Talking about worlds*

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Abstract

I explore the logic of the conditional, using credence judgments to argue against *Duality* and in favor of *Conditional Excluded Middle*. I then explore how to give a theory of the conditional which validates the latter and not the former, developing a variant on Kratzer (1981)'s restrictor theory, as well as a proposal which combines Stalnaker (1968)'s theory of the conditional with the theory of epistemic modals I develop in Mandelkern 2019a. I argue that the latter approach fits naturally with a conception of conditionals as referential devices which allow us to talk about particular worlds.

Keywords: logic of conditionals; *Duality*; *Conditional Excluded Middle*; probabilities of conditionals

1 Introduction

This paper begins with 'a detail in the semantics of the conditional' (Stalnaker 1980): the status of *Conditional Excluded Middle* (*CEM*), which says that 'If p, q or if p, not q' is always true. *CEM* is in tension with another principle, *Duality*, which says that 'If p, q' and 'If p, might not q' are contradictories. I argue that when we focus on credence judgments, rather than assertability or disagreement judgments, it becomes clear that *Duality* is false; these judgments also provide further support for *CEM*. The main theory of conditionals which validates the latter and not the former is that of Stalnaker 1968. I argue that Stalnaker's theory, together with his proposal about how to interpret 'might'-conditionals from Stalnaker 1980, cannot

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make sense of conditionals with complex modal consequents. Such conditionals provide natural motivation for a restrictor approach along the lines of Lewis 1975, Kratzer 1981. Standard versions of that approach validate *Duality*, not *Conditional Excluded Middle*; I explore an alternative version which validates the latter, not the former. Then I discuss some drawbacks of that approach, and revisit the Stalnakerian theory, arguing that it can make sense of 'might'-conditionals after all, provided we take a suitably sophisticated approach to the meaning of 'might'. I argue, finally, that this latter approach fits naturally with a conception of conditionals as referential devices which allow us to talk about particular (possibly distal) worlds; and, more generally, with a unified approach to reference to individuals, times, and worlds, along the lines advocated by Schlenker (2004, 2006).

2 *CEM* and *Duality*

I begin by reviewing the controversy over *CEM* and *Duality*. I use 'If p, q' to range over conditionals, both indicative and subjunctive; what I say is meant to apply to both.[1] *CEM*, again, says that 'If p, q or if p, not q' is always true—thus, for instance, that 'If you flip the coin, it will land heads, or if you flip the coin, it will land tails' is true no matter what. *Duality* says that 'If p, not q' and 'If p, might q' are contradictories, i.e., that exactly one of them is always true; thus e.g. that if 'The coin will land heads if flipped' is true, then 'The coin might land tails if it is flipped' is false, and *vice versa*.

CEM and *Duality* are jointly consistent, but they cannot plausibly both be true. Suppose they were, and suppose that 'If p, might q' is true. Then by *Duality*, it would follow that 'If p, not q' is false; by *CEM*, it would follow that 'If p, q' is true (making classical assumptions about the Boolean connectives). So then 'If p, might q' would entail 'If p, q'. But this cannot be right. 'If Matt flipped the coin, it might have landed heads' does not entail 'If Matt flipped the coin, it landed heads'.

This creates a puzzle, because both *CEM* and *Duality* are *prima facie* attractive. Consider first the case for *CEM*. First, negated disjunctions 'Not (if p, q, or if p, not q)' (equivalently, 'Not (if p, q) and not (if p, not q)') strike us as odd. Even Lewis

1 I use quotation marks for both quotes and corner quotes.

(1973), who argues against *CEM*, concedes that a sentence like (1) sounds like a contradiction:

(1) It is not the case that if Bizet and Verdi were compatriots, Bizet would be Italian; and it is not the case that if Bizet and Verdi were compatriots, Bizet would not be Italian.

As Lewis notes, the oddness of (1) can be brought out by following it up with 'Nevertheless, if Bizet and Verdi were compatriots, Bizet either would or would not be Italian', which is surely true.

(1) is, however, a somewhat artificial construction, since wide-scope negation over conditionals is not particularly natural. A second, and perhaps more compelling, argument for *CEM* comes from Higginbotham (1986, 2003), who observes that conditionals under quantifiers behave in a way that conforms to *CEM*. For instance, note that (2a) and (2b) strike us as equivalent:[2]

(2) a. No one passed if they goofed off.
    b. Everyone failed if they goofed off.

Assume that passing and failing are contradictories, and that sentences like (2a) and (2b) have the logical forms they seem to have: that is, they comprise quantifiers taking scope over open conditionals, as in (3):[3]

(3) a. No x (x passed if x goofed off).
    b. Every x (x failed if x goofed off).

How can we predict that (2a) and (2b) are equivalent, as they seem to be? That (2b) entails (2a) is predicted on any reasonable theory. What about the other direction? The validity of the inference from (2a) to (2b) follows immediately if *CEM* is valid. By the standard semantics for 'No', 'No x (x passed if x goofed off)' is true just in case 'Every x (Not: x passed if x goofed off)' is true. If *CEM* is valid, 'Not: x passed

2 We can also look at conditional under negative attitude verbs like 'doubt', as Cariani & Santorio (2018) suggest (in a different context) and Cariani & Goldstein (2018) suggest in arguing for *CEM*.

3 See von Fintel & Iatridou 2002 for motivation for this assumption. One way to circumvent any complexities concerning the structure of such sentences is to look at exchanges like: 'Of which students is it true that they [passed/failed] if they goofed off?' The answers 'None' and 'All' to the two questions, respectively, feel equivalent, an observation with the same upshot as Higginbotham's.

if x goofed off' entails 'x failed if x goofed off'. And so 'No x (x passed if x goofed off)' entails 'Every x (x failed if x goofed off)', as desired.

By contrast, if *Duality*, instead of *CEM*, is true, then 'Not: x passed if x goofed off' is equivalent to 'x might not have passed if x goofed off'. But then (2a) is predicted to be equivalent, not to (2b), but rather to (4):

(4) Everyone might have failed if they goofed off.

But this is wrong: (2a) and (4) are not equivalent. To make this concrete, imagine a classroom where Teacher promised the students that, if a student goofed off, Teacher would flip a coin: if it landed heads, the student would pass; if tails, the student will fail. Now suppose we don't know what actually happened. In this scenario, it seems like we know (4) to be true, but we don't know (2a) to be true; so these are inequivalent, contrary to the predictions of *Duality*, but in line with the predictions of *CEM*.

These points are robust across different kinds of conditionals, including both indicative and subjunctive conditionals; thus, for instance, (5a) strikes us as equivalent to (5b), not (5c); likewise for (6):

(5) a. No student will pass if they goof off.
b. $=$ Every student will fail if they goof off.
c. $\neq$ Every student might fail if they goof off.

(6) a. No one would have passed if they had goofed off.
b. $=$ Everyone would have failed if they had goofed off.
c. $\neq$ Everyone might have failed if they had goofed off.

In general, then, 'No x (if p(x), q(x))' looks equivalent to 'Every x (if p(x), not q(x))', not to 'Every x (if p(x), might not q(x))'. This is predicted by *CEM*, but is inconsistent with *Duality*. This provides powerful motivation for *CEM*.[4]

But, while this case for *CEM* is compelling, there is also a case to be made for *Duality*. One motivation for *Duality* comes from facts about disagreement and co-

4 Leslie (2009) argues against this conclusion; see Klinedinst (2011) for a response. Again, question/answer pairs like those discussed in Footnote 3 let us make this same point while circumventing some of the relevant structural complexity. See Huitink (2009), Kratzer (In Press) for further discussion of quantified conditionals.

assertability: speakers who assert sentences of the form ‘If p, q’ and ‘If p, might not q’ are felt to be in disagreement with each other, and it is generally infelicitous for a single speaker to assert both of these. Thus Bob and Sue are felt to be disagreeing in (7) and (8):

(7) a. [Bob:] If the coin was flipped, it landed heads.
b. [Sue:] No! If the coin was flipped, it might have landed tails.

(8) a. [Bob:] If the coin had been flipped, it would have landed heads.
b. [Sue:] No! If the coin had been flipped, it might have landed tails.

Likewise, an assertion of a sentence like (9) or (10) is felt to be quite odd:

(9) #If the coin was flipped, it landed heads; and if the coin was flipped, it might have landed tails.

(10) #If the coin had been flipped, it would have landed heads; and if the coin had been flipped, it might have landed tails.

These facts about disagreement and co-assertability are, again, robust across different kinds of conditionals; and they are, of course, immediately explained if ‘If p, not q’ and ‘If p, might q’ are contradictories, as *Duality* holds.

The second argument for *Duality* is more abstract. The argument is that *Duality* gives us a nice characterization of the meaning of ‘If p, might q’: according to *Duality*, this just is the negation of ‘If p, not q’. Lewis (1973) argues that there is no other natural way to account for intuitions about the meaning of conditionals with this form. This idea gained support from Kratzer (1981, 1986)’s influential restrictor semantics for the conditional. The key idea, which we will explore in more detail shortly, is that ‘if’-clauses serve to restrict a modal in the consequent of conditionals. When no overt modal is present, Kratzer assumes there is a covert ‘must’. Given the consensus that ‘must’ and ‘might’ are themselves duals, *Duality* falls out immediately from the restrictor approach.

In addition to arguments for *CEM* and *Duality*, there are a number of important arguments against them in the literature. I will not survey the extant arguments against *Duality* here; I am largely in sympathy with them, and will be adding my own in the next section. Since I will be defending *CEM*, though, it is important to acknowledge the main argument against *CEM*, which is based on the observation that it often feels as though *neither* ‘If p, q’ *nor* ‘If p, not q’ is true. This is the case, for instance, with Quine’s famous pair:

(11) If Bizet and Verdi were compatriots, Bizet would be Italian.

(12) If Bizet and Verdi were compatriots, Bizet would not be Italian.

Neither of these is obviously true. *CEM* predicts that their disjunction *is* clearly true, from which it is a short step to the counterintuitive claim that at least one of (11) or (12) is true. But there are ways to either block this step, or else become more comfortable with it. Stalnaker (1980) showed that we can block this step given a supervaluationist account of the indeterminacy of conditionals. On such an account, p is true *simpliciter* just in case p is true according to every admissible valuation; false *simpliciter* just in case false at every admissible valuation; and otherwise indeterminate. Then it could be that both (11) and (12) are indeterminate, and thus fail to be true (or false) *simpliciter*; but their disjunction nonetheless will always true *simpliciter*. Hawthorne 2005 argues that we can make the counterintuitive claim that one of (11) and (12) is true more palatable, by building on an epistemicist approach to vagueness (Williamson 1994), and holding that one of these conditionals is always true—we just don’t (and perhaps can’t) know which. I will not explore these avenues in detail, or try to choose between them, though I should note that the latter approach looks easier to square with the fact that we have determinate probability judgments about many conditionals which would plausibly be indeterminate on a supervaluationist approach. Having said that, my main point here is that there are at least *prima facie* reasonable paths of response available to defenders of *CEM* in response to this obvious concern.[5]

5 Lewis pushes a more abstract version of this objection: in his semantic framework, *CEM* commits us to similarity orderings on worlds being well-orders, which is metaphysically implausible. But this worry seems wrongheaded: if natural language demands that the relevant orderings have a certain structure, then we should follow its lead, and then figure out how best to intuitively characterize the orderings in question, rather than letting our intuitions about metaphysics dictate our semantics. See §7 for further discussion of this point. Among other things, *CEM* commits us to the *Limit* assumption, which cannot be straightforwardly defused with supervaluations or epistemicism (though see Swanson 2012). Again, *Limit* feels metaphysically implausible; but, again, from the point of view of logic and language, *Limit* is very natural: as Herzberger (1979) observes, the assumption is equivalent to the very plausible claim that, for any p, if ‘If p, q’ is true for all q in some set of sentences Γ, then Γ must

What is the present standing of *CEM* and *Duality* in the literature? My impression is that *Duality* tends to come out ahead. Among linguists, Kratzer's *Duality*-friendly theory is dominant, and it has become increasingly popular among philosophers in recent years as well; prominent alternatives given by linguists, like von Fintel (1997, 2001)'s dynamic strict conditional, likewise invalidate *CEM*. Among philosophers since Stalnaker, while some arguments for *CEM* and against *Duality* have been mounted, this stance seems to remain a minority position: Cariani & Goldstein (2018), in defending *CEM*, characterize it as a 'conditional heresy'; Williams (2010) writes: 'Folklore…has treated [*CEM*] as the clear loser in the face of Lewis's 'might' arguments'; in a memorable turn of phrase, van Fraassen (1976) describes *CEM* as 'the peculiar Stalnaker principle first denied by Lewis'.[6]

How do philosophers and linguists who invalidate *CEM* account for the data which seem to speak in its favor, like Higginbotham's conditionals? The main line of response is due to von Fintel 1997. Von Fintel argues that, in a wide range of domains (modal and otherwise), covert universal quantification comes paired with a *homogeneity assumption* (see Schlenker 2004, Križ 2015, Cariani & Goldstein 2018 for recent discussion): in a quantificational structure ⌜Q(p)(q)⌝, where 'Q' is a covert quantifier, there is a going assumption that all the relevant p-things are alike with respect to q—either they are all q, or they are all $\overline{q}$. We assume a Kratzerian/Lewisian theory on which bare conditionals (conditionals without an overt modal or conditional in the consequent) contain a covert universal modal (more on this shortly). Then 'If p, q' will come along with a homogeneity assumption which, *when satisfied*, will guarantee that 'If p, q or if p, not q' is true. 'If p, q or if p, not q' won't *always* be true, for the homogeneity assumption won't always be satisfied. But in the cases in which it's not, the conditional will not be assertable, and so *CEM* will always *seem* to be true in cases in which we actually contemplate conditionals. The promise of this kind of approach is that it can account for all the

be consistent provided p is possible. Again, I think we should let intuitions about language and logic drive our model theory, rather than *vice versa*.

6 For pro-*CEM*/anti-*Duality* views, see e.g. Stalnaker 1984, McGee 1985, DeRose 1994, Heller 1995, DeRose 1999, Weatherson 2001, Cross 2009, Williams 2010, Swanson 2012, Klinedinst 2011, Rothschild 2013, Cariani & Goldstein 2018. For anti-*CEM*/pro-*Duality* views, besides Lewis's and Kratzer's, see e.g. Bennett 1974, Groenendijk et al. 1996, Bennett 2003, Gillies 2004, 2007, Yalcin 2007, Williams 2008, Gillies 2010, 2009, Kolodny & MacFarlane 2010, Starr 2014, Stojnić 2016, Holliday & Icard 2017, Santorio 2018.

CEM-friendly data we've seen, while also validating *Duality*, without collapsing 'If p, might q' to 'If p, q'. Something along these lines seems to me to be the best way to stand strong against *CEM* and in favor of *Duality*.

3 Credences

But this approach is not satisfying. For while facts about assertability and disagreement provide *prima facie* motivation for *Duality*, when we turn our attention to *graded* judgments about conditionals, it becomes quite clear, I will argue, that *Duality* is not valid.[7]

Suppose that Mark is holding a fair coin. He has his back to us. We see some motion, but are not sure what has happened. Jane says the following:

(13) [Jane:] If Mark flipped the coin, it landed heads.

Now suppose you and I are talking about what Jane said. I ask: 'What do you think of what Jane said? What is the probability that what she said is true?' If you are like most respondents, you will say that there is a .5 chance that what Jane said is true. Now suppose that Steve says the following:

(14) [Steve:] If Mark flipped the coin, it might have landed tails.

Now I ask you: 'What do you think of what Steve said? What is the probability that it is true?' If you are like most respondents, you will say that there is a very good chance that it is true: it is something we can be certain, or nearly certain of. In other words, (14) has a chance of being true somewhere near 1.

We can elicit similar judgments with counterfactuals. Suppose that Mark is holding a fair coin, but doesn't flip it. Now consider the following:

(15) [Jack:] If Mark had flipped the coin, it would have landed heads.

7 Dorr & Hawthorne (2018) independently make essentially the same argument. Similar credence arguments have been made in a corresponding debate about future contingents (Prior 1976, Belnap et al. 2001, Cariani & Santorio 2018). See Edgington 1986, DeRose 1994, Eagle 2007 for closely related but different credence arguments concerning conditionals. DeRose's argument goes by way of judgments about assertability rather than credence judgments; Eagle's involves conditionals which embed 'would' and 'might' under 'likely', which raise complexities which the present argument avoids (and is more open to the kind of Kratzerian response I consider below).

(16) [Sue:] If Mark had flipped the coin, it might have landed tails.

What is the chance that what Jack said is true? Most people say it has a .5 chance of being true. What is the chance that what Sue said is true? Most people say that it has a chance near 1 of being true.

With these judgments in mind, let me turn to my argument against *Duality*. According to *Duality*, (13) and (14) are contradictories: each is equivalent to the negation of the other (assuming for simplicity that heads and tails are the only possible outcomes of a flip). Likewise, according to *Duality*, (15) and (16) are contradictories. Assuming that credences are well-modeled by a probability function, rational credence in p and rational credence in $\overline{p}$ necessarily sum to 1. Then it follows that if you assign .5 credence to (13), you *must* assign .5 credence to (14). Conversely, if you assign credence near 1 to (14), you *must* assign credence near 0 to (13). Likewise, *mutatis mutandis*, for (15) and (16). In short: *Duality* rules out as irrational the credence assignment that nearly everyone takes to be rational in these cases. And so *Duality* is starkly at odds with our intuitive credence judgments about conditionals.

Let me consider some responses to this argument. A first response is to challenge the judgments. But these judgments are those of the vast majority of people. And they seem robust under reflection: the cases in question are simple (they do not involve complex reasoning of any kind), and informants do not seem at all inclined to revise their judgments on further reflection (unlike in cases of well-known probabilistic fallacies, where informants generally change their judgments once they see an error in their reasoning). Moreover, since *Duality* predicts these credence judgments to be *impermissible*, all I need for my argument against *Duality* is that it is *rationally permissible* to have the judgments I have elicited here; I do not need the stronger claim that this is rationally required. Such a weak claim seems hard to challenge. In conversation, some have suggested to me that they can access the intuition that your credence in (13)/(15) should be 0 (stressing 'would' in the latter helps). But I do not think it can be maintained that this is rationally required. One way to see this is to compare the present case to one in which Mark flips a coin which we know is double-tailed. In that situation, it is clear that you *are* rationally required to have 0 credence in (13)/(15). But that case is intuitively very different from our case, where we know that the coin in question is fair; this contrast helps make clear the intuition that, in our case, it is perfectly permissible to have credence around .5 in (13) and (15).

A second response is to argue that rational credence does not obey the rule that credence in a proposition and its complement should sum to 1. To maintain *Duality*, we would have to accept in particular that rational credence in a proposition and in its complement can sum to *more than* 1; this does not seem like a plausible response.[8]

A third response pursues a broadly Kratzerian error theory about the judgments in question (see especially Kratzer 1986, Bennett 2003: p. 251, Egré & Cozic 2011, Kratzer 2012, Rothschild In press). The idea would be that the judgments in question are perfectly rational, but they are not judgments about what they appear to be about. For instance, when subjects say that (13) has a .5 chance of being true, what they are *actually* judging is that the following sentence is true:

(17) If Mark flipped the coin, there is a .5 chance that it landed heads.

And thinking that (17) is true, the thought would be, does not commit speakers to thinking that (13) has a .5 chance of being true.

But this does not seem like a plausible response. I was careful in formulating the questions about probability above *not* to make them questions about whether sentences like (17) were true, but rather to make them questions about the probability of *what Mark/Jane/Jack/Sue said* being true. It's very natural that these judgments go together—that the probability of (13) being true is .5 just in case (17) is true. This is something I can happily take on board. By contrast, the error theory we're considering here must deny exactly this assumption: it must maintain that (17) is true, but deny that the probability of (13) is .5 (or else likewise, *mutatis mutandis*, for (14)). This seems a very uncomfortable position to be in. One way to maintain such a disconnect would be to claim that we cannot even really *think* about the probability of a conditional; all we can think about is the truth or falsity of probabilistic conditionals. But this is a very strange thought; and, again, I do not know of any evidence for it. It's important to be clear that the error theory we are countenancing as a response here is a much deeper error theory than one Bennett (2003) goes in for, on which speakers who say 'There is an *n* chance that, if p, then q' in fact mean 'If p, then

8 One motivation for this kind of position could come from non-classical logics; see Williams (2012), Hedden (2013) for discussion. That motivation, however, does not seem particularly relevant or helpful to the defender of *Duality*.

there is an *n* chance that q'. That is a relatively mild error theory, on which judgments about the truth of one sentence are mistaken for judgments about the truth of another sentence, closely related to the first by movement of a probability operator; this sort of error theory fits naturally with a Kratzerian vantage point on conditionals, and seems plausible enough. But much more than this is required to account for the present judgments: what is needed is the much stronger, and I think less plausible, error theory which says that judgments about the *probability* of one sentence are confused for judgments about the *truth* of a different sentence, one related to the first only by the addition of an operator absent in the first.

Absent a more convincing response, we should conclude, then, that *Duality* isn't true. Binary phenomena like assertability and disagreement make *Duality* look attractive. But graded judgments show that *Duality* runs counter to clear intuitions about rational credence in conditionals.

Credence judgments provide not just an argument against *Duality*, but also further support for *CEM*. Consider pairs like the following in a set-up as above:

(18) a. [Jane:] If Mark flipped the coin, it landed heads.
b. [Luke:] If Mark flipped the coin, it landed tails.

As we saw above, it seems clear that your credence in what Jane says should be .5. By perfectly parallel reasoning, your credence in what Luke says should also be .5. Parallel considerations go for the counterfactual variants in (19):

(19) a. [Jack:] If Mark had flipped the coin, it would have landed heads.
b. [Sue:] If Mark had flipped the coin, it would have landed tails.

Now keep focusing on pairs like those in (18) and (19), but change the coin from a fair coin to a weighted one. If the coin is weighted in favor of heads at a ratio, say, of 2:1, then what should your credence be in the conditionals of each pair? Intuitively, $\frac{2}{3}$ and $\frac{1}{3}$, respectively. What if it is weighted in favor of tails at a ratio of 5:4? Then your credence should be $\frac{5}{9}$ and $\frac{4}{9}$, respectively. And so on. The important thing here is that, in every case, *rational credence in the pair of conditionals sums to 1*. That, of course, is precisely what *CEM* predicts (assuming that rational credence in a proposition and in its complement always sum to 1), since *CEM* predicts that, in every case, the disjunction of the two conditionals is a logical truth.[9] So credence judgments provide further inductive support for *CEM*.[10]

4 Stalnaker's semantics

Credence judgments thus show *Duality* to be false, and they provide further inductive support for *CEM*. Given this, we are on the market for a theory of the conditional that invalidates *Duality* and validates *CEM*. The task of the remainder of the paper will be exploring theories that fit the bill. I will begin by critically examining the *locus classicus* (and, essentially, the only extant contender)[11] for such an approach, namely that of Stalnaker 1968, Stalnaker & Thomason 1970, Stalnaker 1980.

Stalnaker's semantics says that 'If p, q' is true just in case q is true at the closest world where p is. Stalnaker makes this precise using the formal mechanism of selection functions; I will present things in an equivalent way using the apparatus of *order functions* $<(\cdot)$ which take worlds to well-orders on worlds, representing comparative similarity of worlds relative to a given world.[12] We stipulate that the minimal element of $<(w)$ is always w (i.e., w is strictly more similar to w than any other world). Then, where $\textsc{min}_{<(w),\mathrm{p}}$ is the minimal world w' according to $<(w)$ such that $[\![\mathrm{p}]\!]^{<,w'} = 1$:[13]

$$\textit{Stalnaker semantics}: [\![\text{If p, q}]\!]^{<,w} = [\![\mathrm{q}]\!]^{<,\textsc{min}_{<(w),\mathrm{p}}}$$

9 And the two conditionals are jointly incompatible on any reasonable theory, given the consistency of the antecedents.

10 Santorio (2017) corrals this inductive evidence into a stronger argument for *CEM*, by showing that a probabilistic form of *CEM* follows from Stalnaker (1970)'s thesis that the probability of a conditional goes by way of the probability of its consequent on its antecedent—a thesis which cannot always hold (Lewis 1976), but seems to hold in a wide range of cases (see Douven 2015 for discussion of recent empirical motivation).

11 Cf. the approach of McGee 1985, which differs from Stalnaker only in ways that go beyond our interest in this paper.

12 A well-order is a well-founded linear order, i.e. an order which is total, anti-symmetric, and transitive, and which is such that any subset has a least member—e.g., the standard ordering on the positive integers.

13 If there is no such world, then we let the minimal world be an absurd world λ which makes every sentence true. I use italic letters to stand for the proposition expressed by the corresponding Roman sentence letter, suppressing relativization to an order function for brevity; likewise throughout, *mutatis mutandis*. As usual I will leave off the world superscript to indicate abstraction over worlds, e.g. $[\![\mathrm{p}]\!] = \lambda w'.[\![\mathrm{p}]\!]^{w'}$.

The first thing to note about Stalnaker's semantics is that it validates *CEM*. A conditional 'If p, q' is true iff the minimal p-world is a q-world. If it is, 'If p, q' is true; if not, then the closest p-world must instead be a $\overline{q}$-world, and so 'If p, not q' is true instead—guaranteeing that the disjunction of the two is always true.

What about *Duality*? To assess this we have to say more about how we interpret 'might' in the consequent of a conditional. The most obvious option is to interpret it *in situ*. Then 'If p, might q' will mean: the closest p-world is one where 'Might q' is true. But, as many, including Stalnaker (1980), have observed, this won't work. A quick way to see this is the following. Stalnaker's semantics validates *Strong Centering*: if p and q are both true, then so is 'If p, q'. Now suppose that Mark is about to flip a coin, but we don't know the outcome. Then 'It might land tails' is true. Suppose in fact that Mark will flip the coin, and it will land heads; so 'The coin will land heads' is true (but we don't know it). Then 'If the coin lands heads, it might land tails' is predicted to be true, if we interpret the 'might' *in situ*. But this does not sound true; indeed, it sounds like the kind of thing that cannot be true. So we cannot flatfootedly interpret 'might' *in situ* in Stalnaker's conditional.

A natural way of making sense of the infelicity of sentences like 'If the coin lands heads, it might land tails' is to say that the 'might' is scoping over the conditional 'If the coin lands heads, it will land tails', which would straightforwardly account for its infelicity. This is exactly what Stalnaker (1980) proposes: he argues that, in general, when 'might' is in the consequent of a conditional, we interpret it as taking wide-scope over the corresponding bare conditional. That is, 'If p, might q' has the logical form 'Might (if p, q)'.

This approach generally gives natural truth conditions for 'might'-conditionals (see DeRose 1994, 1999 for discussion and defense). And it has two important upshots for *Duality*. First, 'If p, q' and 'If p, might not q' are not predicted to be contradictories. Instead, these stand to each other roughly as do p and 'Might not p', which are not contradictories on any reasonable theory of 'might'. So *Duality* is invalid—as desired. But, crucially, Stalnaker's theory still explains the data that motivated *Duality*. For, given natural assumptions about 'might', p and 'Might not p', despite being jointly consistent, are felt to be in disagreement with each other, and are not co-assertable, for familiar, broadly Moorean reasons: asserting the first expresses that the speaker knows p; asserting the second expresses that she does not.[14] So, despite invalidating *Duality*, Stalnaker still has the resources to make sense of the motivation for it.

I am sympathetic with the spirit of this line on 'might' conditionals. But the details, I believe, are not viable. Problems come from conditionals with complex consequents. Consider in particular a conditional with a 'might' which scopes over part but not all of the consequent. Abstractly, the problem is that we cannot wide-scope the 'might' over the whole conditional, since part of the consequent must intuitively escape its scope. More concretely, suppose I hear a crash from the next room, and I suspect that John has knocked over a vase which Sue really loves, but which I suspect Mark rather doesn't like. I can truly say the following:

(20) If John broke the vase, then Sue will be furious, but Mark might be happy.

On the most flat-footed implementation of Stalnaker's theory of 'might' conditionals, the 'might' which appears superficially in the consequent in fact takes scope (at the relevant level of semantic computation) over the whole conditional, so (20) is predicted to have the logical form (21):

(21) It might be that (if John broke the vase, then Sue will be furious, but Mark will be happy).

The problem is that (21) and (20) are obviously not equivalent: (21) feels much weaker. In particular, (20) communicates that I am sure Sue will be furious if John broke the vase, whereas (21) does not communicate this at all.

A natural response would be to maintain that (20) has the logical form of two conjoined conditionals, as in (22):

(22) If John broke the vase, then Sue will be furious, and it might be that, if John broke the vase, Mark will be happy.

This line of response, however, runs aground on slightly more complicated cases—in particular cases in which proportional quantifiers take scope over a complex consequent. Suppose that an NGO is considering finding plaintiffs to file suit against a tobacco company, but hasn't actually found any plaintiffs yet. Based on the group's strategy for finding plaintiffs, we know that, if a suit went forward, a third of the

14 Actually, there has to be more to the story than this: see §6 for further discussion.

plaintiffs would win outright, a third would lose outright, and a final third would not win outright, but *might* get a settlement. In this case (23) seems true:

(23) If a suit were filed, then most of the plaintiffs either would win or might get a settlement.

Now note that 'most of the plaintiffs' has to be interpreted in the consequent of the conditional, since the plaintiffs exist only under the supposition that a suit is filed: there are not yet actual plaintiffs (so we cannot coherently wide-scope the nominal quantifier, as in 'Most of the plaintiffs are such that, if a suit were filed, then they either would win or might get a settlement'). So how do we extend a Stalnakerian line to (23)? The first option is to say that 'might' takes wide scope over everything, as in (24):

(24) It might be that (if a suit were filed, then most of the plaintiffs either would win or would get a settlement).

But (24) is weaker than (23). For instance, consider a scenario in which, if a suit were filed, a third of the plaintiffs *might* win, but also might not win, and definitely won't get a settlement; the second third might get a settlement, but definitely wouldn't win; and the final third definitely wouldn't win or get a settlement. In that scenario, it strikes me that we clearly know that (24) is true, whereas we do not know that (23) is true. So these don't mean the same thing.

A second option, recapitulating the response above, is to say that (23) is equivalent to the following disjunction of conditionals:

(25) If a suit were filed, then most of the plaintiffs would win, or it might be that if a suit were filed, then most of the plaintiffs would get a settlement.

The problem, of course, is that (25) is clearly not equivalent to (23): the proportional quantifier in the consequent gums up this strategy. In our initial scenario, where (23) was true, (25) is clearly false: it's neither the case that most of the plaintiffs would win (two-thirds would certainly not win); nor is it the case that it might be that most of the plaintiffs get a settlement (two thirds certainly will not get a settlement). So (25) and (23) do not mean the same thing.

5 The restrictor analysis

Stalnaker's strategy for interpreting 'might' conditionals thus runs into trouble from conditionals with suitably complex consequents. We need a sensible way to interpret 'might' *in situ*. There is a theory of conditionals that is explicitly designed to account for the interaction of 'if'-clauses with overt modals: namely, the restrictor theory. Starting with the work of Lewis 1975, developed by Kratzer 1981, 1986, research in this tradition has argued that the role of 'if'-clauses is to restrict the domains of modals in the consequent of conditionals: when there is no overt modal, we assume there is a covert modal. This seems to be exactly the sort of framework we need to deal with the cases above. As I mentioned above, the standard development of the restrictor analysis, in Kratzer 1981, validates *Duality* and not *CEM*. In this section, I will introduce Kratzer's theory and show how we can modify it to validate *CEM* and not *Duality*.[15] In the next section, I'll return to Stalnaker's theory and explore more sophisticated avenues for interpreting epistemic modals *in situ* in the context of his theory.

On Kratzer's approach, sentences are evaluated relative to two parameters, one which provides a set of worlds, and one which orders those worlds.[16] The first parameter is a *modal base* function f which takes any world to a set of worlds which includes that world; the second is an order function $\preceq(\cdot)$ which takes any world to partial pre-order on all worlds, with the property that for any world w, w is minimal in $\preceq(w)$ (we also make the simplifying assumption that $\preceq(w)$ is well-founded). Then Kratzer's semantics for 'if'-clauses goes as follows:

Kratzer 'if': $[\![\text{If p, q}]\!]^{f,\preceq,w} = [\![\text{q}]\!]^{f^{p^{\preceq}},\preceq,w}$

$f^{p^{\preceq}}$ is the limitation of the modal base to p: in other words, it is the smallest function such that $\forall w' : f^{p^{\preceq}}(w') = f(w') \cap [\![\text{p}]\!]^{f,\preceq}$. So 'if'-clauses have a very simple role: namely, to restrict the modal base with their content. The crucial second step of the

15 The restrictor analysis was in fact developed in quite different ways in Kratzer 1981 and Kratzer 1986, respectively (see Schulz 2009 for helpful discussion). I focus on the first of these analyses here, which I think is more promising. Very briefly, the reason for this is that the second analysis holds that, at some level of logical form, conditionals actually have the structure 'Modal(p)(q)'. What do we say, then, about conditionals with complex consequents? It seems to me that this version of the restrictor view faces the same objection just sketched against Stalnaker's view.

16 I depart from Kratzer's presentation slightly, to make the comparison with other approaches clearer.

restrictor theory is to posit that the consequent of a conditional always contains a modal. In particular, bare conditionals—conditionals which lack an overt modal or conditional in their consequent—generally contain a covert 'must' with scope over their consequent, with the semantics given here:

$$\textit{Kratzer 'must'}: [\![\text{Must p}]\!]^{f,\preceq,w} = 1 \text{ iff } \forall w' \in \text{MIN}_{f,\preceq,w} : [\![\text{p}]\!]^{f,\preceq,w'} = 1$$

$\text{MIN}_{f,\preceq,w}$ is the set of minimal worlds in $f(w)$ according to $\preceq(w)$.[17] 'Might' is treated as the dual of 'must', as usual. So 'must' says that all closest worlds are p-worlds, and 'might' says that some closest world is a p-world. Putting this together with Kratzer's semantics for 'if', we get the following: 'If p, q' is true just in case all the closest p-worlds are q-worlds (assuming there is a covert 'must' taking scope over q). 'If p, might q' is true just in case some closest p-world is a q-world.

This approach avoids the objection raised above to Stalnaker's approach: it has no problem interpreting epistemic modals *in situ* in the consequents of conditionals. In particular, sentences with the form 'If p, might not p' will never be (non-trivially) true, since the domain of the epistemic modal will be restricted to p-worlds. But Kratzer's theory lands on the wrong side of the *CEM* vs. *Duality* debate. Again, because 'must' and 'might' are duals, and because Kratzer assumes that bare conditionals contain a covert 'must', Kratzer validates *Duality*: 'If p, q' and 'If p, might not q' are contradictories. And Kratzer invalidates *CEM*: since there can be more than one closest p-world, it may be that some closest p-worlds are q-worlds and some are $\overline{q}$-worlds, in which case neither 'If p, [must] q' nor 'If p, [must] not q' will be true (using brackets to indicate covert material).

But this vice of Kratzer's theory is separable from the virtues of the restrictor approach: we can give a simple variant which retains those virtues, while invalidating *Duality* and validating *CEM*. The central idea is to keep Kratzer's semantics for 'if', but posit a different covert modal for bare conditionals: instead of 'must', we assume bare conditionals contain a covert modal which selects a unique closest world. The result matches Stalnaker's theory for bare conditionals, but in a way which deals more easily with overt modals.

17 I.e. the set of worlds $w' \in f(w)$ such that no world in $f(w)$ is strictly better than w' according to $\preceq(w)$.

In more detail, we define a *selection modal* 'σ' as follows; this generalizes proposals for the semantics of 'will' given in Cariani & Santorio 2018, Kratzer In Press.

(26) $[\![\sigma(\text{p})]\!]^{f,\preceq,<,w} = [\![\text{p}]\!]^{f,\preceq,<,\text{MIN}_{f,<,w}}$

f, $\preceq(\cdot)$, and $<(\cdot)$ are all defined as above, and $\text{MIN}_{f,<,w}$ is the minimal world in the well-order $<(w)$ which is in $f(w)$.[18] Hence ⌜σ(p)⌝ on this semantics is true just in case p is true at the closest world in the value of the modal base. We leave Kratzer's semantics for 'if', 'must', and 'might', unchanged (simply generalizing them all by relativizing them to the well-order function).

The crucial change from Kratzer's view is that in bare conditionals, rather than assuming a covert 'must', we assume that a covert 'σ' takes scope over the consequent of the conditional. Assuming that the matrix modal base parameter is uninformative (i.e., takes every world to the whole set of worlds), this has the result that our truth conditions for bare conditionals coincide with Stalnaker's: 'If p, σ(q)' is true just in case the closest p-world is also a q-world. We thus validate *CEM*: it will always be the case that either 'If p, [σ] q' is true or that 'If p, [σ] not q' is true, since the closest p-world will always be a q-world or a $\overline{q}$-world. And, of course, we invalidate *Duality*. 'If p, q' is true at w just in case the closest accessible p-world to w according to $<(w)$ is a q-world. 'If p, might not q' is true just in case, among the closest accessible p-worlds to w according to $\preceq(w)$, there is a $\overline{q}$-world. Nothing prevents both these conditions from obtaining at the same time.

But how do we account for the assertability and disagreement intuitions that motivated *Duality*? Here we can give an account similar in spirit to Stalnaker's, by stipulating a connection between the order functions $<$ and $\preceq$. In particular, we can hold that for any world w and proposition p, the closest p-worlds according to $\preceq(w)$ are exactly those worlds which, *for all the relevant evidence in w entails*, are the closest p-world according to $<(w)$. In the non-conditional case, this predicts that 'might' and 'must' quantify over all the worlds which are such that, for all the relevant evidence entails, are the actual world, matching standard treatments (e.g. Kratzer 1977, 1981). When 'might' appears in a conditional of the form 'If p, might q', however, this will be felt to be a claim that, *for all we know*, the closest p-world

18 If $f(w)$ is empty, then we stipulate $\text{MIN}_{f,<,w}$ is λ, the absurd world.

is a q-world. This straightforwardly accounts for the data motivating *Duality*, in precisely the same way that Stalnaker does: 'If p, q' and 'If p, might not q' will not be co-assertable, and will be felt to be in disagreement, because the latter entails that the relevant evidence leaves open that the closest p-world is a $\overline{q}$-world, and thus that the relevant evidence leaves open that the former is false.

Unlike Stalnaker's approach, however, the present approach inherits all the virtues of Kratzer's restrictor analysis in its ability to interpret overt modals *in situ*, avoiding the objection to Stalnaker's account given above.

A final virtue of the present approach vis-à-vis the standard implementation of the restrictor theory is that it accounts for intuitive differences between the meaning of 'If p, q' and 'If p, must q'. Kratzer's approach predicts these to be equivalent: the latter simply makes overt what the former contains overtly. But the meaning of these can diverge, as Rothschild 2013 notes (citing Benjamin Spector). Suppose that Mark is holding a fair coin and then turns his back to us. Compare:

(27) [Jane:] If Mark flipped the coin, it landed heads.

(28) [Michael:] If Mark flipped the coin, it must have landed heads.

Suppose that Mark turns around and tells us that he did flip the coin, and it landed heads. Then it seems clear that Jane spoke truly in asserting (27), even if it was just a lucky guess; but Michael's claim in (28) does *not* strike us as true. And so (27) and (28) do not mean the same thing, *pace* Kratzer. By contrast, this divergence is just what the present approach predicts, since on the one hand, 'If p, q' says that the closest p-world is a q-world, while, on the other, 'If p, must q' says that the relevant evidence entails that the closest p-world is a q-world. So, roughly, we predict that (27) only says that the closest flip-world is a heads-world, while (28) says, moreover, that this is *known* to be so: this predicts a difference in their meaning which seems to match intuitions.

6 Stalnaker with local contexts

Assuming that bare conditionals contain a covert selection modal, rather than a covert 'must', lets us marry what is attractive about the restrictor analysis—its ability to make sense of overt modals *in situ*—with what is attractive about Stalnaker's theory—validating *CEM*, not *Duality*. This is a step forward. But this approach has several drawbacks which I want to highlight now.

First, this approach shares with the standard implementation of the restrictor theory the peculiar assumption that conditionals which lack overt modals are always saturated, at the level of logical form, with unpronounced covert modals. There is something intuitively unsatisfying about this assumption. Reflecting on learnability helps make this concern more acute. How do we learn to insert covert modals in all the needed places? And how do we learn which modal to put in? We can imagine a wide array of options that would seem to be open to children concerning what kind of modal we put in (existential? universal? epistemic, deontic, metaphysical?) as well as when to insert them (always? sometimes? never?). How do children (both within and across languages) converge on the correct combination? The restrictor theory must answer these difficult questions. Perhaps an answer can be given; but a theory without covert modals would of course avoid this explanatory debt.

A second worry about this approach is that it shares with the restrictor theory certain logical peculiarities concerning complex conditionals. Most worryingly, if we agglomerate successive conditional antecedents in the manner of the restrictor theory, we will invalidate the principle that conditionals of the form 'If p, then p' are always true. The basic issue is that, on restrictor theories, the interpretation of conditionals depends on the content of the restriction (i.e., the content of the modal base); and this can change within a sentence. So, in 'If p, then p', the second p is interpreted relative to a different modal base than the first—namely, relative to one restricted to *p*-worlds. But that means that, if p itself contains a conditional, then the conditional can be interpreted differently in the two instances, and so 'If p, then p' can fail to be true. I spell this point out in more detail in Mandelkern 2018, 2019b. The principle that 'If p, then p' is a logical truth seems to me very natural, and so this concern strikes me as very serious for any restrictor-style theory, including the present implementation.

A final concern about this approach comes from more general considerations about epistemic modals. Exploration of the embedding behavior of epistemic modals shows that they have restricted readings *across the board*, not just in the consequents of conditionals (see e.g. Groenendijk et al. 1996, Aloni 2000, Yalcin 2007, Dorr & Hawthorne 2013, Mandelkern 2019a). So, for instance, it looks like in a sentence of the form ‘p and might not p’, the ‘might’ is restricted to *p*-worlds, making such sentences inconsistent. Evidence for this comes from the fact that sentences with this form are not only incoherent (which might be explained on pragmatic grounds) but also embed in incoherent ways, as when embedded under attitude predicates or disjunction:

(29) #Suppose it’s not raining and it might be. Yalcin 2007

(30) #Either I won’t win but I might, or I won’t lose but I might. Mandelkern 2019a

It seems unlikely to me that the kind of modal restriction we find in sentences like (29) and (30) is different in kind from the kind of modal restriction we find in ‘might’ conditionals. Further evidence that there is just one phenomenon here, not two, comes from Santorio (2017)’s observation that ‘If p, q; and if p, might not q’ not only strikes us as incoherent, but also embeds incoherently, like conjunctions of the form ‘p and might not p’:

(31) #Suppose, first, that the picnic will be cancelled if it rains; and, second, that the picnic might not be cancelled if it rains.

This shows that simply predicting such conjunctions to be pragmatically incoherent, as both Stalnaker’s wide-scoping view and our variation on the restrictor theory do, misses something important about the relation between bare conditionals and ‘might’ conditionals.

With these points in mind, I want to return to Stalnaker’s theory of the conditional, and explore the combination of that theory with a *general* theory of the interpretation of embedded epistemic modals. As I argued above, Stalnaker’s wide-scoping proposal about ‘might’ conditionals is not plausible in general; what I will suggest here, following suggestions by Cian Dorr (p.c.), is that, given a suitably sophisticated theory of epistemic modals—a theory I have motivated on independent grounds elsewhere—we can in fact interpret epistemic modals *in situ* in Stalnaker’s conditional.

Recall that a simple motivation for the wide-scoping route that Stalnaker proposes is to account for the infelicity of sentences with the form ‘If p, might not p’. As we saw above, the infelicity of such sentences is not straightforwardly explained by Stalnaker's semantics for conditionals. But, reflecting on sentences like (29) and (30), this doesn't look so surprising: in fact, the infelicity of sentences with the form 'If p, might not p' looks like an instance of a general pattern by which epistemic modals get restricted by something like their local information. A general theory of how epistemic modals are restricted by their local information may therefore be able to account for the data in question while leaving 'might' in place. There are different such theories on offer. Broadly speaking, we might try to account for this behavior on pragmatic grounds, following Dorr & Hawthorne 2013; or on semantic grounds, the route I advocate in Mandelkern 2019a. Since my sympathies are with the latter route, that's the one I'll explore here, though the general picture that results is compatible with a pragmatic implementation as well.[19]

The theory I give there, the *bounded* theory, starts with a standard modal account of 'might' as an existential quantifier (essentially Kratzer's account, but we can simplify by eliminating the order function):[20]

Standard 'might': $[\![\text{Might p}]\!]^{\kappa,f,w} = 1$ iff $\exists w' \in f(w) : [\![\text{p}]\!]^{\kappa,f,w'} = 1$.

f is still a modal base; κ is a *local context*. Following Schlenker 2009, the local context of a clause of a sentence is the unit of information which represents the information already available for the interpretation of that clause: in other words, whatever information you could add as a conjunct at that point in the sentence that is guaranteed not to change the meaning of the sentence as a whole (see Schlenker 2009 for a more formal explication). More on local contexts in a moment; the general details are not important for present purposes. The key idea for the purposes of epistemic modals is that *local contexts restrict epistemic modals' domain of quantification*. In particular, epistemic modals presuppose that local context worlds can access only local context worlds; I call this the *locality constraint*.

Bounded 'might': $[\![\text{Might p}]\!]^{\kappa,f,w}$ is defined iff $\forall w' \in \kappa : f(w') \subseteq \kappa$;
where defined, true iff $\exists w' \in f(w) : [\![\text{p}]\!]^{\kappa,f,w'} = 1$.

19 In Mandelkern 2019a, I argue that facts about order make trouble for pragmatic approaches, but those issues are largely independent of present concerns.

20 In Mandelkern 2019a, I put modal bases into the object language; while that move is probably necessary, it doesn't matter for present purposes, so I keep a modal base in the index for simplicity.

At a high level, this kind of restricting should look familiar: it is a lot like the kind of restricting that Kratzer's theory attributes to the antecedents of conditionals. Crucially, however, this formulation is what is required *in general* to account for the interpretation of epistemic modals in sentences like (29) or (30); see Mandelkern 2019a for much more extensive explication and motivation.

The question for the present is what this theory predicts in the context of Stalnaker's theory of the conditional. To answer that, we must ask what the local context is for the consequent of a conditional given Stalnaker's theory of the conditional. Given Schlenker's operationalization of the notion of local context, this amounts to the question: what information can we add to the consequent of a conditional which is guaranteed not to change the interpretation of the conditional, whatever else goes there? One thing we can surely add is the information in the antecedent: since we must evaluate the consequent at an antecedent-verifying world, adding the antecedent as a conjunct to the consequent can't possibly change the truth value of the conditional (in other words: 'If p, then q' and 'If p, then p and q' are guaranteed to be semantically equivalent, in Stalnaker's theory). In fact, given what we've said so far, the antecedent is all we can add to the consequent while being guaranteed not to change the truth-value of the conditional as a whole. As Mandelkern & Romoli (2017) (who also make the present reasoning more formal) discuss, this is not altogether satisfying: we want the local context for the consequent of an indicative conditional to also include the global context. But, as we discuss there, this can be achieved by taking into account standard assumptions about the contribution of indicative mood; this is not relevant for present purposes, so we can set aside this issue. So, adding local contexts and modal bases into Stalnaker's theory, we have:

$$\textit{Stalnaker with local contexts}\text{: } [\![\text{If p, q}]\!]^{\kappa,f,<,w} = [\![\text{q}]\!]^{[\![p]\!]^{\kappa,f,<},f,<,\text{MIN}_{<(w),\text{p}}}$$

Just to emphasize, although we are now representing local contexts explicitly, their value is already determined by Stalnaker's theory plus Schlenker's algorithm for calculating local contexts: importantly from the point of view of explanatory power, we are not making any stipulations here, just applying a general algorithm to Stalnaker's semantic framework.

So let's see what the combination of Stalnaker's semantics with the bounded theory of modality gets us. Think about a sentence with the form 'If p, might not p'. The local context for the 'might' claim will be *p*. Furthermore, by Stalnaker's semantics, we will evaluate the consequent at a *p*-world. By the locality constraint of the bounded theory, this means that, at the world where we evaluate the consequent, if the 'might'-claim is well-defined, we will only be able to access *p*-worlds. But by the core truth-conditions of 'might', that, in turn, means that this sentence will be false. So sentences with this form will always either be false or undefined, accounting for their infelicity. Importantly, we account for their infelicity while leaving the 'might' *in situ*, which means we avoid the objection I raised above to Stalnaker's wide-scoping account. Finally, this account straightforwardly accounts for the incoherence of 'If p, q, and if p, might not q', whether embedded or not: given the left conjunct, the local context for the consequent of the right conjunct will entail *q*, meaning the conjunction as a whole cannot be both well-defined and true. Indeed, this explanation is exactly parallel to the explanation the bounded theory gives of the incoherence of 'p and might not p', in line with the intuition that these are two faces of the same problem.

7 Talking about worlds

Let me briefly review the dialectic to this point. Stalnaker's theory validates *CEM*, but it can't plausibly interpret epistemic modals *in situ* in the consequents of conditionals, given standard approaches to epistemic modals. Stalnaker proposed wide-scoping epistemic modals to deal with this, but, as I've argued, this runs into trouble when we have complex consequents. One way of responding to this is to take on the ideology of the restrictor theory of conditionals, but hold that the restricted modal in bare conditionals is not a 'must' but a selection modal. This lets us validate *CEM* while keeping overt modals *in situ*. But precisely the flexibility which lets us switch from a *Duality* to a *CEM* theory in the restrictor framework should give us pause: there is too much flexibility here for this to be an explanatorily satisfying theory. What's more, restrictor theories in general, including this variant, have implausible logics. Finally, this approach deals with epistemic modals in consequents of conditionals in an *ad hoc* way, missing an important generalization about restricted readings of epistemic modals across the board. By contrast, if we couple Stalnaker's theory with a more sophisticated approach to epistemic modals, like the bounded theory, we can plausibly interpret epistemic modals *in situ*. We thus account for their interpretation on the grounds of a general and independently motivated theory, one which also accounts for the incoherence of 'If p, q and if p, might not q' even when that conjunction is embedded. We also avoid the logical perplexities that any variant on the restrictor theory entails.

We thus avoid the latter two of the objections I raised above to my variant on the restrictor theory. What about the first objection, about explanatory power? On this front, it's not immediately clear how much of an improvement the Stalnakerian theory is. True, we don't have to insert covert modals all over the place, but we somehow have to know what kind of ordering to use in evaluating conditionals: if we use anything other than a well-order, we won't be guaranteed to have a unique closest antecedent world, and we'll end up with a very different picture—indeed, we'll end up with a *CEM*-invaliding theory like that of Lewis 1973. This is a particularly worrisome issue, I think, because there are two vantage points from which that alternative picture looks much more natural. The first is thinking of conditionals fundamentally as tools for talking about orderings on worlds—the kind of picture that comes out of Lewis 1973. For if you just reflect on what kind of similarity relation is appropriate to worlds, 'a well-order' is not an intuitive reply. It's very natural to think that, even once you make clear which features of similarity you are focusing on, we would still countenance at least ties in closeness among worlds (if not more outré phenomena like infinite descending sequences); indeed, Lewis (like others since) appealed to exactly that intuition when arguing against *CEM*. Second, if we think of conditionals as fundamentally quantificational structures, as the Kratzer/Lewis restrictor tradition does, *CEM* again looks unnatural: to obtain *CEM* in a quantificational system, we'd have to have, in essence, quantification over singleton sets, which is not common in natural language, if it exists at all.[21]

In this final section, I want to say something to try to assuage this explanatory worry. Here is one picture of the functional role of conditionals. Following ideas developed most explicitly in Schlenker 2004, 2006, we can think of conditionals as referential structures:[22] a conditional is a mechanism for talking about a particular world—namely, the closest antecedent one. Non-conditional sentences let us talk about the actual world; conditionals let us talk about particular (possibly non-actual)

21 See Schwarzschild 2002 for arguments it does exist.
22 Lewis (1973) likewise explores the application of his theory of conditionals to the nominal domain.

worlds. The role of 'if'-clauses is simply the one of focusing attention, not on the actual world, but rather on the nearest antecedent-world.

Thinking about conditionals from the point of view of reference to worlds, rather than similarity, helps with our explanatory worry. This is because there is evidence that reference *in general* in natural language works with the mechanism of well-orders. The standard way of modelling pronominal reference to individuals treats pronouns as variables, whose value is determined by a variable assignment—which, in turn, is just a well-order of individuals (as in e.g. Heim & Kratzer 1998). In more recent, and more sophisticated, developments of systems of reference and anaphora, sequences of individuals continue to play a central role, as in the dynamic tradition growing out of Heim 1982 (see also e.g. Groenendijk & Stokhof (1991), Dekker (1994)). In Dekker's system in particular, the role of pronouns is (simplifying a bit) to refer to the final (or penultimate, or antepenultimate, etc.) individual in a sequence of individuals.[23]

From a formal point of view, thinking about conditionals as in the first instance referential mechanisms might lead naturally to some changes to the formalism. For instance, to bring out the analogy between reference to individuals and reference to worlds, we could dispense with world parameters, and instead work directly with well-orders on worlds, as in van Fraassen (1976)'s models of Stalnaker's semantics. We might in particular hold that, at the level of logical form, the consequents of conditionals contain pronouns, overt or covert, which refer to worlds.[24] If natural language contains world variables/pronouns, we must say something about what to do with unbound world variables. The natural, and standard, thing to say is that they are always set to the world of evaluation. A different thing we could say, though, is that they are always set to the minimal world in the salient ordering on worlds. We could stipulate that the world of evaluation is always minimal in matrix orderings. Conditional antecedents, however, would have the role of changing the ordering (if necessary) to ensure that some antecedent-world is treated as minimal. These moves

23 Uncertainty about which individual is being talked about is represented using sets of sequences; pronouns are then treated as projection functions over that set. At a high level, this looks a lot like a semantic encoding of the supervaluationist approach to Stalnaker's semantics.

24 See Partee 1973, 1984, Enç 1997, Stone 1997 for arguments that there is such pronominal reference in general; Iatridou (1991) argues that 'then' is the overt realization of a world pronoun; see Fodor (1970), Keshet (2008), von Fintel & Heim 2011: Ch. 8 for some different motivations for the idea that natural language contains world variables.

would bring the parallel between Stalnaker's theory and theories of nominal and temporal anaphora even closer.

In short, then, if we start by thinking about conditionals as mechanisms we use to talk directly about a given world—the closest antecedent world—a well-order based theory like Stalnaker's looks much more natural than if we start by thinking about closeness orderings on worlds. Of course, this way of thinking does not *force* well-orderings on us; we could think about conditional antecedents as denoting *pluralities* of worlds, as Schlenker (2004) advocates in a similar framework; in that case, *CEM* would be invalid. So the present way of thinking about conditionals still leaves open the question of why reference to an individual world is the tack we in fact take, rather than reference to a plurality of worlds. This strikes me, however, as a fruitful question to ask from the point of view of theories of reference, rather than theories of orderings on worlds in particular. If we start by thinking about similarity orderings on worlds, well-orders seem like the last place we would end up; if we start, rather, by thinking about reference to worlds, well-orders seem like a very natural place to start. This approach allows us to then ask fruitful questions about why we in fact default to singular rather than plural reference in the case of worlds: Is there a general bias for individual rather than plural reference? If so, what explains it? If not, what explains the phenomenon in the case of conditionals? It also allows us to ask us about cases in which this default may be overridden. For instance, generic conditionals like 'If John goes to dinner, he stays for dessert', are much more plausibly analyzed as quantifying over cases in which John goes to dinner, and do not seem to validate *CEM* (intuitively, it could well be that neither that conditional nor its internal negation 'If John goes to dinner, he doesn't stay for dessert' is true, if, say, John stays for dessert half the time). A plural analysis of these generic conditionals seems much more plausible than in the case of the kinds of conditionals that we have focused on; from the present point of view, we can ask what it is about these conditionals that selects for plural rather than singular reference to worlds.

In short, then, while Stalnaker's theory leaves explanatory questions open, those questions seem much more tractable than the corresponding explanatory questions for any version of the restrictor theory—provided, that is, that we think about conditionals as, in the first instance, tools for talking about possibly non-actual worlds.

8 Conclusion

I have argued that credence judgments show that *Duality* is false, and provide further support for *CEM*. I argued that the best *CEM*-friendly theory of the conditional, however—namely Stalnaker's theory, together with his assumption that epistemic modals wide-scope—is not satisfying. I then presented two theories which validate *CEM* but do not have trouble with overt modals. The first accomplishes this on a parallel with Kratzer's restrictor theory, but assumes that bare conditionals contain a covert selection modal, rather than a covert 'must'. The second combines Stalnaker's theory of the conditional with my bounded theory of epistemic modality. While both of the resulting theories strike me as worth serious study, I have suggested that the second theory is more promising, because it builds on a more general theory of the interpretation of epistemic modals; has a more plausible logic; and has a better explanation of what conditionals are: devices which allow us to talk about worlds—not just the actual world, but also the world that would obtain if such-and-such were the case.

I have left many questions open. Among these: first, I have not tried to account for the differences between indicative and subjunctive conditionals (see e.g. Stalnaker 1975, von Fintel 1998); this is a topic I take up in the present framework in work in progress Mandelkern 2019b, where I also explore at greater length the logic of restrictor theories of the conditional. Second, I have focused on the interpretation of conditionals which are either bare, or contain overt epistemic modals in their consequents. Even in this limited range, while I have addressed some core challenges, I have left many questions unanswered; for instance, what exactly does the accessibility relation for epistemic modals represent, particularly in subjunctive conditionals? (It can't in general be compatibility with the counterfactual evidence of any people, since we can have subjunctives which take us to worlds where there are no people at all.) Even more questions remain outside this range. For instance, epistemic modals are not the only modals which have been claimed to have restricted readings in the consequents of conditionals: deontic modals (see e.g. Frank 1996, Geurts 2004, Khoo 2011) and adverbs of quantification (e.g. Lewis 1975) likewise seem to exhibit such behavior. This was, indeed, Lewis's original motivation for a restrictor-style theory. If we eschew such a theory, as I have suggested here, can we nonetheless make sense of these readings? This requires careful further exploration. Third, I have not tried to give a general account of judgments about the probabilities of conditionals. The judgments that played a central role in my argument *are* predicted by my theory—first, since we invalidate *Duality*, we predict that credences in 'If p, q' and 'If p, might not q' need not sum to 1; second, since the disjunction of 'If p, q' and 'If p, not q' is a logical truth on my theory, their probabilities (as long as they are disjoint) will sum to 1—both of which are in line with our observations above. But more needs to be said about our judgments in general about the probabilities of conditionals; here, again, we can hopefully appeal to the kind of general considerations which have been put forward recently e.g. in Rothschild 2013, Bacon 2015, Khoo 2016 for making sense of these. Another option here, suggested by the analogy I have drawn out between conditionals and individual reference, is to build on van Fraassen (1976)'s models, which is one route to partially validating *The Thesis* that probabilities of conditionals coincide with conditional probabilities (see Stalnaker & Jeffrey 1994, Kaufmann 2009 for further developments).

A final set of questions comes out of my argument that Stalnaker's theory of the conditional fits naturally into a unified way of thinking about reference to individuals, times, and worlds. I have drawn out some parallels across these domains, but there are many questions of detail that this left unanswered, of the kind I sketched in the last section. What is the best way of thinking about these referential mechanisms? Are there underlying psychological processes that commonalities—and differences—across these domains? These are questions that are, somewhat surprisingly, made pressing by a detail in the semantics of the conditional: namely, the validity of *Conditional Excluded Middle*.

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