded probability concept underdetermined. Here, too, a certain caution is called for. If the other party's behaviour turns out to mismanage *K* – if it is defective *K*-reasoning – that is some reason not to attribute to him an interest in *K*-reasoning.

How, then, did Kahneman and Tversky know that their subjects were working with an *aleatory* conception of probability (and making a bad fist of it)? The received answer is that they instructed their subjects so to do. Given the experimental results, their subjects turned out to be either insubordinate or aleatory misfits. An alternative possibility is that the Kahneman-Tversky subjects surrendered to non-aleatory urges triggered by the propositional content of the experimental information, in ways that call into question the calculus' assumption of probabilistic independence.

Thesis four. Scant resources have a benign influence on human reasoning. St. Augustine speaks of “the *eros* of the mind”. Here is Peirce to the same effect, though with less passion, in which love is demoted to an itch:

The action of thought is excited by the irritation of doubt and ceases when belief is attained; so that the production of belief is the sole function of thought ([Peirce, 1931-1958]).

This is a salutary reminder. In a way, it is so obvious as to be effortlessly forgettable that reasoning is wholly without point or value except as facilitating something else. When Leibniz proposed *Calculemus*, the last thing he had in mind was calculation for its own sake. In the main, we value reasoning for the role it plays in belief-fixation and decision-making. Accordingly, reasoning is judgeable only in relation to an agent's cognitive agenda and the cognitive resources available for him in advancing it. Reasoning is also sometimes involved in more purely dialectical or rhetorical ends. Argumentation theorists

are quite right to take note of this; and quite wrong to give it so central a place in their speculations. In its most basic employment, reasoning is an aid to cognition. In contexts of cognitive *disagreement*, it is unavoidable that various dialectical constraints be honoured, if only to discourage question-begging and what Aristotle called “babbling”. But these constraints flow not from the nature of reasoning but rather from the nature of conflict management.

Agents of all stripes, ourselves as well as NASA and MI5, operate under press of scant resources. These include *information*, *time* and *computational capacity*, and, often enough, infrastructural and cultural encouragement, and, of course, money. There is however, a marked difference between individual and institutional agents. In most matters, institutional agents command resources in quantities that individuals could not begin to manage. Agents tend to set their targets in light of the resources available for facilitating their attainment. This serves to mark off a further difference between individual and institutional agents. Given the comparative scantness of his cognitive resources, an individual agent will set targets of concomitantly comparable *modesty*.

In most things, then, individuals fall considerably short of the standards championed by mainstream logicians. But it is a considerable mistake to equate these shortfalls with the cognitively subpar, still less with a failure of rationality. The reason for saying so, in large part anyhow, is that in most things the standards of deductive and inductive logic are embedded in cognitive agendas that it would be unreasonable for the individual to set for himself.

J.S. Mill was on to something important when he observed in *A System of Logic* ([Mill, 1959]) that inductive reasoning is the proper preserve of societies rather than individuals. Yet it seems that we simply cannot forbear in telling our students, year in and year out, that their ampliative reasonings are subpar to the extent that they fall below the standard of inductive strength.

Reasoning, I say, facilitates cognition. Cognition succeeds when certain conditions are met. One has only to look at actual – and largely successful – human practice to see that our cognitive behaviour implies a fallibilist epistemology. That being so, it is part of a human reasoner’s rationality that he reason with a circumspection appropriate to his recognition, *going in*, that his procedures embed the practical certainty of occasional error.² If logic is a science of reasoning, it must take into account – indeed it must honour – the fallibility of the human reasoner when reasoning as he should. The two mainstream logics leave this duty largely unperformed. Deductive logic embeds an epistemology of Cartesian error-elimination, and inductive logic embeds an epistemology of scientific rectitude that attends such things as drug trials by Health Canada. It cannot be surprising, therefore, that these logics are in the main massively wrong for cognitive beings like us. Pascalianized inductive strength may be fine for some of what NASA does, but the individual who is presently seized with an on-rushing tiger experience would be well-advised to forgo the experimental method.

² For present purposes, let’s say that an error is something that its committer has an implied interest in reversing himself on. One of the great virtues of fallibilism is the pressure that it puts on theorists to pay serious attention to the multiple things that collect under the name of “error”. Fallacy theorist have a considerable stake in this.

Thesis five. Theoretical progress and conceptual change are connected. Now that the Humanities are awash in the various scepticisms of what laughably is called “post modernism”, Stephen Toulmin’s heterodoxies about conceptual change may strike us as small beer. The trouble with post modernism is not its relativities and its constructivisms; the trouble rests with the pinheads in English Departments, Faculties of Law and Education, and elsewhere, who construct the arguments on their behalf. If there is a “fallacy of understatement”, it could not better be instanced than by the assertion that Stephen Toulmin is no pinhead. In Toulmin’s hands, and later in Paul Thagard’s, ([Thagard, 1992]) a central idea is that scientific advancement is driven by conceptual change. This is a principle thesis of *The Philosophy of Science*, where it is defended with subtlety and power. In Toulmin’s telling, conceptual change comes with new ways of modelling correlations. Modelling is a way of seeing things, and how one sees things is a function of what one is able to see, and what one is interested in seeing. These are a large part of what makes for the restrained historicism that flavours Toulmin’s epistemology.

When *The Philosophy of Science* made its brazen, cocky appearance, the philosophical mainstream was scandalized. It wasn’t that the book didn’t receive some good reviews (all of Toulmin’s books have found receptive critics), but rather that the book’s doctrines remained decidedly a minority position among philosophers of science.³ This is decidedly odd. At mid-century, English-speaking philosophy – especially its more technical branches – was agog over revolutionary attainments in arithmetic, semantics and physics. In each case, the prime movers of these transformations were quite aware of the conceptual changes that drove their theories forward – Cantor’s “infinite”, Tarski’s “truth”, and Heisenberg’s “particle”.

The conceptual changes occasioned by modelling phenomena in new ways arise from a form of *ambiguation*. This is meaning-change on purpose. Logicians and argumentation theorists of all stripes are determinedly hostile to ambiguity, a lingering influence of Aristotle no doubt. Perhaps the most common complaint made by these practitioners, against those who make progress by changing the subject, is a kind of red herring fallacy. But the trouble, if trouble there is, is not with the meaning-changer’s reasoning; the trouble rests with the fallacy theorist’s affection for naïve realism. If the history of science has anything of metaphysical moment to tell us, it can only be the incompatibility of scientific progress with that kind of realism. Argumentation theorists, accordingly, should lighten up and give their attention to the subtleties of reasonings that both occasion and flow from this creative kind of ambiguation.

Thesis six. Logic should investigate the cognitive aspects of reasoning and arguing. If what we have said about reasoning as a facilitator of cognition is so, and if logic retains (or re-engages with) its historic mission as a science of reasoning, then logic must take account of what cognitive agents are like, what they are interested in and what they are capable of. Given that beings like us come rigged with psychologies as standard equipment, the hostility of logic to psychologism is risibly inapposite. Of course, speaking of ambiguity, psychologism has attracted its own hefty multivocality, ably sorted out in [Jacquette, 2003]. Not everything that “psychologism” has meant or might

³ In the early 1960s, Toulmin gave a standing-room-only lecture at the University of Michigan. He was introduced by the benign William Alston, who banteringly averred that the visitor was the most refuted philosophical writer of the day.

come to mean is right for the logic of cognitive systems. But it can be said with some confidence that logic's toleration of psychologism must embrace the idea of reasoning in the service of cognition and take due notice of reasoners as performers of cognitive tasks. Frege's contempt was another thing. He associated psychologism with two (inequivalent) views that he detested. One is the "Millian" doctrine that the principles of logic are empirical generalizations. The other asserts that the principles of logic can attain no greater degree of objectivity than that rendered by what has come to be called "intersubjective agreement". We find ourselves oddly positioned. At first blush, we would have thought Frege right in his insistence that there is no place for psychology in the theory of sets. Tarski could say the same for model theory, Post for recursion theory, and Gentzen of proof theory. If, as a great many mainline logicians assert, this is all there is to logic, then logic has no room for psychology because logic makes no room for cognitive agents. But, upon reflection, given what he actually takes psychologism to be, it is striking how difficult it has become in the last fifty years to sustain Frege's claims with confidence and assurance. In the one case, Frege has Putnam to contend with, in his insistence that quantum theory has given to logic an empirical cast ([Putnam, 1975]). In the second, Russell and Hilbert demand a hearing, each arguing, for somewhat different reasons, the theoretical legitimacy of stipulations when accepted by the requisite research communities ([Russell, 1903], [Hilbert, 1935]).

Whatever may be said about the four princely domains of mathematical logic -- set theory, modal theory, proof theory and recursion theory -- the past thirty years has seen the re-engagement by logic of agent-based reasoning. If I may put it this way, logic proper has had a role in this transformation, what with the emergence of belief dynamics, situation logics, dialogue logics, time and action logics, among many others.⁴ A second source of change is computer science and AI, what with their emphasis on non-monotonicity, default and defeasible reasoning. A third has been informal logic, and argumentation theory more generally.⁵ A theme that runs through all these developments is that theories of reasoning must attend to how human beings actually do reason. Where disagreement exists, it turns on the problem (or anyhow the challenge) of validating those norms of reasoning which nearly everyone seems to agree are, on occasion, violated in actual practice. I shall return to this important issue when we examine our next thesis, just below. For now, it suffices to say that the admissibility of psychological factors into any logic that is serious about cognitive agency is a no-brainer.

⁴ We might note in passing that the papers of the so-called Woods-Walton Approach to the fallacies – against which Michael Scriven would bray with characteristic *□lan* – was set largely in this sector of logic's own transformations ([Woods and Walton, 1989]). In those early days, Walton and I would repose the burdens of *petitio principii* on Kripke models for intuitionistic logic. We would take it as given that relevant logic *à la* Pittsburgh and Canberra would suffice for irrelevancies of inference. And so on. The two of us have since wised up, and now pursue somewhat different, and more comprehensive, methodological paths, concerning which it is unlikely that we are both right ([Walton, 1995], [Woods, 2004]).

⁵ Other important sources are cognitive psychology and empirical economics. Case-based analyses of administrative and corporate decision-making have been with us for a hundred years. The common law is centuries old. Logicians are just starting to take note of these. Stephen Toulmin is an exception. They have been in his sights, one way or another, since *The Place of Reason in Ethics*. See also *The Abuses of Casuistry* and *Return to Reason*.

More generally, it is helpful to conceive of a cognitive agent as a device that executes a certain *cognitive psychology*. An open question is the extent to which normative considerations could be handled by a requisite *epistemology*.

Thesis seven. Ideal models are unsuitable for normativity. Writing at mid-century, Toulmin allows that in

[i]n practice, of course, we do not always adopt the most satisfactory methods of argument – we generalize hastily, ignore conflicting evidence, misinterpret ambiguous observations and so on. We know very well that there are reliable standards of evidence to be observed, but we do not always observe them. In other words, we are not always rational; for to be ‘rational’ is to employ always these reliable, self-consistent methods of forming one’s scientific beliefs, and to fail to be ‘rational’ is to entertain the hypothesis concerned with a degree of confidence out of proportion to its ‘probability’. ([Toulmin, 1950, p. 164])⁶

Here is Toulmin in his first book, *The Place of Reason in Ethics*, making a point that to this day is gospel among theorists of human behaviour. ([Cohen, 1982] and [Gigerenzer and Selten, 2001] are conspicuous exceptions.) This is the idea that when humans reason in ways that fail to comport with the relevant theories – deductive and inductive logic, probability theory, decision theory, among others – they perform irrationally.⁷ Perhaps you will agree with me that just three years later, Toulmin had wisely modified his position. (See again the quotations from *Philosophy of Science* at the beginning of our discussion of thesis three). What Toulmin was saying in 1953 is that when a piece of human reasoning fails to honour the requisite theory, then *two* possibilities are open for consideration. One is that the defections reveal a lack of rationality. The other is that the theory whose principles are defected from don’t apply to the cases in question. Much of our discussion so far has it that it is a theme of Toulminian import that when these theories are first-order logic, inductive logic, the probability calculus and rational decision theory, it is nearly always a mistake to suppose that human reasoning, upon pain of irrationality, must conform to their theorems.

Against this is the insistence by a great many practitioners of these disciplines that the defected-from principles are normatively valid, and that it follows from this that a failure to comport with them is indeed rationally subpar. If this is right, it embodies a hugely important insight into human rationality, one that sustains a pessimism rivalling the anti-cognitivism of *Genesis*, running roughshod over the Charity Principle, which “requires that we make the best, rather than the worst possible interpretation” ([Scriven, 1976, p. 71]). This is something to pause over. It convicts us all of widespread irrationality, and it makes us massive cognitive misfits.

Still, these theorists have reasons for their view of the matter, the two most prominent of which are: first, that the principles of these disciplines are articulable in

⁶ It is not wholly clear whether Toulmin’s quotation marks are intended to be admonitory rather than merely emphatic.

⁷ Even more emphatic is Reichenbach in *The Rise of Scientific Philosophy* ([Reichenbach, 1951, p. 308]): What gives priority to science? (he asks). “Who can judge about the theory of knowledge if he has not seen knowledge in its most successful form?” Malcolm, reviewing the book, thought this “primitive nonsense”.

ideal models in which they are analytic; and, second, that their normative force is guaranteed by a kind of reflective equilibrium. Both of these claims are problematic. The analyticity claim is beggared by question-begging. If, for example, someone says that it is not true that belief is closed under consequence, it is unavailing to counter with the assurance that there is an ideal model in which the it is analytically true. Not only does “it’s analytically true” not work *as a rejoinder* to “it’s not true”, but being true-in-a-model makes no claim, just as it stands, on being true in the world, so to speak.⁸ Close kin of the analyticity rejoinder is the true-in-a model response. So, again, someone says that various would-be normative principles don’t hold. The response is that there is a model *M* in which these principles are true. This might be true, but it is irrelevant. Anything true-in-*M* might actually be false.

The reflective equilibrium defence of the normativity of ideal models falters at the starting gate. It may be true that by and large the cognitive behaviour of beings like us is in reflective equilibrium with what could with justice be called the principles of right reasoning. But there is not a jot of support in this for the proposition that the principles privileged by these ideal models *are* the principles of right reasoning for beings like us. Accordingly, we may think it best to abandon these defences, and direct our quest for the normative elsewhere. On one approach, a good place to look is indeed our general practice, but not because it is in reflective equilibrium with the proffered ideal models, but, again, rather more because our run-of-the-mill reasoning doesn’t kill us: We survive, we pass on our ratiocinative devices to the descendent class, we prosper, we do particle physics, and occasionally we build great civilizations. Either we do these things on the basis of such knowledge as our cognitive processes are capable of attaining for us, or in the absence of it. If the former is true, the pessimism of the ideal model approach is unjustified. If the latter is true, the value of knowledge is debased.

Thesis eight. The Can Do Principle should be applied with caution. In most things cognitive, the *Can Do Principle* bids the individual to try to tailor the advancement of his agendas to principles he is already at home with and to problem-solving techniques over which he has attained a certain mastery. *Can Do* requires the problem-solver not to start from scratch if he can help it. *Can Do* is one of the principal canons of the cognitive economies in which agents of all types operate. But there are limits. A hammer’s usefulness in pulling nails gives it no leg up in the removal of paint from walls of one’s dining room. Using a valuable and versatile tool for a task for which it is unsuited is a misbegotten, false economy. When the misapplication is inadvertent, there is room to postulate the presence of the *Make Do Principle*. *Make Do* is a degenerate case of *Can Do*. It has a twofold appeal. It allows the cognitive agent the comfort of doing something rather than nothing. It is also abetted by the ideal model methodology of normativity, for which it is tailor-made. In his more composed moments, the reasoner will see the disutility of methods and applications of principles that have no standing in the problem-space at hand. But not only is this something that he sometimes cannot see, but, not seeing it, *Made Do* is given a degree of encouragement by the attractive example of scientific progress via conceptual change.

⁸ Let us note for the record that *Toulmin*’s models are not normative but abductive. They are ways of seeing which facilitate our accounting for data ready to hand.

Any probability theorist who knows his onions will be aware that after Pascal, probability *changed*. This presents us with a fundamental question: When *K* is a new conception of something, does it extinguish its predecessor-concept, or does it foster a new ambiguity which leaves the old concept standing? The probability theorists' inclination is to see its axioms on the analogy of the principle of rectilinear propagation, after which the scientific concept of light both changed *and* was laid open to an improved scientific understanding. Why could we not proclaim a similar law for probability -- the principle Pascalian compounding -- thanks to which the concept of probability both changed *and* was made susceptible to a better theoretical (i.e., mathematical) understanding? Perhaps we could, but doing so does not answer this fundamental question. Certainly we don't expect an individual's ordinary reasoning about light to comport with the rectilinear propagation principle. An individual who managed to live his life wholly innocent of the disclosures of optical geometry might well run up an error-free record in his reasonings about light and shadow in rainy Vancouver. This tells us that the laws of optical geometry are not normative for ordinary reasoning about light. Probability theory (and deductive logic and decision theory) are quite different in their normative presumptions. I wonder about this. Granted that the aleatory theorems are binding on certain patterns of reasoning "in statistical mechanics and parts of quantum mechanics ... [or concerning] particular questions of statistical technique: questions to be answered in terms of the theory of curve-fitting, significant deviations and so on", it is left wide open as to what principles apply to individual agents when they estimate the probability of Lisa being a lawyer with a big firm who works on environmental issues. Unreflective resort to the calculus of probability gives Toulminian offence for its procrusteanism (cf. [Toulmin, 1953, p. 126]).⁹ By my lights, and to the same effect, it is a matter of *Make Do*. It is "looking for the wrong things."

Although they never put it in these terms, one of the central accomplishments of advancers of the informal logic research programme, which *The Uses of Argument* did so much to motivate and shape, is the stiff resistance they have shown to procrusteanism and *Make Do* in the analysis of human reasoning and argument.¹⁰ It is a welcome turning in the human sciences, but not as yet one graced by a place in the mainstream. More needs to be done. Professor Toulmin, I trust that you are listening.

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⁹ To be clear, Toulmin's target in this instance is Eddington, not Pascal.

¹⁰ Mind you, in giving validity so free a rein, pragma-dialectics is something of a hold-out.

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