

Subjective Modality

by

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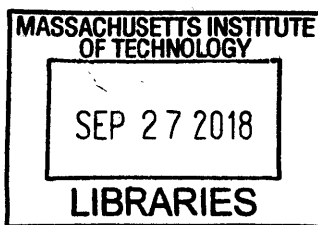
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Abstract

This dissertation focuses on subjective or epistemic readings of the modals ‘might’ and ‘should’ and considers how they fit into broader theories of modal vocabulary.

Chapter 1, ‘What the Future “Might” Brings’, develops a puzzle about epistemic modals and tense, showing that future tensed epistemic modals are surprisingly marked in cases of predictable forgetting. It gives a solution whereupon epistemic modals are monotonic: their domains only shrink going forward in time. It is noted that this property is also a feature of *circumstantial* modals and a new general picture of how epistemic and historical modality are related is proposed.

Chapter 2, ‘Putting “Ought”s Together’, argues that deontic but not epistemic ‘ought’s appear to obey the inference pattern Agglomeration. It gives a new semantics for ‘ought’, where it is an existential quantifier over best propositions, and shows how this semantics together with pragmatic features of deontic contexts can explain the differing inferential properties of deontics and epistemics.

Chapter 3, ‘More Miners’, generalises the now infamous miners problem to epistemic ‘ought’s. It shows that conservative non-probabilistic solutions do not extend to epistemic cases with the same structure. It solves the problem using probabilistic orderings over propositions and draws some morals about the metasemantics of such orderings and the role of neutrality in the semantics of deontic modals.

Thesis Supervisor: Robert Stalnaker

Title: Professor Emeritus of Philosophy

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In the Preface to *Frege: Philosophy of Language* Michael Dummett writes: ‘I am always disappointed when a book lacks a preface: it is like arriving at somebody’s house for dinner, and being conducted straight into the dining-room’. I feel Dummett’s remark applies just as well to acknowledgements, but I am also the kind of person that cannot abide delay once it is polite to eat. So let me make some heartfelt, but brief, remarks of thanks.

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Introduction

This thesis is organised not so much by a unifying thesis, but by a preoccupation with unification in the semantics of modals.

Unification can both illuminate and confusate. When modals pattern alike, unification promises an explanation. When they pattern differently, unification can seem to stand in the way. This thesis examines, directly and indirectly, cases of both kinds involving epistemic ‘might’ and ‘should’. Chapter 1 develops a puzzle about epistemic modals and future tense and shows how monotonicity, a property attributed to circumstantial modals, can account for the problem. Chapter 2 focuses on differences in the inferential properties of the deontic and epistemic ‘ought’ and offers a semantic and pragmatic package to make sense of this data. Chapter 3 focuses on a now classic problem in semantics, the miners problem, and shows how the problem can be illuminated by considering parallel cases in the epistemic realm.

Whether or not there is a unifying thesis, two big picture ideas emerge from this preoccupation with unification. The first is about the relation between epistemic and circumstantial modals. Recent literature, particularly recent expressivist views, have tended to emphasise differences between epistemic modals and other flavours of modality. Time, however, is a place where they seem strikingly similar, or so I argue. This is some reason to reconsider more orthodox views that keep the epistemic and circumstantial closer together.

The other is about the basic structure of the semantics for modals. Classically, they are treated as quantifiers over possible worlds. In this dissertation (and in other co-authored work), I explore the fruits of treating modals as quantifiers over sets of *propositions*. This, I argue, opens up new space for ‘ought’ to have a dis-

tinctive quantificational force; and it integrates better, I think, with the probabilistic orderings modal semantics sometimes needs to work with.

Chapter 1

What the Future ‘Might’ Brings

It is easy to talk about what we know right now, what we knew yesterday and what we will come to know tomorrow. We can also talk about what might be now and what might have been the case yesterday. But what about what might be tomorrow?

I show that we can indeed talk about what, in the future, might be, but that it is more complex than we might have expected. Surprisingly, in cases of *predictable forgetting*, speakers cannot describe their future states of mind with epistemic modals under future tense. This is in spite of the fact that they can foresee that they *will* be able to describe their state of mind with an epistemic modal. It is also in spite of the fact that no similar barrier exists for past epistemic modal claims.

I argue that this phenomenon shows us something important about epistemic modality. It undermines a very general approach to the semantics of epistemic modals, one which draws a very tight connection between epistemic modality and knowledge. To predict the data here, I offer a new approach to the semantics of epistemic modals, one on which epistemic modals collect information gathered within particular intervals. Further, I suggest the puzzle provides evidence for a different general picture of how epistemic and historical modality are related, one which offers a more unified account of the different flavours of modality.

1.1 The Target

It is natural to think that ‘might’-claims simply provide us with another way of talking about a given body of knowledge. That is, a sentence like

(1) It might be raining

means something like

(2) It’s not known to not be raining.

Most theories of epistemic modals take this thought at face-value and say that epistemic modals quantify over whatever the relevant knowledge state in the context is. These theories differ over *whose* knowledge they take to be relevant. Candidates include the speaker’s knowledge, the group’s knowledge or perhaps even just the knowledge of whoever the speaker has in mind at the time of utterance. But they all share the thought that epistemic modals track the knowledge of *some* agent or agents.

Here is a way of making this idea more precise and general:

Knowledge: ‘might ϕ ’ is true at a world w and time t iff ϕ is compatible with what the relevant agent(s) know in w at t .

Knowledge is an important and seemingly plausible claim about epistemic modality, one that originates in Moore (1962) and can be traced through Hacking (1967), Teller (1972) and DeRose (1991).¹ This is a general understanding of what epistemic modality consists in: what is epistemically possible at a given time is whatever is compatible with the knowledge of a certain set of agents at that time. Similarly, what is possible in the past or in the future is whatever is compatible with the past or future knowledge of those agents.

Nonetheless, I will argue that **Knowledge** is false: it makes the wrong predictions about how epistemic modals embed under future tense.² Epistemic modals do not simply track what agents know at a particular time. Rather, I argue, they

¹However, it generalises from those accounts by abstracting on both the world and time parameters.

²In English, it is not a tense morpheme but the auxiliary ‘will’ that performs the role of future tense. I ignore this complication in what follows.

are sensitive also to what those agents knew in the past. Even as agents themselves lose information over time, the set of epistemic possibilities never expands. As a consequence, epistemic modals do not pattern with knowledge attributions under future operators in the way predicted by **Knowledge**.

To predict this, I argue we should replace **Knowledge** with **History**:

History. \lceil might ϕ \rceil is true at a world w and time t iff ϕ is compatible with what is known in the relevant interval before and up to t .

According to **History**, epistemic modals collect the information that agents gather over time. Though the difference between **History** and **Knowledge** is subtle, the resulting big picture differences are significant. Epistemic modals do not simply track the knowledge of a particular agent, as knowledge attributions do. In particular, they never lose information: once something becomes necessary, it remains necessary.³ According to **History**, epistemic modals are not knowledge attributions in a different guise, but rather perform their own special function. They are devices for collecting and talking about the information obtained over time in abstraction from the deficiencies of the agents who gather that information.

1.2 Predictable Forgetting

We will be concerned with cases of *predictable forgetting*, cases where an agent at one time t knows some proposition p but also knows that by some later time t' she will have lost her knowledge that p .

Before getting started, we should note that predictable forgetting is commonplace. It is perhaps easiest to find when we receive detailed information. I can predict that I will forget that the person I just met is called John, that John lives in Apartment 211, that his phone number is such and such. These are all cases of predictable forgetting. In what follows, we will focus on somewhat more involved cases, as it is easiest to see that these are indeed cases of predictable forgetting. But, in assessing the importance of this phenomenon, we do well to remember that it is pervasive.

Take the following example:

³That is, when we hold fixed the context. See section 5 for more details.

Keys. On Monday night, Alice leaves her keys in the kitchen. But she always forgets where she has left the keys overnight. In fact, she can foresee that tomorrow morning she will go looking for them in her bedroom.

What kinds of epistemic modal claims can an agent make when they suffer from predictable forgetting? Here I make three observations.

Alice cannot report her future ignorance by saying:

- (3) # Tomorrow (I'll look in my bedroom because) the keys might be on the nightstand.
- (4) # Tomorrow (I'll look in my bedroom because) it will be possible that the keys are on the nightstand.
- (5) # Tomorrow I will search every drawer in my room which might contain the keys.

These sentences should all have readings where the tense scopes over the modal. Nevertheless, they sound marked. Summing up:

Observation 1. Whenever a speaker knows that ϕ , they cannot assert '*FUT* might $\neg\phi$ ', even if they know they will no longer know ϕ at some later time.⁴

Now continue the case to see what Alice's less informed, future self can say:

Keys *contd.* Alice wakes up on Tuesday morning and has indeed forgotten where the keys are. She goes looking for them upstairs.

⁴Notice that this markedness also projects under supposition. Imagine that, rather than finding herself in it, Alice simply imagines the scenario above. The following is then true:

- (i) Alice supposes that she knows the keys are not in her bedroom, but that she will forget this.

Can she *suppose* that it will in the future be possible that they are somewhere else? It seems she cannot. Consider:

- (ii) Alice supposes that she will look for the keys in the bedroom because they might be on the nightstand.

Here Alice sounds as if she is supposing something that contradicts her initial supposition.

Imagine Alice says

- (6) The keys might be on the nightstand.

What she says is perfectly assertable for her; there's no obvious mistake Alice makes in uttering this sentence. Imagine yourself in her situation: if asked where the keys are, you would probably reply with something like (6). This pattern also holds generally in cases of predictable forgetting:

Observation 2. At the later time when the speaker has forgotten ϕ , she may assert 'might $\neg\phi$ '.

Finally, let us consider what Alice can say about her past self. We see that, unlike future 'might'-claims, past 'might'-claims are not blocked when speakers talk about times when they had less knowledge.⁵ Let's continue the case:

Keys contd. On Monday night Alice's partner asks her what she was doing rummaging around in their bedroom the previous morning. Alice was in fact looking for the keys, which her partner had placed in the living room.

Imagine Alice replies

- (7) Yesterday I was searching our bedroom because the keys might have been in there. (It turned out they were in the living room.)
- (8) Yesterday I was searching every drawer that the keys might have been in. (It turned out they were in the living room.)

Strikingly, this is perfectly natural, despite the fact that Alice currently knows that the keys were not in the bedroom. We observe that:

Observation 3. A speaker who currently knows that ϕ *can* assert 'PAST might $\neg\phi$ ', if they know they previously did not know ϕ .

⁵von Stechow and Gillies (2008) notes this feature of past-tensed epistemic modals, but not the contrast with the future.

Although present knowledge interferes with our assessment of future ‘might’-claims, it does not interfere in the same way with our assessment of past ‘might’-claims.

There are two surprises here. The first is the combination of Observations 1 and 2. Why in cases of predictable forgetting is there a difference between what in the future I *will* be able to say about myself using an epistemic modal and what I can *now* say about future self? We noted that, when uttered at a later time, the bare modal sentence sounds true.⁶ But generally if a speaker can see that ϕ will be assertable for her without error at some time t , then ‘*FUT* ϕ ’ should be assertable without error earlier than t . For example, if I can foresee that it will be perfectly correct for me to say tomorrow ‘It is raining in Topeka’ then it should be correct for me *now* to say ‘It will rain in Topeka’. Why does this pattern not hold here?

The second surprise is the combination of Observations 1 and 3. They show that there is some asymmetry between past- and future-tensed epistemic modals. Present information that ϕ interferes with our assessment of ‘*FUT* might $\neg\phi$ ’ but not ‘*PAST* might $\neg\phi$ ’. This is also strange: why does the direction in time matter?

1.3 What Knowledge Predicts

We will now make precise why predictable forgetting is a challenge for **Knowledge**.

Let us first restate **Knowledge** more precisely:

Knowledge: $\llbracket \text{might } \phi \rrbracket^{c,w,t} = 1$ iff ϕ is compatible with what the relevant agent(s) in c know in w at t .

To get predictions out of **Knowledge**, we will make some general assumptions about the semantics for modals and tense.

We will assume a roughly Kratzerian treatment of modal vocabulary, according to which a modal is a quantifier over a set of possible worlds; this set is the *domain of quantification* of the modal. We construct the domain with a *modal base*, f_c , a

⁶For those who might have eavesdropper type worries, we can at least note there is a marked difference in acceptability between sentences like (3) and (4) on the one hand and like (6) on the other.

contextually supplied function from a world and a time to a set of propositions. The intersection of such a set is a set of worlds and will be the domain of quantification of the modal. More formally, we have:⁷

- (9) $\llbracket \text{might } \phi \rrbracket^{c,w,t} = 1$ iff $\exists w' \in \cap f_c(w,t) : \llbracket \phi \rrbracket^{c,w',t} = 1$
('might ϕ ' is true at $\langle c, w, t \rangle$ iff all worlds in the domain at w, t are ones where ϕ is true.)

Throughout we will work with a very simple view of tense, where tense simply to shifts the time of evaluation:

- (10) $\llbracket \text{PAST } \phi \rrbracket^{c,w,t} = 1$ iff $\exists t' : t' < t \wedge \llbracket \phi \rrbracket^{c,w,t'} = 1$

- (11) $\llbracket \text{FUT } \phi \rrbracket^{c,w,t} = 1$ iff $\exists t' : t < t' \wedge \llbracket \phi \rrbracket^{c,w,t'} = 1$

where $t < t'$ just in case t is earlier than t' . Simply put, past tense shifts the time of evaluation of the embedded clause to the past and future tense shifts it to the future.⁸

Combining our semantics for 'might' and future tense gives us the following schematic entry for future 'might'-claims:

- (12) $\llbracket \text{FUT might } \phi \rrbracket^{c,w,t} = 1$ iff $\exists t' : t < t' : \exists w' \in \cap f_c(w,t') : \llbracket \phi \rrbracket^{c,w',t'} = 1$

That is, 'FUT might ϕ ' is true at a time t just in case there is a ϕ -world in the modal base as evaluated at some point later than t . Similarly, for past 'might'-claims we have:

- (13) $\llbracket \text{PAST might } \phi \rrbracket^{c,w,t} = 1$ iff $\exists t' : t' < t : \exists w' \in \cap f_c(w',t') : \llbracket \phi \rrbracket^{c,w',t'} = 1$.

'PAST might ϕ ' is true at a time t just in case there is a ϕ -world in the modal base as evaluated at some point earlier than t .

Knowledge tells us precisely what worlds will be in the domain at a given time

⁷This formulation is essentially that of von Stechow and Gillies (2008). That theory in turn is an adaptation of Kratzer (1977), Kratzer (1981) and Kratzer (1991) which adds a time-parameter to the modal base.

⁸Note that all the points I make here go through on a referential theory of tense along the lines of Partee (1973) or Abusch (1997). Nor does does my argument rely on thinking of 'will' as a tense rather than a future-oriented modal, as Enc (1985), Abusch (1997) and Cariani and Santorio (2018), among others, take it to be. (See Ogihara (2007) for the specifics of these debates.)

t . It says that there is some agent(s) such that the modal base takes as input a world and a time and delivers a set of propositions known by that agent in that world and at that time. We will assume that the relevant agent is the speaker. (Our problem will be shared by more complex views formulated in response to the problem of disagreement.) That is, the modal base, f_c , represents the knowledge of the speaker in c : when given a world w and a time t , it outputs the set of propositions known by the speaker in w at t . Given (12), this means that 'FUT might ϕ ' will be true at a time t just in case there is some later time t' at which the speaker's knowledge will not entail ϕ . Similarly, given (7) 'PAST might ϕ ' will be true at t just in case there is some earlier time at which speaker's knowledge did not entail ϕ .

It should now be clear that this semantics fails to predict Observation 1. We know Alice will forget the location of the keys; nor will she know anything which entails where they are. So when (3) – (5) are read as future 'might'-claims, they are predicted to be straightforwardly true. Moreover, *Alice* knows this fact and so she too should be in a position to tell that (3) – (5) are all true. Far from predicting that these sentences are marked, **Knowledge** predicts that they should be sensible things for Alice to say. They are not.

I have focused on solipsistic contextualism, a theory thoroughly undermined by the problem of disagreement.⁹ What about more sophisticated forms of contextualism, like those in von Fintel and Gillies (2011) and Dowell (2011)? The added sophistication does not help.

For instance, take the proposal from Dowell (2011): on this view the speaker can select whatever modal base she likes, so long as she can reasonably expect her audience to figure out what she has in mind. But in predictable forgetting cases, it would be reasonable to take the relevant knowledge to be the speaker's: that modal base would make the relevant examples true and relevant, since, on that interpretation, the speaker would be explaining her future actions by appeal to her future ignorance. But this clearly cannot be what is going on here, for our example sentences were unassertable in the contexts we sketched.

The problem here is more general and extends to other sophisticated contextualisms. *Adding* extra readings of our examples sentences won't solve the problem

⁹See in particular MacFarlane (2011).

if the speaker reading is still available. To predict Observation 1, we need to *rule out* a certain reading. No sophisticated account explains why it would be ruled out.

One might wonder whether the problem is that **Knowledge** is a *contextualist* thesis.¹⁰ As we stated it, we took ‘relevant’ to mean ‘relevant in the context of utterance’. What about relativist versions of **Knowledge**? These add an extra parameter to the index, a context of assessment parameter and it is the assessment parameter that supplies the relevant knowledge. Where f_a is the set of propositions known by the *assessor* in w at t :¹¹

$$(14) \quad \llbracket \text{might } \phi \rrbracket^{c,w,t,a} = 1 \text{ iff } \exists w' \in \cap f_a(w, t) : \llbracket \phi \rrbracket^{c,w',t,a} = 1$$

In other words, ‘might ϕ ’ is true at $\langle c, w, t, a \rangle$ just in case what the assessor in a knows at t is consistent with ϕ . Notice that relativism too encodes a thesis like **Knowledge**: here the modal base tracks the knowledge of a certain agent, this time the assessor.

To see what relativism predicts about ‘*FUT* might ϕ ’ in **Keys** we need to consult what *we the assessors* will know the next day. To the relativist’s credit, it does seem plausible here that in this scenario we expect that our own knowledge will persist through until Tuesday. If that is right, then we predict our ‘*FUT* might ϕ ’ examples are all false: *we* won’t forget that $\neg\phi$ and so ‘might ϕ ’ will be false when fed times from Tuesday.

However, for the same reason, the relativist predicts that, in all our examples, ‘might ϕ ’ is false as said on Tuesday: again, we still know that the keys are in the kitchen when we assess what Alice says on Tuesday. So, the relativist predicts we should find ‘might ϕ ’ as said on Tuesday to sound just as bad as ‘*FUT* might ϕ ’ does when said on Monday. The relativist faces a similar dilemma to contextualism: it cannot predict both Observation 1 and 2. The contextualist predicts Observation 2 but not Observation 1; the relativist predicts Observation 1 but not Observation 2.

The shape of the problem is becoming clear. Even if **Knowledge** can be altered

¹⁰It is also a static, rather than a dynamic thesis. But again, this is not the source of the problem. Willer (2013)’s system, the most worked out dynamic theory of how ‘might’s and tense interact to date, faces this problem too.

¹¹This is the semantics given in MacFarlane (2011) with a time parameter added. The objections that follow apply just as well to Stephenson (2007) and Egan (2007).

in some way to predict Observation 1, it is not at all clear how it could predict *both* of Observations 1 and 2. It is hardwired into **Knowledge** that the truth of what she says on Tuesday is sufficient for the truth of her future-‘might’ claims on Monday. If what Alice says the next day is true, then the keys being on the nightstand *will* be compatible with what the relevant agents know on Tuesday. But then what Alice says on Monday is true after all. Predicting Observation 2 seems to come at the cost of failing to predict Observation 1.

Likewise, **Knowledge** will have a hard time predicting Observation 1 and 3 together. According to **Knowledge**, epistemic modals simply track the knowledge of the relevant agent. For this reason, it correctly predicts that Alice can make past-tensed modal claims like (7). But for the very same reason, it fails to predict Observation 1. Since, according to **Knowledge**, modals simply track the evolution of some body of information, present information should be irrelevant in both the past *and* the future.

1.4 Tensing Modals

To get the puzzle going, we needed to assume that tense *can* scope over epistemic modals. But this assumption is sometimes rejected.¹² In this section, I show the assumption is correct: there are perfectly felicitous, genuine examples of past- and future-shifted epistemic modals.

1.4.1 Evidence of Shifting

We have already seen evidence that epistemic modals can have *backwards*-shifted readings. Recall the example from von Stechow and Gillies (2008): suppose I discover that the keys are not in the drawer. You ask why I looked there. I say

(15) The keys might have been in the drawer.

Plainly I am not saying that for all I know *now*, the keys were in the drawer. Rather I am saying that, as far as I knew when I looked, the keys were in the drawer. But

¹²As, for example, in Groenendijk and Stokhof (1975) and Iatridou (1990) and Hacquard (2006).

the sentence can only get this meaning if the past tense scopes over the modal.¹³

We can also find unproblematic forward-shifted readings. For instance, take the following case:^{14,15}

Shortlist. The department has made a hire and you and I are trying to figure out who got the job. We don't know very much about the candidates might be, but we know we will see the shortlist tomorrow. We will contact everyone on the shortlist to see if they got the job.

You say to me:

- (16) Let's drop this matter for now. Tomorrow we will contact everyone who might have been hired.
- (17) Let's drop this matter for now. Tomorrow we will contact every possible hire.

Each of these sentences talks not about who might have been hired given our current evidence, but rather everyone who might have been hired, given our future evidence after we have talked to the head of the department. To bring this out, suppose today we think John might have been hired, but tomorrow we find out that he wasn't. If we didn't contact John, it would be bizarre for me to claim that you are not following up on what you said in (16) or (17). This is because you are talking about your future and not your present evidence.

These data, particularly the forward-shifting data, make an important point: we cannot explain the badness of our future 'might'-sentences (3) – (5) by denying that forward-shifting is possible. This makes our situation much more puzzling. If future shifted readings exist, then why are they not available in **Keys**?

¹³If this reading is hard to hear, one way to bring it out is to compare it to a case where in a fit of madness I look for the keys in the fridge. Were I to say

- (i) The keys might have been in the fridge.

what I say is clearly false. Moreover it clearly contrasts with (15) above.

¹⁴Dorr and Hawthorne (2013) discuss a similar case.

¹⁵These sentences are also evidence against the claim in Hacquard (2011) that apparent tense shifting is always due to a tacit 'because': there's no obvious place to put the 'because' here that will get us the right reading.

1.4.2 Explaining away shifting

There is good *prima facie* evidence for shifted readings. But maybe we should say that that is all it is — *prima facie* evidence. After all, if we could explain away these examples, this would solve our puzzle. Our predictable forgetting data would be explained by the fact that trying to embed epistemic modals under tense simply is not possible.

A natural thought is that the examples from the last section really involve an elided attitude ascription. When we hear

(18) The keys might have been in the drawer.

what we are really evaluating is

(19) I thought that the keys might have been in the drawer.

When we hear

(20) We will interview everyone who might have gotten the job.

we are really evaluating

(21) We will interview everyone who we will think might have gotten the job.

This can make all the difference. If an elided attitude verb is present, then it is no longer clear that shifting should be attributed to tense. Instead, we might say, as Abusch (1997) and Hacquard (2006) do, that the modal must be evaluated at the same time as the embedding attitude verb: when the attitude is evaluated in the past or future, the modal too would have to be evaluated in the past or future. If this is the case, then we could deny that tense ever directly shifts an epistemic modal. The closest we can get to past or future epistemic possibility would be our past and future *beliefs* about it.

This supposes that *all* uses of shifted modals can be glossed as sentences involving an attitude. But this is in fact not the case. First note that when we report attitudes towards non-modal propositions, we can go on to *endorse* the attitude we had. For instance we can say

(22) I thought that Roger was in the kitchen. And in fact he *was* in the kitchen.

It is quite clear that the second sentence should not be thought of as involving an elided attitude verb. The sentence

(23) #I thought that Roger was in the kitchen. And in fact I thought he *was* in the kitchen.

is highly marked. This is what we would expect: the second sentence adds nothing to the first, despite what the use of ‘in fact’ would suggest. Adding an extra attitude, elided or not, makes the second sentence redundant. So we should not expect elided attitudes in discourses like (22).

Now note that we can also endorse our attitudes to shifted modal propositions. Consider the stark difference in the following minimal pair:

- (24) a. I thought the keys might have been in the drawer and I was right: they might have been.
b. I thought the keys might have been in the drawer and I was right: # I thought they might have been.

In the last clause of (1), we are heard as endorsing a backward shifted reading. But as we showed, in this context, it is *not* plausible that the backward-shifting can be attributed to an elided attitude. Just as in non-modal contexts, (24-b) shows that adding an extra attitude renders the sentence marked. This is because adding it makes the sentence redundant: in (24-b) the speaker is heard as repeating themselves. Our elision strategy is then insufficiently general: it cannot explain shifting in all the places it appears. By contrast, the more natural view, that tense does the shifting, has no difficulty here.

But not only is the elision approach insufficiently general, it also *overgenerates* in other areas. That is, it generates felicitous shifted readings in places we do *not* want them. To see this we need only look back to **Keys**. We noted that Alice cannot say

- (3) #Tomorrow I’ll look in my bedroom because the keys might be on the night-stand.

Now observe that in **Keys** it is perfectly fine for Alice to say:

(25) Tomorrow I will think the keys might be on the nightstand.

If we are countenancing elided attitudes, then the contrast here is puzzling. Why is there no elided attitude in (3)? After all, if it were interpreted this way, it would not be marked, but perfectly intelligible.

The absence of this reading casts doubt on the elision strategy. There is no obvious reason for elision to apply in the cases we want it to and not in the cases we do not. In both kinds of cases, speakers are heard as trying to explain past or future actions in terms of what they know at the time; but in only one of them are they successful. The elision strategy gives no insight as to why this should be.

Why then have theorists like Hacquard been so eager to rule out tense shifting? In Hacquard (2006) and Hacquard (2011) it is noted that certain sentences seem to lack the expected tensed readings. Take:

(26) Mary must have taken the train.

(27) Tomorrow Marikos might be dead.

Out of the blue, the only epistemic reading of (26) is one which talks about our *present* knowledge; and it is hard to get any epistemic reading of (27). Likewise it is hard to get epistemic readings when modals are embedded under temporal adverbials:

(28) #Usually Mary might take the train.

Hacquard's thought here is the absence of such readings is readily explained if we predict that tense simply *never* shifts the relevant knowledge.

There are a few things to say here. First of all, the kind of data that Hacquard and others cite seem to be restricted to the case of the modal auxiliaries 'might' and 'must'. When it comes to examples like

(29) It wasn't possible that Mary took the train.

(30) Tomorrow it will still be possible that Marikos is dead.

it is easier to hear shifted epistemic readings. It is also easier to hear shifted epistemic readings for epistemic adjectives:

(31) John was never a possible hire.

(32) Tomorrow John will still be a possible hire.

This suggests that the conclusion that tense *never* scopes above epistemic modals is too quick: even if it holds of the modal auxiliaries, it may not hold for other expressions we use to express epistemic possibility. This is important because the puzzle about predictable forgetting is not just about the modal auxiliaries: (4), for example, is marked but does not use an auxiliary.

Secondly, a pragmatic explanation seems possible here. If aspects of the syntax and semantics of *modals* are supposed to explain the data, then we should not expect to see similar data for knowledge attributions. But note that even tensed knowledge attributions can sound odd out of the blue. Consider for instance:

(33) Q. How did Mary leave town?
a. [A.] ?? I knew she took the train.

(34) ?? Tomorrow I won't know whether Marikos is dead.¹⁶

Both of these sentences can sound odd out of the blue.¹⁷ We notice the same pattern with temporal adverbials:

(35) ??Usually I don't know whether Mary will take the train.

There is a natural explanation of these data: out of the blue, it is not obvious why knowledge states other than our current ones would be *relevant*. When we are wondering how Mary left town or whether Marikos is dead, why would we care about what I knew in the past or will know in the future about these matters?

This invites a pragmatic explanation of Hacquard's data. Whenever we choose between different possible readings of (26) and (27) we search for readings that

¹⁶Though, like (30), this improves if we insert 'still'.

¹⁷I grant that, without the prior question, (33-a) sounds better out of the blue than (34). But it also seems that, without the question, it is easier to quickly come up with a context in which (33-a) is relevant.

are not just true but also *relevant*. Since the missing readings are not relevant, we do not select them. This predicts that when context makes it clear *why* other knowledge states would be relevant those shifted readings should appear. But this is exactly what we have observed: our *prima facie* cases of shifting are precisely cases that make clear why other knowledge states would be relevant.

We tried to explain our original observations by placing the blame on tense. But this does not appear to be the right approach. At least some of the time, tense *appears* to embed epistemic modals in the way we would expect. And, on further consideration, the most straightforward explanation of this fact seems to be the right one: it really is tense that does the shifting.

1.5 An Interval-Based Solution

I will now outline a semantic solution to our puzzle. In this section, I spell out an *interval*-based approach to epistemic modal bases, on which modal bases track the knowledge accumulated within a certain interval and not just at a time, and show how such an account can predict our data.

1.5.1 An overview

Throughout we will keep the schematic Kratzerian semantics from earlier. That is, we have

$$(9) \quad \llbracket \text{might } \phi \rrbracket^{c,w,t} = 1 \text{ iff } \exists w' \in \cap f_c(w, t) : \llbracket \phi \rrbracket^{c,w't} = 1$$

Our task here is to find a kind of modal base which would predict our three observations.

Observations 1 and 3 show that we want to predict an asymmetry between epistemic modals under the future and the past. We can do this if we allow future modal bases to contain certain propositions known at *past* times but not vice versa.

Here is an alternative to **Knowledge** which would predict this:

History. ‘might ϕ ’ is true at a world w and time t iff ϕ is compatible with what has been learned in w in the relevant interval before and up to t .

Like **Knowledge**, **History** amounts to a theory about the output of the modal base. Instead of simply collecting together the propositions an agent knows at a given time, epistemic modal bases will collect both the propositions known by the agent at that time and also propositions known at times within a certain interval before the input time. More generally, it is hardwired into epistemic modals that they collect the information accumulated over time, regardless of whether the relevant agents continue to possess that knowledge. Thus, while epistemic modals still have something to do with the knowledge of the relevant agents, they can also come apart from that knowledge in interesting ways.

This delivers the asymmetry we want. If an agent now knows ϕ , ϕ will remain in the modal base, even as we look at times in the future. More generally, modal bases only ever grow as we move forward in time. But information only has to be carried forward and so we are free to predict the asymmetry resulting from Observations 1 and 3.

To predict Observation 2, we must ensure that ϕ does not remain in the modal base when the less informed future agent asserts ‘might ϕ ’. We predict this by positing a certain kind of context-shifting. We will say that epistemic modals collect the information in certain privileged intervals of time, ones where no information loss occurs.

1.5.2 The semantics

History says that what is possible depends on what has been learned in the relevant interval. Accordingly, we need to say something about what the relevant interval is.

Rather than looking at an agent’s entire history, our modal bases will look at intervals that start with the most recent episode of information loss. Given a time t , define \mathcal{I}_t , which we’ll call a *partial history*, as follows:

$$(36) \quad \mathcal{I}_t = \{t' : \text{either } t < t' \text{ or } t' < t \text{ and } t' \text{ is not earlier than the most recent episode of information loss to relative to } t.\}$$

Intuitively, to construct a partial history for a given t , we take an agent’s entire history and remove the part before the most recent (relative to t) episode of infor-

mation loss.

We then define our modal bases relative to partial histories. Where \mathcal{I}_{t_c} is the partial history corresponding to the time of the context c :

(37) **(Partial History)**

$$f_c(w, t) = \{p: \text{the relevant agent knows } p \text{ at some } t' \text{ where } t' \leq t \text{ and } t' \in \mathcal{I}_{t_c}\}$$

(The output of the modal base at w and t is the set of propositions known in w at any point in \mathcal{I}_{t_c} up to t)

When fed a time t (and world w), an epistemic modal base will deliver the set of propositions known at times up to t (in w) in the partial history \mathcal{I}_{t_c} . In other words, modal bases simply collect the facts known within a given partial history up until t . To find the domain of quantification $\cap f_c(w, t)$ we find all the propositions known at any time in \mathcal{I}_{t_c} before t and intersect that set of propositions.

Note two important features of this account. Firstly, information gets carried forward: when t' is later than t , if p is in the modal base at t then it must also be in the modal base at t' . (In formal terms, these epistemic modal bases are *monotonic*: when $t < t'$, $f(w, t) \subseteq f(w, t')$.) This is because modal bases collect not just what is known at a given time, but also what is known up until that time (within the relevant partial history). As a result, the set of epistemic possibilities can only get *smaller* as time passes (if it changes at all).

Secondly, moving from one partial history to another introduces a context-shift. Whenever information is lost, the relevant partial history changes; and since the modal bases are anchored to partial histories, the modal base too will change. This feature in fact makes a good deal of sense on a **History**-based picture. Were the context not to shift, information would continue to accumulate in the modal base across contexts of utterance, despite the fact that the relevant agents do not have access to that information. This is undesirable: the more time passes, the fewer epistemic modal claims an agent would be justified in making. (It predicts I would not, for instance, be justified in telling you that on this day a year ago, I might have eaten cornflakes for breakfast, for I know my past knowledge settles the matter one way or the other!) If we say that modal bases collect what is known from some point closer to the context of utterance, the resulting context-shifting

solves the problem.

Now recall our case:

Keys. On Monday night, Alice leaves her keys in the kitchen. But she always forgets where she has left the keys overnight. In fact, she can foresee that tomorrow morning she will go looking for them in her bedroom.

Alice says

- (3) #Tomorrow I'll look in my bedroom because the keys might be on the nightstand.

Keeping the semantics from earlier, we still have the following truth-conditions for future 'might'-claims:

$$(12) \quad \llbracket \text{FUT might } \phi \rrbracket^{c,w,t} = 1 \text{ iff } \exists t' : t < t' : \exists w' \in \cap f_c(w, t') : \llbracket \phi \rrbracket^{c,w',t'} = 1.$$

Let f_{Monday} be the modal base supplied by Monday's context of utterance. To evaluate (3), we need to see whether $\cap f_{Monday}(w, t_{Tuesday})$ contains worlds where the keys are on the nightstand.

Partial History tells us that $f_{Monday}(w, t_{Tuesday})$ contains all propositions known by Alice at any time in *Monday's* partial history up until Tuesday. Since she knows on Monday that the keys are in the kitchen, $f_{Monday}(w, t_{Tuesday})$ will contain the proposition that the keys are in the kitchen. This means that, in Monday's context, $\cap f_{Monday}(w, t_{Tuesday})$, the domain for the modal evaluated at Tuesday will *not* contain worlds where the keys are on the nightstand. So when Alice says (3), it is false. It will not be possible tomorrow that the keys are on the nightstand because they are *now* known not to be. Moreover, Alice should *know* it is false: her semantic knowledge and knowledge of the case are enough to guarantee this. Hence (3) is marked. This explanation extends to all of our marked future 'might'-sentences and so explains Observation 1.¹⁸

We also rightly predict that Alice's knowledge only gets carried *forward*. Consider again the continuation of the case:

¹⁸Moreover, since more generally there is no point of evaluation at which 'must $\phi \wedge \text{FUT}(\text{might } \phi)$ ' is true, we also correctly predict the embedding data from footnote 5.

Keys contd. On Monday night Alice’s partner asks her what she was doing rummaging around in their bedroom the previous morning. Alice was in fact looking for the keys, which her partner had placed in the living room.

Alice says

(7) Yesterday I looked in our bedroom because the keys might have been there.

Again, we give past ‘might’-claims the following semantics:

(13) $\llbracket \text{PAST might } \phi \rrbracket^{c,w,t} = 1$ iff $\exists t' : t' < t : \exists w' \in \cap f_c(w, t') : \llbracket \phi \rrbracket^{c,w',t'} = 1$.

To see whether (7) is true, we need to see whether for some time t on Sunday $\cap f_{Monday}(w, t_{Sunday})$ contains worlds where the keys are in the bedroom.

Partial History predicts that it will. $f_{Monday}(w, t_{Sunday})$ will contain just the propositions known by Alice up until Sunday (within Monday’s partial history). Until the point at which she found them, Alice did not know where the keys were. So $\cap f_{Monday}(w, t_{Sunday})$ will contain worlds where the keys were in the bedroom. Hence (7) is true. This explanation extends to predict Observation 3 more generally.

Notice that **Partial History** predicts the asymmetry brought out by Observations 1 and 3 precisely because it is monotonic: this ensures that past information will always get carried forward, making our future ‘might’-claims false; but future information is not carried backwards, allowing our past ‘might’-claims to be true. (In fact, this feature will be shared by any **History**-based view.)

Now we want to predict the contrast between what Alice can say on Monday and what she can say on Tuesday. Consider the continuation of the case:

Keys contd. Alice wakes up on Tuesday morning and has indeed forgotten where the keys are. She goes looking for them upstairs.

We noted Alice can now say

(6) The keys might be on the nightstand.

We predict this by our appeal to partial histories. Since something is forgot-

ten overnight, we can see that different partial histories correspond to Monday and Tuesday. Given our rule for modal bases, this means f_{Monday} will be different from $f_{Tuesday}$. Moreover, since on Tuesday Alice does not know where the keys are, $f_{Tuesday}(w, t_{Tuesday})$ will not contain the proposition that the keys are in the kitchen; nor will it be entailed by the propositions in the modal base. So $\cap f_{Tuesday}(w, t_{Tuesday})$ will contain worlds where the keys are on the nightstand. Given our semantics, this predicts that (6) as said by Alice on Tuesday is true and so is assertable. In fact, we predict Observation 2 more generally.

Importantly, the introduction of partial histories means that modal bases are not monotonic *across* contexts, not even across contexts involving the same agents later in time. This combination of monotonicity within a context and non-monotonicity across contexts is exactly what allows us to predict the contrast between what Alice can say on Monday and on Tuesday, while at the same time predicting the asymmetry between past and future ‘might’-claims.

We have seen a new framework for epistemic modals, **History**, that says epistemic modals collect together what agents know within certain privileged intervals, *partial histories*. Moving to intervals predicts the striking asymmetry displayed by predictable forgetting cases. Forgotten information gets carried forward to future modal bases, predicting Observation 1, but not backwards to past modal bases, predicting Observation 3.

I have also suggested that epistemic modals are context-sensitive in a new way. Information loss changes the relevant interval for an epistemic modal and so changes the domain. As a result, information does not necessarily carry over *across* contexts, even as we move forward in time. This lets us predict the striking combination of Observations 1 and 2: the corresponding ‘might’-claim is felicitous in the future because it talks about a different interval to the infelicitous future ‘might’-claim.

1.6 Conflicting Data

We’ve now seen how the theory predicts our three observations. Now it is time to think about some data that seems to conflict with it.

1.6.1 Monotonicity violations in the past?

The account I have suggested is fully monotonic: modal bases are not merely monotonic from the context time onwards, but monotonic from *any* point in time onwards. An important test case for this monotonicity property is one where the agent can remember that she has forgotten some piece of information but cannot remember precisely what that information was.

Suppose that Alice has woken up and remembers that she either knew the keys were in the living room or that they were in the kitchen. She says

- (38) a. Either the keys had to be in the kitchen or the keys had to be in the living room.
b. But now they could be in either place.

This speech sounds true and so poses a *prima facie* problem for proposals aiming at full monotonicity. If (38-a) is true, then (38-b) should be false. After all, on fully monotonic proposals, what *was* necessary remains necessary. On the semantics I have given, the modal base for (38-a) should in fact be empty: since the relevant time yesterday falls outside the partial history, no proposition known then will be in the modal base. This means that the domain of the modal will in fact include *all* possible worlds. Hence, each disjunct of (38-a) is false: there are worlds where the keys are not in the kitchen and worlds where the keys are not in the living room.

However, with a slight modification of our semantics, we can give a natural pragmatic explanation of what is going on here. First, define our modal bases as follows: where \mathcal{I} is a partial history

$$f_{\mathcal{I}}(w, t) = \begin{cases} \{p: \text{there's a time } t' \text{ not later than } t \text{ s.t.} & \text{if } t \text{ is in or later than } \mathcal{I}; \\ t' \in \mathcal{I} \text{ and } p \text{ is known at } t'\}, & \\ \text{undefined,} & \text{otherwise.} \end{cases}$$

This semantics predicts that the domain of quantification for the modals in (38-a) will be undefined because the relevant time will be before the relevant partial history. This means that (38-a) will itself be undefined.

Generally, when the default interpretation of a modal or quantifier phrase would be undefined, we attempt to save the utterance: we look for other candidate interpretations on which the sentence would be defined.¹⁹ When accounting for this, we predict that when assessing (38-a), we search for some salient modal base other than the default to save (38-a). And there is indeed such a modal base to be found. The speaker remembers that on Monday she knew where the keys were and so should be in a position to tell that the modal base from *Monday's* context of utterance, $f_{Monday}(w, t_{Monday})$, either contains the proposition that the keys are in the kitchen or that entails the keys are in the living room. I suggest then that in order to make the sentence interpretable we interpret the modal in (38-a) as quantifying over $\cap f_{Monday}(w, t_{Monday})$. This predicts that (38-a) is true. We then revert to the default in interpreting (38-b).

There is an asymmetry here: the modal base is undefined when fed times before the relevant interval but not times after it. Is the asymmetry well-motivated? In fact, I think it is. We can think of the modal base as a kind of Fregean definite description: it picks out the propositions known within \mathcal{I} by t . When t falls before \mathcal{I} , the corresponding definite description would suffer from presupposition failure: there simply are no propositions known within \mathcal{I} by t . On the other hand, it will be defined when t falls after \mathcal{I} : the propositions known within \mathcal{I} by t are just all the propositions ever known in \mathcal{I} by t . Seen from this perspective, the asymmetry in fact is quite intuitive.

I have taken pains to explain away this kind of example; so why not instead opt for a *partially* monotonic solution, one where modal bases are only monotonic from the context-time onwards? A solution with this monotonicity property could predict (38-a) and (38-b) to be true in the one context.

I find it hard to see what kind of modal base would generate this kind of monotonicity in a principled way. It is particularly hard to see why the context time should be privileged. By contrast, my account gives a principled explanation of the full monotonicity principle I appeal to: it follows from the fact that epistemic modals collect the information in a particular interval. The challenge for a partially monotonic account would be to give a similar principled account of where its

¹⁹For example, von Stechow (2001) appeals to this kind of mechanism to explain the felicity of Sobel sequences.

monotonicity properties come from.

Our way of implementing full monotonicity together with attested pragmatic effects gives a good explanation of what is happening in such cases, even though they might have appeared to be counterexamples to full monotonicity. Moreover, the full package of my view plus a pragmatic explanation of these cases offers, I think, a better story overall than any obvious proposal that makes modal bases only forward monotonic.

1.6.2 Question-sensitivity

One might worry that this proposal predicts too much sensitivity to forgetting. For example, we do not want the fact that Alice forgot what she ate for breakfast on Sunday to interfere with the truth of a past-shifted sentence like (7). As it stands, however, it looks like this is exactly what we predict: since Alice has forgotten something in the meantime, the modal base for Sunday will simply be empty.

On an intuitive level, the problem is that the proposal is insensitive to the fact that what Alice had for breakfast on Sunday is *irrelevant* in the present context. To fix it we will let the context-shifting be generated not by just any kind of forgetting but forgetting of relevant propositions.

To cash this out, we need to say more about what relevance might be. To do this, we will move from tracking what is known in particular intervals to what is known in particular inquiries. We represent inquiries with an ordered pair $\langle I, Q \rangle$, where I is some interval and Q is a question. Following the usual approach to the semantics of questions, we let questions be sets of their answers. Put a different way, questions are partitions of the set of worlds: a question will be a set of sets of possible worlds the union of which is the set of all worlds.²⁰

We'll now say that a pair $\langle I, Q \rangle$ is a partial inquiry at t just in case I begins right after the closest past forgetting (or defeat) to t of some proposition in Q . This encodes our original idea: the relevant propositions are the ones which provide a partial answer to the question.

Finally, we need to say a bit more about how context picks out a partial inquiry.

²⁰See Hamblin (1973), Karttunen (1977) and Groenendijk and Stokhof (1985) for various specific versions of this proposal.

Before, we had a one-to-one relationship between times and contexts: for each context we had exactly one maximal ideal inquiry segment. This is no longer the case: given that inquiries are now individuated partly by a question, we will have many different maximal inquiry segments which include a given context. However, it is relatively easy to settle which inquiry segment should be selected. It is widely accepted that in a given context there will be a *question under discussion*, a question which the parties in the conversation are attempting to answer at that moment.²¹ This will determine what modal base is selected for epistemic modals. We'll say that f_c is the f_I such that $t_c \in I_I$ and where Q_I is the question under discussion in c .

Let's put this together to see how this handles the problem we started with. We noted that forgetting irrelevant information, like what she had for breakfast on Sunday, shouldn't block Alice from uttering (7). Now, in this context, the question is something like *Where were the keys?* and the elements of the question will be propositions like *The keys were in the living room*, *The keys were in the bedroom* and so on. Clearly the proposition that Alice had corn flakes for breakfast on Sunday is not the union of any elements of the question under discussion. Hence, the partial inquiry here will in fact include the interval across which Alice forgets this fact. As a result, the past tense here will be able to shift the value of the modal base back to a time that is still in the relevant partial inquiry, a time where Alice has not determined where the keys are. This allows us to predict that (7) is true.

1.7 A New Picture

We now have an extensionally adequate solution to our problem. I want to close by considering some bigger picture upshots **History** has for the study of modality more generally.

We have been concerned with epistemic modality, but there is another much-discussed kind of modality, *historical* (also called *circumstantial*) modality. Consider the sentence

(39) It hasn't been decided yet who will get the job. Alex might get it and so

²¹See Roberts (1996) for the canonical discussion of this idea.

might Billy.

There is a reading of this sentence where the ‘might’-claim talks about what is possible *given the relevant facts*.²² Rather than depending on what we know, the truth of this kind of modal statement depends on what holds in the world more generally. This is a historical reading of the modal.

It is often thought that there must be a sharp distinction between epistemic and historical modality.²³ One upshot of my discussion, I want to suggest, is that there is an important *temporal* respect in which the two are closer together than has been appreciated.

While both readings involve the semantics in (9), epistemic and historical modality involve different kinds of modal base. Historical modals get a modal base like the following:²⁴

$$(40) \quad f(w, t) = \{p : p \text{ is a proposition about an interval that does not extend past } t \text{ and } p \text{ is true at } w.\}$$

Notice that, as stated in (40), historical modality will be monotonic. This is no accident. Among other things,²⁵ this captures a very natural asymmetry in how we think about the past: once something becomes settled it remains settled; but what lies in the future can remain open. For example, suppose that it rains at time t . Before t , we will want to say that it is historically possible that it will not rain at t . But once we get to t and it in fact does rain it will no longer be historically possible for it to have not rained at t . So when t is in the future, it is possible that it will not rain at t , but once t is in the past it is no longer possible.

We have in effect seen that epistemic modals seem to display the same kind of behaviour. When something becomes settled by becoming epistemically necessary, it remains settled as we go forward in time. Or equivalently, when something is

²²See Condoravdi (2002) for further discussion of this example.

²³In linguistics, a classic expression of this view is in Kratzer (1991). In philosophy it has recently been reiterated by Williamson (2016). But, perhaps most importantly, such a distinction seems implicit in many of the recent *expressivist* theories of epistemic modals, such as Yalcin (2011) and Moss (2018).

²⁴This is Khoo (2015)’s formulation.

²⁵Condoravdi (2002) argues this is also required to predict the absence of certain historical readings.

epistemically open in the future, it must be open *now*. (All the while holding the context fixed, of course.) When compared to theories of historical modality, our epistemic asymmetry in time begins to look like an epistemic analogue of the historically open future.

That this analogue should exist is extremely surprising, particularly given the presumption that there is a sharp distinction here. Why should it turn out that both kinds of modal base are monotonic, when clearly there are potential meanings that are not? Why would the two core flavours of modality be monotonic?

One thought more or less in line with **History** is that epistemic modality is in fact a more circumscribed form of historical modality. ϕ is historically possible in a world at a time t iff ϕ is compatible with the relevant facts in that world that hold up to and including t . We might say that ϕ is epistemically possible at t just in case it is compatible with the known facts in a given partial history up to and including t .

Thus, instead of being a distinct form of modality, epistemic modality would be a subspecies of historical modality, one that limits itself to facts about particular partial histories. Spelled out, instead of **Knowledge**, we would have

Historical: $[[\textit{might } \phi]]^{c,w,t} = 1$ iff ϕ is compatible with the (relevant) known facts in w about what is known in the relevant interval up to t .

Interestingly, this would reduce the asymmetry between past and future in cases of predictable forgetting to the more familiar asymmetry between past and future in historical modals. On a picture based on **Historical**, it will be precisely the monotonicity property which accounts for the difference between past and future in epistemic modals. As we saw, it is precisely the monotonicity properties of historical modals that predict the fixity of the past principle and so capture the asymmetry between past and future. In this way, we can see that a **Historical**-based picture treats this asymmetry as a special case of the usual asymmetry between past and future that we see with historical modals.

Historical gives a clear explanation of why epistemic modals are monotonic. They simply track the facts in the world relevant to this form of historical modality. It reduces the surprising asymmetry we started with to the more familiar asymmetry present in historical modals. Much more would need to be said before **Historical**

could be accepted. But this is reason enough, I think, for **Historical** to warrant further investigation.

1.8 Conclusion

We started with a puzzle for the received picture of epistemic modality. That picture seemed to suggest that epistemic modals under tense should pattern just as knowledge attributions do. We have seen that this prediction is false: future shifted epistemic modals exist but do not pattern like knowledge ascriptions.

To explain the data, I offered a new picture of epistemic modality. Instead of simply tracking some body of information at a time, epistemic modals collect the information gained within a certain interval. Moreover, epistemic modals turn out to be context-sensitive in a surprising way: instances of relevant forgetting can shift the choice of modal base. Finally, on this picture, the difference between epistemic and historical modality is much less pronounced than commonly thought. I argued that this is reason to take seriously a view on which there is no sharp distinction between epistemic and historical modality.

A proposal like **History** can help solve our puzzle; it gives us a general picture which makes sense of why epistemic modals have the monotonicity properties they do; and a close variant gives an explanation for the surprising fact that epistemic and historical modals have the same monotonicity properties. This is no small feat and gives us good reason to take **History** seriously.

Chapter 2

Putting ‘Ought’s Together

Take a sentence like

- (1) John ought to do his homework.

What does this sentence say? One thing it involves is an ordering on possibilities: it communicates that possibilities where John does his homework are better than those in which he does not.

The classic view of ‘ought’ is a way of spelling out this idea. It says that something ought to be true just in case it is true in *all the best* possibilities in some ordering. This semantics has proven very robust and, despite many challenges, remains the view to beat.

I argue the classic semantics faces a serious predicament. A semantics for ‘ought’ needs to be general: it must capture not just *deontic* readings of ‘ought’ like in (1), but also *epistemic* readings of ‘ought’. But there is an important divergence in the inferences deontic and epistemic ‘ought’s license. Deontic but not epistemic ‘ought’s appear to obey the inference pattern Agglomeration. The classic semantics makes no distinctions and validates Agglomeration in full generality.

We are forced, I argue, to depart from the classic semantics. Instead, I give ‘ought’ a weaker quantificational force, where ‘ought ϕ ’ means that *some best proposition* entails ϕ . I give a pragmatic account of the difference between epistemic and deontic orderings to predict the differing entailments of deontic and epistemic ‘ought’s, all the while keeping a unified semantics for ‘ought’. Deontic

'ought's are subject to a pragmatic constraint that prevents Agglomeration failures.

I argue my semantics is superior to contrastivist accounts of these phenomena on two grounds. It gives a better treatment of epistemic 'ought's generally; and it validates Inheritance, which I argue is a desirable feature of the semantics. I close by exploring how moral dilemmas might be approached in my semantics.

2.1 Epistemic 'Ought'

'Ought' and 'should' are perhaps most easily read deontically, but can also be read non-deontically. Suppose that Jane has been told the bus left 30 minutes ago and it usually takes 40 minutes to her nearest bus stop. She might truly say

(2) The bus ought to arrive in 10 minutes.

Here Jane says nothing about whether the bus arriving in 10 minutes is a good thing or not. (We could even suppose it would be better for all parties if it arrived late and this would not affect the truth-value of what she says.) Rather she is making a prediction about when the bus will arrive. Call this an *epistemic* reading of 'ought'.

Whatever this non-deontic 'ought' means, we should expect a unified account of both it and the deontic 'ought'. When we find a single modal word expressing a variety of modal flavours, we try to account for it with context-dependence, saying that the variation in flavour results from different kinds of parameters being supplied by context. This move is particularly compelling when the multiplicity of readings is robust across languages, as it is for the non-deontic 'ought'. We do not want to say that, by chance, the two flavours are expressed by the same word and that this chance event repeats itself across languages.

Going forward, I will call this reading 'epistemic' because I hear it as saying that it is probable, given Jane's evidence, that the bus will arrive in 10 minutes. Some do not accept this characterisation of sentences like (2) and suggest that these apparently epistemic readings are in fact *normality* readings of 'ought'. Yalcin (2016) argues that a sentence like (2) means something like:

(3) Normally the bus would arrive in 10 minutes.

Most of what I say here will be neutral on this question. All we need to grant for now is that there is indeed some *non-deontic* ‘ought’.

All this being said, epistemic and deontic ‘ought’s diverge in what inferences they validate, or so I will argue.

2.2 Agglomeration

Consider this inference pattern:

Agglomeration: $\text{ought } \phi, \text{ought } \psi \models \text{ought } (\phi \wedge \psi)$

Agglomeration is a kind of multipremise closure principle for ‘ought’s: it says that whenever two things ought to be the case, so too is their conjunction. (The principle that corresponds to single premise closure for ‘ought’s is called *Inheritance* and is the focus of section 9.)

I will argue that deontic and epistemic ‘ought’s do not behave alike with respect to Agglomeration. Deontic ‘ought’s seem to obey it, but epistemic ‘ought’s do not.

2.2.1 Deontic Agglomeration

To make the case that deontic Agglomeration is valid, I’ll note how natural it is generally and then respond to some putative counterexamples.¹

Start with a simple example. Suppose that I ought to help Alice and that I ought to help Billy. It is hard to see how I could avoid the conclusion that I ought to help Alice and help Billy. It is just eminently plausible that if there are two things, each of which I ought to do, then I ought to do *both*. And we can keep agglomerating, even as we add further obligations. If I should help Alice, Billy *and* Carol, or Alice, Billy, Carol *and* Daniel, then in each case I ought to help all of them. And so on.

Some nonetheless think there are counterexamples to deontic Agglomeration. Jackson (1985) provides the following:

¹Of course, some think moral dilemmas are counterexamples to Agglomeration. I set these cases aside for further discussion in section 10.

Chariots. Attila and Genghis are driving their chariots towards each other. If neither swerves, there will be a collision; if both swerve, there will be a worse collision . . . but if one swerves and the other does not, there will be no collision. Moreover if one swerves, the other will not because neither wants a collision. Unfortunately, it is also true to an even greater extent that neither wants to be ‘chicken’; as a result what actually happens is that neither swerves and there is a collision.

Here the following sentences are supposed sound true:

- (4) Attila ought to swerve.
- (5) Genghis ought to swerve.

But the following does *not* sound true:

- (6) Attila and Genghis both ought to swerve.

This is contrary to what Agglomeration predicts, or so says Jackson.

The objection presupposes all of these sentences are evaluated in the one context. But are they? Famously, there is a distinction between ‘ought’s that say what particular agents should do, the *ought to do*; and ‘ought’s that say how the world would be if things went best, the *ought to be*. On either way of distinguishing between these different kinds of ‘ought’s, the counterexample fails.

The true readings of the premises seem to involve the ‘ought to do’ sense. (4) is naturally heard as saying

- (7) Attila should make it the case that Attila swerves.

and (5) is naturally heard as saying

- (8) Genghis should make it the case that he swerves.

But importantly, it is not the same *ought to do* in each case. To bring this out notice that both of the following sound *false*:

- (9) Attila ought to make it the case that Genghis swerves.

(10) Genghis ought to make it the case that Attila swerves.

This suggests there is an equivocation in the counterexample, even when we focus just on the *ought to do*. For there can be many *ought to dos* even in a given case. In ours, there is what *Attila* ought to do and what *Genghis* ought to do. But on neither notion are *both* of the premises true.

When we move to an ‘ought to be’, then it is no longer clear that the premises are true anymore. The sentences

(11) It ought to be that Attila swerves.

(12) It ought to be that Genghis swerves

do not seem true here. It certainly should be the case that (exactly) one of them swerves. But it does not matter which one: the worlds where things go best include worlds where Attila swerves and worlds where Genghis swerves. So, on the ‘ought’ to be, the counterexample falters too. The premises are simply false.

Pace Jackson, his example is not one where Agglomeration fails. So what does happen when we have two best options, ϕ and ψ , both best relative to the same kind of ordering, and when their conjunction is not among the best options? Here is a simple example:

Dessert. I have three options for dessert: cannoli, cheesecake, and apple pie. The pie and the cannoli are the tastiest. I can order as many dishes as I like, but I will definitely feel ill if I have more than one dessert.

Clearly the premises of the Agglomeration inference are just not true here. Both of the following are false:

(13) I ought to have pie.

(14) I ought to have cannoli.

Rather what is true is a disjunctive ‘ought’:

(15) I ought to have pie or cannoli.

I'll call this property of the deontic 'ought' *Indifference*:

Indifference. In cases where ϕ_1, \dots, ϕ_n are jointly best, 'ought $\phi_1 \vee \dots \vee \phi_n$ ' is true.

Indifference is importantly connected to Agglomeration. It tells us that in the cases where we would look for Agglomeration to fail, the premises of the inference will not be true in the first place. Instead it is the *disjunction* of the best options that ought to be true.

2.2.2 Epistemic Agglomeration

Agglomeration looks far less natural for the epistemic 'ought'. Counterexamples are easier to find and are, I think, more decisive than their deontic cousins.

Take the following example:

The Office There are 26 workers, Alice, Bob, Carol, ..., and Zadio, in our office. On average, they all come into work 17 days out of 20.

All of these sentences sound true here:

- (16) Alice should be in work today.
- (17) Bob should be in work today.
- (18) Carol should be in work today.

...

- (19) Zadio should be in today.

But, on the reading where 'should' takes wide scope, it does not seem to follow that:

- (20) Everyone should be in today.

This is contrary to what Agglomeration predicts. If (16) – (19) are true, then (20) should be too.

Note as well that, unlike deontic counterexamples to Agglomeration, context-shifting is a much less promising explanation of examples like these. For one thing, our explanation of **Chariots** will clearly not carry over to the epistemic case. There we relied on the context-sensitivity, in particular the *agent*-sensitivity, of the *ought to do*. But there is no parallel for *ought to do* when it comes to the epistemic ‘ought’. The epistemic ‘ought’ displays none of the *ought to do*’s sensitivity to the subject of the prejacent.

Undoubtedly, there are other context-sensitive features of the epistemic ‘ought’ to appeal to. But there is a very general reason to think that no such explanation will work. Suppose we found some other source of context-sensitivity for the epistemic ‘ought’, so that in **the Office** at least some of the sentences are evaluated in different contexts. If that source of context-sensitivity is to render Agglomeration *valid*, we know that, in each context, at least one of the sentences must be *false*. So, other things being equal, we predict that at least one of the *negations* of the sentences in **the Office** should have a true reading.

But there is no true reading of any of the sentences:

(21) It’s not true that Alice should be in work today.

(22) It’s not true that Bob should be in work today.

(23) It’s not true that Carol should be in work today.

...

(24) It’s not true that Zadie should be in today.

No way of elaborating on the sentences helps either. A sentence like

(25) It’s not true that Alice should be in work today. (After all, not everyone will be in.)

still sounds false. Notice here the contrast with **Chariots**: there we can easily hear a true reading of a sentence like

(26) It’s not true that Attila ought to swerve. (After all, *Genghis* could swerve.)

We have an important divergence that any semantics for ‘ought’ will have to account for: Agglomeration looks to be valid for deontic ‘ought’, but not for epistemic ‘ought’.

2.3 The Predicament

These data create a dilemma for what, following von Fintel, I’ll call the *classic* view of ‘ought’.

While there are different ways of spelling out the classic view, they are all based on the idea that ‘ought’ is a universal quantifier over worlds. To fix ideas, let’s take a (simplified) Kratzerian version of this view, as outlined in von Fintel (2012):

$$(27) \quad \llbracket \text{ought } \phi \rrbracket^{c,w} = 1 \text{ iff } \forall w' \in \text{BEST}(w): \llbracket \phi \rrbracket^{c,w'} = 1$$

That is, ‘ought ϕ ’ is true iff ϕ is true throughout some set of best worlds. Differing versions of the view differ on how to determine a world is best. For the purposes of this chapter, this is a difference without a difference: all of these accounts agree on the basic logic of ‘ought’. (In the next chapter, we will have more to say about where the orderings come from.)

Our data from Agglomeration pose a dilemma for the classic view. If the classic semantics is meant to be adequate for all flavours of ‘ought’, then Agglomeration should be valid generally. It is easy to see why: if ‘ought ϕ ’ and ‘ought ψ ’ are true, then all the best worlds are ones where ϕ is true and where ψ is true. But then all the best worlds must be ones where ‘ $\phi \wedge \psi$ ’ is true. So ‘ought ($\phi \wedge \psi$)’ is true.

Notice this reasoning is general: we made no suppositions about what the relevant flavour of ‘ought’ might be. So the classic view is committed to Agglomeration not just for deontic ‘ought’, but for *any* flavour of ‘ought’. This means that any counterexample to Agglomeration undermines the classic view, as a view about ‘ought’s generally.

This does not leave us with many options. One option is to deny that we should strive for a unified semantics for ‘ought’. But, as we noted already, an ambiguity strategy does not look promising. Epistemic ‘ought’s occur in a number of languages, something an ambiguity strategy does not predict. Moreover, a spe-

cial ambiguity treatment for ‘ought’ sits badly with what we already know about modals, namely that modal vocabulary generally can express a variety of different flavours of modality as the context changes.

Another option is to deny there really is a divergence between epistemic and deontic ‘ought’s with respect to Agglomeration. Even putting aside the responses already given, this response seems to miss something important. Counterexamples to epistemic Agglomeration are very easy to construct: it is not hard to think of a case where it fails. But even *prima facie* counterexamples to deontic Agglomeration are much harder to find, and when they are found, the judgements are far more fragile. The ease with which we can find counterexamples in the one case but not in the other is significant. It strongly suggests a genuine divergence between epistemic and deontic ‘ought’s.

The final option is to accept the divergence and aim for a unified theory, but drop the classic semantics. This might not seem all that promising either, at first. After all, won’t it be true that, no matter what entry we give to ‘ought’ we will have a problem predicting one half of the data?

Not so. We will certainly have a problem if we start with a very strong semantic entry for ‘ought’. If our semantics validates Agglomeration, no choice of ordering will make a strong semantic entry behave as if it had a weaker semantics. But if we start with a weaker semantic entry for ‘ought’, we can allow for Agglomeration to fail in principle but also for it to appear as if it were valid when given certain orderings. Supplying particular kinds of orderings can make ‘ought’ look as if its semantic entry were stronger than it is. This is the option I will pursue.

2.4 The Semantics

Here is the basic idea. I change the classic semantics in two ways: instead of being a quantifier over the best worlds in some ordering, I will take ‘ought’ to be a quantifier over the best *propositions* in some ordering. And instead of being a universal quantifier, I will take ‘ought’ to be an *existential* quantifier. So ‘ought ϕ ’ will say that ϕ is entailed by a best proposition.

Let’s state the semantics in slightly more detail now. Like in the standard Kratzerian semantics, we will add a modal base, *f*, to the index. This is a func-

tion from worlds to sets of propositions, representing the background information against which we evaluate an ‘ought’. We will add another function g to the index also, an ordering function. This function will take a world and a modal base and return an ordering over propositions, $\lesssim_{w,f,g}$.

Using these parameters, we can define the set of best propositions that entail the information in the modal base at a world:

$$(28) \quad PBEST(w, f, g) = \{p \subseteq \cap f(w) : \neg \exists q \subseteq \cap f(w) : q \prec_{w,f,g} p\}$$

This allows us to state the semantics precisely:

$$(29) \quad \llbracket \text{ought } \phi \rrbracket^{c,w,f,g} = 1 \text{ iff } \exists p \in PBEST(w, f, g) : \forall w' \in p : \llbracket \phi \rrbracket^{c,w',f,g} = 1$$

‘ought ϕ ’ will be true just in case there is *some* proposition at the top of the ordering that entails ϕ .²

This semantics is a natural fit for epistemics. But how could it give a plausible meaning for the deontic ‘ought’? The important point I will try to exploit is that, when we put particular constraints on orderings, the existential force of ‘ought’ will be equivalent to *universal* force. A simple example is where the domain contains just one best proposition: in that case some best proposition will entail ϕ just in case every best proposition does. In such cases, ‘ought’ will look like it obeys Agglomeration. If the unique best proposition entails ϕ and entails ψ then it entails $\phi \wedge \psi$. On my account, it is just this feature that deontic uses of ‘ought’ will exploit.

Before I go on, a few words about the kind of orderings I will be working with. I aim to be as neutral as possible about what deontic orderings track. There is a large and convincing literature that a semantics for ‘ought’ should aim for neutrality. Various different sources of value can supply an ordering to the semantics, if the context is right.³

I also aim to be neutral about the epistemic case, but there are fewer plausible

²There are some important precedents for this view. The idea that ‘ought’ involves something like a layer of existential quantification has been explored by Fraassen (1973), von Fintel (2012), Horty (2012) and Swanson (2016). There are nonetheless important difference between their views and mine: all of those proposals aim to capture moral dilemmas and, as a result, do not obviously carry over to epistemic Agglomeration failures. My move to quantification over *propositions* is what helps here.

³See, among others, Carr (2015), Charlow (2016), Lassiter (2016) and the discussion in the next chapter.

candidates here. One plausible ordering tracks *probability* and reflects whether or not a proposition passes a contextually supplied threshold.⁴ So no proposition will be strictly better than ϕ whenever the probability of ϕ passes the given threshold. This is for the simple reason that ‘ought ϕ ’ can be true, even when we are not certain that ϕ . Here is a way of formalising this:

$\phi \lesssim_{w,f,g} \psi$ iff, where θ_c is the contextually determined threshold, one of the following conditions holds:

1. $P(\phi | \cap f(w)) > \theta_c$; or
2. $P(\phi | \cap f(w)) \geq P(\psi | \cap f(w))$

A different thought, touched on earlier, is that the relevant ordering is really a *normality* ordering. This kind of ordering is much more straightforward. We would have $\phi \lesssim_{w,f,g} \psi$ just in case ϕ is at least as normal as ψ . I will not choose between them.⁵

2.5 Just Enough Agglomeration

A naive application of my semantics predicts Agglomeration failures in both epistemic and deontic cases. In both kinds of cases, the natural orderings supplied by certain contexts will rank some ϕ and ψ among the best propositions, but not their conjunction.

I will break the solution into two halves. This section will give a pragmatic story about why Agglomeration-violating orderings are ruled out of the semantics in the deontic case. I’ll start by identifying a key difference between deontic and epistemic Agglomeration-violating orderings. Then I will argue for a pragmatic constraint which prevents orderings with that property from entering the semantics. In the next section, I’ll argue that alternative orderings are available to deontics that predict Indifference.

⁴Because of the arguments in Yalcin (2016), this can at best be a necessary condition for an epistemic ‘ought’. A plausible additional constraint is that ϕ not only be probable, but that learning its negation would also lower one’s credence in some salient background proposition. I will ignore these complications here.

⁵I am open to the possibility that both probabilistic and normality readings of ‘ought’ exist, but again I will not attempt to settle the issue here.

2.5.1 Distinguishing Epistemic and Deontic Agglomeration Failures

The difference between Agglomeration-violating orderings in deontic and epistemic cases is, I'll argue, that the set of best propositions can be consistent in epistemic cases but in deontic cases they will be contextually inconsistent.

This is clearly borne out in some of the cases. **The Office** clearly confirms the claim about epistemics. There the set of best propositions is plainly consistent. When we say something like 'Alice ought to be in today', we are clearly not saying it is *only* Alice that is likely to be in. We are not taking a stance on the likelihood or normality of the other workers being in.

A certain class of deontics cases fits in easily here too. Here is a simple example where ϕ -ing and ψ -ing are equally good, but contextually inconsistent:

Going Home. There are three different routes home, A, B and C. A and B get us home equally quickly, but C takes longer. All we care about is how quickly we get home.

A naive application of the semantics would suggest Agglomeration will fail here: *we take A* and *we take B* will be among the best propositions; *we take A and B* will not, simply because it is not possible. Clearly this kind of case fits with the distinction I am attempting to draw.

Other kinds of case are trickier. Take instead a case where ϕ -ing and ψ -ing are equally good and better than anything else, but $\phi \wedge \psi$ -ing is worse than doing either individually:

Dessert. I have three options for dessert: cannoli, cheesecake, and apple pie. The pie and the cannoli are the tastiest. I can order as many dishes as I like, but I will definitely feel ill if I have more than one dessert.

Again, it looks like Agglomeration will fail here: having pie and having cannoli are equally good and are better than any of the other options. In particular, they are both better than having pie *and* cannoli. But isn't it consistent to have both pie and cannoli?

Here we need to be careful about precisely what propositions are at the top of the ordering. If the propositions are *I have pie* and *I have cannoli*, then we have a problem. However, another possibility, one consonant with my theory, is that *I only have pie* and *I only have cannoli* are at the top of the ordering. This is what I think is in fact the case here.

Start with some linguistic evidence. When we say that having pie is among the best options, it would be natural for a scalar implicature to strengthen *I have pie* to *I only have pie*. This is unremarkable in the embedded case. If I say

(30) I'm going to have the pie.

you will, via scalar implicature, hear me to say

(31) I'm going to have only the pie.

No surprise then that scalar implicatures should operate in the embedded case too.⁶

Notice as well that when we control for the scalar implicature, we change how we talk about the case. One way of controlling for the scalar implicature is by adding disjunctions that rule out the implicature. The sentence

(32) John ate some or all of the cookies.

does *not* carry the implicature that he didn't eat all of them.⁷ Now consider what happens when we describe **Dessert** this way:

(33) Having pie or pie and cannoli is among the best options.

This simply sounds false. Having pie or pie and cannoli clearly leaves open than I have pie along with cannoli. But then it cannot be better than having pie and

⁶By contrast, notice there is no corresponding implicature for a sentence like:

(i) Alice is in work today.

as uttered in **the Office**.

⁷On some ways about thinking about implicature, this is not quite a case of cancellation. For example, according to Chierchia et al. (2009), the 'some' here is covertly exhaustified and so equivalent to 'some but not all'; but this stronger meaning is washed out by the other disjunct. My point here is simply that sentences like (32) are *as a whole* equivalent to sentences like 'John ate some of the cookies', minus the scalar implicature.

cannoli after all.

In addition to the cases, I think there is also a theoretical motivation for drawing the line this way. Suppose we have a deontic ordering where ϕ and ψ are both better than $\phi \wedge \psi$. The natural question to ask is, where do the non- ψ portion of ϕ and the non- ϕ portion of ψ fall on the ordering? Are $\phi \wedge \neg\psi$ and $\neg\phi \wedge \psi$ ranked higher or lower than ϕ and ψ ?

They must, I think, be both ranked higher than ϕ and ψ in order to balance out the comparative badness of $\phi \wedge \psi$. If pie is better than pie and cannoli, then having *just* pie will need to be better to weigh out the badness of having it as part of two desserts. In general, other things being equal, when ϕ and ψ are better than $\phi \wedge \psi$, the propositions $\phi \wedge \neg\psi$ and $\neg\phi \wedge \psi$ will have to be better still. But this means that in such cases the set of best propositions will be inconsistent.⁸

The same is not true of epistemics. Take an epistemic ordering where ϕ and ψ are both better than $\phi \wedge \psi$. Where do the non- ψ portion of ϕ and the non- ϕ portion of ψ fall? Here there is no reason to think that $\phi \wedge \neg\psi$ must be better to balance out the lower ranking of $\phi \wedge \psi$. This is clearest in the case of probability: ϕ has to be at least as probable as anything it entails; and often will be more probable than both $\phi \wedge \psi$ and $\phi \wedge \neg\psi$. It is even true in the case of normality. The fact that some ways for ϕ to happen are abnormal need not take away from the normality of ϕ more generally. No particular way for Alice coming into work has to pull extra weight to make up for the fact that everyone's showing up is abnormal.

Consistency then is the dividing line between deontic and epistemic cases.

2.5.2 A Constraint

The next step is to motivate a constraint which would rule out deontic Agglomeration failures.

I suggest the following pragmatic constraint applies to deontic orderings:

Consistency: g is a permissible ordering source in c only if when

⁸There are limits to this thought. What about bad realisations of ϕ that are weird or intuitively far away possibilities? One might think that the rest of ϕ need not pull extra weight in this case. If that is so, then my point should be understood as applying to propositions that are all subsets of the modal base, the information held fixed in the context. Since the background information will rule out far away cases, they will not enter into the comparisons relevant here.

ϕ and ψ are in $PBEST(w_c, f_c, g_c)$, then $\phi \wedge \psi$ is consistent with $\cap f(w_c)$.

This constraint says that, whatever the best propositions are, they must be jointly consistent with the background information in the context.

This principle is, I think, metasemantically plausible. To get a grip on it, consider first the following generalisation of an ought-implies-can principle:

Oughts imply joint ability: if one ought to ϕ_1 , ought to ϕ_2 , ..., and ought to ϕ_n , then one can (ϕ_1, ϕ_2, \dots and ϕ_n)

This principle says that one is able to do all at once all of the things she ought to do; she is not forced to choose between the obligations she will satisfy and the ones she will not. It seems to share its motivation with the ought-implies-can principle (which it is strictly stronger than). Just as you might have to be able to satisfy your obligations for them to be obligations, this principle says you must be able to satisfy all of your obligations all together, for them to be your obligations.

Consistency is essentially a pragmatic version of **Oughts imply joint ability**. It says that whatever propositions are best, it must be possible to ensure they *jointly* obtain. This means that 'ought ϕ ' and 'ought ψ ' will be true together only if ϕ and ψ are consistent given our background information. Given the relation between 'ought'-claims and action, it is plausible that they should be governed by Consistency. If 'ought's are to guide our action, we should not want them to direct us to do things which, taken altogether, we cannot do.⁹

2.5.3 Predictions

With all this in hand, we can show that cases like **the Office** are genuine failures of Agglomeration and that cases like **Going Home** and **Dessert** are not.

Start with **the Office**. Our threshold probability ordering from earlier gives a smooth account of this case. Suppose that in **the Office** the threshold is .8. Then all of the propositions *Alice is in work today*, *Billy is in work today*, ..., and *Zadie*

⁹It is claimed that the inference from 'ought' to 'can' is meant to hold only for the *ought to do*. However, this claim is about the *agential* 'can'. My constraint concerns the circumstantial 'can', one much weaker than ability and plausibility connected both to the *ought to do* and the *ought to be*.

is in work today are in $PBEST(w, f, g)$, but not *everyone is in work today*. This means that each of

(16) Alice should be in work today.

(17) Bob should be in work today.

(18) Carol should be in work today.

...

(19) Zadio should be in today.

will be true but

(20) Everyone should be in today.

will be false. Clearly then Agglomeration fails.

Normality orderings seem to make the same prediction. It is certainly more normal for Alice to be in work today than not; and for Billy to be in work today than not; and so on. But it is not more normal for them *all* to be in than not; it would be very unusual for everyone to turn up on a given day. This then suggests that it is less normal for everyone to be in than it is for Alice to be in. For, if they were equally normal, then it would be more normal for someone to be absent than for Alice to be in, and that does not seem true here.¹⁰

Now for deontics. Start with cases like **Going Home**. The natural ordering in this case clearly violates the constraint. *I take route A* and *I take route B* are both among the best propositions, but are not jointly consistent against the background information. This ordering is clearly ruled out and so we avoid generating true Agglomeration-violating readings of the sentences

(34) I ought to take route A.

(35) I ought to take route B.

Finally, return to **Dessert**. There, as we have seen, there is reason to think

¹⁰Of course, I have not ruled out that the proposition that Alice is in and that someone is absent are *incomparable* with respect to normality; but I see no good reason to think that should be true here.

that the propositions at the top of the natural ordering will be *I only have pie* and *I only have cannoli*. These propositions are not consistent and so our consistency principle rules this ordering out: we will not have readings of the sentences

(13) I ought to have pie.

(14) I ought to have cannoli.

where they are evaluated against an ordering that puts *I only have pie* and *I only have cannoli* at the top. Again, our consistency principle prevents **Dessert** from being an Agglomeration failure.

Our constraint appears to do its work: epistemic ‘ought’s will permit Agglomeration failures; deontics will not.

2.6 Indifference

We are not yet done. We do not simply want to avoid deontic Agglomeration failures. We want to predict Indifference.

Since the most natural orderings supplied by the context are banned, we need an account of where the actual orderings come from. The basic thought here will be that, in deontic cases, there are multiple orderings available to the semantics, which correspond to different ways of discriminating between options. In particular, there will be orderings available which lump together equally good options into their disjunctions.

2.6.1 Dividing up the options

Return to **Going Home**, our case from before. Suppose I am asked what is the best way to go home. Consider these possible replies:

(36) Taking route A and taking route B are the best things we can do.

(37) Taking route A or taking route B is the best thing we can do.

Clearly these replies are equivalent. Saying either would answer the question in the same way. But, on a natural way of understanding the semantics of ‘the best

thing we can do', it cannot be precisely the same ordering in each case. For (36) suggests that the propositions *I take route A* and *I take route B* are at the top of the relevant ordering; but the use of the definite in (37) suggests that the proposition *I take route A or take route B* is the *unique* proposition at the top of the ordering.

The difference between these orderings, I suggest, comes down to different ways of treating our options. One way of thinking about our options distinguishes them at least as finely as the choice situation does. We could however make distinctions between our options only when there is a difference between how good they are. In other words, we could lump together options that result in equally good outcomes. So, when we have an ordering over propositions something like

$$\phi_1, \dots, \phi_n < \rho < \sigma$$

there will also be available an ordering which lumps together the equally good options, putting the union of the best propositions at the top, like so:

$$\phi_1 \vee \dots \vee \phi_n < \rho < \sigma$$

Suppose then that whenever the finer-grained ordering is available, so too is the coarser-grained one. This would explain the equivalence we noted above.

Now, these data do bear a close resemblance to the free choice inference. It's been observed that disjunctive permissions give rise to the following (felt) inference:

- (38) a. You may go left or right.
 b. \sim You may go left and you may go right.

But the resemblance is imperfect. First, note that 'best' is a stronger notion than permission. Something's being among the best things to do entails that it is permitted, but not vice versa. We can coherently say things like:

- (39) You may go left, but it's not the best thing to do.

Now note that we do not get an analogous free choice effect for notions *stronger* than possibility. Neither of the following seem like good inferences:

- (40) a. I ought to go left or right.

- b. \leadsto I ought to go left and I ought to go right.
- (41)
- a. He probably went left or right.
 - b. \leadsto He probably went left and probably went right.

Since *this* is the inference we would need, our data cannot boil down to the free choice phenomenon.

So, I have argued, whenever we have an Agglomeration-violating ordering available, we have an Agglomeration-satisfying ordering too, which lumps equally good options together into their disjunction. This is exactly what we need. We will have Agglomeration-violating orderings only when we have multiple best propositions. Whenever we have an ordering with multiple best propositions, we will also have an ordering which fails to discriminate between those options and lumps them into one option. This means we will have an ordering which puts the *disjunction* of the old best propositions at the top.

This dovetails nicely with our pragmatic constraint. If both of these orderings are potentially available for the semantics, and our pragmatic constraint rules out one of them, then it would be natural for the other to be selected. This then will predict Indifference: since Agglomeration-violating orderings will be ruled out, the disjunctive orderings will be selected. And those orderings predict Indifference.

2.6.2 Predictions

Now let's see with how this completes our story about deontic cases. Return to one of our cases from earlier:

Going Home. There are three different routes home, A, B and C. A and B get us home equally quickly, but C takes longer. All we care about is how quickly we get home.

Here we want to predict that the sentence:

- (42) We ought to take route A or route B.

is true. The natural ordering on propositions suggested by the case is as follows:

we take route A \equiv *we take route B* $<$ *we take route C*

We saw that our constraint rules out this ordering. The pragmatics will search for another ordering instead and will find the modified ordering which replaces the old best propositions with their disjunction:

(43) *we take route A or take route B* < *we take route C*

Since in this case there is just one best proposition, the one that entails we take route A or route B, we predict that (42) is true. Moreover, we avoid predicting failures of Agglomeration, for according to this ordering, neither of

(44) We ought to take route A.

(45) We ought to take route B.

are true. This is simply because *we take route A or take route B* neither entails that we take A nor that we take B.

The same holds for cases like **Dessert**, repeated below:

Dessert. I have three options for dessert: cannoli, cheesecake, and apple pie. The pie and the cannoli are the tastiest. I can order as many dishes as I like, but I will definitely feel ill if I have more than one dessert.

Here we want to predict that

(46) I ought to have cannoli or pie.

is true. Again we already saw that our pragmatic constraint rules out the natural ordering that would violate Agglomeration. Since that natural ordering is unavailable, we will search for others. The disjunctive ordering we have seen will be available and will predict the truth of (46).

2.6.3 Formally Defining the Relation

When thinking about these disjunctive orderings, one might worry about the status of the old best options. Where do they fall on the ordering? Must they then fall *below* their disjunction?

By more carefully defining the new relation from the old, we can show why we do not have this problem. To fully define this procedure, we can rely upon the fact that relations are (or can be represented by) a set of ordered pairs. For any given relation, we define a new relation which replaces every pair containing a proposition entailing ϕ or ψ , replacing it with $\phi \vee \psi$ substituted in its stead:

$R_{\phi \vee \psi}$ is the relation formed by:

1. Removing from R any pair that contains $\phi \vee \psi$;
2. Removing from R any pair that contains some ρ which entails $\phi \vee \psi$ and replacing it with a pair with $\phi \vee \psi$ substituted for ρ .

So if we have ϕ and ψ that are inconsistent, we can form a relation which replaces ϕ and ψ with their disjunction.

Given this definition, it is straightforward to state what we want our relation to be. Say that ϕ_1, \dots, ϕ_n are the propositions in $PBEST_R$. We then let our new relation be $R_{\phi_1 \vee \dots \vee \phi_n}$. More specifically, when R is the salient deontic relation, the ordering source supplied by context, g_c , will be such that $g_c(w_c)$ is $R_{\phi_1 \vee \dots \vee \phi_n}$. Given our definition, this has just the properties we want: the relevant ordering will always have the disjunction of the old best propositions at the top.

Here is an important technical upshot of this construction. Suppose that ϕ and ψ were at the top of our original ordering. On our new construction they will be incomparable with any other proposition: there will be no proposition, not even themselves, which they are related to on the new construction. But notice this means that there is no proposition which is strictly better than ϕ or better than ψ . The unfortunate upshot of this is that, given its definition, ϕ and ψ end up back in $PBEST$; after all, there is no proposition strictly better than either of them!

Fortunately, there is a simple trick we can use here. ϕ and ψ are in the remarkable position of being incomparable to every proposition, even themselves. We can simply add to the definition of $PBEST$ that the best propositions are at least comparable to *something*. This will ensure that ϕ and ψ do not end up among the best propositions. Formally stated then, we would have:

$$PBEST(w, f, g) = \{p \subseteq \cap f(w) : \neg \exists q \subseteq \cap f(w) : q \prec_{w, f, g} p \text{ and } \exists q : p \preceq_{w, f, g} q\}$$

On the orderings our semantics will care about, the best propositions will also be related to themselves. So this adjustment will only make a difference in the cases where we are using the construction outlined above.

2.7 Interim Summary

The story here is rather complex, so let us take a moment to review. In order to permit Agglomeration failures at all, we have weakened the semantics for ‘ought’. ‘Ought’ is an existential quantifier over best propositions and so ‘ought ϕ ’ and ‘ought ψ ’ can be true, while ‘ought $(\phi \wedge \psi)$ ’ is false.

An unsophisticated application of this semantics predicts too many Agglomeration failures: in general, whenever ϕ and ψ are best but $\phi \wedge \psi$ is not, it will fail. This is desirable in the epistemic but not the deontic case. To distinguish between them, I argued that the deontic case is special in a number of ways. I argued that the deontic orderings that would induce Agglomeration failure are inconsistent at the top. I argue that there is reason to think that, at least in the deontic case, there is a pragmatic constraint that the set of best propositions be consistent. Finally, there are alternative, disjunctive orderings available to the semantics when the default is ruled out.

Before we move to responding to my proposal, it is worth dwelling for a moment on how my semantics for ‘ought’ fits into work on modal vocabulary generally. My move in raising the type of ‘ought’ dovetails with other work on the semantics of possibility and strong necessity modals. The notion that possibility modals involve quantification over propositions has been explored by, among others, Moss (2015) and Mandelkern et al. (2017). There is a particular kind of continuity with the latter view. Mandelkern et al. (2017) take ability modals to quantify over *actions* saying that ‘S can ϕ ’ is true iff there is some (practically available) action A such that if S tried to A , S would ϕ ; and ‘S must ϕ ’ is true iff all practically available actions A are such that if S tried to A , they would ϕ .

A natural generalisation of this idea substitutes *best propositions* for available actions, with possibility and necessity modals getting the following entries, respectively:

$$(47) \quad \llbracket \text{can } \phi \rrbracket^{c,w,f,g} = 1 \text{ iff } \exists p \in PBEST(w, f, g) : \exists w' \in p : \llbracket \phi \rrbracket^{c,w',f,g} = 1$$

$$(48) \quad \llbracket \text{must } \phi \rrbracket^{c,w,f,g} = 1 \text{ iff } \forall p \in PBEST(w, f, g) : \forall w' \in p : \llbracket \phi \rrbracket^{c,w',f,g} = 1$$

The Mandelkern et al. (2017) semantics will result when the best propositions are the set of closest worlds where you try to do an available action; they will be the propositions that say the agent performs a representative attempt to A , relative to the world of evaluation.¹¹

On my proposal, ‘ought’ falls somewhere between these entries for possibility and necessity: it is stronger than a possibility modal, because it requires entailment by a best proposition and not mere consistency; it is weaker than a necessity modal, because it does not require entailment by *all* best propositions.¹²

To be sure, the type-raised view is an emerging, rather than established view. But it does hold out promise to illuminate a number of independent issues and it is interesting to note the continuity between it and my proposal for ‘ought’.

2.8 Contrastivism

The natural competitors to my account are *contrastivist* accounts of ‘ought’. I have two reasons for preferring my view. The first is that my view offers a better account of the epistemic ‘ought’.

Contrastivists think that a sentence of the form ‘ought ϕ ’ will be true iff ϕ is *better* than its alternatives. We can turn this (admittedly vague) statement into a semantic entry along the following lines:

$$(49) \quad \llbracket \text{ought } \phi \rrbracket^{c,w,f,g} = 1 \text{ iff for every } \psi \in ALT(\phi) : \phi <_{w,f,g} \psi$$

There is room for many different views about when one proposition is the alternative to another. But the core idea is that closely related sentences can have different alternatives. The alternatives for ϕ and ψ can be disjoint, even if ϕ entails ψ . The alternatives for ϕ and for ψ can be disjoint from the alternatives from $\phi \wedge \psi$. This

¹¹Note that, because Mandelkern et al. use a CEM-validating selection function, these propositions will be *singleton* sets of worlds. This is essential for capturing the duality of ‘can’ and ‘cannot not’.

¹²Though note that the kind of weakness my account aims to capture, the non-validating of Agglomeration, is distinct from the kind of weakness discussed by, among others, von Fintel and Iatridou (2008).

variation in the alternatives is what generates contrastivism's distinctive logic.

Contrastivism is my natural competitor because many contrastivists share my aim of invalidating Agglomeration.¹³ They do so by saying that, while ϕ is better than its alternatives and ψ is better than its alternatives, $\phi \wedge \psi$ is not better than its (distinct) alternatives. The trouble is that almost all contrastivist views violate *deontic* Agglomeration. This is no accident. We saw that Jackson, a contrastivist, attempts to counterexample deontic Agglomeration. But we also saw that there is good reason to be skeptical of that claim.

Contrastivists could amend their view by using the kinds of orderings I've argued for. My deontic pragmatics could be plugged into their semantics to avoid Agglomeration failures: since the offending propositions are simply deleted from my orderings, we will predict Indifference. But, perhaps surprisingly, combining contrastivism with plausible epistemic orderings predicts too *few* Agglomeration violations in the epistemic case.

A core fact about alternatives is that every proposition p is inconsistent with its alternatives; and in general the union of the alternatives of ϕ will be $\neg\phi$. A core fact about probabilities is that when ϕ entails ψ the probability of ϕ cannot be higher than the probability of ψ . Taken together, this means that, for the contrastivist, ϕ being more probable than $\neg\phi$ is *sufficient* for the truth of an epistemic 'should': when ϕ is more probable than $\neg\phi$, it *must* have a higher probability than any of its alternatives.

The same is not true for a threshold view like mine. When the threshold is greater than 0.5, a proposition can be more probable than its alternatives and still fail to pass the threshold. Take a simple, abstract case: p has probability 0.7, q a probability of 0.55 and the threshold of probability is 0.7. A threshold view predicts that 'should p ' will be true, but not 'should q '. A contrastivist will predict that *both* are true.

This difference cuts against the contrastivist. We can see this perhaps most clearly by turning our schematic case into an Agglomeration failure. Suppose that

¹³Many, but not all. Cariani (2013a), Cariani (2013b), and Cariani (2016b) defend a contrastivist semantics which validates Agglomeration. Unfortunately, on any way of selecting alternatives, it validates Agglomeration in full generality and so cannot be adapted to our epistemic Agglomeration failures.

Alice, Billy and Carol each come into work about 70% of the time, but they come into work together about 55% of the time, just over half of the time. Here I think it is true to say each of:

(50) Alice should be in work today.

(51) Billy should be in work today.

(52) Carol should be in work today.

But it does *not* sound true to say:

(53) Alice, Billy and Carol should *all* be in tomorrow.

My view can easily accommodate these judgements, if we suppose the relevant probability threshold is 0.7. The contrastivist view, on the other hand, *cannot* predict this pattern of judgements: the fact that Alice, Billy and Carol come into work together more often than not should suffice for the truth of (53).¹⁴

Would contrastivism do better here with a normality ordering? I am skeptical. Again, it looks like, for instance, *Alice will be in work today* is more normal than its negation, but also that *Alice, Billy and Carol should all be in tomorrow* is *slightly* more normal than its negation. There is perhaps is more room to manoeuvre here. It might be claimed that individual alternatives to ϕ can be more normal than the union of those alternatives. But it is not clear what exact selection of alternatives would yield this in our case.

What if we alter the view slightly, so that contrastivism says that a proposition ϕ needs to be *significantly* better than its alternatives for 'ought ϕ ' to be true? This works in our present case: Alice, Billy and Carol come into work together more often than not, but maybe not significantly more often than not. It is questionable, though, when we move to deontics. Suppose that route A is somewhat shorter than route B and, other things being equal, we prefer to take a shorter routes home. Here it seems that

(54) I should take route A.

¹⁴I should note that Cariani's brand of contrastivism will not have this problem. But since it validates Agglomeration in full generality, it is not my main target here.

is true. But if a proposition is required to be *significantly* better than its alternatives, it looks like this should be false. While good in the epistemic case, this amendment is inappropriate for deontics. Deontics clearly care about small differences between options, even if they are small ones. After all, it would be strange to take the somewhat longer route, if all one cares about is how quickly they get home.

Perhaps surprisingly then, threshold and contrastivist views of the epistemic ‘ought’ come apart. Threshold views predict more cases of Agglomeration failure than contrastivists. This is to the credit of threshold views, as Agglomeration does indeed seem to fail in these cases.

2.9 Inheritance

The second advantage I claim over the contrastivist is more controversial. Consider the following inference:

Inheritance If $\phi \models \psi$, then $\text{ought } \phi \models \text{ought } \psi$

Just as Agglomeration is a multi-premise closure constraint on ‘ought’, Inheritance is a single premise closure constraint on ‘ought’. It says that whatever any individual ought entails is itself something that ought to be. Contrastivists typically *invalidate* this inference; my semantics *validates* it. I want to argue that validating Inheritance is a good feature.¹⁵

Contrastivists will say that validating Inheritance is bad because there are counterexamples to it. Consider the famous example adapted from Ross (1941):

- (55) a. We ought to mail the letter.
 b. \nrightarrow We ought to mail the letter or burn it.

Inheritance predicts that (55-b) follows from (55-a), but it does not seem to.

¹⁵Another indirect argument for this comes from results in Cariani (2016a). Cariani shows that very few contrastivist theories invalidate Agglomeration without also invalidating the very plausible inference pattern he calls Weakening:

$\text{ought } \phi, \text{ought } \psi \models \text{ought } (\phi \vee \psi)$

Weakening falls directly out of my semantics, however, as it is valid whenever Inheritance is.

Let me start by echoing a defensive point others have made.¹⁶ Pragmatics will naturally interfere here. (55-b) is less informative than (55-a); asserting (55-b) would be surprising if we are in a position to assert (55-a). Gricean considerations would then suggest we interpret (55-b) as saying something stronger, perhaps implicating that burning the letter is permissible.¹⁷ Saying exactly *how* this works is not trivial of course, but the starting point is a natural one.

But the strongest point against the contrastivist and in favour of Inheritance, I think, is that Inheritance is *needed* to explain various data. This is the line taken by von Fintel (2012); but since his data largely concern ‘have to’ and not ‘ought’, I give two more data points which support Inheritance.

First, let us consider ‘knows whether’-reports. Unsurprisingly, when ϕ entails ψ it is marked to ascribe knowledge that ϕ and deny knowledge of whether ψ . For instance, consider:

(56) #John doesn’t know whether the restaurant is to the right or the left; but he knows that it is to the left.

Now note that if we deny Inheritance, then it should be coherent to ascribe knowledge that ought ϕ but deny knowledge whether ought ψ , even when ϕ entails ψ . But this does not seem to be borne out:

(57) #John doesn’t know whether we should mail the letter or burn it; but he knows that we should mail it.

The response here might be that, in the imagined context, if you know that if you should mail the letter, then you should not burn it! This would explain the markedness of this example, but only goes so far. Consider instead adding disjuncts that, rather than being deontically compatible, simply seem to go beyond known permissions and obligations. For instance, even if I should mail a letter of reference, it does not follow that I should reach out to the department in person. But neither does my obligation to mail the letter rule out my reaching out to the department. It does not speak to my following up, one way or the other.

¹⁶See, in particular, Wedgwood (2006) and von Fintel (2012).

¹⁷Presumably this would go via the free choice inference from ‘may ($\phi \vee \psi$)’ to ‘may $\phi \wedge$ may ψ ’. See Kratzer and Shimoyama (2017) and Fox (2007) for scalar implicature accounts of this inference.

Now consider the same knows-whether construction applied to this case:

- (58) #John does not know whether we should mail the letter or reach out to the department; but he knows we should mail the letter.

This still sounds marked. Denying Inheritance leaves one badly placed to explain this: the question of whether we should mail the letter or reach out to the department is not settled by the question of whether we should mail the letter. Accepting Inheritance makes for a very simple explanation.¹⁸

A different consideration in favour of Inheritance comes from cases of less than complete cooperation. Normal conversation presumes that speakers will share all the information they have on the relevant questions. But sometimes this presumption is cancelled. A good example of this are cases that involve games or competitions of some kind. Take the following:

Maze. You are navigating your way through a maze and you get to a point where there are there are two paths to the east and two paths to the west. I am tracking your progress and giving you some hints as to where to go next. The southeast path is the correct one for you to take.

Suppose now that I say to you

- (59) You should go east.

It seems that what I have said to you is true. After all, you should go south east; and if you go south east, you thereby go east.

But without Inheritance we cannot give this explanation of what I have said. Without Inheritance what I have said is misleading: even if you should go south east, this does not settle whether you should go east. In fact, most contrastivist proposals will predict what I have said is *false*: they tend to be built on the idea that 'ought ϕ ' is true only if all of the relevant ways of doing ϕ are sufficiently good. But either way, it should be misleading of me to say that you should go east.

¹⁸It might be pointed out that 'knows whether' has an alternative question reading, where it asks which of two exhaustive and exclusive alternatives (given the contextual information) are known. But there is no reason to think that this reading would be prominent in the case: mailing a reference letter and following up with a department by no means exclude each other.

There is surely more to be said about Inheritance. I hope to have strengthened the case that Inheritance is valid. Not only can the counterexamples be explained pragmatically, but certain data resist explanation without it. Another thing I hope to have shown is that a denial of Agglomeration need not bring with it a denial of Inheritance. Appealing to contrastivist intuitions is one way of spelling out failures of Agglomeration, but it is not the only way. My semantics allows for failures of Agglomeration but does so while maintaining the validity of Inheritance.

2.10 Dilemmas

I want to finish with a topic that has been conspicuously absent, *moral dilemmas*.

Moral dilemmas might be thought to be a genuine source of counterexample to Agglomeration for the deontic 'ought'. Take the following case:¹⁹

Sophie's Choice. Sophie is forced to choose which of her children is going to be sent to the labour camp and which is going to be killed. If she chooses neither child, then both will be killed.

Here it is possible to get a true reading of both of the following sentences.

(60) Sophie ought to save her daughter.

(61) Sophie ought to save her son.

But obviously the agglomerated 'ought'

(62) Sophie ought to save her daughter and her son.

is not true here, for Sophie cannot do both of these things together. So, if all of these are evaluated in the same context, we have a failure of Agglomeration. Since my semantics is designed to predict Agglomeration is valid in the deontic case, I do not predict this.

¹⁹This is an example of what Sinnott-Armstrong (1985) calls a *symmetric dilemma*, where there is no morally relevant difference between the sources of the obligations. Symmetric dilemmas are perhaps the more challenging case to account for; everything I say here will carry over to asymmetric dilemmas, like that of Sartre's famous student.

Let me make a more concessive point first. Though my semantics does not predict Agglomeration failures in cases of moral dilemmas, there is a natural modification that does. To state this modification, it is important to first get clear on when dilemmas arise.

As von Fintel has pointed out, dilemmas do not simply arise when we have more than one best option; otherwise cases like **Going Home** or **Dessert** would be classed as dilemmas when they clearly are not. What is distinctive about dilemmas, I suggest, is that the best options are *incomparable*.²⁰ Take first the natural language data. In **Going Home**, for instance, it seems true to say

(63) Taking route A is just as good as taking route B.

We cannot say something analogous in **Sophie's Choice**. There it sounds *false* to say

(64) Saving her son is just as good as saving her daughter.

So the way we talk seems to offer some support for this idea.

Further evidence comes from mild sweetening: making one of the options slightly morally better does not resolve the dilemma. Suppose that Sophie can choose between having her daughter saved and having her son saved *and* treated slightly better in the labour camp. Having her son saved *and* treated slightly better in the labour camp is better than just having her son saved; but improving this option does not resolve the dilemma. Since neither option is *better* than the other, we are left with the conclusion that the two options are incomparable.

We can use this understanding of dilemmas to restate the pragmatic constraint. Before we said that the intersection of the best propositions must be consistent with the background information. We simply weaken this to apply to just the *comparable* best propositions. Instead of applying the constraint to the whole set of best propositions, we take the biggest sets of comparable best propositions and apply it to them. Or more formally:

²⁰I mean this simply in the formal sense that, for the contextually supplied ordering, neither is at least as good as the other. Ethicists may want to make further distinctions among such cases. Chang (2002), for instance, distinguishes between cases where objects are *on a par* and where objects are incomparable in some further ethical sense. I suspect dilemmas will result however we fill out these further details.

g is a permissible ordering source in c only if for every maximal consistent set S of *comparable* propositions in $PBEST(w_c, f_c, g_c)$, for every $\phi, \psi \in S$, $\phi \wedge \psi$ is consistent with $\bigcap f(w_c)$.

This principle can be motivated along the same lines as the stronger one. The thought here would be that it should be possible to jointly do all the things one ought to do, given a particular source of value or obligation.²¹ While the things one ought to do may be inconsistent when all taken together, they should at least be locally consistent.

This is all to say that, if one thinks that dilemmas should be treated as bona fide failures of Agglomeration, like our epistemic examples were, then there is a natural tweak to my pragmatics which delivers this. However, I am sceptical that this is ultimately the right thing to say about dilemmas.

The first thing to note is that, as von Fintel (2012) has observed, dilemmas arise for ‘must’ as well as ‘should’. This is clear from the example we already saw. We just as easily could have considered:

(65) Sophie has to save her daughter.

(66) Sophie has to save her son.

and noted that both seem to have true readings here.²²

This should make us much more reluctant to lump these cases in with the failures of Agglomeration I have discussed. Agglomeration for ‘must’ is very plausible and Agglomeration for epistemic ‘must’ is impeccable. So whatever it is that accounts for dilemmas is unlikely to be the same thing that accounts for the Agglomeration failures I have focused on.

Noting connection with ‘must’ also points us towards what I think is a more plausible explanation of dilemma cases. ‘Must’ is what I will call *pseudo-factive*: as Ninan (2005) observes, it sounds incoherent to say

(67) You must clean your room, even though you aren’t going to.

²¹This will have to be understood in a fairly fine-grained way to give a proper treatment of symmetric dilemmas. Different people will be understood as being distinct sources of obligations.

²²Some hear dilemmas for strong necessity modals more easily by using ‘have to’ instead of ‘must’.

Though they surely don't entail them, 'must's then seem to carry some commitment to the truth of their prejacent. Seen from this perspective, the existence of dilemmas for 'must' is surprising. If *both* (60) and (61) are true, then there should be a felt commitment to both John's joining the army and his looking after his mother. But since at most one of these can hold, on the face of it, it should be puzzling why (60) and (61) sound jointly acceptable.

Now notice that when there are explicitly multiple orderings in play, the pseudo-factivity of 'must' disappears. If I say something like:

(68) According to your father, you must be home by 9; and according to your mother you must be in bed by 10

it does not seem to carry the same commitment to the prejacent. To see this observe that I can continue with something like:

(69) But of course, you're not going to do either of those things.

Here my stressing the source of the requirements seems to leave open whether we endorse or are guided by them. And when we leave open whether we endorse or take as binding the source of a requirement, it does not seem to carry the same commitment to the prejacent.

This observation suggests that, like in **Chariots**, we should appeal to multiple ordering sources in accounting for dilemmas. For if both (65) and (66) were interpreted relative to the same ordering source we should expect felt commitments to two contradictory propositions. On the other hand, if there were multiple ordering sources at play, neither of which were completely endorsed or taken to be binding, we would not expect such commitments. Positing multiple ordering sources in fact explains why (65) and (66) can be coherently said together, despite the usual pseudo-factive behaviour of 'must'. It also seems to sit well with the natural ethical understanding of dilemmas. In such cases there are two distinct sources of obligations, neither of which we can be completely guided by because they push us to do incompatible things.

To give a fully general account of dilemma cases, we need to separate dilemmas from the kinds of Agglomeration failures that motivate my semantics. Though

my theory could naturally be extended to cover them, ultimately I suspect the correct account of dilemmas will distinguish them from the kinds of Agglomeration failures we get with 'ought'.

2.11 Conclusion

Agglomeration is not a valid inference for 'ought', though it looks to govern certain of its flavours. Raising the type of 'ought' gives us the logical space we need to invalidate Agglomeration, but it must be used with care. To fill the gap, we can appeal to special pragmatic features of deontic contexts which will conspire to make Agglomeration seem valid.

The resulting semantics has substantial advantages over the main semantics for 'ought' on offer. The classic view, as we have seen, has little to offer in accounting for epistemic Agglomeration failures. But my semantics also has advantages over contrastivist views that also attempt to invalidate inferences like Agglomeration. It gives a more natural treatment of the epistemic 'ought' and validates Inheritance, something which I have argued is an advantage.

Chapter 3

More Miners

Kolodny and MacFarlane (2010) introduced the infamous miners case into the literature on deontic modals:

Miners. Ten miners are trapped either in shaft A or in shaft B, but we do not know which. Flood waters threaten to flood the shafts. We have enough sandbags to block one shaft, but not both. If we block one shaft, all the water will go into the other shaft, killing any miners inside it. If we block neither shaft, both shafts will fill halfway with water, and just one miner, the lowest in the shaft, will be killed.

In this case I can truly say:

- (1) I ought to block neither shaft.
- (2) If the miners are in shaft A, I ought to block shaft A.
- (3) If the miners are in shaft B, I ought to block shaft B.

The classic semantics for ‘ought’ cannot predict this and so the miners case looms large among its challenges.

I aim to generalise this semantic puzzle: analogues of Kolodny and MacFarlane’s case exist for *non-deontic* readings of ‘ought’. Information-sensitivity, a controversial property postulated to account for **Miners**, is needed for the non-deontic, as well as the deontic, ‘ought’.

I show that these epistemic cases defeat conservative responses to the problem. Solutions that attempt to address the problem without measure-theoretic notions fail to generalise adequately to the epistemic case. I show that accounts based on probability can succeed and I conclude by arguing that the real philosophical questions surrounding the miners puzzle are ones about the relationship not between semantics and decision theory but semantics and probability.

3.1 The Problem

The classic semantics for ‘ought’ and ‘should’, as laid out in von Fintel (2012), seems to make a strong assumption about the role that uncertainty plays in the determining these orderings. It appears to assume that there is *no* interaction between uncertainty and the ordering: varying the information held fixed in the background makes no difference to the ordering. This, it is thought, is why the miners case makes trouble for the classic view.¹

As we saw in the last chapter, the classic view assumes that ‘ought’ and ‘should’ are necessity operators: ‘ought ϕ ’ is true just in case every world in the modal’s domain is one where ϕ is true. More precisely:

$$(4) \quad \llbracket \text{ought } \phi \rrbracket^{c,i} = 1 \text{ iff } \forall w' \in \text{BEST}(i) : \llbracket \phi \rrbracket^{c,i[w_i \rightarrow w']} = 1^2$$

For our purposes, this part of the classic view will not be under dispute.³

In the last chapter we glossed over how the domain, $\text{BEST}(i)$, is supposed to be determined. According to the classic semantics, two ingredients combine in a straightforward way to yield the set of worlds ‘ought’ quantifies over.⁴

First of all, BEST is constrained by a contextually supplied ordering on worlds, \leq : only top \leq -ranked worlds can be elements of BEST . Following Kratzer (1981)

¹Kolodny and MacFarlane presented **Miners** as a counterexample to modus ponens. (See Dowell (2012), Willer (2012) and Bledin (2015) for further discussion.) But, as was first clearly recognised by Charlow (2013) and Cariani et al. (2013), the problem posed by **Miners** goes beyond modus ponens; and Khoo (2013) has shown that the classic package, as I lay it out here, *already* invalidates modus ponens. But it still gets the wrong results in **Miners**.

²Here i is a variable over indices and $i[w_i \rightarrow w']$ is the index formed by replacing the world in i with w' .

³Though it has been disputed by many: see, for instance, Cariani (2013b), Lassiter (2011).

⁴Kratzer (1977), Kratzer (1991).

and Kratzer (1991), that ordering is determined by an *ordering source*, g , which represents, for each world, what the relevant priorities are. To do this, we let the ordering source be a function from worlds to sets of propositions, so that $g(w)$ yields us the set of priorities at that world. Following Kratzer, our ordering will generally be defined as follows:

$$(5) \quad w_1 \leq_{w,g} w_2 \text{ iff } \{p \in g(w) : w_1 \in p\} \supseteq \{p \in g(w) : w_2 \in p\}$$

In other words, w_1 is at least as good as w_2 relative to $\langle w, g \rangle$ just in case w_1 makes true all the propositions in $g(w)$ w_2 does and possibly more.

The domain of ‘ought’ is also constrained by some body of information. We represent this in the semantics with the modal base, f , a function from worlds to sets of propositions.⁵ These propositions represent the relevant information we take to be held fixed in the background; for us, they will be the propositions *known* by some particular agent. On the classic theory, the role of the modal base is simply to restrict the domain of quantification: only worlds in the intersection of the modal base can feature in the domain of quantification.

For the classic semantics, the story ends there. The domain of quantification of the modal is just the set of top $\leq_{w,g}$ -ranked worlds compatible with the information in the modal base.⁶ In other words,

$$(6) \quad \text{BEST}(w, f, g) = \{w \in \cap f(w) : \neg \exists w' \in \cap f(w) : w' <_{w,g} w\}$$

Notice, there is no interaction between the two features which constrain the domain: given $g(w)$, the ordering will not change, even if we vary the modal base.

Before turning to the miners case, we will need to say something about conditionals. I adopt throughout Kratzer’s restrictor theory of conditionals, which says that conditionals restrict the modal bases of modals in their consequents.^{7,8} Where $f + \phi$ is the modal base such that $f + \phi(w) = f(w) \cup \{\phi\}$, conditionals like (2) and (3) have the following truth-conditions:

⁵When it does not cause confusion, I sometimes use the term ‘modal base’ to pick out what is strictly speaking the intersection of modal base.

⁶Here and throughout I make the limit assumption in stating the classic semantics.

⁷Kratzer (1991), Kratzer (2012).

⁸As Charlow (2013) shows, the problem still arises even if we adopt other theories of the conditional, such as those of Stalnaker (1968) and Lewis (1973).

$$\llbracket \text{if } \phi \text{ then ought } \psi \rrbracket^{c,w,f,g} = 1 \text{ iff } \forall w' \in BEST(w, f+\phi, g) : \llbracket \psi \rrbracket^{c,w',f,g} = 1$$

'if ϕ then ought ψ ' is true just in case all the best worlds which are ϕ -worlds are also ψ -worlds.

We are now in a position to appreciate the problem **Miners** poses. Let us make some assumptions about the modal base and ordering source. I do not know the location of the miners, so $\cap f(w)$ will contain worlds in which they are in shaft A and worlds in which they are in shaft B.⁹ Given that I have not made up my mind about what to do, the modal base will also contain worlds in which I block shaft A, block shaft B and block neither. Since this is the only relevant information here, we can simplify and represent my knowledge with this set of worlds:

$$\cap f(w) = \{(A, blA), (A, blB), (A, blN), (B, blA), (B, blB), (B, blN)\}$$

We'll take $g(w)$ to say that I should save as many miners as I can; or in other words,

$$g(w) = \{I \text{ save } 10 \text{ miners}, I \text{ save } 9 \text{ miners}, \dots, I \text{ save } 1 \text{ miner}\}^{10}$$

Given these parameters, the ranking on worlds will be

$$(A, blA), (B, blB) < (A, blN), (B, blN) < (A, blB), (B, blA)$$

The best worlds are those where I block the shaft the miners are in, followed by those where I block neither, followed by those where I block the wrong shaft.

To see what this predicts, look to Figure 3.1. The circled set in figure 3.1(a) is $BEST(w, f, g)$, the best worlds compatible with the evidence. The set circled in figure 3.1(b) is $BEST(w, f + A, g)$. Since all the worlds circled in figure 3.1(b) are worlds where I block shaft A, we predict (2) is true: all the best worlds where they are in A are worlds where I block A; for analogous reasons, we predict (3) is true. However, we don't get the right prediction for (1). As Figure 3.1(a) and

⁹It is shown in Cariani et al. (2013) how the problem arises for a circumstantial modal base. As we are about to see, we can see more generally that the problem is independent of the particular choice of parameters.

¹⁰I use italicisation to refer to propositions i.e. ' p ' denotes the propositions that p .

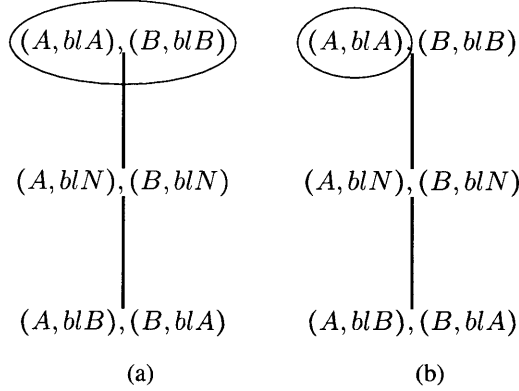


Figure 3.1

3.1(b) show, $BEST(w, f, g)$ will have to be a superset of $BEST(w, f + A, g)$. This means there will be some worlds in $BEST(w, f, g)$ where I block shaft A. But then (1) is false.¹¹

Kolodny and MacFarlane’s diagnosis is that, to make the right predictions, $BEST$ must be defined in such a way that makes it *information sensitive*:¹²

$BEST$ is information sensitive iff there exist f_1, f_2 and w such that:

1. $\cap f_1(w) \supseteq \cap f_2(w)$
2. $BEST(w, f_1, g) \cap \cap f_2(w) \neq \emptyset$
3. $\exists w' : w' \in BEST(w, f_2, g) \ \& \ w' \notin BEST(w, f_1, g)$

More intuitively put, changes in information must be able to sometimes *reorder* possibilities.

To see why the miners case seems to involve information sensitivity, look to figure 3.2. To get (1) right, we let $BEST(w, f, g)$ be the set circled in figure 3.2(a); to get (2) right, we let $BEST(w, f + A, g)$ be the set circled in figure 3.2(b). This

¹¹Subsequent papers, such as Charlow (2013) and Cariani et al. (2013), have shown that the problem is not due to our selection of parameters. In general, we cannot both keep the background ordering of worlds fixed and predict the truth of (1), (2) and (3).

¹²Kolodny and MacFarlane’s term is *serious information dependence*.

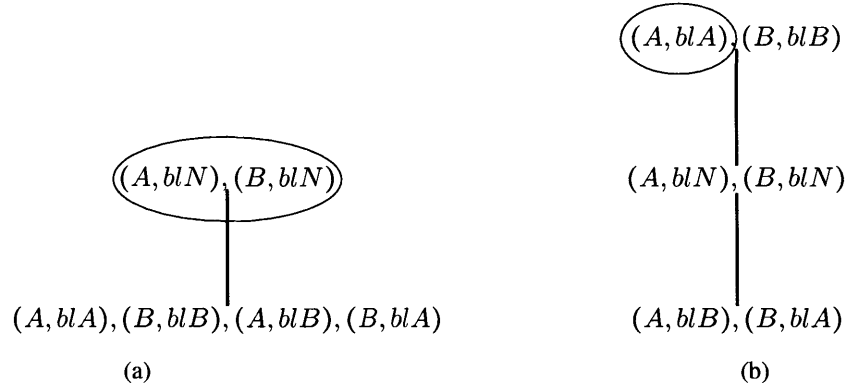


Figure 3.2

gets the judgements right, but it is only because the background ordering shifts that we are able to do this.^{13,14}

3.2 Conservativity

The literature has gone in different ways from this point, taking various morals from the case. I will try to carve out what seem to me the key questions here. In doing so, I will try to get clear on what reasons there might be to favour the various conservative impulses the literature has displayed.

¹³Here is the formal explanation of why information sensitivity is required. Condition 1 follows from the set up of the case and the restrictor semantics: we leave open possibilities where the miners are in A, so $\cap f(w) \supseteq \cap f + A(w)$. Condition 2 follows also from the set-up of the case: the best worlds, the ones where I block neither, include both worlds where the miners are in A and worlds where they are in B. The crucial condition is condition 3. This condition is met just in case $BEST(w, f + A, g)$ contains something that was not originally in $BEST(w, f, g)$. And indeed, there is such a world, (A, blA) .

¹⁴In fact, something somewhat stronger than information sensitivity is needed: $BEST(w, f, g)$ and $BEST(w, f + A, g)$ will have to be *disjoint*. However, information sensitivity captures the main conceptual contrast with the classic semantics, the idea that possibilities get ranked higher as we get more information.

3.2.1 Pragmatic solutions

The first, most straightforward question is whether we really need to add information-sensitivity to our semantics. When semantic explanations fail, it is natural to turn to pragmatics for an answer. By doing so, we might explain the judgments in **Miners** without altering the classic semantics. We'll call a theory that gives that tries to do without any information-sensitivity a *very conservative* theory.

Adding information-sensitivity has met with strong resistance in some quarters.¹⁵ For some, information-sensitivity is a deeply dubious property. Charlow (2013) for instance asks how it could be possible that certain worlds get *better* as more information is added.¹⁶ But this reads too much into the semantics: even when the modal is deontic, our ordering need not represent how good worlds themselves are. Preference orderings can surely change as we get more information: which possibilities seem best to me can change as I gain more information.

That being said, resistance here *is* well-motivated, even if not by the reasons that have been given. Adding information-sensitivity would result in a theory more expressive than the classic theory. As well as the readings provided by the classic semantics, we now predict new possible interpretations of modals where shifting the modal base shifts the ordering. But we should prefer less expressive theories where possible: if we can postulate fewer possible readings and still capture the data, then that is what we should do. In this case, we should wonder if we can capture the appearance of information-sensitivity using some pragmatic mechanisms.

The main kind of very conservative response denies that (1), (2) and (3) are all evaluated within the same context. In particular, it claims that the ordering source used to evaluate (1), the 'subjective' ought, is different from that used to evaluate (2) and (3), the objective 'ought'. As outlined in von Fintel (2012),¹⁷ such a strategy can successfully predict the judgements. Suppose the ordering source for (1) were

$$g_{(2)}(w) = \{ \text{If we know where the miners are, our chosen action yields} \}$$

¹⁵See, for instance, von Fintel (2012).

¹⁶It is not clear that this is Charlow's final view on the matter. (See, for instance, Charlow (2016).) But this thought does seem to account for some of the suspicion of information-sensitivity in the literature.

¹⁷Von Fintel attributes it to unpublished notes by Kratzer.

the optimal outcome for the miners, If we do not know where the miners are, our chosen action yields a still acceptable outcome for the miners and would not yield a less acceptable outcome if they weren't where they in fact are}

We then get the result that (1) is true. If we suppose that the ordering source for (2) and (3) is

$$g_{(2)}(w) = \{I \text{ save } 10 \text{ miners, } I \text{ save } 9 \text{ miners, } \dots, I \text{ save } 1 \text{ miner}\}$$

we predict true readings for both.

Context-shifting strategies are only as plausible as the claim that context might supply those parameters. But these particular parameters are plausible. There is a genuine difference between the subjective and the objective 'ought': the former tracks what we should do given what we know and the latter tracks what would be best for us to do given all the facts. What's more, it gives us an understanding of the case which is intuitively satisfying. This approach cannot be accused of dreaming up *ad hoc* parameters to solve the problem.

3.2.2 Non-probabilistic Solution

There is another aspect of the classic semantics at stake, even if we admit information-sensitivity. If 'ought' is information-sensitive, there is a serious question about where the information-sensitivity comes from. MacFarlane and Kolodny give no clear guidance here — nothing in their system tells us anything about how it is to be generated. But our semantics should be predictive. Given a plausible story about the context, it should tell us *why* information-sensitivity comes into play in cases like **Miners**.

The classic semantics gives us a very clear story about where our orderings come from: they are constructed out of sets of propositions by appeal to entailment. Something like this story might yet hold up, even if the classic semantics must be altered in other ways. This brings us to our second question: can miners cases be explained using only possible worlds machinery? This question is an important one about the structure of our theory of modal vocabulary and its relations

to other important concepts. We'll call a theory that answers *no* to this question a *moderately conservative* account.

It is striking that the judgements in the miners case track natural judgements about the expected utilities: blocking neither shaft has the highest expected utility; and conditional on the miners being in A, blocking A has the highest expected utility (and similarly for B). But such measure-theoretic tools carry far more information than non-measure-theoretic ones: they tell us not just how possibilities are ranked, but carry information about *how much* better certain possibilities are than others. Before allowing these kinds of structures to access our semantics for modals, we should want good reason to think they are needed.

A leading moderately conservative theory is that of Cariani et al. (2013). This semantics allows information-sensitivity but remains close to the spirit of the Kratzer framework in constructing its orderings. Cariani, Kaufman and Kaufman (CKK from henceforth) add a decision problem to the Kratzer semantics, a set of propositions representing the actions available to an agent in a given scenario. For instance, in the miners case, the decision problem δ would be $\{I \text{ block } A, I \text{ block } B, I \text{ block neither}\}$. What ends up being important is not just the decision problem but also the decision problem as restricted by the modal base. Such a restriction is obtained by intersecting each member of the decision problem with the modal base. In our example, the decision problem restricted by f would be $\{I \text{ block } A \text{ and the miners are either in } A \text{ or } B, I \text{ block } B \text{ and the miners are either in } A \text{ or } B, I \text{ block neither and the miners are either in } A \text{ or } B\}$.

Importantly, the relevant orderings on worlds, though information-sensitive, are still generated by means of entailment. An ordering is defined on the members of the restricted decision problem and used to create a corresponding ordering on worlds. A member of the decision problem p is at least as good as another q just in case p entails all the same ordering source propositions as q and maybe more. More precisely:

$$(7) \quad p \lesssim_{w,f,g} q \text{ iff } \{r \in g(w) : p \subseteq r\} \supseteq \{r \in g(w) : q \subseteq r\}$$

A world is then taken to be just as good as the restricted decision problem proposition of which it is a member. Where $\Delta_{\delta,f}(w)$ denotes the decision problem proposition (as restricted to f) containing w , we say that $w' \leq_{w,f,g,\delta} w''$ just in case

$\Delta_{\delta,f}(w') \lesssim_{f,g,w} \Delta_{\delta,f}(w'')$. Our clause for the modal is more or less as before:

$$(8) \quad \llbracket \text{ought } \phi \rrbracket^{c,w,f,g,\delta} = 1 \text{ iff } \forall w' \in BEST(w, f, g, \delta) : \llbracket \phi \rrbracket^{c,w',f,g} = 1$$

where $BEST$, similar to before, is defined as:

$$(9) \quad BEST(w, f, g, \delta) = \{w \in \cap f(w) : \neg \exists w' \in \cap f(w) : w' <_{w,f,g,\delta} w\}$$

3.3 Epistemic Miners Cases

Both reactions seem well-motivated, given the data we have seen so far. But, I'll argue, they do not appreciate how general the problem is. I'll show that non-deontic analogues of miners cases exist. Later I will argue they are problematic for the kinds of approaches we just saw.

Having isolated the epistemic 'ought' in the last chapter, we can show it gives rise to cases analogous to **Miners**. Take the following case:

Exam. Alex and Billy are the top math students in their class and will take their weekly algebra exam tomorrow.

- Alex does best in 66% of the exams.
- Supposing Billy studies tonight, Billy will probably get the best grade: out of exams he studied for, Billy did best in 66% of them.
- Supposing that Billy doesn't study, Billy will certainly not do best. Alex did better in all of the exams that Billy didn't study for.
- Billy always lets a fair coin toss decide whether he will study. He studies just in case it comes up heads.

Imagine we are asked who will do best and consider the following replies:

(10) Alex should do best.

(11) But, if turns out that Billy studied, then he should do best.

Both seem true here. The first seems true because, given what we know, it is more likely that Alex will do best. The second seems true because, were we to learn that the coin came up heads, we would think it more likely that Billy will do best.

Just as in **Miners**, the classic semantics cannot predict the truth of both (10) and (11). We can see that $BEST(w, f, g)$ should both contain worlds where Billy studies and worlds where he doesn't. After all, neither of the sentences

(12) Billy should study.

(13) Billy should fail to study.

have a true reading here. To predict (10), we need the set of best worlds to entail the proposition that Alex does best. So we want $BEST(w, f, g) \subseteq \text{Alex does best}$ and $BEST(w, f, g) \not\subseteq \text{Billy studies}$. To predict (11), we want the set of best worlds which are worlds where Billy studies to be ones where Billy does best. In other words, we want $BEST(w, f + \text{Billy studies}, g) \subseteq \text{Billy does best}$.

For the same reasons as in **Miners**, we cannot get all three. Let AB be the proposition that Alex does best, BB be the proposition that Billy does best and BS be the proposition that Billy studies. Suppose we have an ordering like that in Figure 3.3. The circled set in Figure 3.3(a) is $BEST(w, f, g)$. The updated modal base we use to evaluate (11) must be consistent with $BEST(w, f, g)$: as we said, $BEST(w, f, g)$ neither entails that Billy studies nor that Billy doesn't study. As a result, $BEST(w, f + \text{Billy studies}, g)$ must be the circled set in Figure 3.3(b), a subset of $BEST(w, f, g)$. But if $BEST(w, f + \text{Billy studies}, g)$ is a subset of $BEST(w, f, g)$, then $BEST(w, f + \text{Billy studies}, g)$ also entails that Alex studies. We then fail to predict that (11) is true. So if $BEST(w, f, g)$ contains both worlds where Billy studies and ones where he doesn't, if we make (10) true, we are forced to make (11) false.

Figure 4 shows how information sensitivity helps here too. In Figure 3.4(a) we have an ordering which makes (10) true, with $BEST(w, f, g)$ entailing that Alex does best. In Figure 3.4(b) we have an ordering that makes (11) true, with $BEST(w, f + \text{Billy studies}, g)$ entailing that Billy wins. Since the ordering has

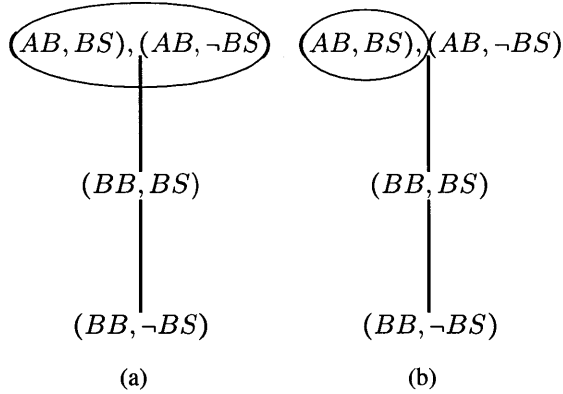


Figure 3.3

changed, *BEST* must be information sensitive.^{18,19}

Running it through the classic semantics reveals that **Exam** has the same problematic structure as **Miners**. We shall now see that unlike the original case, the epistemic miners case is also problematic for our two responses.

3.4 Not Very Conservative

We run into problems when we try to extend the context-change strategy approach to **Exam**.

Before we used the subjective/objective ‘ought’ distinction to get separate ordering sources for (1) and (2) and (3). Now we have to posit multiple ordering sources in **Exam**, distinguishing the ordering source used to evaluate (10) (call it $g_{(10)}$) and that used to evaluate (11) (call it $g_{(11)}$). The first issue is that there is no natural analogue of the subjective/objective ‘ought’ when the relevant ‘ought’

¹⁸Here is the reasoning, more formally: by definition, $\cap f(w)$ is a superset of $\cap f(w) + \text{Billy studies}$; thus the first condition of our definition of information sensitivity is satisfied. Moreover, $BEST(w, f, g)$ is consistent with $\cap f(w) + \text{Billy studies}$ and so there is a world in $BEST(w, f, g)$ which is also in $\cap f(w) + \text{Billy studies}$; thus the second condition is satisfied. Finally, since $BEST(w, f, g)$ and $BEST(w, f + \text{Billy studies}, g)$ are disjoint, the third condition is satisfied.

¹⁹There is one notable point of dissimilarity between **Exam** and **Miners**. **Exam** does not have the same disjunctive syllogism form as **Miners**. But the semantic problem generated by the miners case does not depend on the form of the original example. Even in **Miners**, we only needed to consider (1) and (2) to get a problem going.

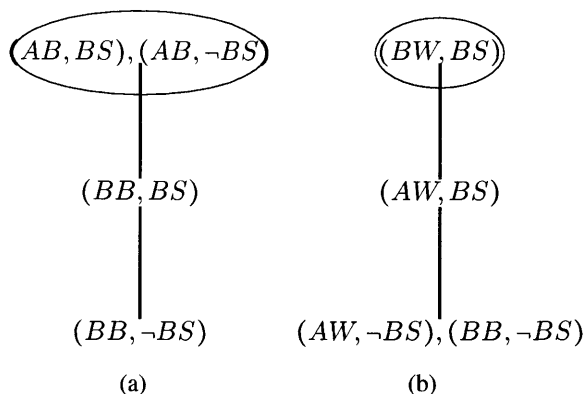


Figure 3.4

is epistemic. The epistemic ‘ought’ is subjective through and through. One could of course cook up an ordering source to play the role of the objective epistemic ‘ought’. But this would betray the motivations of this response: its strength was supposed to be that it relies on a very natural distinction among deontic ‘ought’s.

The second issue is a problem of overgeneration. Notice that if $g_{(10)}$ is available in the context, then we predict that it should be available to evaluate the conditional:

(14) (Even) if Billy studied, Alex should get the best results.

If this were the case, (14) should have a true reading. It would be heard to say:

(15) Even if Billy studied, it is still the case that, just given what we know *now*, Alex should get the best results.

But this is not the case: (14) has no true reading here. The context-shifting strategy thus overgenerates here: it predicts that, in addition to (11), we should also have a true reading of a conditional like (14). This is a bad prediction: overgeneration is the hallmark of too much context-sensitivity.

The proponent of this strategy will have to say that, for some reason, the ordering source used to evaluate (10) is not available for (14). This is puzzling, particularly when (10) and (11) are uttered in sequence. They would be claiming

the context shifts in such a way that, instead of giving (14) a true and non-trivial reading, it delivers instead a *false* reading of the sentence. None of the familiar mechanisms of context-change, such as accommodation in the sense of Lewis (1979), fit this profile. When context changes, it rarely shifts to make otherwise true utterances to *false*.

3.5 Not Moderately Conservative

Adding non-probabilistic parameters was reasonable when **Exam** was not on our radar. Now the situation is quite different. Let us return to Cariani et al. (2013) and see how their reliance on deontic machinery prevents their theory from generalising.²⁰

The first problem is how to interpret the decision problem parameter. Decision problems model the choices an agent must make in a given scenario; but in the scenario we outlined, there is no such choice at issue. In such a case, CKK say that the decision parameter should be set to the set of all worlds. This is designed to make the decision problem redundant: the orderings can only change if the decision problem has more than one cell. This answers the problem of how to interpret the parameter in non-deontic cases, but does not address **Exam**. Here the decision problem must be non-trivial or we will have no information-sensitivity.

Probably the best way to generalise the view is to think of the decision problem as some salient partition of the modal base. In **Exam** the decision problem might be

(16) {*Alex does best, Billy does best*}.

Even still, when we give the semantics a plausible ordering source, it does not make the right predictions.

Take a probability based ordering source:

²⁰Charlow (2013)'s solution seems to face similar issues. For him information-sensitivity is generated by the interaction of two ordering sources, one tracking what is deontically best and another tracking what is actionable. Information sensitivity is generated by the fact that, against different modal bases, different propositions will be actionable. Again, it's not clear how to extend this idea to epistemic cases, as there is no obvious parallel for the actionable propositions.

$$(17) \quad g(w) = \{\phi: \phi \text{ is probable in } w\}$$

To simplify things, suppose that the only things probable on our evidence are that Alex does best, that if Billy studies, Billy does best and that if Billy does not study, Alex does best. This gives us the following:

$$(18) \quad g(w) = \{\textit{Alex does best}, \textit{If Billy studies, Billy does best}, \textit{If Billy doesn't study, Alex does best}\}$$

To predict the truth of (10) we want the following ordering on decision problem cells:

$$(19) \quad \textit{Alex does best} < \textit{Billy does best}.$$

Our current choice of ordering source delivers this. Only *Alex does best* entails any ordering source proposition (namely itself). To predict (11) we want a new ordering, namely:

$$(20) \quad \textit{Billy studies and Billy does best} < \textit{Billy studies and Alex does best}.$$

But this is not the ordering we get. The proposition *Billy studies and Alex does best* entails *Alex does best*. *Billy studies and Billy does best* clearly does *not* entail *Billy does best*. But then it cannot be that *Billy does best* entails all the ordering source propositions that *Alex does best* does. We do not get (20). At best, the two cells can be *incomparable*.²¹

Deontic approaches like CKK's yield information sensitivity, but not in all the right places. Refining decision problems does not generate information-sensitivity for the epistemic 'ought'.

3.6 The Way Forward

Let us survey the ground we have covered. We saw two kinds of responses that dominate the literature on the miners problem. Although they differ in their semantics, deep down these responses share a common assumption. They both assume

²¹Notice that, if we wanted to use instead a normality based ordering source, we would get more or less the same problem. Something like *Alex does best* will have to be among the propositions to predict the truth of (20); but no refinement of the *Billy studies* cell will entail this proposition.

that it is something uniquely deontic that explains information sensitivity. One does this by writing particular kinds of deontic parameters into the semantics, the other by appealing different readings of modals.

I have argued the assumption is false. The interesting phenomenon at play in miners cases cannot be essentially a deontic one because structurally analogous cases exist in the epistemic realm. This should bolster our confidence that information-sensitivity is really needed for ‘ought’ and ‘should’: there is no room for the kind of pragmatic manoeuvres we saw in the original case.

This is further evidence for a *probabilistic* solution to the miners problem. The shifts in ordering we need both for **Miners** and **Exam** are typical of probabilities and cognate notions. Gaining information can make certain possibilities more likely and others less so; similarly, gaining information can make our estimations of how good something is likely to be go up or down. Relating information sensitivity to probability gives a good general explanation of how it arises.²² To show this, I will give a toy semantics for ‘ought’ and show how it can use probabilistic notions to account for miners cases. I will then compare some of its important features to other probabilistic solutions.

3.7 A Toy Semantics

To integrate probabilities into our semantics, I will amend the classic account so that it works with orderings over propositions rather than orderings over worlds.

Rather than quantifying over the best *worlds*, the toy semantics for ‘ought’ quantifies over the intersection of the best *propositions*. To spell this out, say that g is a function from worlds and modal bases to orderings on propositions. Again, we define *PBEST* as a function that collects together the best propositions:

$$(21) \quad PBEST(w, f, g) = \{p \in \cap f(w) : \neg \exists q \in \cap f(w) : q \prec_{w,f,g} p\}$$

²²Carr (2015) shows that there are cases analogous to **Miners** involving ‘probably’. However, I do not think that this is enough to show information-sensitivity is probabilistic. Information sensitivity is crucially a property of a *modal semantics*: it says that their domains behave in a certain way as information shifts. If ‘probably’ instead has a scalar semantics, as Yalcin (2010) and Lassiter (2011) among others argue, then it has no domain of quantification and so trivially is not information sensitive.

Our semantics for ‘ought’ quantifies over the intersection of that set:

$$(22) \quad \llbracket \text{ought } \phi \rrbracket^{c,w,f,g} = 1 \text{ iff } \forall w' \in \cap PBEST(w, f, g) : \llbracket \phi \rrbracket^{c,w',f,g} = 1$$

So ‘ought ϕ ’ is true just in case the intersection of the best propositions contains only ϕ -worlds.

This semantics can make use of probabilistic orderings. To get an ordering reflecting expected utilities, g would be as follows:

$$(23) \quad \phi \lesssim_{w,f,g} \psi \text{ just in case } EU(\phi|f(w)) \geq EU(\psi|f(w)).$$

For the epistemic ‘ought’, we want an ordering that tracks probabilities only up to a threshold probability. After all, ‘ought ϕ ’ can be true even if ϕ does not have probability 1. To achieve this we construct an ordering that ignores differences in probability above a given threshold:

$\phi \lesssim_{w,f,g} \psi$ iff, where θ_g is the threshold, one of the following conditions holds:

1. $P(\phi|f(w)) > \theta_g$; or
2. $P(\phi|f(w)) \geq P(\psi|f(w))$

This ordering effectively ignores differences in probability once the threshold value is passed.

Before we show how our machinery helps with miners cases, note that these are not the only orderings the semantics can use. It could, for instance, take orderings on propositions based on Kratzerian orderings on worlds. Following Kratzer we can lift a Kratzerian ordering to an ordering on propositions by saying that $\phi \lesssim_{w,f,g} \psi$ iff for all $w' \in \psi$ there is a $w'' \in \phi$ such that $w'' \leq_{w,g} w'$. In the cases where the classic semantics does well, we can mimic its results using this kind of lift.

Let us return to our problem cases. In **Miners**, we take advantage of conditionalisation and its effect on expected utilities. Given the set up, the miners are just as likely to be in A as they are to be in B; and outcomes where I save more miners have greater utilities than those where I save less. So let’s imagine that we have probability and utility functions P and U as follows:

$$P(A) = P(B) = 0.5$$

$$U(A \wedge blA) = U(B \wedge blB) = 1$$

$$U(A \wedge blB) = U(B \wedge blA) = 0$$

$$U(A \wedge (\neg blA \wedge \neg blB)) = U(B \wedge (\neg blB \wedge \neg blA)) = 0.9$$

When we conditionalize P on $\cap f(w)$, this will not change the probabilities above. When we do the expected utility calculations,²³ the resulting order on propositions is:

$$block\ neither < block\ A \cong block\ B$$

Thus *block neither* has the highest expected utility and so, given our semantics, we predict (1) to be true in this context.

When we conditionalise on $\cap(f + A(w))$, the probabilities change and so does the ordering on propositions.²⁴

²³We can see that conditionalising P on $\cap f(w)$ will make no difference to any of the values of P which we have specified. So the value assigned to blA will be

$$\begin{aligned} U(A \wedge blA)Pr(A) + U(B \wedge blA)Pr(B) = \\ 1.(0.5) + 0.(0.5) = 0.5 \end{aligned}$$

which will be the same as the value assigned to blB ; whereas as the value assigned to $(\neg blA \wedge \neg blB)$ will be

$$\begin{aligned} U(A \wedge (\neg blA \wedge \neg blB))Pr(A) + U(B \wedge (\neg blB \wedge \neg blA))Pr(B) = \\ (0.9).(0.5) + (0.9).(0.5) = 0.9. \end{aligned}$$

²⁴Our new probabilities will be

$$\begin{aligned} P(A) &= 1 \\ P(B) &= 0 \end{aligned}$$

Recalculating the expected utilities, the value assigned to $(\neg blA \wedge \neg blB)$ will be equal to

$$\begin{aligned} U(A \wedge (\neg blA \wedge \neg blB))Pr'(A) + U(B \wedge (\neg blB \wedge \neg blA))Pr'(B) = \\ (0.9).1 + (0.9).0 = 0.9. \end{aligned}$$

but the value assigned to blA will be

$$\begin{aligned} U(A \wedge blA)Pr'(A) + U(B \wedge blA)Pr'(B) = \\ 1.1 + 0.0 = 1. \end{aligned}$$

The value assigned to *block B* will

block A < block neither < block B

block A now has the highest expected utility. Hence, given our semantics, when the modal base restricted to the worlds where the miners are in *A*, all the worlds in $BEST(w, f + A, g)$ will be ones where we block shaft *A*. Given the restrictor view of conditionals, it follows that (2) is true here. By similar reasoning, we also predict the truth of (3).

Let's turn now to **Exam**. Again, by taking advantage of conditionalisation, we can predict the truth of our sentences:

(10) Alex should do best.

(11) But, if turns out that Billy studied, then he should do best.

Given the set up, the probabilities should be

$$P(\textit{Alex does best}) = 0.66$$

$$P(\textit{Billy does best}) = 0.33$$

$$P(\textit{Alex does best} \mid \textit{Billy studies}) = 0.33$$

$$P(\textit{Billy does best} \mid \textit{Billy studies}) = 0.66$$

Suppose the threshold probability is 0.5. Conditionalising on $\cap f(w)$ makes no difference, as that information is already reflected in the probabilities. Hence, the proposition that Alex does best will pass the threshold and, by our semantics, Alex does best in all worlds in the domain. Hence (10) will be true.

When we conditionalise on $\cap f + \textit{Billy studies}(w)$, the probabilities do change. The probabilities of *Alex does best* and *Billy does best* are now equal to the conditional probabilities above and the proposition *Billy does best* will now pass the 0.5 threshold. So, relative to our more restricted modal base $f + \textit{Billy studies}$, Billy does best in all worlds in the domain. Given the restrictor analysis of conditionals, we then predict that (11) is true in this context.

$$U(A \wedge \textit{block B})P(A) + U(B \wedge \textit{block B})P(B) = 0.1 + 1.0 = 0.$$

This adds more flexibility to our semantics. Unlike the classic semantics, we can access probabilistic orderings in a natural way. And, unlike Kolodny and MacFarlane (2010), we have a story about where information-sensitivity comes from: it comes from using orderings based on probabilistic information.²⁵

3.8 Ordering Propositions Directly?

The key feature of my toy semantics is that we use orderings over propositions directly, without translating them into orderings on worlds or constructing them out of propositions. Other probabilistic approaches to the miners build probabilistic orderings by appeal to *premise sets*, sets of propositions meant to recapitulate the reasoning that generates our orderings. I want to compare my method to theirs and say why I think mine is better.

An ordering on worlds for the epistemic ‘ought’ could not track the probabilities of the individual worlds. For suppose that all worlds compatible with our evidence are equally probable, but that there are simply more ϕ -worlds than $\neg\phi$ -worlds. If our ordering reflects the probability of each world, then *all* worlds compatible with our evidence will be among the best worlds. But then our semantics for ‘should’ will not reflect the fact that ϕ is more likely than not.

We could let the ordering on worlds track the probability of the most probable propositions (excluding the tautology) containing those worlds, saying that $w \leq w'$ iff $P(\phi) \geq P(\psi)$, where ϕ and ψ are the most probable non-tautologous propositions containing w and w' . But the ordering on worlds is no longer really doing much work here; it is the ordering on propositions that is important. While working with worlds is not out of the question here, but it is less natural than simply raising the type of our orderings generally.

It is also hard to construct probabilistic orderings with the right shape using sets of propositions. Carr (2015) constructs expected utility orderings over worlds using ordering sources like

²⁵This is some evidence against Yalcin’s claim that there is no true *epistemic* ‘ought’. If there are information-sensitive readings of the epistemic ‘ought’ then these, I think, cannot be normality ‘ought’s. Normality does not appear to be an information-sensitive notion: varying the information held fixed does not change how normal possibilities are.

$$(24) \quad g(w, r) = \{\text{I perform the action with the highest } r\text{-expected utility in } w.\}$$

where r calculates expected utility using probabilities and utilities from the context. Cariani (2016b) uses a similar technique uses it to construct orderings on propositions. While the top of such orderings will look like mine, there will be unusual divergences further down. In particular, such ordering sources only distinguish between worlds where I do the action with highest expected utility from those where I do not.²⁶

To see this, take the following variation on the miners problem. In addition to blocking a shaft and doing nothing, I can fetch my supervisor who knows where the miners are. But there is a catch: I don't know whether the supervisor is on the surface or lower down in the mines; and if I don't find her in the first place I look, then eight of the miners will drown before we are able to block the correct shaft.

Here too not blocking a shaft has the highest expected utility. But fetching the supervisor has a higher expected utility than simply guessing a shaft: for in the good cases, I save all the miners in both cases; in the bad cases, fewer miners die if I fetch the supervisor than if I guess the wrong shaft. So the expected utility ordering is:

$$\textit{block neither} < \textit{fetch supervisor} < \textit{block A} \approx \textit{block B}$$

But the ordering we would get from using (24) is:

$$\textit{block neither} < \textit{fetch supervisor} \approx \textit{block A} \approx \textit{block B}$$

This is because, apart from *block neither*, all of the relevant options entail that you do not maximise expected utility. That is, none of them entail *any* ordering source proposition.

This makes the wrong predictions when it comes to certain steadfast condition-

²⁶Actually, Cariani's view is a bit more flexible here: in Cariani (2016b) he suggests the following can be an admissible ordering source:

- (i) $g(w) = \{\{A \mid \text{given } A, \text{ it is at least .9 likely that nine people are saved}\}, \{A \mid \text{given } A, \text{ it is at least .8 likely that ten people are saved}\}\}$

But unless these sets of propositions are multiplied, we will still get an ordering which is coarser than using an expected utility ordering directly.

als. As we saw earlier, the antecedents of steadfast conditionals do not appear to have a conditionalising effect; they merely remove worlds where the antecedent is false from the modal base without changing the probabilities. Such readings will be problematic because steadfast readings will be able to access the lower, counterintuitive segment of the ordering.

Consider here the conditional:

- (25) Even if the miners are in shaft *A*, if I don't do nothing, then I should fetch the supervisor.

This sentence is true here: it is a way of stating that, given what I know now, fetching the supervisor is the next best option after simply blocking neither shaft. Moreover, we can see that we must be dealing with a steadfast reading; if we weren't, then it should be instead true that I ought to block *A*. So our ordering source must be something like:

$$g(w, r) = \{\text{I perform the action with the highest } r^* \text{-expected utility in } w.\}$$

where r^* -expected utility is the current, unconditionalised expected utility.

But this ordering source makes (25) *false*. The antecedent of the whole conditional removes the worlds where I block neither of the shafts. But given our ordering source, we can see that this antecedent leaves the rest of the ordering intact. Moreover, according to that ordering source, worlds where I fetch the supervisor are just as good as worlds where I block a shaft by myself. So, of the remaining worlds in the modal base, in some of the best worlds I block *A* and in some I block *B*. This predicts that on its steadfast reading, (25) is predicted to be *false*, as not all of the best worlds are ones where I fetch the supervisor.

The reason we have this problem is these ordering sources fail to draw enough distinctions between worlds. We could improve our predictions by adding more propositions to the ordering source, like this:

$$g(w, r) = \{\text{I perform the action with the highest } r^* \text{-expected utility in } w, \\ \text{I perform the action with at least the second highest } r^* \text{-expected utility in } w.\}$$

But notice that as we increase the number of equally good options, this machinery gets progressively more unwieldy. For suppose we have a variation on **Miners**, with, say, 10 options none as good as any other, our ordering source will need to be something like:

$$g(w, r) = \{ \text{I perform the action with the highest } r^* \text{-expected utility in } w, \text{ I} \\ \text{perform the action with at least the second highest } r^* \text{-expected utility in } w, \\ \text{I perform the action with at least the third highest } r^* \text{-expected utility in } w, \\ \dots, \text{I perform the action with at least the ninth highest } r^* \text{-expected utility in} \\ w. \}$$

And of course we can make the numbers even greater.

More importantly, these kinds of ordering sources do not sit well with the original motivation for having ordering sources. Ordering sources are intended recapitulate our practical reasoning: they are supposed to capture *how* orderings on worlds (or propositions) are constructed. But these kinds of ordering sources clearly do *not* capture how we construct expected utility orderings. They presume we already have an expected utility ordering on propositions and then constructs the ordering all over again. Particularly in more complex cases, this is clearly not the natural way to construct our orderings: if we already have the numbers, we should just use them directly. This kind of ordering source does not do its job of recapitulating our practical reasoning in a meaningful way.

My use of orderings on propositions is then a justified departure from the classic theory. But in other ways, my toy semantics is quite conservative: like the classic theory, it takes 'ought' to be a universal quantifier. Before moving on, I want to note a reason to think our final theory will have to look more like the theory I propose in chapter 2.

One prediction of the toy theory and the classical theory is the following, which I call **No Lotteries**:

If $\alpha, \beta, \dots, \phi$ are jointly inconsistent, then 'ought α and ought β and ... and ought ϕ ' are true iff the set of best worlds is empty.

The reason is simple. If $\alpha, \beta, \dots, \phi$ are jointly inconsistent then they cannot be all

true at any world. So, trivially, the only set of worlds that makes them all true is the empty set. But given a universal semantics for ‘ought’, ‘ought α and ought β and ... and ought ϕ ’ is true just in case each of α , β , ..., and ϕ are all true in all of the best worlds. So the set of best worlds must be empty.

I call this principle **No Lotteries** precisely because it runs into trouble in lottery cases. Take a one thousand ticket lottery: for each ticket, it is probable that it will not be drawn; but it is also certain that some ticket will be drawn. So the set of sufficiently probable propositions will be inconsistent. But then, given our semantics, the set of best worlds for the epistemic ‘ought’ is empty. This means that *all* epistemic ‘ought’s are trivially true (or suffer presupposition failure)—a dreadful result. And, while lotteries bring out the problem, Hawthorne (2003) gives good reason to think that cases with this structure are widespread.

My theory from chapter 2 does not face this problem. Unlike the toy theory here, there is no quantification over the *intersection* of the best propositions. My entry for ‘ought’ existentially quantifies over that set directly. Furthermore, the consistency constraint I motivated was one for *deontic*, not epistemic ‘ought’s. It easily accommodates lottery-like cases where the set of likely propositions as a whole is inconsistent.

As far as the miners problem goes though, I think we can rest content with our toy theory. The miners problem is about determining how the *orderings* for ‘ought’ and ‘should’ work, not a problem about their quantificational force. We have shown that by appealing to probability, we can tell a unified story about information sensitivity. Using orderings on propositions directly in our semantics is a better way to tell that story.

3.9 Semantic Neutrality

I’ve argued that many existing semantic treatments of miners cases falsely assume they arise only in the deontic realm. Before closing, I want to trace out the consequences of my argument for a recent debate about semantic neutrality. That debate has made a similar assumption and should, I think, be reframed as a debate about how much *probability* we should include in our semantics.

A number of authors have asked how committal our semantics for ‘ought’

should be about the rules that govern rational decision-making. As Carr (2015) points out, CKK effectively account for the original miners case by building in a maximin rule: their semantics predicts ‘ought ϕ ’ is true just in case ϕ has the best worst-case outcome. Similarly, Lassiter (2011) and Lassiter (2016) account for it by building a maximise expected utility rule into the semantics of ‘ought’.²⁷ This has prompted many to ask whether there is something improper about being so committal, whether a semantics should not instead be *neutral* about what the correct decision rules are. Philosophers like Carr think that, though accounts like Lassiter’s are predictive, their predictions are ill-gotten gains.²⁸

But if I am right, the focus of this debate is misplaced. Even if we settle the question of how much decision theory our semantics for ‘ought’ can see, we will not have thereby accounted for the epistemic ‘ought’. Adding decision rules to the semantics for ‘ought’ does not settle what is happening in **Exam**. Committing our deontic talk to decision rules is not a general enough strategy.

However, we can ask analogous questions about the relationship between semantics and *probability* theory. I have built little to no probabilistic structure directly into the semantics. This renders the semantics more flexible, but it also requires more assumptions about the context to be predictive. Lassiter, among others, has argued forcefully against this kind of semantics, claiming that a semantics for ‘ought’ and ‘should’ should encode more structure, not less. For instance, we could build it into our semantics for ‘ought ϕ ’ that it looks to see whether the expected value for ϕ , whatever kind of value context supplies, meets a threshold value:

$$(26) \quad \llbracket \text{ought } \phi \rrbracket^{c,w} = 1 \text{ iff } ExpV_{c,w}(\phi) \geq t_c.$$

When V is utility, ‘ought’ will track expected utility. When V is probability, we will calculate the expected *probability* of ϕ , which, other than in cases of higher-order uncertainty, simply collapses to the probability of ϕ .²⁹

This reformulation of Lassiter’s theory empties it of its normative commit-

²⁷Though note that, as of Lassiter (2017), he no longer gives ‘ought’ a straightforward threshold semantics.

²⁸See also Charlow (2016).

²⁹See, for instance, Christensen (2010).

ments. We cannot, as Lassiter does, stipulate that V is always utility; otherwise, we will fail to capture the epistemic 'ought'. But now context can supply values for V that are not utilities but instead mimic what, say, a maximin decision rule would predict. For instance, in the miners case, our maximin-ising value could assign 1 to the proposition that I block neither shaft and 0 to the propositions that we block A and that we block B. As long as the threshold is above 0, no matter what the probabilities are, we will get the result that what we ought to do is what maximin recommends. There is no longer a built in commitment to maximising expected utilities.

But the probabilistic structure remains no matter the conception of value. No matter what the relevant value is, it is expectations about that value the semantics looks at. It is only probability that remains in the semantics across all variations of the relevant value.

Interestingly, the motivations for neutrality change as we move the question from decision theory to probability. Here the objection from encoding controversial assumptions seems to me to have rather less force. It is *conditionalisation* that generates information-sensitivity, not some specific decision rule. And while conditionalisation has its detractors, it is considerably less controversial than any decision rule.³⁰ It is more or less accepted that something in the vicinity of conditionalisation is needed: even if the correct way to update our credences is not by conditionalisation, it will behave a lot like conditionalisation in most cases. By contrast, it is *not* accepted that something in the vicinity of maximin must be true: competitors like Maximise Expected Utility can make wildly different predictions about what it is rational to do.

We have not said enough in this chapter to adjudicate between these approaches.³¹ The question of how much probability theory our semantics should be able to see remains open. But better appreciating the nature of the miners problem moves the debate in the right direction. We should not ask how much decision theory to build into our semantics to solve the miners problem; that will not help in with the epistemic cases. Rather, as I have shown, we should ask how much *probability* we are allowed to build in.

³⁰See, for instance, Lange (1999) and Arntzenius (2003).

³¹However, Agglomeration and Inheritance will both be relevant here.

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