Aristotelian indeterminacy and the open future

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I explore the thesis that the future is *open*, in the sense that future contingents are neither true nor false. The paper is divided into three sections. In the first, I survey how the thesis arises on a variety of contemporary views on the metaphysics of time. In the second, I explore the consequences *for rational belief* of the 'Aristotelian' view that indeterminacy is characterized by truth-value gaps. In the third, I outline one line of defence for the Aristotelian against the puzzles this induces: treating opinion about future contingents as a matter of *fictional belief* rather than simple belief.

I think this defensive tactic faces major obstacles. The ultimate conclusion, therefore, is that if future contingents are indeterminate, we should find some non-Aristotelian treatment of the indeterminacy involved.

1 Aristotelianism about the open future

Aristotle writes:

> Let me illustrate. A sea-fight must either take place to-morrow or not, but it is not necessary that it should take place to-morrow, neither is it necessary that it should not take place, yet it is necessary that it either should or should not take place to-morrow. ...
>
> One may indeed be more likely to be true than the other, but it cannot be either actually true or actually false. It is therefore plain that it is not necessary that of an affirmation and a denial one should be true and the other false.
> (Aristotle, 1941, §9)

One standard interpretation of Aristotle's writings on the subject of future contingents has him accepting each instance of the *law of excluded middle*:

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$$p \vee \neg p$$

but denying future contingent instances of the *law of bivalence*:[1]

$$T(p) \vee T(\neg p)$$

Future contingents are but one arena among many one might think of as containing "open" or "unsettled" or "indeterminate" questions. Just as Aristotle is supposed to have linked the openness of the future contingents with failures of bivalence, one might in general associate indeterminacy with failures of bivalence. I will call a theory of indeterminacy that combines the preservation of classical tautologies (such as the law of excluded middle) with truth value gaps, "Aristotelian". Of course, this might be unfortunate if exegesis shows Aristotle himself not to be an Aristotelian,[2] and we should be aware that various views might be Aristotelian in preserving classical logic and dropping bivalence, while disagreeing profoundly with Aristotle as to the source or nature of the indeterminacy, even if the topic we focus on is future contingents. Nevertheless, the label will be useful.[3]

[1] An immediate question to ask about this formulation is whether 'p' is used or mentioned in this formulation. At points this matters—when it does, I discuss it in footnotes. For the most part, though, it is safe to read $T(p)$ as the application of an object-language truth operator to p: i.e. "it is true that p".

[2] This interpretation of Aristotle is defended in detail in Crivelli (2004, ch.7). One worry with the interpretation is that Aristotle elsewhere (in the Metaphysics, for example) seems to defend bivalence). Scholars disagree on the matter. For an alternative interpretations of the passages, consistent with bivalence, see for example Whitaker (1996, p.125) (Whitaker's view is that for Aristotle future contingents are either true or false, but it is not settled which truth value they have. For contemporary development and defence of this kind of view of the open future, see Barnes and Cameron (2008).) In another direction, Lukasiewicz (1967) develops a non-classical logic that gives up the law of excluded middle for future contingents. I could, in fact, run something like the argument below in this setting, but this will not count as 'Aristotelian' in my sense (and it seems that the consensus is that it does not approximate Aristotle's view either).

I do not wish to prejudge these exegetical matters. In calling the position under discussion 'Aristotelian', while setting aside the exegetical questions, I am following a fairly common practice: see, for example, Rice (2007).

[3] *Supervaluationism* is one of the leading contemporary treatments of indeterminacy. On a standard presentation, it is Aristotelian. The term is often now linked to a distinctive *semantic* account of the nature of indeterminacy. (Central works on 'semantic indecision' implementations of supervaluationism are Fine (1975) and Keefe (2000). For a stripped-down description of the theory, see Williams (forthcoming).).

In origin, however, the notion of supervaluationism referred to a more technical thesis—a formal device for deparameterizing truth that (within a classical background theory) lead to the distinctive combination of classical tautologies and truth value gaps I have called 'Aristotelianism'. For this original usage, see in particular van Fraassen (1966) and (importantly for our later discussion) Thomason (1970).

In recent times, several theories on which indeterminacy is *metaphysical* rather than semantic in origin have been developed, that are in some respects analogous to supervaluationism. See (Akiba, 2000, 2004; Barnes, 2006; Williams, 2008). The version I sketch in (Williams, 2008) is supervaluationist in the original, technical sense, and consequently Aristotelian.

Contemporary Aristotelianism

What metaphysical pictures might motivate the thesis that future contingents are indeterminate in the Aristotelian sense? Several contemporary views have this upshot. I will not attempt to survey all the options here, but I will sketch two illustrative cases: one an *eternalist* setting, and one in a *non-eternalist* setting.

The eternalist conception of the open future on which we focus is the following. The future and past exist in exactly the same (tenseless) sense as the present does. 'The present' is just the time that we speakers and thinkers happen to be located at. The *openness* of the future derives from a *branching structure* possessed by time. In particular, there (tenselessly) exist futures where this fair coin comes down heads on the next ten flips, and there exist futures where the fair coin comes down tails. The various future events are future-related to our present situation: but they are not past or future related to each other. 'x being in the future of y' is a *partial ordering* of times, and of events that take place in time.

B-theoretic branching time is a *metaphysical* view, not directly a semantic one. It doesn't directly tell us the truth value of the claim 'the coin will come up heads tomorrow' as uttered by me, now. In a non-branching time, we would just appeal to the (unique) coin-flipping event that is tomorrow-related to the present time. The future contingent will be true iff that coin-flipping event involves the coin landing heads; otherwise it is false. But if the future is branching there is no unique such event by means of which to resolve the question.

It is in this kind of context that Thomason (1970) suggests a supervaluational resolution of the truth-status of such claims. The idea is that 'the coin will land heads tomorrow' if on *all the tomorrows future-related to the present time* the coin will land heads. It is false if the coin fails to land heads on the tomorrow of each future branch. In cases where it lands heads on some branches but not others, the future contingent will be neither true nor false. (On the other hand, 'the coin will land heads or it will land tails' is made-true by every branch whatsoever at the tomorrow time of every branch whatsoever—so the disjunction will automatically be true). Thomason's supervaluational semantics for branching time is thus an instance of Aristotelianism.[4]

[4]The 'relativistic' semantics favoured by Macfarlane (2003) has the same underlying structure—except that the 'futures' over which we supervaluate are fixed not only by the time of utterance, but also by the time of evaluation.

B-theoretic branching metaphysics should be taken seriously. Arguably, the best interpretation of the *Everettian* interpretation of quantum mechanics involves this kind of structure.[5] Combining what some argue to be best interpretation of best fundamental science with Thomason's supervaluationist take on the semantics of branching time, and we have an Aristotelian open future.

Let us now turn to non-eternalist views of the future. Such views don't commit themselves to future events with the same ontological status as present events. There are non-eternalist positions which give rise to Aristotelian open-future theses. Here is a simple example of this sort. A *B-theoretic growing block theorist* believes only in the (atemporal) existence of past and present objects and states of affairs. The view is B-theoretic, since there is nothing irreducibly tensed in the metaphysics—the ontological view is just as the B-theoretic eternalist would believe it to be, except that it is "chopped off" exactly where we happen to be standing right now.[6] The distinctive view is that there simply are no future states of affairs of any sort to stand in the 'in the future of' relation to the present. Of course, they think that there *will soon be* such states of affairs—but such tensed truths are to be *explained* in terms of the fundamental, growing block ontology.

How could the B-theoretic growing block theorist deal with future contingents? In one special case, an elegant solution presents itself. Let us suppose that the growing block theorist can help herself to *laws of nature*—and those laws of nature are deterministic. Under this assumption, there will be a unique 'possible big-bang to heat-death' description of the world that exactly duplicates the actual growing block on its present slice, and (b) continues beyond the present moment in accordance with the laws of nature. We can regard the range of such complete world-descriptions as the *possible worlds*, and call the history selected by the recipe just given the *actualized world*. Thus, if the past (in the concrete reality we inhabit) contains

For future contingents which remain unsettled even at the time of evaluation, we have the Thomason results. If the moment of utterance and the moment of evaluation are one and the same, then we have exactly the same distribution of truth values as on the original Thomason proposal—so I won't consider this version separately).

[5] See Saunders and Wallace (2008) for advocacy of this view

[6] The more usual growing-block theorist will be A-theoretic: thinking that additional 'layers of being' *will be* added to the block as time flows on (where the *will be* here is a tensed claim that is not reducible to anything in the ontology. A-theoretic growing block theorists may end up with a position similar to the one sketched below, but they have extra resources to play with if they want to resist the conclusion. They might, for example, simply believe that the truth or falsity of future contingents is a brute matter—perfectly determinate, but not reducible to anything in the ontology itself.

dinosaurs, the actualized world-description will represent *there being dinosaurs*. But the construction of the actualized world will secure more. If present-and-past facts and laws of nature allow in principle one to derive the existence of intelligent robots, then by construction the actualized world will represent *there being intelligent robots*. We can then secure an account of the truth and falsity of future contingents by saying that for something to be true is it for to be *represented to be true by the actualized world*.[7]

Since the laws of nature have been assumed to be deterministic, there's no particular reason to think that the future is 'open' in this case—and in particular, no reason to believe that future contingents will be truth-value gaps. But what of the *non-deterministic case*? Determinism allowed us to select a *unique* 'actualized' big-bang-to-heat-death history. But if the laws of nature are not deterministic, there may be *multiple* such complete histories compatible with the laws of nature and the past-and-present facts.

There are two obvious options that present themselves. In such a case, we might say that the 'actualized' world is a *branching* one—the very sort of history that a B-theoretic branching theorist holds to be the way that reality itself is. Or we might allow for there to be *multiple actualities*—we replace the unique 'actualized' world of the deterministic setting with a multiplicity of diverging 'actualized' worlds. Either way, the story about how truths get fixed must be complicated. If there are multiple actualities, then we can't just say that to be true is to be represented as true by the actualized world—for there is no unique such world. If there is a branching future, then we could retain the old story—but we will be back to the issue that confronted the B-theoretic branching eternalist—it's not immediately clear what it is for a future contingent to be true in a branching history (even once we pick out a 'moment of utterance').

The supervaluational solution is an obvious candidate in both cases. In the multiple actualities setting, we hold that to be true is to be represented as true by each actuality.[8] In the branching-world version, we rerun the Thomason semantics to tell us what the truth values of future contingents uttered at moments within the branching world should be—and then just let

[7] What of the growing block theorist's claim that 'future things don't really exist'? I think that the right response is to introduce an operator *Really* or *Fundamentality*. for 'fundamentally, *p*' to be true, *p* has to be true, not at the actualized world, but rather at a world-description exactly matches reality, in particular by failing to represent the existence of anything other than past or present objects.

For further discussion of this kind of operator, and the sort of resources that can be used to flesh out the sort of framework introduced above, see Williams (2007).

[8] See in particular Williams (2008).

the truth value of the future contingent as before be fixed by the truth-value it is represented to have by the unique actualized branching world. Either way, if it is an indeterministic matter whether the coin will land heads or tails, then we have Aristotelian indeterminacy back—it will be neither true nor false that the coin lands heads.

2 From Aristotelianism to Rejectionism

In the previous section, we outlined Aristotelian or truth-value gap conceptions of indeterminate future contingents. We shall now set aside the specific case of indeterminacy for a while, and argue for some distinctive general characteristics of Aristotelian indeterminacy.

A natural first thought about Aristotelian indeterminacy is the following: that given an indeterminate p will be neither true nor false (i.e. their negations aren't true either), from an external perspective we shouldn't believe a future contingent, nor its negation.

I think we can argue for a strengthened version of this result: that if we are certain that p is indeterminate in the Aristotelian way, we should have no positive degree of belief in p, and no positive degree of belief in $\neg p$ either. We should utterly *reject* both. So Aristotelianism, I will argue, commits us to rejectionism.

I know of two arguments for this conclusion. The first rests on the idea that *truth norms belief*—and more specifically, on a generalization of this rule to partial beliefs in the spirit of Joyce (1998). The second rests on distinctive *logical* theses that the Aristotelian is arguably committed to, together with a general thesis about how logic constrains rational belief.

The idea that truth norms belief can be expressed as follows. One should: believe p only if p is true.[9] So formulated, the truth-norm applies to the notion of *all-or-nothing belief*. I wish to work instead with *partial beliefs* or *credences*—a measure of how strongly we believe p (where believing to degree 1 is maximal, and believing to degree 0 is minimal). How should we generalize the plausible thought that truth norms belief to this setting?

[9] Some clarification is in order. First, one may *blamelessly* fail to meet this constraint—if for example, one has misleading evidence for p's truth, when p is in fact not true. But the idea here is that, from an external perspective, there is a *fault* with a system of beliefs that violates this norm.

See Williamson (2000, ch.11) for a discussion of norms of assertion (in that case, in terms of knowledge rather than truth) that defends the distinction between violations of a norm that are *mere faults*, and those for which it is appropriate to *blame* an agent.

Here is a suggestion. For a specific proposition p, we can measure the difference between our degree of belief in p, and its truth value. The truth norm for graded belief will then tell us that we should minimize this difference. For example, let $t(p) = 1$ iff p is true, and $t(p) = 0$ otherwise. Then define an 'aleithic inaccuracy score' as $|t(p) - b(p)|$—the absolute difference between a measure of the truth value of p and the degree of belief.[10] A normative constraint on partial belief can be put as follows

one should: minimize the aleithic inaccuracy score of one's degree of belief in p

Suppose now p is indeterminate—neither true nor false, so that $t(p) = 0$ and $t(\neg p)$=0. To *minimize* the inaccuracy score—i.e. to match ones beliefs as closely as possible to truth value measures—then we'd ideally like to have $b(p) = 0$ and $b(\neg p) = 0$. So if the normative scoring constraint is correct, both p, and $\neg p$, should be utterly rejected when p is indeterminate in the Aristotelian way.

Of course, this argument will convince only insofar as the truth-norm on graded belief is accepted. But we can conclude at least this much: someone who is an Aristotelian indeterminist, and wants to avoid the rejectionist conclusions just sketched, owes some alternative elaboration of the way that truth-values normatively engage with degrees of belief.

The second argument for rejectionism given Aristotelianism turns on a normative role for *logic*. To build up this case, we have to first explore some distinctive logical features of the Aristotelian setting. In the next subsection, I will focus on arguing that these features are present in the Aristotelian setting, before moving on to consider the rejectionist consequences.

[10]The notion of aleithic inaccuracy score, and the use of it to impose normative constraints on partial belief, is inspired by that of Joyce (1998), who uses the notion of score (in the context of a classical, bivalent assignment of truth values) to argue that degrees of belief should conform to the classical probability calculus (see in particular ibid p.579 "the norm of gradational inaccuracy" and p.589 "expected inaccuracy minimization").

Rather than simply 'defining' the score as we have above, Joyce offers an *axiomatic* characterization of formal characteristics that score-functions should have, and argues that probabilistic degrees of belief minimize overall scores for all one's beliefs. Lots of the interest and complexity of Joyce's work derive from the fact that he is trying to construct a score *for an overall assignment of degrees of beliefs to propositions*. But in the text above, I concentrated on a norm for a single belief. But given Joyce's axioms, we can plausibly derive the rule in the text as a special case. Joyce's axiom of dominance (p.593) tells us that, all else equal, the accuracy of one's credence in p "always increases as it approaches [the truth value of p]". Thus, all else equal, minimizing the absolute difference between truth value and credence in p is a way of minimizing the inaccuracy score of the overall credence function.

(It should be noted that Joyce's work takes place with a background assumption of bivalence, so it would be a proper extension of his work to apply it to the case at hand.)

Logic induced by truth value gaps

Consider sentences of the type:

$$p \wedge \neg T(p)$$

Are such sentences logically consistent? In the special case of supervaluationism, Williamson (1994) argues that they are inconsistent. Arguably, the point generalizes to any Aristotelian framework.

The generalized argument has as a starting point a certain general characterization of consistency and consequence:

1. ϕ follows from Γ iff the truth of each member of Γ guarantees the truth of ϕ
2. ψ is logically inconsistent iff an explicit contradiction follows from ψ

"Guarantees" is left intentionally imprecise here: we might read it as "necessitates", or characterize it in terms of quantification over admissible interpretations as on Bolzano-Tarskian characterizations.

We can now argue that sentences of the form $p \wedge \neg T p$ are logically inconsistent. To see this, by (1) and (2) we need to show that the truth of $p \wedge \neg T(p)$ guarantees the truth of an explicit contradiction; i.e. that $T(p \wedge \neg T(p))$ guarantees the truth of an explicit contradiction. But, it seems, we can show this. Distributing T across the conjunction and using factivity, we get to $T(p) \wedge \neg T(p)$—an explicit contradiction. That's an explicit contradiction; but of course, we wanted the *truth* of an explicit contradiction. But by the classical rule of explosion, from a contradiction we can reach any claim we like, and so from the above we can in particular derive the truth of some contradiction.

More formally:

1.	$T(p \wedge \neg Tp)$	assumption	1
2.	$T(A \wedge B) \supset (T(A) \wedge T(B))$	premise	2
3.	$Tp \wedge T(\neg Tp)$	instance of 2, MP	1,2
4.	Tp	conjunction elimination, 3	1,2
5.	$T(\neg Tp)$	conjunction elimination, 3	1,2
6.	$T(A) \supset A$	premise	4
7.	$\neg Tp$	instance of 6, 5, MP	1,2,6
8.	$Tp \wedge \neg Tp$	conjunction introduction, 4,7	1,2,6
9.	$(A \wedge \neg A) \supset B$	classical tautology	9
10.	$T(0 = 1)$	instance of 9, 8, MP	1,2,6,9

The above formulation aims to rely on only the least controversial rules of inference: conjunction introduction and elimination, and modus ponens for the material conditional. This allows us to highlight the assumptions on which the proof rests, which amount to a single classical tautology (the schematic sentence-form of the rule known as 'explosion') and two rather obvious principles governing truth: that it distributes over conjunction and that it is 'factive'.[11]

[11] In fact, the above argument will need to be adapted to the details of this or that setting. If consequence in the relevant sense is a relation among sentences, then we will need to examine where in the above we are using and where we are mentioning the sentence '*p*'.

The main interpretation of consequence where this may matter is on the Bolzano-Tarski elucidation, where 'truth-preservation' is understood to be: truth preservation *on any logically admissible interpretation*. For the truth predicate that is *used* in premise 1 will then be relative to one interpretation (call it *i*). This leads to two worries. First, in order that we genuinely have a contradiction at 8, it needs to have the form $T_i\ulcorner p\urcorner \wedge \neg T_i\ulcorner p\urcorner$. But for this to be the case, we need an instance of 6 to read: $T_i(\ulcorner\neg\ulcorner p\urcorner \text{ is true}\urcorner) \supset T_i\ulcorner p\urcorner$. That is, we need to get from the truth-on-i of " $\neg\ulcorner p\urcorner$ is true" to the lack of truth-on-i of "*p*". And one can imagine circumstances in which that is resisted. For example, suppose that *i* is an unintended interpretation in which quotation works strangely, so that the symbol "*p*", which we can assume would normally be a name for the sentence *S*, in fact designates the first sentence of this essay. Or alternatively, suppose that 'true', which would normally be apply only to true sentences were on *i* to express the property of *being written in red ink*. On those deviant interpretations of the vocabulary, the needed inference simply wouldn't go through.

The moral here is that in order to evaluate whether the argument for which we've given a schematic formulation above really goes through, we need to look at the details of the theory of consequence involved. In particular, we need in effect to treat 'truth' and auxiliary devices such as quotation as logical constants, which keep their intended interpretation on all logical admissible interpretations. Resisting this allows us to resist the argument for the inconsistency of the sentence mentioned.

Getting into the details of this or that elaboration of the Aristotelian case need not weaken the overall argument. On a standard supervaluationist take on these issues, orthodoxy has that formulation of (1) in terms of a certain truth (or determinacy) *operator* (it is true that…) can be shown to be inconsistent. Worries about quotation can then be evaded. I argue in Williams (forthcoming) that the orthodox description of these cases still requires treating the truth-operator as a logical constant, which is not obviously the right thing to do.

Nevertheless, I think there is value to presenting the more general argument for which these issues are suppressed. For some would like to characterize consequence in more object-language terms: as necessary truth-preservation for some suitably strong sense of necessity. So on at least some readings, the argument has a good

What we have argued for is that sentences of a certain type are inconsistent; that is:

$$p \wedge \neg T p \models$$

We already emphasized that the Aristotelian setting is non-revisionary of classical logic in the sense that familiar classical tautologies (such as the law of excluded middle) are retained. But the above argument shows that we can extend to this general setting a further point that Williamson makes about the supervaluational setting. This is that while classical *tautologies* might be retained, certain classical *inference rules* will not be validity-preserving. In fact, we can argue that inference rules such as reductio, proof by cases, conditional proof and the like must be given up.[12] Suppose for example that reductio was a legitimate rule of inference. Then for any p, we could argue from the above to the claim that $\neg(p \wedge \neg T p)$ is tautologous (by reductio); which is just to say that one direction of the T-rule is a tautology:

$$\models p \supset T p$$

Notoriously, instances of this direction of the T-scheme conflict with the presence of the kind of truth value gaps in which the Aristotelian believes. Various arguments have been given illustrating this (Williamson, 1994; Wright, 1992). For completeness I set out one below:

chance of going through as stated. Furthermore, if the above argument breaks down on this or that characterization of consequence, it will, I think, be instructive to see *where* it breaks down.

[12]The general point that classical rules are seemingly undermined by standard supervaluations is noted at several points, right back to van Fraassen (1966). The particular instance given here is, I believe, due to Williamson, and is a particularly nice illustration, technically.

1.	$\neg Tp \wedge \neg T\neg p$	premise	1
2.	$\neg Tp$	conjunction elimination	1
3.	$\neg T\neg p$	conjunction elimination	1
4.	$p \supset Tp$	premise	4
5.	$\neg p \supset T\neg p$	premise	5
6.	$\neg p$	modus tollens, 2,4	1,4
7.	$\neg\neg p$	modus tollens, 3,5	1,5
8.	$\neg p \wedge \neg\neg p$	conjunction introduction	1,4,5

All the moves featuring in this argument are licensed classically. So something has to go if we are to retain the distinctive Aristotelian claim that in certain cases bivalence fails. In fact, the reductio step that bridges the gap between our two arguments, converting the inconsistency of instances of $A \wedge \neg TA$ into premises 4 and 5 of the above argument, is the obvious candidate for rejection.[13]

Consequences of logical revisionism

Quite generally an Aristotelian setting is committed to a certain kind of logical revisionism—a revisionism often associated with supervaluationism. Many see this revisionism as a major objection to the supervaluational framework, and given the above, presumably they would extend this charge against any Aristotelian treatment of indeterminateness. The thought is that standard inferential *practice* is represented faithfully by classical inference rules. Adopting a theory on which the token inference moves are of an invalid type objectionably threatens the standard classical rationale of inferential practice.

I argue in Williams (forthcoming) that *this* objection to supervaluationism (and by extension, Aristotelianism) is a red herring. Even though the classical inference rules are undermined, we can spell out in a systematic way inference rules extremely close to the classical ones which are

[13]If we are to be Aristotelians, and so preserve all classical tautologies, then our options in resisting the argument are severely limited. The conjunction elimination and introduction steps in both arguments can be eliminated by appeal to corresponding conditionalized sentence-forms together with modus ponens. So our choice is essentially between two very compelling principles governing T (distribution and factivity), the rule modus ponens, and the rule reductio. Especially since a restricted form of reductio can be saved (see below), the last is the obvious principle to give up. This meshes with the orthodox response to these puzzles in the case of supervaluationism.

supported by the system. So though we lose the letter of the classical rationale for various token inferential moves, we get a replacement rationale: there is no good argument that I can see that the revisionism induced undermines inferential practice.

The usual complaint against the sort of logical revisionism induced by failures of bivalence is unpersuasive. However, there is a second manifestation of the revisionism that is potentially far more philosophically rich. This is revisionism over *partial beliefs*.

The key principle needed for this manifestation of the revisionism is the following logic-probability link:[14]

> Improbability should never increase over a valid argument: the conclusion of a valid argument should be at least as probable as its premise. More generally, the improbability of the conclusion should be less than or equal to the sum of the improbabilities of the premises.[15]

In particular, this ensures that any tautology must have a maximal probability, and that any contradiction must have a minimal probability. We can assume that these are 1 and 0 respectively.[16]

As is familiar, probabilities admit of various interpretations. Two relevant interpretations for our purposes are:

- The partial belief interpretation: probabilities as degrees of belief (cf, e.g. Kaplan, 1998)
- The epistemic interpretation: probabilities as the probability of truth given our evidence (where evidence is identified with what we know). (Williamson, 2000, cf.)

Construed doxastically, the probability-logic link tells us that validity norms partial beliefs: that there is something bad about assigning greater credence to the premises of a valid argument than its conclusion. I think the principle is best precisified in this setting as a *consistency constraint* on rational partial belief. Specifically *if one has a degree of belief in each of the premises*

[14]I will be using 'probability' here in a way that does not presuppose that it obeys the standard Kolmogorov axioms. It will be convenient to label certain systems 'non-classical probabilities' which others might prefer to call non-probabilistic mathematical representations of uncertainty. The issue, I take it, is purely terminological.

[15]In fact, we shall only need to appeal to this generalized version in one special case: to argue that if we have degree of belief 1 in a conjunction, we should have degree of belief 1 in each conjunct.

[16]1 and 0 might be treated simply as notations for the maximal and minimal elements of whatever structure of probability values we are working with. In principle we need not assume that the values are linearly ordered. However, then what result we get from our constraint will depend crucially on the details of the formulation. My working assumption is that, as with classical probability theory, the values can be linearly ordered and represented by the reals in the range [0,1]. But this is a strong structural assumption and it would be interesting to see how far we could get weakening it.

and conclusions of a valid argument, then one will be pro tanto irrational unless those degrees of belief are related in the way that the probability-logic connection prescribes.[17] Construed epistemically, the probability-logic link tells us that evidential support respects logic: the degree of evidential support for the conclusion of a valid argument can never be less than for its premises. On both interpretations, I take it, the probability-logic link is compelling.

The probability-logic link in conjunction with our earlier results on the logic of the Aristotelian/truth-value gap treatment of indeterminacy gives us the *rejectionist* results mentioned earlier. Let p be a known failure of bivalence.[18] We have of course: $A,B \models A \wedge B$. By the probability-logic link, the improbability of $A \wedge B$ must be less than or equal to the sum of the improbabilities of A and B, that is:

$$1-\rho(A)+1-\rho(B) \geq 1-\rho(A \wedge B)$$

First let $A = p$ and $B = \neg Tp$:

$$1-\rho(p)+1-\rho(\neg Tp) \geq 1-\rho(p \wedge \neg Tp)$$

But the $p \wedge \neg Tp$ is inconsistent, and so must be probability 0.[19] $\rho(p \wedge \neg Tp) = 0$. So we get:

$$1-\rho(\neg Tp) \geq \rho(p)$$

By exactly parallel reasoning we have:

[17] One issue here is whether one needs to be *aware* of the validity in order for this constraint to be in force. I'm inclined to think that we don't *begin* to be irrational if we realize that certain beliefs we hold are mutually inconsistent—rather, we *discover* an irrationality that was there all along. So I favour the unrestricted formulation. But one could hedge the principle in the way just indicated, and with slight adjustments the argument below could proceed as stated.

[18] I will in what follows often use 'known' as a shorthand for 'known with subjective and evidential certainty' i.e. as entailing full partial belief and evidential probability 1. Partial results follow from less strong assumptions, and obviously doxastic and epistemic status can be disentangled, but this convention will save routine repetition. For a similar reason I continue to speak in terms of generic 'probability' rather than the various subjective interpretations just mentioned.

[19] This result was stated above. It follows from the probability-logic link given that *everything* follows from an inconsistency. In particular, statements with arbitrarily low probability follow, and since probability can never decrease across a valid argument (by the link) the probability of $\neg Tp \wedge p$ must be minimal, i.e. it must be probability 0.

$$1 - \rho(\neg T \neg p) \geq \rho(\neg p)$$

But in cases where the probability of a failure of bivalence is 1, the probability of each conjunct must be 1.[20] It's easy then to see that the above constraint forces $\rho(p) = \rho(\neg p) = 0$.

So both p and $\neg p$ should be probability zero in the envisaged circumstances. Doxastically, this tells us that it would be pro tanto irrational to be certain that p is a truth value gap, but assign non-zero credence to p (or indeed, to $\neg p$). This is the just the *rejectionist* consequences of Aristotelian indeterminacy for which we were aiming. And we get further results if we run the argument with other interpretations of the probabilities involved. For example, epistemically construed, it tells us that neither p nor $\neg p$ have any evidential support, given that we know that p is a truth value gap.

These results show that in the truth value gap/Aristotelian setting classical rules for probability break down. For example, though p and $\neg p$ should have probability zero, their disjunction, $p \vee \neg p$, remains a tautology and so (by the same principles) should have probability 1. Thus a disjunction has probability 1 while its disjuncts have probability 0. Whether interpreted doxastically or epistemically, some new non-classical treatment of probability is needed. This is the promised revisionary implication of Aristotelianism: something far more dramatic than the rather limited departures from classical inference rules mentioned earlier.

Initial evaluation

If is sympathetic to an Aristotelian treatment of indeterminacy, then one is committed to a distinctive *rejectionist* take on how the information that p is indeterminate should impact on one's credences in p itself—viz, that you should reject p. This is doubly supported—both by the natural characterization of the normative role that truth should play vis a vis partial belief, and also by normative constraints that logicality imposes. The logicality result has wider impact as well—not just subjective probability, but also evidential probability arguably must obey the logicality constraint and thus be reduced to zero in indeterminate cases.

One might again be tempted to think of the bare results just sketched as a major objection

[20] If we wanted to be really careful, we could argue for this by another application of the probability-logic link.

to the Aristotelian treatment of indeterminacy (and likewise to its supervaluational instance). There are various things one might say. Flat-footedly, one might claim that various of the above results were analytically false of probabilities. That isn't very dialectically convincing: one might just drop talk of 'probabilities' in favour of talk directly of degrees of belief/trust and measures of evidential weight (etc). We would need *arguments* to say that these creatures don't function in the way just described: one can't settle matters via a piece of terminological stipulation.

Of course, there are familiar arguments that probability theory is the right way to represent (rational) degrees of belief. Dutch book arguments, generally, aim to show exactly this. I won't try to respond to these arguments right now: others have done this before me.

A complaint to which I have considerable initial sympathy is the following. We need *some* theory of partial beliefs, to integrate with decision theory (and so our account of rational action), and the kinematics of belief (and so our account of how it is rational to respond to new information), and so forth. In a non-Aristotelian classical setting, this important role can be played by classical probability functions, and we can help ourselves to classical decision theory, Bayesian theories of belief updating and the like. But if we go Aristotelian, we deprive ourselves of this well-worked through body of theory, and commit ourselves to redo all that work. Of course, if it can't be redone, then this is a direct objection to Aristotelianism. But the mere fact that we open up anew all these questions is enough to give us pro tanto reason to prefer theories that are not so revisionary.

This complaint can be to a certain extent assuaged. There are, in fact, extant theories of degrees of belief and the like which behave in the ways that Aristotelianism requires. Dempster-Shafer 'belief functions' are a certain kind of generalization of probability functions that allow features such as disjunctions to be assigned a value that exceeds the sum of the values assigned to their disjuncts.[21] Moreover, Dempster-Shafer theories have been argued *independently of anything in the current debate* to be an appropriate model for rational beliefs in the presence of indeterminacy.[22] Of course, that is not to say that there are no *objections* or *problems* with extending this framework to decision theory and belief kinematics. But there is extant work

[21]For Dempster-Shafer theory, see Halpern (1995).

[22]See especially Field (2000), Field (2003b), and Field (2003a). For critical discussion see Weatherson (ms, 2002). Weatherson makes explicit connections between supervaluationism and Shafer functions.

tackling these issues.[23]

The connection between the Aristotelian theses about indeterminate cases and the distinctively non-classical interpretations of subjective and epistemic probabilities gives us a lot of independent traction on those theses. One reaction, the outlines of which we have been briefly sketching, is to argue *in wholly general terms* for or against this non-classical take on the core notions. Standard arguments for classical probabilities give us one direction of this general tactic. Recent work by Hartry Field illustrates that one might argue in the other direction, for revisionism.

I doubt these wholly general arguments will deliver clean results.[24] I am far more optimistic about the prospects of getting arguments *in selected, specific cases* about the appropriateness or otherwise of the non-classical model of partial beliefs.

Various specific cases suggest themselves. Elsewhere, I have explored the consequences of this kind of rejectionism about indeterminacy for our thinking about indeterminate cases of personal identity, and for its impact on our degrees of belief on conditional propositions. However, the specific case that we shall be interested in here is the one for which Aristotle first formulated his thesis: the (putative) indeterminacy of *future contingents*.

[23] See Halpern (1995) for references to the literature.

[24] I am particular dissatisfied with appeals by philosophers to intuitions about their partial beliefs in indeterminate sentences and conjunctions of indeterminate sentences. The kind of operational fixes one normally uses for partial belief (in terms of betting behaviour, for example) are hard to apply to paradigm cases of vague predicates, for example. And I simply do not put much faith in philosophers' appeal to introspection in such cases to carry any dialectical force, suggestive as they might be. I would be more impressed by a properly conducted empirical survey of reported levels of confidence in controverted cases. There is one case (again, very localized) where such work has been done: the empirical studies on the folk judgements of conditionals in Evans and Over (2004). In other work, I argue that if we take this data literally then (1) certain of the conditionals covered by the survey should be regarded as indeterminate in truth value; and (2) that the treatment of this vagueness cannot be supervaluationistic, since the probability-logic link would enforce verdicts on the truth-values of such conditionals incompatible with the judgements we're trying to accommodate.

Still, even if a survey appeared to support classicism or non-classicism over the reported levels of belief or confidence in paradigm indeterminate cases, many lines of resistance remain open. For one thing, it might be that our actual (as opposed to idealized) degrees of belief go haywire in such special cases, and should be *criticized* if they doesn't line up in the right way with best theory. For another thing, the link between phenomenological confidence and the theoretically interesting notion of degree of belief is very much open to question. I might be very nervous, and feel no phenomenological confidence, in a test which I know I'm almost certain to pass. In such circumstances, my betting behaviour—and so degrees of belief, in the sense that makes them relevant to decision theory, for example—might reflect near certainty that I will pass, while an introspective report on confidence will give a different result.

If we focus on evidential probability rather than degree of belief as the central theoretical notion, there's even less of an obvious connection between the kinds of things one might discover by introspection and one's actual probabilities.

3 Opinion about indeterminate future contingents

Beliefs about future contingents seem to be *paradigmatically* ones where we have an array of discernable credences. Faced with an fair coin in midair, my degree of belief that it will come down heads is roughly 0.5; as is my degree of belief that it will come down tails. And of course, that dovetails with betting odds in the expected way. Similarly, examples like tomorrow's potential sea battle give cases where my subjective and evidential probabilities can have quite definite, positive values, depending on the course of my experience to this point.

But the philosopher who takes the Aristotelian line, it appears, should give up all this. Given that the coin's landing is "open", and that one believes(/has evidence) that it is neither true nor false that the coin will land heads, our arguments tell us that the sum of probabilities in 'it will land heads' and 'it will land tails' will be substantially less than 1. In the limit, as already argued, one who is certain (/knows) that bivalence fails in this case will assign no probability at all to either p or its negation. This all seems absurd. Unless we introduce some fancy footwork, Aristotelian indeterminacy about future contingents stands refuted.

Before we go on to consider what fancy footwork the Aristotelian could try out, let me note that this does seem to be a special case. I can't see how to argue so strikingly against the supervaluational version of Aristotelianism on its home turf: paradigmatic instances of vague predicates. As Hartry Field has argued, there's some intuitive appeal to 'suspending judgement' on the proposition that some man is bald, when the man concerned is clearly a borderline case of baldness. I don't know of any terribly convincing arguments that the rejectionist implications of Aristotelianism are unacceptable in *that* setting.

In the case of future contingents however, we have a rich independent take on what our degrees of partial beliefs should be. Support comes from several directions. If, for example, we think that future contingents presently have different *chances* of obtaining, then the following kind of instruction seems compelling: to conform one's credence in p to a (known) *chance* of p.[25] This is incompatible with assigning zero credence to all future contingents.

Another independent fix on credences in future contingents comes from a standard take on instrumentally rational action. The amount of money I am willing to pay for a gamble that

[25] See Lewis (1980).

pays off 1 iff a fair coin coming up heads ten times in a row is far less than I am willing to pay for a gamble that pays the same amount iff the coin lands tails once during this sequence. If my credence in the first prospect is roughly $(1/2)^{10}$ and the second $1-(1/2)^{10}$, then we can rationalize this behaviour: the expected amount of money I receive on the first gamble is standardly taken to be the weighted sum of the money I receive on each element of some partition of the possible outcomes—where the weights are given by my degrees of belief. Thus the expected amount of money of the first gamble will be $1 \times (1/2)^{10} + 0 \times (1-(1/2)^{10}) = (1/2)^{10}$, whereas the expected amount of money I will receive on the second gamble is: $0 \times (1/2)^{10} + 1 \times (1-(1/2)^{10}) = 1-(1/2)^{10}$. But consider applying this recipe if we following the rejectionist implications of Aristotelianism. Our degrees of belief in the coin landing heads every time will be 0, as will our degree of belief that this will not happen. So the 'weights' attached to the partition of outcomes will be zero, and the expected amount of money we will get back from each gamble will be zero. We lose any prospect of explaining in terms of expected value why the first gamble is better than the second.

So there's more than intuitive repugnance in play here. We would need to rebuild all sorts of theoretical connections if we tried to defend the Aristotelian view of the open future. This sort of pressure is missing in a putative 'borderline cases of vague predicates' application. For there's no obvious chance-probability connections to appeal to, nor are there any obvious cases where expected-value rationalizations of action need to be saved.

Opining

An Aristotelian might respond as follows:

> Let's suppose that, given the openness of the future, we should not believe future contingents to any positive degree (in fact, we should reject them utterly). That conclusion, however, is particular to *belief*. But there *does* seem to be something odd about having positive beliefs about a future, once we are aware that there is really no fact of the matter for beliefs to target.
>
> The real challenge in this situation is not to rescue *belief*—a positive doxastic attitude, normed by truth. Rather, we adopt some non-truth normed attitude (call it opining) to future contingents. It will be this opining rather than belief that should be used to rationalize action, which should match known chances, and so forth.

This response has a delicate path to tread. We want our strength of opining in 'the coin will come up heads ten times in a row' to match the known chances of that event. But—granting that we know that that proposition is indeterminate—should we also opine that the proposition is indeterminate? More specifically, can we opine strongly *that the coin will not come up heads ten times in a row, and it is indeterminate whether that coin will come up heads ten times in a row*. If so, then the structure of opining—whatever else we say about it—will not respect the normative constraints of logic. But presumably we do want opining to be related to belief at least to the extent that there'll be something wrong with opining that *the coin will land heads and it's not the case that the coin will land heads*—we want *some sort* of logical constraints on opining, otherwise it'll be ill-suited to play the role of belief.

The cleanest line through this issue, it seems to me, is to take opining to be beholden to the same logical constraints as ordinary beliefs. However, we should deny that it's appropriate to *opine* that future contingents are indeterminate. What we *opine* to be the case will be inconsistent, therefore, with what we *believe* to be the case. This isn't unprecedented, of course: the contents of belief-like states like *imaginings* or *pretendings* can be inconsistent with our beliefs.

We cannot *simply* identify opining with imagining or pretending, however: we don't have a free hand in what we opine. Given a present set of beliefs (in particular, beliefs about present chances) there are rational constraints on how we should opine. Somehow, then, we must tread a line between the too-unconstrained model of pretence, and the over-constrained model of states that import all our ordinary beliefs.

One model for this kind of attitude—constrained by some parts of our knowledge, but inconsistent with the totality of what we believe—is *fictional belief*. We have something like a belief that Sherlock Holmes lived in Baker Street. *Relative to the background fiction* this is not an optional opinion to have—it's what we *should* be doing, given what is true in the fiction. In many areas where folk opinion outruns easily-understood ontology, the model of fictional belief has been appealed to. If there are (really) no numbers, perhaps what we should say is that what folk opinion that two is prime tracks truth of the fictional claim: according to the fiction that there are numbers, two is prime. If there are really no macroscopic objects, perhaps the folk opinion that there are tables and chairs should track the truth of the fictional claim: according to the fiction that microscopic objects compose, there are tables and chairs. What we should *fictionally believe* about the locations of tables and chairs is not something we have a free hand in. Granted we know that there are some atoms arranged table-wise in location *L*, then we must on pain of irrationality fictionally believe that there is a table in location *L*, if the background fiction tells us that things arranged table-wise compose a table. And yet our fictional beliefs in these areas need not conflict with maintaining our (hypothesized) considered beliefs that numbers and microscopic things don't exist.

An appealing option for the Aristotelian, therefore, is to identify opinion about the future with fictional belief. What is the relevant fiction? This might vary depending on the underlying metaphysics. For the growing block theorist, the fiction might be that future states of affairs exist. For the B-theoretic branching eternalist, the fiction might be that only a single future branch exists. The hope would be that opinings, construed as fictional beliefs, would be fit to play the role that future beliefs play for those who deny that the future is open.

I want to finish with three kinds of worry about this proposal. The first concerns the kinds of attitude I should have towards the proposition "the coin landed heads yesterday, and will land heads again tomorrow". The first conjunct is something I believe, but the second conjunct is something I should be opining about. So do I opine about the conjunction, or believe it (or at least: what attitude do I report in ordinary contexts when telling of my level of confidence in the conjunction)?

Here's a second kind of worry. As an open future theorist, I may believe that there is no fact of the matter about whether the coin will land heads tomorrow. But suppose it's presently undecided also when the coin-flipping will take place tomorrow. I might also have the opinion that there the coin flip will happen tomorrow afternoon, and (so) there will be no fact of the matter at lunchtime tomorrow about whether the coin lands heads in the afternoon. But can this opinion be justified by the fictional belief story? I can certainly opine that the coin-flip will happen tomorrow afternoon. But if my opining this involves engaging in the fiction that there is a unique existing future, I shouldn't opine that there'll be no fact of the matter about which way it lands.

These first two worries suggest, I think, that the fictional belief move is unlikely to give us everything we want. But it may yet give us enough to be interesting. Even if it fails to give a neat unified story about our beliefs about the indeterminacy of the future itself, if we restrict ourself to opinions concerning first-order matters of fact, then it seems to do remarkably well. This is enough to justify investigating further.

The final worry I will outline, is, I think, more serious. The following are two possible accounts of the relationship between *truth in fiction* and *fictional belief*.

> THE SIMPLE OPERATOR MODEL OF FICTIONAL BELIEF
>
> For p to be *true* in a fiction F, is for the following to hold: In F, p. Fictional beliefs are simply ordinary beliefs whose content is given by the operator. Thus, fictionally believing that p is a matter of believing, in the ordinary way, that in F, p.

Now, this model might sound crude. Engagement in fictions (and ordinary folk engagement with future contingents) simply does not seem to involve taking an attitude to explicitly fiction-involving propositions. However, this kind of criticism presupposes that claiming that so-and-so has a belief that in F, p, need not be taken to imply that they would report their belief in this explicit form; or that the conditional nature of their belief be transparent to them. This isn't obviously correct: the operator might be *tacit*, justified as the most charitable interpretation of the truth-conditions of the sentences they actually assent to or carry 'in their belief box'.

An alternative view might take the following form:

> THE FICTIONAL-TRUTH NORMED MODEL OF FICTIONAL BELIEF
>
> For p to be *true* in a fiction F conveyed by explicit text T, is for the following to hold: In F, p. Fictional beliefs are states *normed* by fictional truth. Thus, one ought: fictionally believe that p iff it is true that in F, p.

The simple model has the advantage of extending in a natural way to partial beliefs; degrees of fictional belief are just degrees of belief in the fictional content. The normed model arguably has a matching result. For fictional belief in p and simple belief in *in F, p* are normed by one and the same state of the world: they are correct only if *in F, p*. One would think, therefore, that for the fully informed, the degrees of fictional belief should match degree of belief in the corresponding fictional content.

On either model, therefore, to investigate the structure of opining construed as fictional belief, we should look at what partial beliefs are appropriate to contents such as: *in the fiction that* *there is a unique existing future*, the coin lands heads. What the Aristotelian needs, therefore, is a notion of fictional truth that will return the right results. Thus, if a coin is fair, we need a notion of fictional truth that makes one half the correct degree of belief in the proposition *in the fiction that there is a unique existing future, the coin hand heads*. If the coin is known to be biased in various ways, their belief in the corresponding proposition should match the bias.

It is not obvious that such a notion of truth in fiction is available. Consider, for example, Lewis's counterfactual analysis (Lewis, 1978). Lewis suggests that *in F, p* expresses the counterfactual *if F were to be told as known fact, then p*. In the present context, therefore, the content of the earlier proposition would be:

> If 'there is a unique existing future' were told as known fact, then the coin would land heads.

We can assess the plausibility of the suggested Aristotelian strategy by assessing whether one's degree of belief in this counterfactual should match that which is desired: one half, in the case where the coin is fair.

In fact, many would *deny* that we should adopt this attitude to the counterfactual. On Lewis's own view, a counterfactual is true if the consequent is true at all the antecedent-satisfying worlds most similar to actuality.[26] But most would think that among worlds with a unique existing future, worlds where the coin lands heads and those where the coin lands tails will be *equally similar* to actuality. Since the consequent is not true at *all* the closest antecedent-worlds, the counterfactual is false. So the fully informed should be pretty confident that the counterfactual is false—and certainly not adopt a 0.5 degree of belief in the proposition.

There are other possibilities. Robert Stalnaker suggests that the truth of a counterfactual turns on whether the consequent is satisfied at the *closest* antecedent-world.[27] His account presupposes that there are never any ties for 'closest'. In cases of *apparent* ties, such as the one we have here, he suggests that closeness ordering of worlds is indeterminate: either the heads world or the tails world is closest, but there is no fact of the matter which one is which.

Stalnaker's suggestion is, *pace* Lewis, counterfactuals like the one we're considering aren't probably false. However, on the Stalnakerian view they are probably *indeterminate*. Our ques-

[26]See Lewis (1973)

[27]See in particular Stalnaker (1980).

tion about the degrees of fictional belief that *this coin will land heads* turns on the question about the degrees of belief that *if 'there is a single existing future' were told as known fact, then the coin would land heads*. And on a Stalnakerian understanding of that conditional, we end up again asking: what kinds of degree of belief can we (rationally) have in a proposition which we are pretty certain is indeterminate? If one is Aristotelian about indeterminacy in general, then in the particular case of *conditionals*, as much as indeterminate future contingents, we have an answer—the conditional (and its negation) must each be rejected. So construed, the Stalnakerian line ends up in the same place as the Lewisian analysis of conditionals: we end up rejecting the fictional content, rather than allowing for the opining of strength 0.5 which the Aristotelian needed for their fictionalist story to run.

4 Conclusion

I presented arguments that an Aristotelian view of indeterminacy (principally, a view that sees indeterminacy as manifesting in truth-value gaps) puts strong constraints on the level of confidence it is rational to invest in p, where p is known to be indeterminate.

I do not think there are any *general* reasons for thinking this is an unsustainable view of the cognitive role of indeterminacy. But there are *specific* areas in which it will cause concern. The case of the open future is one. Aristotelianism about the open future is a popular option in the literature, following the lead of Thomason (1970) (and in recent times, Macfarlane (2003)). And we have sketched how this position emerges as a natural consequence of well-known eternalist and non-eternalist views.

If Aristotelian view of the open future has the consequences for credences in future contingents I argued for, one might think this was an immediate reductio of the position. I think this would be too quick. There is room for maneuver: I have sketched on possible model, whereby opinion about the future is a matter of *fictional belief* rather than *standard belief*—with the relevant fiction being that there is a single, existing future.

The fictionalism strategy seems to me a promising route for the Aristotelian route. But we have seen that details will be crucial to defending its viability. In particular, the following combination lands us back into trouble:

1. A model of opining as fictional belief
2. A fictional-truth normed model of fictional belief.
3. A Lewisian conditional analysis of fictional truth
4. A Lewisian or Stalnakerian analysis of the truth conditionals of conditionals
5. An Aristotelian model of the indeterminacy of conditionals

I have not argued that these are obligatory moves. One or other of (2-5) could be denied. I shall leave this as a standing challenge to those wishing to develop a detailed version of the opining defence of Aristotelian about future contingents.

My preferred response to the above puzzles, however, is to suggest that Aristotelianism about the open future should be rejected. Rejecting Aristotelianism is not yet to reject the open future itself—for we might think that future contingents are indeterminate, but not think that they are neither true nor false. Working out the details of this view, however, is a matter for another day.[28]

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[28]One proposal of this form is given in Barnes and Cameron (2008). An alternative would be to work within a *degree-theoretic* conception of indeterminacy, where the degree of truth of sentences form an 'expert function' for our credences—we should aim to make our credences in a proposition match its degree of truth. Degree-theories are often associated with non-classical logic; but for present purposes the most interesting versions are versions that preserve classical tautologies—views of the kind sketched by Lewis (1970), Kamp (1975) and Edgington (1997).

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