

Quantified Conditionals and Conditional Excluded Middle*

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Abstract

Higginbotham (1986) observed that *quantified conditionals* have a stronger meaning than might be expected, as attested by the apparent equivalence of examples like *No student will pass if he goofs off* and *Every student will fail if he goofs off*. Higginbotham's observation follows straightforwardly given the validity of *Conditional Excluded Middle* (as observed by von Fintel and Iatridou (2002)), and as such could be taken as evidence thereof (e.g. Williams 2009). However, the empirical status of CEM has been disputed, and it is invalid under many prominent approaches – notably Lewis 1973 for counterfactuals, also Kratzer 1979, 1991. More acutely, Higginbotham's observation holds even for quantified counterparts of conditionals that appear *not* to obey CEM (Higginbotham 2003), and the standard way of explaining (away) such apparent counterexamples to the principle, à la Stalnaker 1981, does not directly yield an account of our apparent truth-conditional intuitions about the quantified counterparts (as reported by e.g. Leslie 2009). This paper provides an explanation for the latter intuitions within Stalnaker's framework, the upshot being that CEM does remain a viable explanation, in principle, for Higginbotham's observation.

1 Introduction

Higginbotham (1986) observed that the following pair of *quantified conditionals* appear to be equivalent (assuming passing and failing to exhaust the

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options):¹

- (1) a. No student will pass if he goofs off
- b. Every student will fail if he goofs off

Quantified conditionals seem to straightforwardly involve binding into a conditional, both its antecedent and consequent. As such it seems that the following should hold:

- (2) a. (1a) is true iff \forall student x , ‘ x will pass if x goofs off’ is false
- b. (1b) is true iff \forall student x , ‘ x will fail if x goofs off’ is true

But (one might think) ‘ x will pass if x goofs off’ and ‘ x will fail if x goofs off’ can both fail to be true of some particular student, say Tom. Intuitively this is the case where both outcomes are possible in the event that Tom goofs off: maybe he gets lucky, maybe not. But then the equivalence of (1a) and (1b) precludes the analysis in (2), for it can yield different truth values for the sentences. For example, supposing ‘ x will pass if x goofs off’ is false of all other students in addition to Tom, (1a) will be true, but (1b) will be false.

Thus, what Higginbotham observed was that the truth conditions of conditionals like ‘ x will pass/fail if x goofs off’ seem to change when they appear embedded in quantified contexts.² Throughout this paper I’ll refer to such conditionals as *instances* of their quantified counterparts ((1a)/(1b)), where ‘ x ’ is a variable implicitly understood as assigned to an element in the domain of the quantifier, or a term denoting such an element.

Higginbotham’s observation could be taken to indicate a failure of compositionality (Higginbotham 1986). In addition, or alternatively, it could be taken to indicate that quantified conditionals have a more complicated logical form than that of quantification into a conditional as (2) would have it (von Stechow 1998, Leslie 2009). However, the reader may have noticed already that some semantics for the conditional do not “allow for” the situation that is problematic for the straightforward analysis in (2). In particular, a semantics

¹Higginbotham (1986) didn’t put things this way, but it seems to be the received understanding of the data; cf. von Stechow and Iatridou (2002), Leslie (2009), Williams (2009), and Higginbotham’s own, later work (2003).

²It is certainly relevant to giving an account of Higginbotham’s observation, how such conditionals behave under other “negative” embedding expressions, e.g. negation itself. To negate a conditional requires sentential negation (‘it is not the case that’), and there are known complications to its interpretation. However, see §2.2 for some other relevant cases and remarks.

for the conditional that validates the *Conditional Excluded Middle* (CEM) precludes precisely the possibility that ‘x will pass if x goofs off’ and ‘x will fail if x goofs off’ are both false:

CEM *if p, q* \vee *if p, $\neg q$* (for arbitrary p, q)
 thus: *if p, q* is true or *if p, $\neg q$* is true

Given that the semantics also disallows the (normally absurd) possibility that *if p, q* and *if p, $\neg q$* are both *true*, the validity of CEM is equivalent to the following: *if p, q* is false iff *if p, $\neg q$* is true.³ But then the equivalence of (1a) and (1b) *does* follow – trivially – under the analysis in (2) (as pointed out by von Fintel and Iatridou (2002) and Higginbotham (2003)). To see this, it suffices to observe that what follows the comma in (2a) can be freely interchanged with what follows the comma in (2b).

So one way to resolve the tension between the equivalence of (1a) and (1b) and the supposition in (2) that they involve straightforward quantification into a conditional, is to defend the view that CEM is valid. In the case above this means denying or explaining away any intuition that ‘x will pass if x goofs off’, and ‘x will fail if x goofs off’ are both false.⁴ Most notable for defending such a position is Stalnaker. In defending a semantics for conditionals that validates CEM, Stalnaker has claimed that putative coun-

³A basic aspect of the meaning of conditionals is that *if p, q* and *if p, $\neg q$* are incompatible, and no semantics that I know of fails to make them so, except in the degenerate case that *p* is a contradiction.

⁴An alternative explanation that would resolve the tension would be to suppose that (a) the relevant conditionals have a *reading* on which they are both false, but (b) there is *another* reading on which CEM holds, and only it can arise in quantified contexts. In fact such an explanation could likely be given independent motivation, at least for indicative conditionals, given the approach of Kratzer (1979, 1991). On her approach *if*-clauses serve to restrict a covert modal, and it is allowed that one interpretation for that modal is an epistemic one. Suppose it could be argued that when judged false instances of (3a) and (3b) are interpreted epistemically. von Fintel and Iatridou (2003) claim that quantifiers in English cannot scope over (overt) epistemic modals, which would rule out an epistemic interpretation for the conditional when embedded in (3a), and (3b), given Kratzer’s approach. Given (b) the equivalence of the latter would follow. von Fintel and Iatridou presage such an explanation, claiming that quantifying into conditionals that feel epistemic seems to be difficult (von Fintel and Iatridou 2002, §3.4). I am not sure of the status of the relevant claims. In any event, I would note that the pattern of judgments Leslie claims for (3a)/(3b) and their instance would seem to be no different for *counterfactual* versions of them. Since positing an epistemic reading for counterfactuals seems implausible, so seems the line just sketched, as a fully general account; see fn. 15.

terexamples to it are best understood as cases of joint *indeterminacy* rather than joint falsity (1981). The primary purpose of this paper is to suggest that Stalnaker’s particular strategy can yield a plausible account of Higginbotham’s equivalences. The challenge to this strategy is an observation due to Leslie (2009), about our (apparent) truth conditional judgments about quantified counterparts of conditionals that seem not to obey CEM.

2 Quantified conditionals and conditional excluded middle

In a recent paper, Leslie cites the kind of problematic situation discussed in the previous section to argue against the view that CEM plus the view in (2) is what explains Higginbotham’s observation (2009). (The latter view she calls the “simple solution”.) In the paper she develops an alternative account, to which I return below and in §3.2. Leslie points to the following kinds of examples, which – crucially – she reports to each be false:

- (3) a. No fair coin will come up heads if (it is) flipped
- b. Every fair coin will come up tails if (it is) flipped
- c. This (fair) coin will come up heads if (it is) flipped
- d. This (fair) coin will come up tails if (it is) flipped

(Assume landing heads and landing tails to be mutually contradictory – no coins land on their edge.)

As I understand it, Leslie’s intuition is that both (3a) and (3b) and all of their instances, e.g. (3c) and (3d), respectively, are false – on the relevant readings – because they contradict the laws of chance. (I infer this based on the analysis that she proposes, to which I turn shortly).⁵ In the case of (3a)/(3b), chance precludes what she takes them to assert, the certainty of

⁵It is important to clarify the basis for the reported judgments since there may be another, ‘generic’ construal of the examples, on which (3c) says something like: ‘in most cases of a flipping, this coin will land heads/tails’. (A proposition that, incidentally, people judge to be false or highly improbable, even when the number of flippings considered is sufficiently small to make such a judgment irrational; see Tune 1964; Tversky and Kahneman 1971; Kahneman and Tversky 1972.) I assume that the intuitions Leslie reports hold even when the sentences are disambiguated to avoid a generic reading – e.g. by making the antecedent ‘if flipped right now’ – and thus assume that the generic construal is separate from the issues at hand.

a uniform tails outcome for all (hypothetical) flippings. In the case of their instances, chance entails that either a tails or a heads outcome should be possible for a given coin, but the assertion is (according to Leslie’s intuition) precisely that only one outcome is possible, in the very same sense of possibility.⁶ The problem is as described in §1. Given the picture in (2) and that all instances of (3a) and (3b) are indeed false, (3a) should be true and (3b) false, in direct conflict with the intuition that they are equivalent.

I want to stress that the heart of the matter is the judgment that *instances* of (3a) and (3b) are genuinely false. The (putative) falsity of both (3a) and (3b) is perfectly compatible with CEM – as would be the falsity of ‘No fair coin will come up *tails* if flipped’ and ‘Every fair coin will come up *heads* if flipped’ in addition.⁷ However, given that these quantified conditionals and their instances are all genuinely false, some line of analysis that does not appeal to CEM would seem to be needed. I turn briefly to Leslie’s own such analysis next, before returning to the examples in (3) in §3.

2.1 *if*-clauses as quantifier restrictors

Leslie (2009) follows von Stechow (1998) in proposing that the equivalence of (for example) (1a) and (1b) owes to the possibility of the *if*-clause functioning to directly restrict the (nominal) quantifier. Her innovation is to take this as an even further generalization of the Lewis/Kratzer approach to conditionals, which treats *if*-clauses as restrictors of (possibly covert) temporal or modal operators rather than as part of (their own) binary connective structure. The innovation is meant to avoid the problem that the basic nominal restrictor analysis wrongly obviates the modal aspect of the interpretation (as pointed out by Higginbotham (2003)). According to the basic analysis, (1a) and (1b) have logical forms essentially equivalent to those of the following:

- (4) a. No student who goofs off will pass
 b. Every student who goofs off will fail

⁶There is a risk that truth is being confused with assertability. On nobody’s account of the conditional is it likely that a normal speaker could have grounds for asserting (3c)/(3d), etc. See this paper’s concluding remarks.

⁷Suppose for example that half of the fair coins satisfy ‘x will come up heads if x is flipped’, and half satisfy ‘x will come up tails if x is flipped’. Then all four quantified sentences are unproblematically false.

The problem with (4a)/(4b) is that they do not seem to be falsified by anyone’s “potential”. They are judged (to have been) true if nobody who does happen to (turn out to) goof off passes. (1a)/(1b), however, can be false in this case if among the students who will not goof off is one clever enough to pass even if he does.

Leslie’s proposed amendment is to layer a modal on top of (4a)/(4b), so that (1a) and (1b) are true, respectively, iff

- (5) a. \forall (relevant) world w , no student who goofs off in w passes in w
 b. \forall (relevant) world w , every student who goofs off in w fails in w

The further assumption is that we have to consider (among the relevant ones) worlds in which the exceptional but hardworking student goofs off, thereby falsifying the sentence as desired. As I understand it the covert modal is taken to be exactly that which is posited by Kratzer’s analysis for (other) conditionals that contain no overt modal or temporal operator for the *if*-clause to restrict. In quantified conditionals, the *if*-clause just happens to restrict the nominal quantifier instead.⁸

Before returning to consider further the problem posed by examples like (3) (for CEM-based accounts), I want to consider a few potential problems for Leslie’s generalization of the nominal restrictor account. This is useful to further clarify some aspects of the data, and to motivate trying to retain CEM and the picture in (2).

2.2 problems with the revised restrictor analysis

Beyond the idea of nominal restricting *if*-clauses, the obvious property of Leslie’s analysis is that, unlike (2), it puts the quantifier *inside* a modal context. Both aspects of the analysis raise problems: I start with cases problematic for the latter, and then return to the former.

A first issue is the following. While the *if*-clause does seem to be interpreted within a modal context in quantified conditionals, the *overt* restriction of the quantifier seems not to be, fairly systematically. So, for example ‘No student would have failed if he had cheated’ expresses a claim about actual students (or so it seems to me), rather than the kind of generalization about

⁸The *if*-clause is presumably be free to restrict the covert modal even in quantified conditionals, except that this puts the pronoun outside the scope of the quantifier, thus precluding binding.

students and cheating that is expressed by a sentence whose overt structure is closer to Leslie’s analysis of quantified conditionals. (Consider: ‘Necessarily, there could be no student who cheats and fails’). To be clear, even if this data point is correct, it is not a problem *formally* for Leslie to derive it (e.g. by actuality operators, in the simple case). The issue would be to *explain* why it is so. No explanation is needed under (2).

Related to this, an issue arises about accounting for the non-monotonic behavior of conditionals – which I assumed them to display equally when embedded in quantified contexts, as when standing on their own. Suppose that there are two boys in the class, John and Bill, to whom the domain of the quantifier in (6a) is restricted, and suppose that (6b) and (6c) are both true.

- (6) a. No/neither boy would have gotten away with it if he had cheated
- b. John would have gotten caught if he had cheated
- c. Bill would have gotten caught if he had cheated

What I have in mind is the following scenario: Bill is honest but loyal to John, and him cheating on his own accord is an impossibility. In situations in which he does cheat, it’s only to help John cheat, in which case he’ll also do *anything* for John, including taking the blame so that John doesn’t get caught. Bill is not similarly loyal to John.

In other words, for each boy, the closest possible world(s) in which that boy cheats are ones in which that boy gets caught; but in Bill’s world(s), John is *not* caught. (2) combined with Stalnaker or Lewis’s analysis of conditionals has no problem making all of (6a)-(6c) true in this kind of situation, and I suppose that they can be. Leslie’s analysis gives:

- (7) In all (relevant) worlds w , no [of the two] students who cheats gets away with it

(Let’s grant that in the worlds that are relevant, the students are just John and Bill, so that the previous issue is kept separate.) What are the relevant worlds? If for example they are (stipulated) to simply be the union of the closest antecedent worlds for (6b) and (6c) according to the Lewis/Stalnaker analysis, (6a) will wrongly come out false, since in “Bill’s” worlds, John does get away with cheating. For the very same reason, taking the intersection won’t work either.

Of course, there are many domains of (relevant) possible worlds that

will make (6a) come out true on the analysis (7). The question is whether there is such a domain, and a general procedure for determining it, that does justice to our intuitions about the way in which the truth conditions of (6a)-(6c) are connected in the given scenario. If one wants conditionals to be non-monotonic, whether non-monotonicity is to be explained semantically à la Lewis/Stalnaker, or by contextual domain restriction à la von Stechow (2001), it seems to be non-trivial to get Leslie's analysis to relate quantified conditionals appropriately to their instances. This task is trivial under (2), since there is a possibility for the worlds considered to vary (one way or another) with the elements in the domain of the quantifier.

In addition to issues related to the scope of the covert modal, there are basic questions that arise about the appeal to nominal restricting *if*-clauses. It has been claimed that the strengthening effect found in examples (1a) and (3a) is more general, arising under other negative operators like 'doubt' (von Stechow and Iatridou, 2002). While this observation would follow trivially under a CEM based account (as von Stechow and Iatridou note), it would require additional assumptions on Leslie's.⁹ Issues also arise about the syntax-semantics interface, as for Leslie's analysis to generalize to all cases, *if*-clauses will apparently have to achieve their restrictive effect over long distances and out of syntactic islands. For example, the following seem to be equivalent

⁹Another issue about the generalizability of the nominal restrictive analysis crops up with examples involving *multiple* quantifiers binding into an *if*-clauses. In some cases, it seems that to derive the attested truth conditions, the *if*-clause will have to be restricting a quantifier that is higher than one which binds a variable within it (which is impossible). Consider:

- (i) a. No cheerleader's boyfriend will stick around if he knocks her up
b. = Every cheerleader's boyfriend will skip town (=not stick around) if he knocks her up
- (ii) No boy will marry the first girl he kisses if she makes fun of him afterward

The question is what the *if*-clause can be restricting in these examples. It cannot be the quantifier itself, on the plausible assumption that the (possessive) description is lower and itself binds a variable in the *if*-clause. But if it restricts the description, the reader can confirm that the wrong uniqueness implications will be derived. In addition, *if*-clauses seem unable to restrict descriptions in the general case: *The first coin will land heads if flipped* ≠ *The first coin flipped will land heads*. There is a potential way out, however, namely treating the pronouns which appear to be bound by the descriptions as being concealed descriptions themselves.

just as (3a) and (3b):¹⁰

- (8) a. No coin is such that it will land heads if flipped
 b. Every coin is such that it will land tails if flipped

Thus there appear to be very basic issues about the generality and implementability of the kind of account Leslie pursues, in addition to ones with her particular implementation, which do not arise for the simple picture in (2) + CEM. As such it is worth reconsidering the problem posed by (3) for the latter.

3 Indeterminacy

The problem that Leslie pointed out for CEM-based accounts of Higginbotham’s observation was the following. The equivalence of certain quantified conditionals whose instances seem not to obey CEM is at odds with the truth conditions that (2) + CEM predict. So for example (3a)/(3b) are equivalent, but (putatively) false along with all of their instances, and this situation is impossible given (2) and CEM. However – as Leslie acknowledges – some speakers are inclined to judge instances of the relevant quantified conditionals to be *indeterminate* or *lacking a truth value*, rather than false:

One might deny that (7) [‘x will land heads if flipped’, for fair coin x] and (8) [= ‘x will land tails if flipped’, for fair coin x] really are false, and claim instead, for example, that they are simply indeterminate, or lack a truth value. Certainly the defender of CEM as a general principle should argue for some such claim. I will not discuss such a possible defense here, but rather the discussion will proceed on the highly intuitive assumption that this is a genuine counterexample to CEM. It is worth noting, though, that it is far easier to convince oneself that (7) and (8) are indeterminate, than it is to convince oneself that their quantified counterparts (9) and (10) are:

- (9) No fair coin will come up heads if flipped.
(10) Every fair coin will come up heads if flipped.

¹⁰To be fair, this is an issue for the Lewis/Kratzer approach to *if*-clauses more generally.

(9) and (10) strike most people as quite clearly false. Thus even if one is inclined to reject (7) and (8) as counterexamples to CEM on the grounds that they are indeterminate rather than false, one still needs an explanation of why (9) and (10) seem quite clearly false and not at all indeterminate. Any natural extension of the Simple Solution to cases of indeterminacy would predict that the quantified statements should be indeterminate if their embedded conditionals are indeterminate. (Leslie, 2009)

Stalnaker (1981) in particular has argued that putative counterexamples to CEM are rather best understood as cases of indeterminacy. To accommodate this indeterminacy in a compositional setting, Stalnaker recast his earlier, bivalent theory of conditionals – which famously validates CEM – in the framework of *supervaluations*. Under this recasting CEM remains valid (see below), and Higginbotham’s equivalences follow as a consequence.

However, as Leslie correctly alludes in the above, it does not follow from the supervaluational semantics itself that (3a)/(3b) should be judged false if there instances are indeterminate.¹¹ In the remainder of this paper it is argued that there is nonetheless an explanation for this pattern of judgments available in Stalnaker’s framework. A consideration of slightly more refined examples than those considered by Leslie supports this explanation. The upshot is that a CEM-based account of Higginbotham’s effect remains tenable, without further stipulation, at least for speakers with the relevant intuitions. The paper concludes with a comparison to Leslie’s own theory with respect to its handling of the refined data, and with respect to the (apparent) speaker variation in basic intuitions about (3a)/(3b) and their instances.

3.1 Stalnaker 1981

According to Stalnaker (1968)’s influential proposal a conditional *if* p , q is true in a world w iff q is true in the (unique) p -world that is most similar to w . (For the moment I ignore the indicative/subjunctive distinction). On Stalnaker’s theory the validity of *Conditional Excluded Middle* is a consequence of the principle of excluded middle itself: given that there is a unique

¹¹The same applies to other familiar accounts of how semantic indeterminacy or truth-valuelessness of simple sentences is inherited by complex (e.g. quantified) ones – for example, trivalent theories based on strong Kleene logic, or ‘satisfaction’ theories of pre-supposition projection.

closest p -world, either that world is a q -world or not. In later work Stalnaker responded to apparent counterexamples to CEM, such as the following one due to Quine, by claiming them to be best understood as indeterminate rather than as false (1981):

- (9) a. If Bizet and Verdi had been compatriots, Bizet would have been Italian (too)
 b. If Bizet and Verdi had been compatriots, Verdi would have been French (too)

The (putative) indeterminacy of conditionals like (9) amounts on Stalnaker’s view to something like referential failure: there failing to be a unique world which is the closest or most similar antecedent-world. As Stalnaker – and even Lewis – pointed out, there is indeterminacy in the notion of similarity itself. Attending to different features or respects gives potentially different similarity orderings of worlds. As I understand it, Stalnaker (1981) takes the basic semantics to be determinate – to yield either true or else false for a given conditional – up to the point that the underlying notion of similarity employed by speakers is itself determinate. Stalnaker pointed out that, once a plausible view is adopted of how semantic indeterminacy (in general) is inherited by complex sentences – the method of supervaluations – CEM remains valid (as do all “classical” validities).

The key idea of supervaluationism (van Fraassen, 1966; Fine, 1975) is that the truth conditions of sentences take into account various possible ways in which the relevant indeterminacy (or vagueness) could be resolved. These ways are a class of completed, bivalent interpretations – or *precisifications* – of the initial, partial interpretation of the language.¹²

A basic implementation of the supervaluational version of Stalnaker’s semantics goes as follows. Let i_0 be the ‘initial’ semantic interpretation, under which (perhaps) many conditionals are indeterminate/undefined. Definedness and truth of conditionals with respect to an interpretation i – possibly a precisification i_n of i_0 – is as follows:

- (10) a. $p > q$ is defined/determinate in w , w.r.t. i iff there is a unique p -world that is maximally similar to w w.r.t. i [= iff $F(P)(w)(i_n)$]

¹²Or *supervaluations*. On some formulations, truth and falsity are defined over a broader space including not only complete precisifications, but partial or intermediate completions of the base interpretation (e.g. Fine (1975)). The latter can be ignored for present purposes.

- is singleton]¹³
- b. $p > q$ is true_{*i*} in w if q is true_{*i*} in $F(P)(w)(i)$
 - c. $p > q$ is false_{*i*} in w if q is false_{*i*} in $F(P)(w)(i)$

If we make the simplification of ignoring other types of indeterminacy, a precisification i_n (of an interpretation i_0) corresponds to a way of eliminating any indeterminacy in the underlying similarity relation used to interpret conditionals, such that there will be a single world that counts as the closest p -world for any antecedent p . That is, such that $F(P)(w)(i_n)$ is singleton for all propositions P and worlds w . A further condition typically imposed on precisifications – implicit in the name itself – is that they should respect the initial interpretation i_0 in all cases that it already gives true or false:

- (11) *stability*: for all sentences p , for all precisifications i_n of i_0 :
 $(p \text{ is true}_{i_0} \rightarrow p \text{ is true}_{i_n}) \wedge (p \text{ is false}_{i_0} \rightarrow p \text{ is false}_{i_n})$

For a sentence to be true or false *simpliciter* is for it to be true (false) relative to every precisification, rather simply with respect to i_0 .

- (12) a. p is true (simpliciter/“supertrue”) in w if p is true_{*i_n*} in w for every precisification i_n
 b. p is false (simpliciter/“superfalse”) in w if p is false_{*i_n*} in w for every precisification i_n

The (intended) upshot of equating truth/falsity with truth/falsity under every precisification is that certain complex sentences containing an indeterminate conditional will nonetheless still themselves be determinate. This happens precisely when resolving all indeterminacies, in any (allowable) way, settles that a sentence has a particular truth value. The disjunction of (9a) with (9b), for example, will be true, even though neither disjunct is determinate(ly true, or false). This disjunction is true precisely because (by assumption) CEM is valid under every precisification, and thus under every precisification one of its disjuncts will be true. Of course, the very same considerations show that CEM is valid (simpliciter).

That CEM is valid even though many pairs of conditionals *if* p , q , *if* p , $\neg q$ are indeterminate, Stalnaker took to be in accord with speakers’ intuitions.

¹³So for any i_n F behaves as a selection function meeting Stalnaker’s conditions; in i_0 it can fail the condition of uniqueness, possibly returning a non-singleton set of worlds. I sloppily speak of singleton sets of worlds and the worlds they contain interchangeably.

It seems to me plausible in the case of Leslie’s coin flip examples. Consider the following, which seems to me nearly trivial in spite of my uncertainty about the truth of the disjuncts:

- (13) Either this (fair) coin will land heads if you flip it, or it will land tails if you do.¹⁴

Before continuing, it will be useful to consider the conceptual role being played by precisifications in slightly more detail. Cases like (9) are presumably indeterminate because there are just no salient or relevant considerations that make Bizet more Italian than Verdi is French – to put it coarsely – or vice versa. But in principle there are enough respects of similarity which, if put into focus, decide the matter one way or the other. (Maybe if one considers musical style closely enough, Bizet is more imaginably Italian – but in terms of some quirks of appearance, Verdi is more imaginably French.) Precisifications fall into classes corresponding to those respects.

Instances of (3a)/(3b) (‘x will land heads/tails if flipped’) would seem to be slightly different. Their indeterminacy presumably runs deeper, owing to the fact that coin flips are random events. There are not likely to be principled considerations that could make any world in which a flip does take place most like ours in virtue of the outcome of the flip in that world or anything else.¹⁵ Accordingly, it would usually be expected that the space of precisifications will mirror the space of possible outcomes, in the sense that for every fair coin x and for every possible distribution of truth values to every sentence of the form ‘x will land heads/tails if flipped’, there will be at least one precisification. However, it is in principle up for grabs what range of precisifications speakers actually consider when judging particular sentences. We return to this point directly in reconsidering the quantified examples (3a)/(3b) themselves.¹⁶

¹⁴To fairly test intuitions, it’s important to not misconstrue the sentence as ‘This coin will either land heads or tails if you flip it’, which is bound to be nearly trivially true on anyone’s semantics for conditionals, at least in normal worlds.

¹⁵Things are of course different in the case that the antecedent is true, and the relevant coin was/will be flipped. Then the outcome of the flip is *trivially* relevant to determining the closest antecedent world, since per Stalnaker’s definition of the selection function, it will be the actual/evaluation world itself. See fn. 16 for relevant remarks.

¹⁶In comparison to counterfactual and future oriented variants, *past tense* coin-flip conditionals, for which it is already determined whether a flip has occurred and what its outcome was, feel less indeterminate.

3.2 supervaluations and quantified conditionals

Higginbotham’s observation follows straightforwardly under the supervaluational implementation of Stalnaker’s semantics, as a trivial consequence of the fact that truth/falsity (simpliciter) corresponds to truth/falsity with respect to a class of CEM respecting bivalent interpretations (precisifications).

- $\neg(Ax > Bx)$ is true_{i_n} iff $(Ax > \neg Bx)$ is true_{i_n} , since all precisifications i_n respect CEM
- for all quantifiers Q , $Qx\phi$ is true/false iff it is $\text{true}_{i_n}/\text{false}_{i_n}$ for all i_n (and otherwise indeterminate)¹⁷
- thus $Qx\neg(Ax > Bx)$ is $\text{true}_{i_n}/\text{false}_{i_n}$ iff $Qx(Ax > \neg Bx)$ is $\text{true}_{i_n}/\text{false}_{i_n}$,
- and $Qx\neg(Ax > Bx)$ is true/false (simpliciter) iff $Qx(Ax > \neg Bx)$ is

So, for example, $\forall x(Ax > \neg Bx)$ – e.g. (3a), (1a) – is true/false iff $\forall x\neg(Ax > Bx)$ is, and in turn iff $\neg\exists xQx(Ax > Bx)$ – e.g. (3b), (1b) – is.

Since $Qx\neg(Ax > Bx)$ and $Qx(Ax > \neg Bx)$ are true and false under the same conditions, they are indeterminate – neither true nor false – under the same conditions. Obviously, what the latter are depends on what precisifications there are. In the case of (3a)/(3b) (‘No fair coin will land heads if

(i) That (fair) coin landed heads if it was flipped

It seems clear that the sentence can be falsified by showing that the coin was flipped but landed tails (at least on a salient reading). This follows on Stalnaker’s analysis, since by stipulation, if the antecedent holds in the world of evaluation, the closest antecedent world is the world of evaluation itself. As such, *if* p , q is true (in that world) iff p and q are.

One might take the data for past tense conditionals to suggest that the putative indeterminacy of future oriented examples like ‘this (fair) coin will land heads if flipped’, owes simply to the relevant facts about the future being currently open or unknowable (rather than to indeterminacy in the similarity relation, as assumed here). As far as I can tell, the skeleton of the account sketched in this subsection (§3.1) and in the following one (§3.2) could be retained even if this is the case. Rather than exploring the latter point, I would simply point out (again) that all of the relevant data are the same for counterfactuals. ‘This (fair) coin would have landed heads/tails if flipped’ is (for me) indeterminate, and ‘No fair coin would have landed heads if flipped’ is equivalent to ‘Every fair coin would have landed tails if flipped’, with both appearing to be false. (Though see below for a refinement of the latter.) Thus, the skeptical reader can simply replace the examples in §3.1 and §3.2 with counterfactuals, and evaluate the account as one applying to them (only).

¹⁷I.e. quantifiers do not themselves “manipulate” precisifications.

flipped’, ‘Every fair coin will land tails if flipped), the only kind of precisification that makes either sentence true, is one that assigns truth to each of its instances. That is, a precisification on which each fair coin lands tails in the closest world in which it is flipped. All other precisifications yield false: on “most” of them some coins land tails in the closest world in which flipped, but some land heads; on others all land heads. Thus, taking the former kind of precisifications into account, (3a)/(3b) are indeterminate. But if ignored, both are false since then false on all (considered) precisifications.

I would like to propose that it is precisely ignoring the former kind of precisifications that accounts for the tendency of speakers to judge (3a)/(3b) false, in spite of judging their instances to be indeterminate. This explanation rather directly predicts two kinds of attested variability in such speakers’ behavior. First, if one explicitly considers or mentions the possibility – however remote – of a freakish string of coin flips with identical outcome, a (3a)/(3b) no longer seem false, but rather indeterminate as well. (‘Surely it is *possible* that every flipped coin should land the same way.’)

Second, it seems to matter *how many* coins are in the domain of quantification, as made explicit or determined implicitly. If we restrict to a small number of coins, (3a)/(3b) again seem to be as indeterminate as their instances. Suppose there are three coins in a row on a table (presupposed to be fair):

- (14) a. None/each of these three coins will land heads if flipped
- b. No/every coin on the table will land heads if flipped
- c. No/every coin will land heads if flipped [domain understood to be restricted to the coins on the table]
- (15) The coin on the left/in the middle/on the right will land heads if flipped

If I had to, I’d still bet against the sentences in (14) before I would bet against any of their instances ((15)) (or for them for that matter). But I don’t have a sense that the former are more determinate(ly false) than the latter.

Taking stock, the disparity in judgment that Leslie claims between (3a)/(3b) and their instances for speakers who find the latter indeterminate, seems to hinge on the implicit knowledge that there are massively many fair coins, and the absence of a contextual domain restriction, “out of the blue”, to limit the number actually considered. Even the intuition of falsity for the case that all

coins are considered seems to be naïve: it depends on ignoring the unlikely but possible.

Within Stalnaker’s framework, both of these variabilities can be explained as variability in the precisifications that speakers access when interpreting and judging sentences. I find it a priori plausible that consideration of the relevant type of “uniform” precisifications should be forced in the two situations where I have claimed that judgments of indeterminacy resurface: first, when the kind of outcome they represent is explicitly mentioned, and second, when the domain of quantification is small enough to make the probability of uniform outcomes sufficiently high that they cannot be ignored.¹⁸

In addition, it is a well established fact that people systematically overestimate how much “randomness” is guaranteed by processes governed by chance (Tune 1964; Tversky and Kahneman 1971; Kahneman and Tversky 1972). A relevant example is that, when asked to generate outcomes of a series of hypothetical coin flips, the answers that subjects give are systematically far more “random” than alternative outcomes that have equal probability (given the number of flips and that each flip has equal probability of landing heads, or tails). For example, ‘runs’ of heads and tails, are avoided, so that the distribution of heads and tails tends to stay close to 50-50 even over small parts of the series.

A possible hypothesis is that the inclination to ignore “improbable” precisifications and judge (3a)/(3b) to be false, is a manifestation of these experimentally established tendencies. This is consistent with the observation that judgments of indeterminacy resurface when we explicitly attend to improbable, uniform outcomes or consider (very) small domains. If certain correlations could be found between subjects’ behavior on tasks like those described in the previous paragraph and their semantic intuitions about the relevant sentences, this could be taken as independent support for the account.

I would like to introduce one further piece of relevant data before reconsidering Leslie’s account in light of the ‘contextual’ variation in judgments of indeterminacy, and (apparent) speaker variation, introduced in this section.

¹⁸Why then not just explain away the false judgement for large domains as directly reflecting a probability judgement? In a sense this is what I am proposing, but in such a way that the probability judgment is ‘semanticized’ (via “contextual” restriction of the precisifications considered). In support of this, it’s not clear that there is a qualitatively identical tendency to judge other case cases of the vanishingly improbable (‘This coins was flipped 1 million times and never landed heads’) to be false.

Consider the following, the contradictory of (3a)/(3b) as expressed with an existential quantifier:

(16) {Some/at least one fair coin} will land heads if flipped

A pattern of judgments that mirrors those for (3a)/(3b) seems to obtain. With no implicit domain restriction – understood as a claim about all of the fair coins there are – there is a pull to call (16) *true* equal to the one in the opposite direction for (3a)/(3b). But if a small number of coins is considered, by restricting the domain implicitly or explicitly as in (14), the judgment of indeterminacy returns. This follows straightforwardly on the supervaluational approach, given that the same ‘unlikely’ precisifications are ignored as in the case of false judgements for (3a)/(3b). (In the supervaluational framework, $\exists x\phi$ is true/false as long as for every precisification i_n , there is some/no x that makes ϕ true/ i_n . Thus $\exists x\phi$ can be true/false even if ϕ is indeterminate, for all x . In particular, (16) will be true even though there’s no particular coin that is certain to land heads if flipped (again, so long as the relevant precisifications are ignored).)

How does the modified nominal restrictor approach of Leslie fair with respect to (14) and (16)? I’ll focus primarily on the question of whether her approach could give an account of speakers like me. In doing so I will grant for the sake of argument some ancillary explanation for why judgments of indeterminate’ should arise in the first place. (Say, for instances of (14) and (16), i.e. for ‘ x will land heads if flipped’). In addition I will assume that speakers like Leslie do judge (14) and (16) to be false, as her theory predicts. But if not, the following considerations also apply to her approach *as an account of speakers like her*.¹⁹

Unless the tendency to judge (16) to be true (for large domains) is to be explained away, Leslie’s approach will have to make use of essentially the same idea appealed to above, that unlikely outcomes are ignored in context. Her analysis makes it exceedingly easy for any coin-flip quantified to be false. As long as there is a “relevant” world in which it is not the case that Q -many coins that are flipped land heads, a quantified conditional Q *coins will land heads if flipped* will be false. So for (16) to be (literally) true on Leslie’s account, the domain of relevant worlds would have to be restricted to exclude every one in which all flipped coins land tails.

¹⁹... who judge instances of (14) and (16) (etc.) to be *false*.

In addition, and perhaps more severely, the fact that quantified conditionals like (3a)/(3b) are easily falsified on Leslie’s approach makes it difficult to account for the sensitivity of judgments to the size of the domain. Consider (14a). “Out of the blue”, there seems to be no reason why there shouldn’t be a relevant, salient possible world in which one of the three coins is flipped and lands heads (or tails, in the case of the variant with ‘each’) – and I say this as a speaker who judges (14a) to be indeterminate. But on Leslie’s approach this fact should be enough to render a (clear) judgment of falsity.

If correct, these considerations suggest that Leslie’s approach fares no better – and probably worse – at explaining the behavior of speakers with ‘indeterminate’ judgments for instances of (3a)/(3b)/(16). And this is already granting that indeterminate judgments *can* somehow be explained (away) on her approach. But can the supervenient account explain the behavior of speakers like Leslie?

I will not defend a ‘yes’ answer here, but will rather simply point to the following considerations about supervenientism. There are numerous reasons for taking falsity to equate to superfalsity (falsity under every precisification), rather than to mere non-supertruth; for example, maintaining the (apparent) duality of p and $\neg p$. However, it is not implausible that some ‘supervenientist’ speakers should sometimes be pulled to make the other equation, and to reject as “false” assertions that are indeterminate.

This pull might be expected to be especially strong in the case of conditionals like ‘this fair coin will land heads/tails if flipped’, because there is (I suppose) not normally a non-arbitrary way of *forcing* them to be determinate. That is, of attending to enough plausible features of similarity to yield a unique coin flip-world as most similar. For any world that might *seem* to be closest, there will normally be one just like it but in which the coin lands the other way. As such, there is good reason to reject ‘this fair coin will land heads/tails if flipped’: not only does it fail to be (super)true, it stands no principled way of being so, and thus no reasonable speaker could make the

mistake of thinking it expresses a truth.^{20,21}

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²⁰There is a parallel in the case of presupposition. As pointed out by Strawson, expressions that invoke presuppositions sometimes lead to apparent judgments of falsity, rather than oddness or failure, when those presuppositions are not met (Strawson, 1950, 1954, 1964). von Stechow (2004) has argued that this fact is not fatal to a treatment of presuppositions using truth value gaps. In fact von Stechow and Iatridou (2002) propose to treat the (apparent) validity of CEM itself, as arising from a (linguistic/conventional) presupposition associated with conditionals. Rather than supervaluations, they assume a satisfaction type account of presupposition projection to explain how “indeterminacy” induced by failure of this presupposition is inherited by complex sentences. Thus, there is a question – which I leave for the reader – of whether Leslie’s objection to CEM based accounts, and her pattern of judgments, could be addressed under a presuppositional approach à la von Stechow, and how such an approach would compare to the supervaluational one presented here.

²¹If desired the situation could be (at least in part) modeled in the obvious way using a (covert) ‘definitely’ operator, where ‘definitely p’ is true at any precisification just in case it is true at every precisification. A false judgment would arise, by hypothesis, due to judging the conditional as in the scope of such an operator.

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