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On Linking Dispositions and Which Conditionals?

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Manley and Wasserman (2008) have provided a convincing case against analyses of dispositions in terms of one conditional, and a very interesting positive proposal that links any disposition to a 'suitable proportion' of a particular set of precise conditionals. I focus on their positive proposal and ask just how precise those conditionals are to be. I argue that, contrary to what Manley and Wasserman imply in their paper, they must be maximally specific, describing in their antecedents complete centred worlds. This suggests a natural semantics for dispositional expressions, which I briefly explore to argue that it lacks uniformity. I end by suggesting a variation on Manley and Wasserman's view which would preserve uniformity, though at the cost of some new puzzling questions.

1.

Dispositions, it has been agreed by most philosophers, are somehow linked to subjunctive conditionals. To take the philosophers' favourite example, a fragile object is one that would break if it were dropped.

On the simplest version of this, the *Simple Conditional Analysis*, we can think of the link between an ordinary-language dispositional predicate such as 'fragile' and the conditional as proceeding in two steps, as follows.

First step. Analyse the dispositional predicate in terms of its stimulus and manifestation condition: N is fragile if and only if N is disposed to break if dropped.

Second step. Analyse the overt disposition ascription in terms of the corresponding conditional: N is disposed to break if dropped if and only if N would break if dropped.

Of course, we all know now that things cannot be quite so simple. Fragile things may be dropped and yet not break; their fragility may be *finked* (Martin 1994, Lewis 1997) or *masked* (Johnston 1992, Bird 1998 speaks of 'antidotes'). Moreover, a non-fragile object may *mimic* fragility if, upon being dropped, it would by some deviant mechanism be caused to break. Manley and Wasserman (2008; henceforth MW) identify the 'standard reply' to these problems with the simple conditional analysis as a strategy of 'getting specific' (p. 63¹). The strategy of getting specific retains the two-step schema sketched above but modifies the kind of stimulus condition that enters into it. 'N is fragile' now expresses, not simply that N is disposed to break if dropped, but instead some contextually determined very *precise disposition* such as:

(SD) N is disposed to break when dropped on Earth from one metre up onto a solid surface with a Shore durometer measurement of 90A, through a substance with a density of 1.2 kg/m³. (p. 63)

1. All page numbers refer to Manley and Wasserman 2008, unless otherwise indicated.

A precise disposition ascription specifies, in positive terms, some particular stimulus condition that is 'paradigmatic for the manifestation of the disposition' (p. 66), thus excluding the supposedly non-paradigmatic conditions in which the disposition is masked. Thus it is thought to be equivalent to, and analysable in terms of, a *precise conditional* such as:

(EC) N would break if dropped on Earth from exactly one metre onto a surface with a Shore measurement of exactly 90A, through a substance with a density of exactly 1.2 kg/m³. (p. 66)²

MW reject the strategy of getting specific: it

ultimately fails, because a disposition can be masked or mimicked even in conditions that are paradigmatic for the manifestation of that disposition—that is, conditions that will not be ruled out by getting more specific. (p. 66)

As an example of such conditions, MW imagine a very sturdy concrete block with an *Achilles' heel*, which withstands heavy forces but has one weak spot: if 'it is dropped onto a *particular* corner at *just* the right angle with *exactly* the right amount of force, an amazing chain reaction will cause it to break.' (p. 67) These conditions might be paradigmatic for the manifestation of fragility; in particular, they might be just those conditions described in the antecedent of (EC). Hence in a context where we use 'N is fragile' to ascribe the disposition expressed in (SD), we would have to count the block as fragile. But there is no such context; hence we do not ascribe any precise conditional such as (EC) by 'N is fragile'.

There is also the opposed problem of *reverse Achilles' heel* (p. 69): a fragile crystal glass may have all except one of the precise dispositions of the kind ascribed by (SD). It does not follow—as it should, on the proposal of getting specific—that in some one context that object does not count as fragile.

Achilles' heels and their reverse already point us into a particular direction: having *one* of the precise dispositions is not enough to be fragile, and having *all* of them is not necessary. We must find something in between.

That impression is strengthened when MW point out further, purely structural problems for any one disposition—one conditional approach. One such problem is that disposition ascriptions admit of comparatives, while a subjunctive conditional (of both the precise and the imprecise variety) is all-or-nothing: it is either true of an object, or it is not. (Another problem, which MW call 'absent stimulus conditions', will be discussed in section 5 below.)

A conditional is an all-or-nothing matter: given a particular context, it is true or false, and that's all there is to it. On the standard semantics of the subjunctive conditional, 'N would M if C' is true if and only if N M's in *all* the closest worlds where C. Universal quantification does not admit of degrees: if there is one non-M-world among the closest C-worlds, the conditional is false. If one disposition were always correlated with one conditional, then disposition ascriptions would have to be all-or-nothing too; but they are not. One object may be more fragile than another. And intuitively, that fact is correlated with the idea that the first object would break in *more* conditions than the second.

MW's central insight is that we need to make room, as the one disposition—one conditional model does not, for this proportional, degree-admitting nature of dispositions. Their positive proposal aims to do just that, while still trying to maintain the link between dispositions and conditionals that their title promises. Its basic idea is that for an object to be fragile, it is necessary and sufficient that the object have *many* of the precise dispositions such as the one ascribed in (SD): not one, not all, but

2. MW also offer an alternative that has intervals instead of exact values, (IC) on p.67. The alternative faces somewhat different problems, but these are not my concern here.

many. The non-fragile block with the Achilles' heel has one, not many, such dispositions; the fragile crystal glass with the reverse Achilles' heel has many of them, even if it does not have them all. Two objects may both have many of the precise dispositions even if one of them has more than the other. Hence the proposal succeeds in solving the problems I have cited so far.

Or does it?

2.

Let me introduce MW's official statement of their view. It is

(PROP) N is disposed to M when C if and only if N would M in some suitable proportion of C-cases.

Note that (PROP) replaces my intuitive expression 'many' by the more flexible 'suitable proportion' (MW's first version had the stronger 'most'); MW's reasons for preferring this flexibility will be addressed in section 5. For convenience, I will continue to use the term 'many' where it is harmless to do so.

(PROP) uses MW's own terminology of C-cases: where C is a disposition's stimulus condition, a C-case is a specific way in which that condition may be fulfilled. MW do not say explicitly *how* specific a C-case is, but they introduce the term by example: 'the antecedent of every precise conditional like (EC) describes a particular C-case'. Hence a C-case may be thought of as complex type of event—more complex than 'dropping', but general enough to apply across a range of possible worlds.³

Note that (PROP) does not explicitly provide the link between disposition ascriptions and conditionals; but we are to understand that N is disposed to M when C iff 'N satisf[ies] a [suitable proportion] of counterfactuals of the form N would M if it were in C-case_n.' (p. 75) MW thus go with the simple conditional analysis in understanding 'N is fragile' as ascribing an imprecise disposition such as 'N is disposed to break if dropped'. They depart from it at the second step, by linking that overt disposition ascription not to one but to many precise conditionals, each with an antecedent that describes a particular dropping-case.

Let us spell this out in terms of the standard semantics for subjunctive conditionals. According to MW's proposal, where N is a fragile object, many dropping-cases are such that if N were in them, N would break; that is, many dropping-cases are such that, in all the closest worlds where that dropping-case obtains, N breaks. If N has an Achilles' heel, then there will be some dropping-cases such that N will not break in all (or indeed any) of the closest worlds where that dropping-case obtains. That's fine: there are plenty of other dropping-cases such that N does break in all the closest worlds where *they* obtain.

So far, all seems well. But now consider an object *x* with a *probabilistic reverse Achilles' heel*, or *PRA* for short. In each of the dropping-cases, *x* has a 99% chance of breaking. However, *x* also has, in any given dropping-case, a 1% chance of not breaking. Perhaps *x* is simply an odd object; or perhaps our own objects are much like it. As John Hawthorne points out,

On those interpretations of quantum mechanics according to which the wave function for a system delivers probabilities of location, it seems that in any mundane situation, there is

3. MW do not explicitly address the question how general their C-cases are, and I will shortly argue that they should not be general at all. However, the conception of C-cases as types of events that occur in more than one world is a natural reading, and confirmed explicitly in an earlier, related paper (Manley and Wasserman 2007); see the last paragraph but one of this section.

always a small chance of some extremely bizarre course of events unfolding. Suppose I drop a plate. The wave function that describes the plate will reckon there to be a tiny chance of the particles comprising that plate flying off sideways. (Hawthorne 2005, 396)

In any given dropping-case, the chance of the plate flying off sideways is much less than 1%; none the less, given the relevant interpretation of quantum mechanics, they are real, non-zero chances. Hawthorne, in the paper quoted, argues that this spells trouble for the standard semantics of counterfactuals: if, as the probabilities suggest, some worlds where the plate flies off sideways are as close as any world where it lands on the floor, then ‘If I had dropped the plate, it would have landed on the floor’ is false. There are, of course, some strategies that try to deal with this problem. But whatever their merits, intuitively it seems that the plate’s fragility is unaffected either way. In (say) 99% of the relevant worlds where it is dropped, the plate breaks; in 1% it flies off sideways. Whatever we say about the counterfactuals, the plate surely is fragile!⁴

In the original case of reverse Achilles’ heels, the problem was easily neutralized because all the worlds where the crystal glass was dropped in the problematic way but did not break were packed into a single C-case; and what is one C-case in which the crystal glass does not break against many in which it does! The problematic worlds were all of one type, the type described in the antecedent of one conditional like (EC). With the probabilistic version, the problematic worlds cannot be so neatly packed into one C-case; there may be one, and only one, of them in every set of worlds that exemplify a given C-case. They have nothing in common, except for exhibiting a remarkable chance event; but that is hardly a specification that should enter the antecedent of a conditional like (EC).

What has gone wrong? I believe that MW have done a disservice to their central insight by allowing the all-or-nothing nature of the counterfactual, the universal quantifier implicit in its semantics, to retain some small foothold. The PRAs do their destructive work precisely because a given C-case counts towards an object’s fragility only if the object breaks in *all* worlds that exemplify it. The central insight of MW’s paper was that dispositions are not all-or-nothing, they are a matter of proportions, of more-or-less. If that insight is to be applied more thoroughly, we should leave no foothold for universal quantification.

One way of doing this is to simply leave no sets of worlds for the universal quantifier to quantify over without triviality. I said that C-cases are event-types, which typically obtain at more than one world, and it was this that created the problem I have described. To avoid the problem, we might construe C-cases more finely, taking each C-case to obtain at just one world; or we might simply identify each C-case with one world. (A world includes, of course, its entire history both before and after the dropping. So on this interpretation, even if the plate’s breaking upon being dropped is a chance event, it will turn out one way or another in any given C-case.) In fact, while their introduction of the term ‘C-case’ by the example of (EC) pointed us into the more coarse-grained direction I have pursued up to now, MW add a footnote which suggests (but no more than suggests) a strategy along these more fine-grained lines: ‘C-cases are to be construed less coarsely than worlds: a world may contain many dropping-cases’ (p. 75, fn. 19). While this does not preclude that C-cases are, on another dimension, also construed more coarsely than worlds (such that one C-case obtains at many worlds), it does suggest an alternative way of thinking about C-cases: as centred worlds, that is, triples of a world, an object, and a (possibly extended stretch of) time. Indeed, in a closely related paper, Manley and Wasserman (2007) explicitly suggest that ‘[C]-cases are to be understood not as worlds, but roughly

4. I do not wish to endorse the claim that given quantum mechanics, all counterfactuals are false. Rather, I hold the following two theses: (1) even if that claim were true, disposition ascriptions would not be affected; and (2) if it is not true, then there may still be localized examples, that is, individual objects with a PRA. The possibility of one such object is enough as a counterexample to MW’s position as presently construed.

as centred worlds, with the relevant object at the centre being subjected to some specific stimulus condition’.

On this alternative way of construing C-cases, the original problem will no longer arise. Let a *maximally precise* conditional be one that describes in its antecedent an entire centred world. Then on the revised version of MW that I am advocating, N is disposed to break iff many maximally precise conditionals are true. A maximally precise conditional ‘N would break if N were in C-case_n’ is true iff N breaks in all the worlds where C-case_n obtains, that is iff N breaks in the one and only centred world where C-case_n obtains, namely, C-case_n itself. N will satisfy most (or many) conditionals of this type if and only if N breaks in most (or many) worlds that are C-cases.⁵

It is clear that this reading of MW’s proposal solves the problem of PRAs. But it does not appear to be Manley and Wasserman’s own favoured reading. The above-quoted identification of C-cases with possible worlds is immediately retracted in favour of the more coarse-grained conception: ‘One respect in which [C]-cases might differ from centred worlds is that they need not be construed as so fine-grained that they specify different facts not causally relevant to the manifestation condition.’ (Manley and Wasserman 2007, p. 72)

I believe that this is a mistake, and that MW should embrace the fine-grained conception of C-cases as centred worlds. I will argue for this in the next two sections by first addressing an alternative solution to the problem of PRAs, which goes with the coarse-grained conception and is suggested by MW themselves, and then dispelling an apparent reason to favour the coarse-grained reading over my proposed more fine-grained one. I return to my proposed reading in section 5.

3.

In discussing potential problems and revisions for their account, MW suggest a way of dealing with PRAs that differs from the one I have just proposed.⁶ On p. 78, they consider

a view where the laws are indeterministic, and for some C-cases the most we can say is that N would *probably* M in them (rather than that it would M in them). To modify our account for such a view, it would not be enough to replace ‘would’ with ‘would probably’ in (PROP) ... after all, it should count more towards a thing’s degree of fragility if it would break with a very high degree of probability in a given C-case than if it would only break with a moderately high degree of probability.

While the considered replacement of ‘would’ with ‘would probably’ is ‘not enough’ for the purposes of establishing the *degree* of an object’s fragility, MW appear to be satisfied with it for the purposes of simply stating the conditions for an object to be fragile.⁷ The suggestion, then, is that instead of giving up on (EC) and its ilk in favour of maximally specific conditionals, as I have done, we might modify (EC) itself by inserting the adverb ‘probably’.

One question that naturally arises for this suggestion is what the *scope* of ‘probably’ in the revised conditionals is meant to be: is it part of the conditional’s consequent, modifying the sentence ‘N breaks’? Or does it modify the conditional itself, weakening its quantificational force? Semi-formally,

5. For ease of expression, I will identify C-cases with centred worlds (rather than with the maximally specific conditions that obtain at only one world). I will also mostly drop the word “centred”. All that I say can be accordingly rephrased and should still be true.

6. Thanks to David Manley for pointing this out to me.

7. This is confirmed by their explicitly endorsing the reformulation in Manley and Wasserman 2007, p. 73.

the two readings may be distinguished thus: on the first reading, the relevant conditionals are of the form

(1) N is in C-case_n $\square \rightarrow$ probably: N breaks

On the second reading, the standard counterfactual is replaced by something weaker, which we may write $\square \Rightarrow$ (read: if it were the case that ... , then it would-probably be the case that ...), yielding conditionals of the form

(2) N is in C-case_n $\square \Rightarrow$ N breaks.

(1) retains the universal quantification implicit in the counterfactual conditional, but does not require that N *breaks* in the worlds where C-case_n obtains; instead, it requires that N's breaking have a high probability in those worlds. (2), on the other hand, does give up on the universal quantifier. The most straightforward semantics for the non-standard operator $\square \Rightarrow$ is one akin to MW's treatment of dispositions: (2) is true if N breaks, not in all, but in *many* or a *suitable proportion* of the worlds where C-case_n obtains. Both readings of the suggested revision solve the problem of PRAs. None the less, I believe there is reason to prefer the proposal I have outlined in the previous section.

Whichever of the two readings, (1) and (2), is adopted, MW's account of dispositions has it that an object N is disposed to M when C just in case many (a suitable proportion) of the revised conditionals are true. Let us plug the two types of revised conditionals, with their envisaged semantics, into that characterization.

Take (2) first. A typical characterization of a disposition such as fragility, fully spelled out, now reads: N is disposed to break when dropped just in case *many* dropping-cases are such that, in *many* of the closest worlds where that case obtains, N breaks.

Note that the proportional quantifier, 'many', is applied twice: first to event-types, the C-cases; and second to the worlds where any particular such event-type obtains. Pictorially speaking, we first sort the relevant possible worlds into groups or clusters, each cluster corresponding to a particular C-case; we then say that many of those clusters are such that many of the worlds in the cluster are worlds where N breaks. What counts as 'many' has to be fixed by context. In fact, it has to be fixed twice over; there is no reason to be sure that it is the same in both cases. It will be noted immediately that my proposal is simpler: it involves the application of that same quantifier, but it involves it only once. *Ceteris paribus*, that gives us reason to prefer my proposal; the burden of proof is on the advocate of complexity. This burden might be discharged by showing that the more differentiated structure of the more complex proposal provides explanatory benefits which my simpler proposal lacks. But as far as I can see, MW show no such thing in their original article. Alternatively, the burden might be discharged by showing that there is independent reason to prefer the coarse-grained conditionals over my fine-grained ones. I will consider this line of response in the next section.

Let us now turn to (1). Plugging this type of conditional into MW's account of dispositions, we get: N is disposed to break when dropped just in case many dropping-cases are such that in *all* worlds where that case holds, N has a high (objective) probability of breaking. Note that here, again, there are two thresholds to be set by any given context: one for the suitable proportion of dropping-cases, and one for the probability of N's breaking in the corresponding worlds; simplicity, again, is on the side of my proposed solution.

More importantly, on this reading the manifestation of a disposition, as opposed to its objective chance, drops entirely out of the picture. It does not matter whether N actually breaks in any of the C-case worlds, as long as it has the right chance of breaking. (Given plausible assumptions about objective chance, N *will* break in the right proportion of worlds, but that is now irrelevant to its fragility.) This is intuitively puzzling: are we not now describing a disposition *to have a high chance of*

breaking, as opposed to a disposition to break?

I have not refuted either version of MW's suggestion; I have merely shifted the burden of proof. As long as that burden is not discharged, I submit that we have reason to prefer my own suggestion: the identification of C-cases with centred worlds, and accordingly the appeal to maximally specific conditionals.

One way of discharging the burden of proof would be to give us independent reason for preferring the less-than-maximally precise conditionals. One such reason, and hence a potential problem for my proposal, may be extracted from MW and will be addressed in the following section. As a challenge to my proposal, I think it can be met, and it provides some interesting insight into the link between dispositions and conditionals in general. After thus defending my proposed version of MW's view, I will move on to situating it in the larger context of modal semantics. That context gives rise to a new challenge, which will, however, point us in a direction very different from the one explored in this section.

4.

One apparent reason to prefer the more coarse-grained conditionals (such as (EC), but also (1) and (2) in the previous section) arises from the very motivation that MW provide for mending the link between disposition ascriptions and subjunctive conditionals.

Having argued against extant versions of conditional analyses, MW point out that

it is hard to believe that there is *no* interesting connection between conditionals and ordinary dispositional ascriptions. ... The connection need not be reductive, but it should at least explain, for example, the way that ordinary beliefs about dispositions guide action. When we learn that something is fragile, we treat it with care because we know that many kinds of rough behaviour would lead to breaking. A theory of dispositions that dismisses this connection is simply abnegating its explanatory burden. (p. 73)

Note, incidentally, that MW's explanation of the action-guiding power of disposition beliefs does not explicitly appeal to conditionals. The explanation appeals to types of events, probably the C-cases of my first interpretation ('kinds of rough behaviour'), and perhaps even to causal connections ('lead to its breaking'); those things are naturally, but not trivially, connected with subjunctive conditionals.

That said, it still seems intuitively true that when we know of a vase that it is fragile, we take care not to drop it because we know that many conditionals of the form 'if it were dropped thus-and-so, it would break' are true. We cannot quite say what is to replace thus-and-so in the individual conditionals. But (EC) and its ilk appear to be good candidates.

Now, on my proposed reading of MW's thesis, (EC) is not the kind of conditional of which an appropriate proportion must be true if an object is fragile. Rather, we now have maximally specific conditionals that describe in their antecedent a complete centred world. Those, it might be protested, were not the conditionals we had in mind when assenting to MW's motivation for preserving the link between dispositions and conditionals; the maximally precise conditionals are far too specific, and far too complicated, to be action-guiding. How could any such thing play a role in our practical deliberation processes?

Whether or not such protest would be justified in its preference for (EC) over the maximally precise conditionals now at issue, it can be answered. Given the possibility of probabilistic reverse Achilles' heels, 'N is fragile' is not logically equivalent to most conditionals of (EC)'s type being true. But logical equivalence is not the only interesting link that there might be between dispositions and

conditionals. If PRAs are relatively rare, it will be true in *most* cases that if a suitable proportion of the maximally precise conditionals is true (and hence N is fragile), then a suitable proportion of the less-than-maximally precise conditionals such as (EC) will be true too. Knowing that an object is fragile, we could thus be reasonably confident that a suitable proportion of those less-than-maximally specific conditionals is true. We might, of course, be wrong; but that should not prevent us from rationally guiding our actions by what we have good reason to believe.

But what if the case of PRAs is more pervasive—if, to take the extreme case, Hawthorne’s challenge cannot be answered and all conditionals of (EC)’s type are false? Then our apparently reasonable beliefs about the link between dispositions and conditionals of that kind are false. But that would not, in this case, be an isolated error; if Hawthorne’s challenge cannot be met, then almost all our beliefs concerning counterfactual conditionals are false. The error concerning the link between dispositions and conditionals would then be merely one instance of a much more general mistake that we make, and not an objection to the envisaged link (or lack thereof) between disposition ascriptions and conditionals.

5.

On our current understanding of C-cases, (PROP) reads much like a semantic clause for ‘N is disposed to M when C’: it applies a kind of quantification (‘a suitable proportion’, in the original statement: ‘most’) to a restricted set of worlds (the C-cases). Insert quotation marks and the truth predicate on (PROP)’s left-hand side, and you have a set of truth conditions for sentences containing disposition terms. So while MW do not explicitly ask or answer the question of a modal semantics for disposition terms, it would be surprising to say the least if (PROP) was true without also being a guide to that semantics.

According to standard modal semantics (classic treatments: Kratzer 1977 and 1991, Lewis 1979), modal expressions can be interpreted as quantifying over possible worlds. Modal expressions include not only the adverbs ‘necessarily’ and ‘possibly’, but such more wide-spread locutions as the modal auxiliaries ‘can’ and ‘must’, ‘would’ as it occurs in the subjunctive conditional, and dispositional predicates, or rather, their typical suffixes ‘-ile’, ‘-ible’ or ‘-able’. Each modal expression comes with a specific *modal force*, a particular quantifier that is common to all uses of the expression. The modal force is applied to a *modal base*, a (usually restricted) set of worlds which may vary widely from one context to another. The modal force may simply be existential or universal quantification (as in ‘can’ and ‘must’, respectively), but arguably it may also be proportional: as a first pass, ‘probably *p*’ is true in a given context if and only if *p* is true at *most* (or at a very high proportion) of worlds in the contextually determined modal base. What counts as ‘most’ (or as ‘very high’) will be subject to some amount of contextual variation too.⁸

(PROP) fits into that schema in an obvious way. Like ‘probably’, disposition terms have a proportional modal force: that of a *suitable* proportion. Their modal base is a set of worlds restricted by the stimulus condition, C—in other words, it is the set of C-cases.⁹ (PROP) leaves the modal force of dispositional terms deliberately vague so that we ‘can think of contexts as providing the standards for “fragility” by establishing a requisite proportion of C-cases in which an object would break, for

8. I borrow the convenient terminology of modal force and modal base from Angelika Kratzer, though I do not follow her presentation in the details. My presentation is adapted to the spirit of MW, which is closer to David Lewis’s version of the semantics. Kratzer’s account of gradable modals works not in terms of proportional modal forces, but of an ordering source, or less formally: in terms of closeness to a contextually determined ideal.

9. Having *centred* worlds in the modal base is unusual, but I do not see that it does any damage.

example' (p. 76). How does context do that? In our toy semantics of 'probably' above it is plausible that the vagueness of 'most' is a virtue: what counts as probably true varies between contexts, and which proportion counts as most of the cases varies as well. The equivalence is plausible because our intuitions about what is probably true and about what happens in most cases arguably vary together. (MW make an analogous claim in support of their principle (MORE) on p. 82.) What about our intuitions concerning what is fragile and what breaks in a suitable proportion of dropping-cases? 'Suitable' cries out for supplementation, in a way in which 'high' and 'most' do not: suitable for what? As far as I can see, MW do not indicate an answer to that question. (The first answer that comes to mind is that it must be a proportion suitable for capturing the truth conditions of the disposition ascription at issue.) Without such an answer, the modal semantics is hard to evaluate: there are no intuitions concerning suitable proportions that might or might not vary together with intuitions about disposition ascriptions. The semantics is also less informative than we might wish. For it is hard to find a quantifier that corresponds to the elusive 'suitable proportion' and thus characterizes the modal force that is common to disposition ascriptions. The problem is not that there is no such quantifier in English; we could always define one by stipulation. It is that we cannot even begin to see what the appropriate definition would be.

This is not, of course, any simple slip on MW's part; they have very good reason for using the elusive 'suitable'. Consider MW's last argument against the one disposition–one conditional view: the problem of absent stimulus conditions. As MW point out,

there are plenty of dispositions that do not have any particular stimulus condition. Suppose someone is highly disposed to talk, but there is no particular kind of situation that elicits this response in him. He is disposed to talk when happy, when sad, with others or by himself—he is just generally loquacious. ... We are not given an ascription of the form 'N is disposed to give M in C', we are only given 'N is disposed to M ... So with nothing to put in place of 'C', how can we construct a conditional of the form 'N would M if C'? (p. 72f.)

(Note that a disposition without a particular stimulus condition need not be one that manifests spontaneously. It may simply be that the various conditions that trigger its manifestation—in a given individual, or across individuals sharing the disposition—are too diverse to allow for a general description to be put in place of 'C'.)

MW claim to have solved this problem with their positive proposal:

Because the C-cases in our domain need not be restricted in any way, absent stimulus conditions are not a problem for (PROP). We can simply allow that N is loquacious just in case N would talk in a suitable proportion of situations—any situations at all. (p. 77)

Where in the case of dispositions with a particular stimulus condition, we can fill in some non-trivial general characteristic C that applies to all C-cases, no such thing is true of a disposition without such a stimulus condition. In that case, 'C' may be thought of as any tautology; there is no restriction on C-cases except for some very general ones that apply to all C-cases—that N exist in them, that they share our laws of nature, etc.

To see the import of this, consider two dispositions: fragility₁, which is the disposition to break when dropped, and fragility₂, which is the disposition to break in any situation. (MW surmise that fragility really is fragility₂: p. 72.) What are their respective modal forces? Since they are proportional, we may approximate them by percentages. (We cannot really express them by anything so simple, see MW's concerns about comparing proportions of C-cases on p. 79ff.) Take 'N is fragile₁'. Given our supposition, the modal base of the dispositional expression here is restricted: it consists only of dropping-cases, that is, centred worlds in which N is dropped. The suitable proportion may thus be rather high. Given an everyday context, a typical fragile thing will break in at least, say, 70% of the

cases in which it is dropped. It need not always be this high: MW imagine that Fred, who is disposed to become violent in the evening, would become violent on about one third of evenings (p. 75). This disposition thus comes with a modal force of at most 33%.

Now consider the disposition of fragility₂. Its modal base is entirely unrestricted (except in the general ways mentioned above). It includes an enormous variety of situations in which N is safely stored away in a cupboard or used with great caution.¹⁰ In those cases, there would simply be no reason for expecting the glass to break. Perhaps it breaks spontaneously in some minute fraction of them. Most of the manifesting, however, will go on in cases where it is dropped, struck or otherwise suddenly stressed. And those cases are themselves only a minute fraction of the C-cases, cases where the glass is in any situation whatsoever. The percentage of breaking-cases among the C-cases for fragility₂ may plausibly be less than 1%. None the less, the fragile₂ objects differ from the non-fragile₂ objects: while both break only in a minute proportion of cases, the fragile₂ objects break in a larger proportion than the non-fragile ones. Their modal force may be akin not to that of 'probably', but perhaps to that of a nonnegligible possibility.

The 'suitable proportion' that is a dispositional expression's modal force, then, may vary so widely as to be close either to that of (non-negligible) possibility (fragility₂: < 1%), or to that of high probability (fragility₁: 70%). It may lie anywhere between these two (plausibly, that is where the disposition to get angry in the evening is located), and we may expect there to be cases that lie above fragility₁. It seems that the modal force of a disposition expression can cover the entire spectrum from existential quantification ('at least one') to universal quantification ('all')—with the exception, perhaps, of the end-points themselves. But a quantifier that covers the entire quantificational spectrum is no quantifier at all, except in a trivial sense.

The modal semantics for disposition terms suggested by (PROP), then, does not provide what a modal semantics standardly should provide: a quantifier (which may or may not exist in natural language) that non-trivially characterizes the modal force of the kind of modal expression at issue, disposition terms. Granted, MW never claimed to have provided such a semantics. But it would, I think, lessen the attraction of their proposal if there was no link between (PROP) and the semantics of disposition terms.¹¹

The challenge, then, is to provide a credible semantics for disposition terms that is in line with MW's (PROP). There are two possible lines in responding to this challenge (apart from simply refusing it). One is to provide a more substantial reading of 'suitable' that gives us some grasp on how context determines the suitable proportion of C-cases for any given disposition ascription. Another is to 'compress' the range of the quantificational spectrum that may be covered by a dispositional expression's modal force so that we can find or define a non-trivial quantifier to characterize it. I will end by sketching that second line, which is the one I prefer. It may begin by taking a cue from the linguist Angelika Kratzer, who identifies the modal force of 'fragile'¹² as that of possibility. (Kratzer 1981, 64)

We cannot, by hypothesis, increase the modal force of dispositional terms such as 'fragile₂' from

10. Those are not cases of finking or masking, at least not in any usual sense. Finks and masks apply only once a non-trivial stimulus condition is induced; figuratively speaking, they intervene when the trigger has been pulled. In our cases no trigger is pulled, though of course the trivial stimulus condition (being in any situation at all) is present.

11. In addition, the problem of the previous section might resurface: how is knowledge of dispositions to be action-guiding through our knowledge of (or reasonable belief about) the associated conditionals if there is no way of telling whether many or just a few of those conditionals are true?

12. To be precise, Kratzer is writing about German and hence uses the adjective "zerbrechlich". But she gives no reason to doubt that she would apply the same to the English translation that she herself provides, "fragile".

something much like possibility to something more like high probability. But we can decrease the modal force of 'fragile₁' by simply not using its stimulus condition to restrict the C-cases. We might thus depart from the traditional conditional analysis (and any of its specific descendents) at an earlier stage than MW, namely, at the first step: perhaps disposition ascriptions *in general* are to be understood simply in terms of their manifestation conditions, for instance, as 'N is disposed to break', fullstop. In a second step, we might then apply to all disposition terms the strategy that MW apply to dispositions such as fragile₂: N is disposed to M if and only if in a suitable proportion of cases, N breaks. The 'cases' are now any (centred) worlds whatsoever, without any restriction apart from the general ones, for instance, that they share our laws of nature. The 'suitable proportion' can be uniformly low, and close to some such modality as a non-negligible possibility.

One consideration in favour of such a strategy is the observation that our ordinary-language dispositional terms generally specify only the disposition's manifestation (*fragile*, *soluble*, etc.; in some cases, the dispositional adjective made its way into English without the corresponding verb: e.g. *loquacious*, from the Latin for 'to talk', *loqui*). Needless to say, the suggested strategy will be faced with difficulties of its own. One is the question how we are to treat such overtly two-part disposition ascriptions as 'N is disposed to sneeze when near flowers'. Another is the question of how dispositions, so understood, can be action-guiding via our knowledge of any conditionals.¹³

This is not the place to answer those questions. But for those with an inclination both to accept MW's central insight, and to prefer a more uniform semantics of disposition predicates, the effort of answering them may be worth the price.¹⁴

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13. For more on the suggested view, see chapter 2 of Vetter (2010). To indicate my answer to the second question, let me note again that causal notions seemed to surface in MW's explanation of how knowledge of dispositions guide our actions. Knowing that N is fragile *and* that dropping is a typical cause of things' breaking, in the absence of any evidence that N is an atypical object, is sufficient for reasonable belief that many kinds of dropping would make N break.

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