

Dispositions, Conditionals, and Counterexamples

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In an earlier paper in these pages (2008), we explored the puzzling link between dispositions and conditionals. First, we rehearsed the standard counterexamples to the simple conditional analysis and the refined conditional analysis defended by David Lewis. Second, we attacked a tempting response to these counterexamples: what we called the ‘getting specific strategy’. Third, we presented a series of structural considerations that pose problems for many attempts to understand the link between dispositions and conditionals. Finally, we developed our own account of this link, which avoids all of the standard counterexamples and comports with the relevant structural considerations. In this paper, we reply to some objections.

We are grateful to all of our commentators, and to the editor of *Mind* for the opportunity to respond. Our replies are organized alphabetically.

1. Reply to Bonevac, Dever, and Sosa

Bonevac, Dever, and Sosa raise a number of important issues in their paper. But we should begin with some points of clarification.

1.1 Interpretative issues

At the outset of their paper, Bonevac, Dever, and Sosa (BDS) write that we ‘join the chorus of philosophical orthodoxy in holding that [certain] counterexamples doom the project of analysing dispositions in terms of conditionals’ (p. 1144). We found this claim doubly surprising—first, because we were unaware of any such chorus and, second, because we would not join it even if one existed.

Let us begin with the first point. Why think this is the view of philosophical orthodoxy? For evidence, we turned to an earlier

paper in which BDS claim that a long line of philosophers have endorsed a general argument against ‘any conditional analysis of any dispositional property’.¹ The philosophers implicated are David Lewis, Alexander Bird, Mark Johnston, Michael Fara, C. B. Martin, Robert Shope, and Crispin Wright. But as far as we can tell, most of these philosophers are innocent of the charge. For example, the evidence offered against Lewis, Bird, and Fara consists of the following quotes:

The simple conditional analysis has been decisively refuted by C. B. Martin. (Lewis 1997, p. 227)

Thanks to Charlie Martin, the conditional analysis ... has long been known to be incorrect. (Bird 1998, p. 227)

It is now widely agreed that the simple conditional account is mistaken. (Fara 2005, p. 4)

But there are at least three problems with this evidence.

First, BDS misquote. Bird does not write that Martin refuted ‘the conditional analysis’; he writes that Martin refuted ‘the *simple* conditional analysis’.²

Second, BDS over-generalize. Correctly quoted, each author is here rejecting a single version of the conditional approach to analysing dispositions, namely the *Simple Conditional Analysis*:

(SCA) N is disposed to M when C iff N would M if C

Yet on this basis each is interpreted as rejecting *every* analysis of dispositions in terms of conditionals.³

¹ That ‘some conditional analyses fail’ is ‘uncontroversial’; the problem, they say, is that certain counterexamples have ‘been taken to show the impossibility of *any* conditional analysis of *any* dispositional property’ (Bonevac, Dever, and Sosa 2006, p. 274, italics ours).

² BDS also omit a statement of the simple conditional analysis from the middle of the quotation.

³ This is not to deny that Bird and Fara express doubts about the project of analysing dispositions in terms of conditionals (see Bird 1998, p. 232 and Fara 2005, p. 61). However, Fara does not take the standard counterexamples to establish, by themselves, that there can be no conditional analyses of dispositions. Nor does he overlook the possibility of an analysis using non-standard conditionals, such as conditionals that do not license *modus ponens*. So he is hardly a suitable target of BDS’s methodological observations. As for Bird, he appears to despair of a conditional analysis because the problems giving rise to counterexamples start to feel endemic to the project—much the way that many epistemologists have come to despair of a ‘fourth condition’ on knowledge after decades of failed Gettierology. (Contrast BDS’s use of this analogy on pp. 1145–6.)

Third—and most importantly—BDS ignore the fact that David Lewis is a leading defender of the conditional approach. In the very paper they cite, Lewis endorses a conditional analysis of dispositions, namely the *Lewisian Conditional Analysis*:⁴

(LCA) N is disposed to M when C iff N has some intrinsic property B in virtue of which, were it to retain B, it would M if C

In the same way, Mark Johnston is listed as an opponent of the conditional approach, yet he defends a version of that view in the very paper cited (1992, p. 233).⁵

This pattern continues in the present paper, where BDS quote us as writing that ‘the simple account is far too simple. It founders on C. B. Martin’s problem of finks’ (Manley and Wasserman 2008, p. 60). From this they apparently conclude that we reject every version of the conditional analysis. But this is a *non sequitur*. Like Lewis and Johnston, we reject the *simple* conditional analysis, but we do not reject *every* conditional analysis. In fact, we spend roughly half of our earlier paper developing and defending the following link between dispositions and conditionals:

(PROP) N is disposed to M when C iff N would M in a suitable proportion of C-cases⁶

We take (PROP) to capture a necessary connection between dispositions and conditionals, but we withhold judgement on the priority of either side of this equation (whether metaphysical or conceptual). For that reason, we are agnostic about whether or not (PROP) counts as a

⁴ Here it is worth quoting Lewis at length:

Once we scrap the *simple* conditional analysis, what should we say about dispositions? Martin’s own response is radical: a theory of irreducible dispositionality... But those of us whose inclinations are more Fabian than revolutionary, and who still back one or another of the usual approaches to lawhood and causation, may well suspect that Martin has overreacted... Rather than starting with irreducible dispositionality, as Martin does, we shall start with fairly widely shared ideas about properties, causation, lawhood and counterfactuals; and on this foundation *we shall hope to build a reformed conditional analysis of dispositions*. (1997, p. 148, italics ours)

⁵ Moreover, the quotes they offer from Shope do not demonstrate that he held the very strong view ascribed to the ‘chorus of orthodoxy’. Among the philosophers on their list, that view appears to be held only by Martin (1994, p. 7), and perhaps Wright (1992, p. 118).

⁶ For further details, see Manley and Wasserman 2008, Sect. 5.

genuine *analysis*. But we are certainly happy to recommend (PROP) to those who seek such a reduction.⁷

In short, there does not appear to be any chorus proclaiming doom for the project of analysing dispositions in terms of conditionals. And even if there were, we would not join in.

1.2 Methodological observations

Having clarified our position, we can now address the methodological observations made by Bonevac, Dever, and Sosa.

The first methodological observation is that a counterexample to one proposal may not be a counterexample to another.⁸ The second is that one cannot refute a view by simply forming an intention to provide counterexamples.

We agree with both observations, but fail to see their relevance. Like those in the philosophical orthodoxy, we take counterexamples involving finks and masks to provide false instances of (SCA) and (LCA), respectively, and thus to falsify the universal generalizations represented by those schemas.⁹ But we do not take those cases to provide counterexamples to other proposals; nor do we appeal to intentions to provide such counterexamples.

⁷ Compare the view of Gilbert Ryle, whom BDS cite (2006, p. 273) as a paradigm promoter of conditional analyses. Ryle considered 'x knows French', for example, to be a dispositional statement, and offered this sketch of an analysis:

To say that this [man] knows French, is to say that if, for example, he is ever addressed in French, or shown any French newspaper, he responds pertinently in French, acts appropriately or translates correctly into his own tongue. (1949, p. 123)

That is, one analyses 'x knows French' into a cluster of conditionals like 'x would speak French if addressed in French' and 'x would translate correctly if presented with a French newspaper'. But Ryle immediately notes a problem with equating 'x knows French' with this list of conditionals: 'We should not withdraw our statement that he knows French on finding that he did not respond pertinently [to being addressed in French] when asleep, absent-minded, drunk or in a panic' (pp. 123–4). (Note that these are, in effect, masks for the disposition to speak French if addressed in French, anticipating Johnston 1992.)

Ryle's solution is vague but suggestive: 'We expect no more than that he will ordinarily cope pretty well with the majority of ordinary French-using and French-following tasks' (p. 124). As we understand it, the idea is that there are many specific circumstances where French is called for, but we require of the subject only that, in the *majority* of such circumstances, he would respond appropriately. Of course, the resulting view is very much like (PROP), which requires, in effect, that one satisfy a suitably high proportion of a long list of conditionals involving very precise stimulus conditions. Given this similarity, we find it curious that BDS portray Ryle as a champion of the conditional analysis, while we are advertised as opponents.

⁸ For the unabridged observation, see p. 1144.

⁹ See Sect. 2 of our 2008 for further discussion of finks and masks.

The third methodological observation amounts to this: philosophical discussions of conditionals and dispositions should not assume the standard account of counterfactuals and, in particular, should not assume a counterfactual logic that includes centring.¹⁰ On this point we disagree. Every philosophical discussion must take some things for granted, and a discussion of dispositions that assumes a common account of counterfactuals will be of interest to the many people who accept that account.¹¹

In any case, the third observation turns out to be irrelevant, since our arguments do not appeal to centring. To illustrate the point, we will focus on one particularly vivid example. In our earlier paper, we report a famous case of C. B. Martin as follows:

Martin invites us to consider an ‘electro-fink’ — a device that attaches to a dead wire and monitors whether a conductor is about to touch the wire. Were such contact to occur, the fink would instantaneously render the wire live: that is, the fink would confer on it the disposition to conduct electricity if touched by a conductor. And the wire would then conduct electricity. (2008, p. 60)

We claimed that this case undermines (SCA), since Martin’s wire would conduct electricity if touched but it is not, on that basis, disposed to conduct electricity.

In reply, BDS complain that this objection appeals to centring, for they interpret us as moving from

- (1) If the wire were touched, it would be such that if it were touched it would conduct electricity

to

- (2) If the wire were touched, it would conduct electricity

by implicit appeal to contraction (pp. 1155–6).¹² Given (SCA), (2) would imply:

- (3) The wire is disposed to conduct electricity when touched

Since (3) is false, (SCA) would stand refuted. The problem, they say, is that the validity of contraction ‘is a consequence of the weak

¹⁰ More precisely, it should assume neither weak nor strong centring, where *weak centring* puts each world among its own closest worlds and *strong centring* makes each world its only closest world.

¹¹ Compare: Should discussions of modal metaphysics never assume S5? Should discussions of the semantics of proper names never assume a ban on empty domains?

¹² Contraction licenses the move from $P > (P > Q)$ to $P > Q$.

centring of the conditional' (p. 1156). They conclude that examples involving finkish dispositions are 'toothless against an uncentred conditional' (p. 1147) and that our particular argument fails.

But this is not our argument. We do not appeal to (1); nor do we appeal to contraction. Rather, we take Martin's case to generate a *direct* intuition for (2): given the description of the case, it seems obvious to us that *the wire would conduct electricity if touched*. It seems equally obvious to us that (3) is false, in the case described. And that is enough to undermine (SCA), no matter what our position on centring and contraction.

Interestingly, BDS acknowledge this interpretation of Martin's argument in their earlier work (2006, pp. 275–81) and they concede that this direct argument requires neither centring nor contraction. In fact, they admit that this kind of argument refutes the simplest version of the conditional analysis (2006, pp. 278, 280–1).¹³ Their only complaint is that the direct argument does not go far enough:

[T]he conditional fallacy strategy is out for bigger game — it seeks, again, a generic fallacy in the very idea of conditional analysis of dispositional properties. No particular counterexample can bring down this game. (2006, p. 281)

But as we emphasized above, we do not oppose the conditional approach. Since we have no interest in bringing down this 'bigger game', BDS have no objection to our argument. Once again, the methodological observation is irrelevant.

1.3 Achilles' heel

The problem of masks for (SCA) and (LCA) is straightforward. To take a standard example: it would seem that a vase can have the disposition to break if dropped even though it just so happens that, if it were dropped, a mad magician would cause the floor to turn to jelly and, as a result, the vase would not break if dropped. Reflection on such cases suggests that something can have the disposition to M in C even though it is not the case that it would M in C.¹⁴

¹³ There is, of course, some tension between this admission and the outright claim in the present paper that 'masking and mimicking arguments require the centring assumptions that Lewis imposes on his counterfactual conditional, and are toothless against an uncentred conditional' (BDS, p. 1147).

¹⁴ Note, again, that this example is *only* intended to refute specific proposals like (SCA) and (LCA); and it is intended to do so by way of a *direct* intuition that the relevant vase would not break if dropped.

In our original paper, we discussed one way of defending these schemas from this problem (pp. 63–71). The suggestion is that masks only arise because ordinary language does not fully articulate the stimulus conditions of the dispositions that are expressed even by explicit dispositional predicates like ‘is disposed to break if dropped’. A full articulation of those stimulus conditions would exclude being dropped on a jelly surface—or indeed any other ways of being dropped that are not intuitively paradigmatic for the manifestation of the disposition to break if dropped. If this is correct, then perhaps we can find true instances of (SCA) or (LCA)—namely, those in which the antecedent of the conditional contains all the stimulus conditions that belong to a full articulation of the relevant disposition. This we called the *getting specific strategy*.

So, we said, let us imagine that we have a conditional whose antecedent specifies only the most paradigmatic of circumstances for the manifestation of the disposition to break if dropped.¹⁵ It would look something like this: ‘*x* would break if dropped at such-and-such a height, onto such-and-such a surface, through such-and-such a medium, etc.’ Now either this conditional narrows down the relevant aspects of the environment to a *single fully determinate case*—a litmus test—or else it allows for a *range* of cases all of which are paradigmatic for the manifestation of the disposition to break if dropped. In the former case, if we call the resulting litmus case ‘*C*’, the biconditional looks like this:

- (A) *x* is disposed to break if dropped iff *x* would break if dropped in *C*

In the latter case, there is a range of paradigmatic cases that fall within the specification of the various relevant environmental factors: call these cases ‘the *Cs*’. This gives us:

- (B) *x* is disposed to break if dropped iff *x* would break if dropped in one of the *Cs*

Here the consequent is ambiguous among the following three options:

- (B₁) ...were *x* in some case or other among the *Cs*, *x* would break

¹⁵ We do not ourselves think it is plausible that context restricts the relevant stimulus conditions in this way: see n. 38 below.

- (B₂) ... for some case among the Cs, if *x* were dropped in that case, it would break
- (B₃) ... for every case among the Cs, if *x* were dropped in that case, it would break

So we now have four proposals for getting specific: (A) and (B₁)–(B₃). Our paper provided arguments against each of these, and we will not rehearse them all here. BDS focus on our argument against one of the least plausible of these alternatives, namely (A).

The problem, we said, is that (A) is just too easy to satisfy. Whatever fully determinate case *C* we pick, there is a possible object that does not have the disposition to break if dropped, but is such that it would break if dropped in *C*. We need only consider a very sturdy concrete block which is such that, for a huge range of the relevant cases, it would not break if dropped in those cases; but *C* is an *Achilles' heel* for that object. Here we took a toy example, filling in the specifics of *C* by plugging in precise measurements for the variables of height, surface hardness, and so on. We then considered a concrete block that was not disposed to break when dropped, but would break if dropped in exactly that case (pp. 67–8).

In response, BDS write: '*Of course* an object can be such that it would break under exactly these conditions without being fragile...' which we are happy to agree with; but they add 'because these are not the right conditions to test' (p. 1151). To us, the problem is pretty clearly not that the specific conditions we chose in our toy example were the wrong ones to choose. The problem lies in the overly specific nature of the conditions: one cannot use a litmus test involving a single determinate case to analyse a disposition that concerns an object's counterfactual behaviour in a wide range of cases. As long as *C* involves that kind of litmus test, a proposal along the lines of (A) will fall prey to a similar counterexample. But this is not how BDS see it. Seizing on our toy example, they write:

A particular (reverse-)Achilles' heel example refutes a particular hyper-specific dispositional analysis (the First Methodological Observation). But all this serves to show is that that hyper-specific analysis was not a good one ... Manley and Wasserman have, in the end, given us nothing more than the dialectically unacceptable brute commitment to producing a counterexample to every proposed (hyper-specific) analysis, without reason to think that they can fulfil that commitment (the Second Methodological Observation). (pp. 1151–2)

The second methodological observation is that a ‘brute commitment’ to provide counterexamples to each instance of a ‘style of analysis’ does not constitute a ‘general recipe’ for the production of counterexamples (p. 1147). But the particular toy example we chose was not intended as a recipe. It was for illustrative purposes only, like a picture in a cookbook. As for the recipe itself, here are the ingredients:

One instance of (A), specifying a fully determinate litmus case C.

One possible concrete block that does not have the disposition to break if dropped but is such that it would break if dropped in C.

And here are the directions: *combine ingredients; repeat as necessary.*¹⁶

1.4 Structural considerations

In section 4 of our previous paper, we set aside counterexamples and considered facts about dispositional ascriptions that are not explained by any ‘extant theory of the link between dispositions and conditionals’ (p. 71). The goal was to identify structural problems with existing accounts so that they could be avoided in our own.

First, dispositions come in degrees. But if fragility comes in degrees and *being such that one would M in C* does not come in degrees—regardless of how carefully one specifies ‘C’—then fragility is not the property of *being such that one would M in C*. Relatedly, one would hope for a semantic account of comparative constructions like ‘more fragile than’ as well as of the function of degree modifiers applied to dispositional predicates. As we point out in our 2007, it is difficult to see how one might extend previous views like (SCA) to account for these other locutions.

BDS protest that ‘a theory of categoricals need not also be a theory of comparatives’, so none of this amounts to a problem for a

¹⁶ Elsewhere in the paper—p. 75 and especially n. 20—we provide another kind of example that is fatal for litmus-test analyses. Consider again all of the maximally specific circumstances in which a vase might find itself, and suppose we give them a very arbitrary order: C_1, C_2, C_3, \dots . Now consider two vases— V_1 and V_2 —where V_1 would break in all and only the odd-numbered circumstances and V_2 would break in all and only the even-numbered circumstances. That is, if you drop V_1 in one way, it will break; if you drop V_2 in a slightly different way, it will break. If you look at V_2 in one way, it will break; if you look at V_1 in a slightly different way, it will break. And so on. Both vases are fragile—it would be very difficult to keep either from breaking for very long. But there is no condition C_n such that, for each vase, it would break if it were in C_n . So there is no true biconditional of the form: N is fragile iff N would break if it were in C , where C is a fully determinate litmus-test case.

Such examples are even more dramatic when it comes to obviously ‘low yield’ dispositions like *being disposed to become violent in the evening*—it is clearly possible to have such a disposition even if one is not disposed to get violent in most evening-situations.

conditional analysis unless it ‘actively hinders the provision of an independent analysis of dispositional comparatives’ (p. 1147). In reply, we grant that one could pursue an ‘independent’ analysis of dispositional comparatives. But we emphasize that this would run counter to the semantic orthodoxy, which would be a significant cost to the resulting theory. The leading approaches to the semantics of degree make reference to a scale along which objects are ordered by the comparative, and along which a minimal degree (or interval) is required by the corresponding positive.¹⁷ This kind of approach also provides a natural account of the context-dependence of gradable adjectives: context selects where along the scale one must fall in order to satisfy the positive predicate. In this way, semantic orthodoxy provides a unified treatment of comparatives, degree modifiers, and context-dependent gradable adjectives.¹⁸ So the challenge is to make sense of the underlying scale: and our own proposal does exactly that.¹⁹

¹⁷ See Cresswell 1977, Hellan 1981, Hoeksema 1983, von Stechow 1984a, Bierwisch 1989, Moltmann 1992, Lerner and Pinkal 1995, Gawron 1995, Kennedy 1999, Heim 2000, Kennedy 2001. For a semantics of extents or intervals in particular, see Seuren 1978, 1984, von Stechow 1984a, 1984b, Schwarzschild and Wilkinson 2002, Schwarzschild 2005. A growing body of literature on the linguistic significance of scale structure suggests that degree-theoretic representations cannot be avoided in the semantics of these terms; see Rotstein and Winter 2004, Kennedy and McNally 2005.

¹⁸ BDS point out that the approach of Kamp 1975 — see also Lewis 1970, p. 65; Fine 1975; Klein 1980 — which treats the semantics of comparatives as parasitic on the vagueness of the corresponding positives, would not require establishing a scale in the fashion we suggest. For some of the standard reasons why that sort of view does not have much of a following, see von Stechow 1984a; Keefe 2000, pp. 169–70; Kennedy 2007, pp. 40–2. The problem of gradability without vagueness appears particularly pressing. Note also that more recent versions of this view appeal at some level to the notion of being F-relative-to-a-comparison-class (Klein 1980, pp. 13–15; for discussion, see Larson 1988, Ludlow 1989) and it is unclear how to even make sense of that idea using a schema like (SCA).

¹⁹ In a footnote, BDS claim that our own ‘positive account of the mechanism fails’, for the following reason:

An aerospace engineer, when calling a material fragile, has in mind its behaviour at high temperatures on re-entry, while a sculptor, when calling a material fragile, has in mind its tendency to shatter when chiseled. Neither engineer nor sculptor is using *fragile* to mean *more fragile* than the other; they are instead focusing on different aspects of a complex range of fragility-relevant features. (p. 1148)

BDS take this to show that the ‘context-dependency of dispositional predicates cannot be a matter of selecting a threshold for an underlying comparative, since the dimensionality of the context-sensitivity outruns that of the comparative’. But why assume that this is a feature only of the positive? On our view, the context-sensitivity of the positive is partially a matter of determining a threshold and partially a matter of weighting scenarios by relevance in establishing the scale (see pp. 78–9). But uses of the comparative also display this second dimension of context-sensitivity. If O_1 would break in many more chiseling scenarios than O_2 , but in many fewer aerospace scenarios — and they are otherwise the same — the sculptor but not the

Another structural problem concerns dispositions that lack any special associated stimulus conditions. We considered the example of a man who is, in a general way, very disposed to talk, but is not particularly disposed to talk in any special set of circumstances (p. 72). In this sense, his disposition to talk lacks stimulus conditions. (By this we do not mean that, in situations where he *does* talk, there is no trigger or cause for the manifestation of the disposition; we mean that there is no restricted set of cases such that only talking in *those* cases counts as a manifestation of the disposition.) We went on to hypothesize that loquacity may just be *the disposition to talk*. So too, irascibility might be the disposition to get angry, and fragility the disposition to break. (After all, breaking because of a loud noise, a drop in temperature, or for no reason at all appears relevant to an object's degree of fragility.) At any rate, if there are any dispositions that (in this sense) lack stimulus conditions, they will cause trouble for existing versions of the conditional analysis, since those accounts tell us nothing about how to analyse statements of the form 'N is disposed to M'.

In reply, BDS offer three objections. First, they think there might be stimulus conditions for loquacity after all: 'the loquacious man talks given any even minimally talk-apt situation' (p. 1148), but talking while asleep or listening raptly to a symphony is not a manifestation of loquacity. Perhaps. In that case, loquacity is not simply the disposition to talk. For surely, talking while listening raptly to a symphony is a manifestation of the *disposition to talk*. And if A and B are twins in all counterfactual respects except that A but not B would talk while listening raptly to a symphony, then A is more disposed to talk than B. Our point about the absence of stimulus conditions holds for *that* disposition.

BDS's second objection is that loquacity and irascibility might turn out to be 'tendencies, inclinations, [or] propensities' rather than dispositions. Typically the literature uses 'dispositions' broadly, so as to include tendencies, inclinations, and the rest.²⁰ In any case, we assume

engineer is likely to accept ' O_1 is more fragile than O_2 '. (It is also worth noting that the comparative as used in the sculptor's context does not *ignore* aerospace scenarios: if O_3 and O_4 differ only in that O_3 would break in more aerospace scenarios than O_4 would, even the sculptor will grant O_3 is more fragile than O_4 .)

²⁰ For anyone attached to the metaphysical motivations that typically underlie attempts to analyse dispositions in terms of counterfactuals, it would be odd to insist that dispositions can be so analysed, but *inclinations, tendencies, propensities, and proclivities* cannot. 'Habits', which BDS also mention, appear to be concerned with actual manifestation in a way that the others do not. For more on dispositions and habitual statements, see Fara 2006 and Wasserman 2011.

that there is no dispute about whether *the disposition to get angry* and *the disposition to talk* are dispositions.

BDS's third objection is that such dispositions may be analysed in terms of conditionals with vacuous antecedents. Suppose that John has the disposition to talk. We had argued (2008, p. 73) that this does not entail:

(V1) If John were in any situation at all, he would talk

Take a perfectly ordinary case in which John is standing nearby, not talking. In that case, we take it that (V1) is false.²¹ If we were to bet on the truth of (V1), the silence of John would settle the bet.

BDS complain that counterexamples of this sort — like all masking and mimicking counterexamples²² — involve a covert appeal to centring. But, they say, not all possible conditionals feature centring, and without 'an unjustified constraining of logical options, there is no reason to think that absent-stimulus dispositions, if there are any, do not yield to analysis by vacuous-antecedent conditionals' (p. 1149).

Two points about this objection should by now be familiar. First, if one were to invoke centring in an argument against the vacuous-antecedent proposal, one would only require *weak* centring, which is overwhelmingly accepted as a feature of the natural language counterfactual.²³ Second, we did not in fact invoke centring. We appealed to intuitions about a particular case, rather than to a general feature of the counterfactual. The relevant intuition is that (V1) is false in the case where John is actually silent, and that seems correct, whatever one's position on centring.²⁴

²¹ At least, (V1) will be false on any *literal* reading, as opposed to one in which it is some kind of acceptable exaggeration. (In typical cases, of course, pointing out that John is not silent will elicit a response indicating the presence of exaggeration: 'Okay, I didn't mean *any* situation'.)

²² Though see n. 13 above.

²³ See Nozick 1981, pp. 680–81, Penczek 1997, and Vessell 2003 for motivations to reject strong but not weak centring. One notes that even philosophers with views on which it would, in the abstract, be natural to reject weak centring are often determined to save it. (See e.g. the discussion of Bennett in n. 29 below.)

²⁴ Admittedly, in our earlier paper we did at one point make a stronger assumption than was necessary for our argument. We said that (V1) has two readings: 'On one reading it is too strong (requiring that every situation is such that he would talk in it) and on another it is too weak (requiring only that he talk in the closest world in which any situation obtains; i.e. the actual world).' In the second parenthetical remark, we should have remained neutral about whether, on the relevant reading, it is *sufficient* for John to actually talk.

There is also a third point worth making. It is easy to imagine a case in which there are *many* (relevant) nearby worlds in which John does not talk. This is consistent with John having the relevant disposition, but not, intuitively, with the truth of (V1). The point is even more vivid if we focus on what we call ‘low-yield’ dispositions: those that require manifestation in only a very low proportion of the relevant situations. Suppose John is disposed to become violent. This is consistent with John’s serenity in the actual world *and* a high proportion of nearby worlds. Now consider

(V2) If John were in any situation, he would become violent

Only a serious pre-theoretical commitment to a particular style of conditional analysis would lead one to say that (V2) is true in the case described.

1.5 Off-centre

At various points in their paper, Bonevac, Dever, and Sosa charge us with ‘covert appeals to centring’. Our response to this point has been simple: the charges are false, for the relevant arguments do not rely on centring. We now wish to explore in more detail the role of centring in connection with conditional analyses, and to address a positive proposal made by BDS in their previous work.

Consider the following argument against (SCA). According to (SCA), Mary is disposed to kick John in the shins when he speaks to her if and only if she *would* kick him in the shins, were he to speak to her. Now suppose Mary currently has the relevant disposition. A bystander says:

(M1) If John were to speak to her, Mary would kick him in the shins

As the bystander is speaking, John speaks to Mary, but she manages to restrain herself. In this case, the argument runs, it seems that (M1) is false. So the relevant instance of (SCA) is false.

This argument, we claim, does not exhibit a ‘covert appeal to centring’. But one might have used the same case to argue as follows, instead. Assuming that $A > B$ guarantees that *all* minimal sphere A-worlds are B-worlds, weak centring gives us:

(MP) $A \ \& \ A > B$ implies B

And (MP) in turn entails that Mary’s actual restraint falsifies the relevant counterfactual. The problem with this argument, from our

point of view, is that it gets things entirely the wrong way round. (MP) is only plausible because there are so many particular cases in which instances of it are highly supported by intuition (and it resists definitive counterexamples). And this is just such a case: so we ought to be more confident about our verdict in this particular case than we are about (MP) or weak centring in general. After all, a number of candidate principles for the counterfactual—like transitivity and antecedent-strengthening—might seem plausible for the counterfactual in the abstract, but break down in the face of particular cases.

This is not simply an abstract point of logic. There are plenty of reasons why one might be motivated to give up on weak centring or (MP) but still want to retain the intuitive verdict in this kind of case. To illustrate: one might want the assessment of counterfactuals to ignore certain ‘remarkable’ events of astronomically low probability. So, for example, even though the laws of nature do not rule out flipping heads ten million times in a row, we normally tend to think the following counterfactual is true:

- (F1) If I were to flip a coin ten million times, it would not come up heads every time

One might worry that there is no principled reason to treat a world in which I flip heads a million times as more distant than worlds containing other outcomes.²⁵ But then the Lewis semantics will yield the result that (F1) is false, because $A > B$ requires that *all* of the minimal sphere A-worlds must be B-worlds. But suppose we did have a satisfactory way of characterizing extremely ‘remarkable’ events of very low probability (as opposed to boring events of very low probability, like most other specific outcomes of a million tosses) that allows us to cordon off crazy worlds in which those events occur.²⁶ We could then insist that the counterfactual requires only that all closest *non-crazy* A-worlds are B-worlds. Without further modification, this view allows

²⁵ See Bennett 2003, Hawthorne 2005, and Hájek MS. One can invoke scenarios involving improbable quantum events to an even greater rhetorical effect.

²⁶ In a recent paper, Robbie Williams argues that the intuitively crazy outcomes do exhibit an objective feature he calls *atypicality*. But the resulting view is much more sophisticated than the one sketched above. Rather than having the counterfactual ignore atypical worlds, Williams builds affinity for typicality into the notion of closeness. This yields the result that (F1) is true in the ordinary case, because the all-heads world is not as close as worlds with other outcomes. Now, what happens if I actually flip ten million heads? Williams, partly in order to capture intuitive counterfactual judgements, requires that no world can be closer to the actual world than it is to itself. See Williams 2008, p. 396, especially n. 12, and p. 415.

the actual world itself to be forced from the minimal sphere due to craziness. In that case, if I flip ten million heads in the actual world, (F₁) is still true. Presumably giving up on weak centring would be reckoned a cost, but we can imagine someone taking this step for considerations of simplicity.²⁷

Having bitten this bullet, suppose our imaginary theorist considers (M₁). As someone who rejects weak centring and (MP), does she have any reason to insist, contrary to intuition, that (M₁) is true? Of course not. In any ordinary case, Mary's failure to kick John is not an astronomically unlikely 'remarkable' event: so her actual restraint in a non-crazy world settles things. And this is the right result. It is hard enough to bite the bullet in the case where I actually flip ten million heads; one should not accept more counterintuitive verdicts than necessary. The same point holds for (V₁) and (V₂) in the preceding section.

There are other views that reject weak centring but can nevertheless yield a verdict of falsehood for (M₁). Take the probabilistic proposal of Gundersen (2004), which BDS mention as a semantics without weak centring (2006, p. 306). The idea is this:

The closest (A & B)-world is closer than the closest (A & ¬B)-world iff

- (i) $p(B/A)$, is greater than $p(\neg B/A)$, and
- (ii) $p(B/A)$ is greater than $p(B/\neg A)$

Whatever notion of probability Gunderson has in mind, this view will be more permissive than our intuitive judgements when it comes to many ordinary counterfactuals.²⁸ But at least on some ways of spelling out the Mary case, this proposal need not yield the counterintuitive result that (M₁) is true; nor does it imply the truth of (V₁) and (V₂). Centring is, again, irrelevant.²⁹

²⁷ The thought might be: why exempt α from being jettisoned from the minimal sphere? It is worth stressing that neither Williams (see n. 26, above) nor Bennett (see n. 29) are willing to give up (MP).

²⁸ For example, 'If I were to roll a fair die, it would not come up 1 or 2' comes out true.

²⁹ The same point can be made for another kind of proposal for dealing with sentences like (F₁). Jonathan Bennett suggests that the counterfactual ' $A > B$ ' requires only that B obtains at 'an astronomically high proportion of the closest A-worlds' (p. 249). If we leave the account at that, we will end up rejecting (MP): the actual world may contain the fluke. Bennett finds this utterly unacceptable: 'We should all agree that $T > F = F$ absolutely' and so he revises the proposal by fiat, requiring that when @ is an A-world, it must be among the overwhelming majority of A worlds that are B. But one can imagine insisting that the counterfactual should

The point generalizes. Recall our exposition of the electro-fink example, which BDS accuse of appealing to centring. (The idea is that it employs the rule of contraction, which is a consequence of weak centring and (MP).) Our response was to say that we were not appealing to contraction, but simply appealing to intuitions about a particular case. We can now go further and insist that there is simply no reason even for many opponents of contraction to reject those intuitions. (After all, it is not as though opponents of contraction are committed to *anti-contraction*.) The same point holds, *mutatis mutandis*, for other counterexamples to (SCA). They are not ‘toothless against an uncentred conditional.’

1.6 A new sort of conditional?

We now turn to a positive proposal offered in Bonevac, Dever, and Sosa 2006. In that paper, they use the conditional analysis of dispositions to motivate ‘the possibility of a new sort of conditional’, one that ‘does not satisfy some of the traditional presuppositions about conditionals’ (p. 313), and in terms of which one could provide a conditional analysis of dispositions.

It is worth distinguishing between two possible projects here. One project is concerned with analysing dispositions in terms of ordinary language conditionals. Another takes a very ‘abstract perspective on conditionality’ on which all sorts of symbols count as conditionals as long as they play a certain role in conditional proofs (p. 307). It then attempts to define a connective ‘ \rightarrow^* ’ which counts as a conditional in this abstract sense, and is such that:

(S) N is disposed to M in C iff C obtains \rightarrow^* N Ms

It may be worth emphasizing that *no one* in the literature rejects the possibility of *this* kind of conditional analysis. We have all along been interested in the subjunctive conditionals of natural language.

Now, here is a characterization BDS offer of a ‘new sort of conditional’ in terms of which dispositions might be analysed:

[S]uppose that \Rightarrow is a neighborhood conditional, whose purpose is to test the coordination of features across regions of modal space. Thus $\phi \Rightarrow \psi$ would mean, roughly, that some relevant ϕ -region of modal space was in a ψ region. Instead of understanding ‘If that were a bird, it would fly’ as asserting that all normal *that’s a bird* worlds are *that flies* worlds, we might

simply assess most A-worlds, without any special provisions for whether @ is in the majority. Again, on such a view, one would reject (MP) and yet there would be no need for rejecting the intuitive judgements in our cases.

think of it as asserting that *that flies* is sufficiently thick about the neighborhood of *that's a bird* worlds. A conditional with such truth conditions would allow for the possibility of exceptions in a way that differs significantly from that of existing normality conditionals. (BDS 2006, pp. 312–13)

There are two questions for this proposal: Does it do the work required of ' \rightarrow^* ' in providing the analysis (S)? And, if so, does ' \Rightarrow ' correspond to any expressions of natural language?

Regarding the first question, it is worth returning to low-yield dispositions. If Fred's disease is disposed to spread upon contact, disease-spreading worlds need not be particularly thick about the neighbourhood of Fred-contact worlds. In fact, it may be that, intuitively, failing-to-spread worlds are *thicker* around Fred-contact worlds. Likewise for Bob's disposition to be violent in the evening: worlds in which Bob is serene might be thicker in the neighbourhood of Bob-in-the-evening worlds than worlds in which Bob is violent. Perhaps one could allow both ' $A \Rightarrow B$ ' and ' $A \Rightarrow \neg B$ ', but this would put significant pressure on the relationship between ' \Rightarrow ' and any conditional expressions of ordinary language. We suspect that, to mirror the variability and context-dependence of the modal requirements of dispositional discourse, BDS's ' \Rightarrow ' will have to exhibit a high degree of contextual flexibility.

This brings us to our second question. Do any conditionals of ordinary language exhibit flexibility to this degree? BDS take an interest in the *normality* conditional: 'If that were a bird, it would (normally) fly'. But even if we force this 'reading',³⁰ we still get sentences that appear false in the cases described, despite the presence of the relevant dispositions:

(N1) If one were in contact with Fred, his disease would (normally) spread

(N2) If it were evening, Bob would (normally) become violent

Much the same point can be made for another type of expression they discuss, namely the *fainthearted* conditionals examined by Morreau (1997). These include: 'If...then provided conditions are suitable...' and 'If...then under normal circumstances...' and 'If...then other things being equal...'. As Fara (2005) persuasively argues, masking conditions can occur in perfectly suitable cases; and

³⁰ One might take this to involve the effect of an (overt or covert) operator on certain sentences involving the conditional.

the low-yield dispositions we have been discussing make this especially clear.

We have been assuming that the proposed analysis of dispositions would take the form ‘N is disposed to M in C iff C obtains \Rightarrow N Ms’. But it would be a mistake to assume that an analysis in terms of conditionals must have this form. Perhaps one can use natural language neighbourhood conditionals—if there are any—to provide a more refined analysis with a different form.

1.7 Apollo’s machine

This brings us to our own view, which quantifies into an ordinary subjunctive conditional:

(PROP) N is disposed to M when C iff N would M in a suitable proportion of C-cases

On our view, the connection between dispositions and conditionals begins with the comparative. We suggest, for example, that one object is *more fragile than* another—or is *more disposed to break when dropped*—just in case there are more dropping situations in which it breaks. We then say that an object is disposed to break when dropped *simpliciter* just in case it is more fragile than a sufficient number of objects in the relevant comparison class. Thus, a particular vase is *disposed to break when dropped* just in case it would break in a suitable proportion of cases in which it is dropped. More carefully, our suggestion is that one holds fixed the actual laws, as well as the intrinsic properties of the vase. One then considers all of the (nomically) possible circumstances in which the vase is dropped (from a great height, from a little height, onto a hard surface, onto a soft surface, with a fink present, with no fink present, and so on). If the vase would break in enough of these circumstances, it is disposed to break if dropped. If not, not.³¹

Bonevac, Dever, and Sosa conclude their paper with an alleged counterexample to our proposal, involving a sturdy block with an Achilles’ heel that has an *Apollo machine* installed next to it:

The Apollo machine catches the block whenever it is dropped (from any height, through any medium, onto a surface of any hardness), and moves the block to a *particular* height above a surface of a *particular* hardness and releases it. The block then falls, triggering its heel, and breaks. So were

³¹ Whether or not a given proportion of circumstances is enough will depend on context and may vary with different dispositional terms. See our 2008, pp. 74–82 for details.

the block dropped in *any* C-case it would (due to the intervention of the Apollo machine) break. Thus by any standard of suitable proportion, the block breaks when dropped in a suitable proportion of C-cases, and is, according to (PROP), fragile. But *ex hypothesi* the block is not fragile, so (PROP) fails. (p. 1157)

Note that although BDS use ‘fragile’ in their discussion of the case, (PROP) only concerns explicit dispositional predicates, and we reject the assumption that fragility is just the disposition to break if dropped. We did, however, tentatively suggest that fragility is the disposition to break, *simpliciter*. If that is right, the relevant C-cases for fragility are not restricted to dropping cases; so BDS’s example should involve a machine that would, in a wide variety of circumstances in which it is next to a block, cause that block to break. (In this respect it would be much like Lewis’s Hater of Styrofoam (see Lewis 1997, p. 153).)

Even so, a block that happens to be next to such a machine does not count as fragile according to (PROP).³² Of course there are *some* circumstances in which the imagined block would break, for there are some worlds in which the block is victimized by an Apollo machine (or a sorcerer, or some Z-rays, or a rapidly rising floor; see, respectively, Lewis 1997, p. 147; Smith 1977, pp. 440; and Manley and Wasserman 2008, p. 62). In this respect the block does not differ from any other concrete block — on our view, all of these worlds are taken into account,³³ and the fact that some of them are especially nearby for this particular block is irrelevant.³⁴ The key fact is that there are also many more circumstances in which the block does *not* break, since these abnormal factors are absent from most circumstances.³⁵ As a result, it is not fragile, even supposing fragility is just the disposition to break.

³² This is noticed by Alexander Anthony in his excellent 2010 BA thesis (pp. 49–50).

³³ *Modulo* the laws. However, as we discuss in detail (pp. 78–81), there may be contextual factors affecting how stimulus-cases are weighted in the scale itself.

³⁴ That is, at least in the case of intrinsic dispositions (see Manley and Wasserman 2008, p. 78). Nearness does, however, matter for extrinsic dispositions. Perhaps there are contexts where ‘is disposed to break’ expresses an extrinsic disposition, and perhaps if one is in such a context, then a block with an Apollo machine that is very robustly attached will enjoy that extrinsic disposition. But that is as it should be. (We doubt, however, that ‘is fragile’ ever expresses an extrinsic property: see p. 61, n. 4. This suggests that care must be taken in identifying fragility with the disposition to break.)

³⁵ That is, intuitively the proportion of scenarios with Apollo machines, out of all scenarios, will be quite small. See our 2008, pp. 79–82 for more on the relevant notion of proportion.

2. Reply to Choi

Sungho Choi raises three concerns for our earlier paper. First, he objects to our implementation of the getting specific strategy. Second, he rejects our characterization of masking. And third, he responds to our Achilles' heel objection. We would like to briefly clarify our position on these issues.

2.1 *Getting specific*

The Simple Conditional Analysis (SCA) predicts the equivalence of the following two statements:

(D1) This vase is disposed to break if dropped

(D2) This vase would break if dropped

Recall that, in what we call *masking* cases, (D2) is false even though the vase would retain its disposition, were it dropped. For example, the vase may be filled with Mark Johnston PackingTM. Or the glass may be suspended over a vat of honey. In either case, the vase would not break if dropped, but it would remain disposed to break if dropped.

In the second section of our earlier paper, we tried to spell out one strategy that philosophers have hinted at to avoid these objections: the getting specific strategy.³⁶ The idea is that the predicate 'is disposed to break when dropped' expresses a highly specific disposition, relative to a given context of utterance, perhaps something like:³⁷

(D) The disposition 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

The schema offered by (SCA) will now yield a biconditional with an equally specific counterfactual claim on the right hand side. The resulting biconditional avoids some masking counterexamples—like the vat-of-honey case—but not all. In particular, as we pointed out, 'Johnston's original example is still problematic, for example, since a vase filled with anti-deformation packing might have this very specific disposition and yet be dropped on Earth from a metre up onto a solid surface, etc., and still not break' (pp. 63–4). However, we set aside such specific shortcomings of (D) and pretended 'for the

³⁶ This strategy is suggested by, for example, the discussion in Lewis 1997, p. 145.

³⁷ In our original paper, an ascription of (D) appears as (SD).

sake of argument ... that the ascription in question is precise enough' that an instance of the biconditional schema with an explicit ascription of (D) on the left hand side would actually be true (*ibid.*). We then argued that, even granting this, the getting specific strategy would fail.³⁸

Strangely, Choi attributes to us the claim that 'the simple conditional analysis of [D] seems to be free from the problem of maskers' (p. 1161). Then, perhaps unsatisfied with the Johnston counterexample we mention, he offers a different alleged counterexample to this claim.³⁹ But the point of (D) was *not* to provide an example that would itself avoid all masks; rather, it was to illustrate the general strategy that one might pursue in trying to identify unmaskable dispositions that could serve as the semantic values of our ordinary dispositional predicates.

2.2 Masking

Unlike us, Choi is a proponent of the getting specific strategy: he holds that 'the context's semantic contribution to the utterance of "*x* is disposed to break when dropped" ... includes that *x* is dropped from one metre up onto a hard surface and so on' (p. 1165). However, he rejects what we grant for the sake of argument — namely that with dispositions like (D) on the left hand side, we can achieve true

³⁸ However, there is a reason to be sceptical that context restricts the range of relevant stimulus cases in this way, one that is only briefly alluded to in our 2008 (p. 79). After all, breaking when dropped onto a soft surface can *manifest* the disposition to break when dropped. And plausibly any case in which one counts as manifesting a disposition is a case in which the appropriate stimulus conditions for that disposition are in place (see our 2007, p. 74). Moreover, being such as to break if dropped from a millimetre up or onto fluffy mattresses is relevant to the *degree* to which *x* is disposed to break if dropped. After all, two objects that differ only in that one would break if dropped onto soft surfaces and the other would not, will differ in *how* disposed to break if dropped they are. So the most natural account of the relationship between the comparative predicate and the corresponding gradable positive is one on which

the standards for fragility can vary across contexts without any variance in the set of stimulus conditions that are associated with fragility. So for something to satisfy the predicate 'fragile' as uttered by a waiter (as contrasted with 'fragile' as uttered by an aeronautical engineer), it must be such as to break in a much larger proportion of the very same set of [C]-cases ... It would be implausible for the comparative to bring into play a set of [C]-cases different from those invoked by the positive' (Manley and Wasserman 2007, p. 73).

All of this is inimical to the 'getting specific' strategy.

³⁹ He focuses on a case where, if the glass were dropped, a sorcerer would 'change the solid floor below into fluffy mattresses' (p. 1160). He claims that this case is not ruled out by conditions specified in (D), but this is not obvious: unless the floor is solid at the moment of striking, the glass is not dropped *onto* a solid surface.

instances of (SCA). Instead, he holds that the conditions specified by the contextual contribution are not sufficient to rule out what he calls ‘maskers’ — namely factors that ‘would block the manifestation of a disposition even if *its characteristic stimulus* obtains’. The characteristic stimulus is in turn defined as ‘the stimulus condition that can be acquired by taking maximal consideration of the context of ascription’ (p. 1167). In short: there are some factors, call them *maskers*, that are masks (in our sense) and also cannot be ruled out by the getting specific strategy. We are happy to grant that there are such factors (though we do not know of any clear examples)⁴⁰, and we have no objection to Choi’s terminological proposal.

According to Choi, such cases need not worry the proponent of (SCA). This is because, regardless of the context of ascription, ‘when I simply say that *x* is disposed to *M*, I typically mean that *x* is disposed to *M* when no maskers are operative’ (p. 1167). That is, ‘the condition that there are no maskers is required ... by the context-independent meaning of a dispositional ascription’ (p. 1168). Combining this condition with ‘the context’s semantic contribution’, the predicate ‘is disposed to break when dropped’ expresses, in an ordinary context, something like the disposition of being disposed to break when dropped on Earth from one metre up onto a solid surface, etc., *in a scenario without maskers*. Given Choi’s definition of ‘maskers’, this amounts to:

- (DM) The disposition to break when dropped on Earth from one metre up, etc. ... where nothing would block the manifestation of the disposition to break when dropped from one metre up, etc. ...

However, it is unlikely that the expression ‘the disposition to *M* in *C*’ means ‘the disposition to *M* where nothing would block the manifestation of the disposition to *M* in *C*’. After all, the second expression

⁴⁰ If the ‘masker’ must operate extrinsically, Achilles’ heels do not count as ‘maskers’ — though they are still counterexamples to the resulting view (as we will see). And Choi’s examples of maskers that cannot be ruled out by the getting specific strategy are not convincing. Aside from the fluffy-mattress-sorcerer — see the previous footnote — he writes that drinking a bowl of tap water is a masker for the disposition to ‘feel nauseous when I drink very saline water’ (p. 1164). But surely the ‘getting specific’ strategy, applied to this disposition, could rule out cases where plenty of tap water is drunk. Likewise, Johnston’s anti-deformation material seems easily ruled out by a specification of the environment.

contains the first.⁴¹ Moreover, the resulting biconditional replacement for (SCA) would contain the *analysandum* as part of the *analysans*, and as a result could not provide a conceptual or reductive analysis.

We suspect, therefore, that Choi intends for us to *mention* the relevant manifestation. On this view, ‘is disposed to break when dropped’ expresses:

- (DM*) The disposition to break when dropped on Earth from one metre up ... where nothing blocks it from breaking

And, schematically, with ‘C*’ representing the specific conditions supplied by context, we can state the biconditional thus:

- (B*) N is disposed to M when C* in cases where nothing blocks N from M-ing iff N would M if C* and nothing blocked N from M-ing

Note that this is not really a defence of (SCA). Although every instance of (B*) is an instance of (SCA), Choi is apparently abandoning the equivalence of (D1) and (D2), and so acknowledging that (SCA) has false instances. The new idea is that *certain* instances of (SCA) are true; namely those in which the two hidden sets of conditions are explicitly spelled out.

This proposal is very much like that of Johnston (1992, p. 233), and it is subject to many of the counterexamples we discussed in our 2008. From right-to-left: consider our concrete block with an Achilles’ heel that happens to be held in the very unlikely situation that makes it vulnerable. That block is such that if it were dropped in a C*-case (where nothing would block it from breaking), it would break—because as a matter of fact it would hit *just so* if it were dropped in a C*-case, and would break because of its Achilles’ heel. But since that is the only C*-case in which it would break, it does not intuitively have (DM*).⁴²

From left-to-right: this depends on how we construe ‘where nothing would block N from M-ing’. Choi’s use of ‘block’, ‘interfere’, and ‘operate’ suggest that he has in mind extrinsic factors that exert a causal influence. (He also uses the phrase ‘extrinsic interfering factors’ when expressing a similar proposal in his 2008.) Now consider our

⁴¹ One might consider the infinitary expression in which all of the relevant replacements are made, ad infinitum: but it is hard to see what it would take for anything to count as having the property it expresses.

⁴² We are again assuming the intrinsicness of the relevant disposition: see n. 35.

very fragile glass with a reverse Achilles' heel: it has (DM*). As it happens, if the glass were dropped in a case where no outside influence blocks it from breaking, it would still survive the fall because it would hit the floor *just so*; the reverse Achilles' heel is simply a combination of intrinsic factors that render it immune to breaking if dropped *just so*. Or consider a low-yield disposition: Bob has a disease that is disposed to spread upon contact, but as it happens if you were to touch Bob's left hand right now the disease would not spread. It is not that anything external to the disease is blocking its spread in that case: it is just that the disposition to spread upon contact is a low-yield disposition requiring only that a disease spread in some suitable proportion — say, four percent — of the appropriate stimulus conditions.

Alternatively, the notion of *blocking* could be interpreted to include intrinsic factors of an object: on this view, the glass with a reverse Achilles' heel has a factor that keeps it from breaking in this particular fully precise dropping scenario. But what is that factor? It is simply lacking the (causal basis for the) disposition to break if dropped *just so*. But surely we cannot include this as a factor that blocks the glass from breaking. After all, to consider a situation in which *no* such factors are present is to consider a situation in which the relevant object has all the specific dispositions to break in C', for every specific dropping scenario C'. But *everything* is such that it would break if dropped in such a 'situation'. (See the related point in response to Johnston's proposal in our 2008, p. 62, n. 7.)

2.3 Achilles' heel

With respect to Achilles' heel cases, Choi writes:

This issue, however, is on the turf of the contextual part of Lewis's strategy. Recall that it is its non-contextual part that is at work in averting the problem of maskers. If so, Manley and Wasserman's examples end up attacking the part of Lewis's strategy that plays no role in defending (SCA) from the problem of maskers.

However, Choi provides no textual evidence for thinking that his proposal is any part of Lewis's strategy. And if it were part of Lewis's strategy, would it not have been sufficient to point out that the resulting proposal *as a whole* fails to avoid the problem of Achilles' heels? Choi goes on to suggest a way of handling them:

In fact, the defence of (SCA) requires from its proponents no commitment whatsoever on the issue of how to clarify the context's role of fixing the semantic content of dispositional ascription: granted that Manley and Wasserman's (2008, p. 76) own proposal does not suffer from Achilles'

heels or its reverse version, it is an option open to the proponents of (SCA) to develop much the same proposal with respect to the semantic elements saturated by the context of ascription. (p. 1170)

The final idea, then, is to co-opt (PROP) itself for the ‘contextual’ component, and then add a no-masks condition as the ‘non-contextual’ component. The result is that ‘is disposed to break if dropped’ expresses:

(DM**) The disposition to break **in a suitable proportion of dropping cases** *where nothing (extrinsic) would block x from breaking*

This gives the illusion of solving the problem of Achilles’ heels because it is natural to pair it with a quantified-into conditional, as required by (PROP): ‘For a suitable proportion of dropping cases *where nothing would block x from breaking*, *x* would break if dropped in them.’ But note that it is the quantified-into conditional that is doing all the work. A flatfooted application of (SCA) to (DM**) would yield a conditional like ‘If *x* were in one-or-another of a suitable proportion of dropping cases where nothing would block *x* from breaking, *x* would break’—but this clearly falls prey to the problems of Achilles’ heels and accidental closeness.⁴³ The lesson is that the problem of Achilles’ heels is not avoided even by a context-dependent specification of stimulus conditions like the one given in (DM**). The key is that the analysis must employ a context-dependent method of quantifying into the conditional.

What about the idea that ‘is disposed to break if dropped’ expresses (DM**), combined with a (PROP)-inspired approach to its analysis? Here are two problems with this idea. First, it seems insufficiently motivated: given that (PROP) by itself solves the problems of finks, masks, Achilles’ heels, and so on, what additional benefit is gained by adding the italicized clause?⁴⁴ The second problem is this. The current

⁴³ See the ambiguity discussed in Sect. 1.3 above.

⁴⁴ After all, nothing breaks in a scenario where it is prevented from breaking. So why not think that for any suitable threshold *n* out of *m* dropping cases where nothing would block *x* from breaking, we can obtain a suitable threshold for the simpler account of the disposition to break if dropped by adding to *m* the number of dropping cases where something *would* block *x* from breaking? (Of course, this over-simplifies things by treating the number of cases as finite. In fact the relevant proportions will have to be achieved by measures on infinite sets—see pp. 79–82 of our 2008). In other words, by tossing the italicized condition and lowering the threshold a bit, we obtain a simpler view with no loss: namely ours.

In fact, the views come apart in certain *recherché* cases, and where they come apart, we take our view to deliver more intuitive results. For example, suppose there are two objects, A and B, such that *across modal space* (holding fixed the laws), there is a greater proportion of

proposal rightly sets aside the idea that a very specific disposition like (D) is given by ‘the context’s semantic contribution’. But that idea played a crucial role in Choi’s attempt to put what he takes to be the intuitive idea of a ‘masker’ on ‘firm theoretical footing’ (p. 1165). (Recall that a masker is a factor that ‘would block the manifestation of a disposition even if *its characteristic stimulus* obtains’ — that is the stimulus condition acquired by ‘taking maximal consideration of the context of ascription.’) Without that idea in place, we have lost our grip on what ‘maskers’ are supposed to be.⁴⁵

3. Reply to Vetter

There are two main concerns that motivate Barbara Vetter’s very helpful discussion. First, Vetter argues that our view leads to trouble when combined with a certain picture of the relationship between chance and counterfactuals. Second, Vetter worries that, in our analysis, the expression ‘suitable proportion’ must be capable of so much flexibility as to risk triviality. She suggests handling this problem by (in effect) conflating stimulus and manifestation conditions in our analysis. We will reply to these points in turn.

dropping-cases in which A is prevented from breaking than in which B is prevented from breaking. Then it might be that A and B are such that they would break in the same proportion of L-dropping-cases (that is, cases compatible with the laws) in which they are not prevented from breaking, whereas B is such that it would break in a greater proportion of L-dropping-cases *simpliciter*. On the (PROP)-inspired view that Choi suggests here, A is disposed to break in L-dropping cases iff B is. (And, assuming the natural treatment of comparatives, A and B will count as equally disposed to break if dropped in L-cases.) But this seems like the wrong result to us. In the case described, there is some *law-governed* reason why there are more extrinsic interferences that protect A, and so A is actually less disposed to break if dropped in L-cases *simpliciter*, though it is equally disposed to break if dropped in L-cases where nothing extrinsic interferes. Moreover, that disposition is still intrinsic (again, modulo the laws).

⁴⁵ For example: given Choi’s original picture, fluffy mattresses that happen to be on the ground are not maskers for the disposition to break if dropped. But they are extrinsic to the object and would block it from breaking in the sense of bringing it about that the object fails to break. (One might claim that the relevant test is not whether mattresses would block the object from breaking, but whether they would block the object from breaking-as-a-manifestation-of-the-disposition-to-break-if-dropped. This is admittedly another way to interpret Choi’s idea that maskers block the *manifestation of the disposition to break if dropped*. But this would be a vacuous test because the disposition to break if dropped itself is supposed to be identical to the disposition to break if dropped *in the absence of a masker*. But *nothing* extrinsic can block something from breaking-as-a-manifestation-of-that-disposition; if something could, it would count as a masker and would thereby be irrelevant to the manifestation of that disposition.)

3.1 Chance and counterfactuals

Vetter's first concern involves a question we very briefly address in both our 2007 and our 2008 — namely, what should we say about (PROP) if it turns out that most ordinary counterfactuals are false because of indeterministic laws?

Here is some background. Suppose you are holding up a fragile vase in a perfectly ordinary setting that would typically lead to its breaking if dropped. According to some physical theories, the laws are indeterminate and will yield only objective chances for the future locations of the vase's particles. In fact there is an extraordinarily small chance that the vase is dropped and, due to exactly the right combination of quantum events, it simply remains suspended in the air. Let us assume this course of events is not ruled out by the laws; it is simply freakishly unlikely. The problem is that any completely specific course of events in which the vase drops and breaks will also involve a precise distribution of fundamental particles that was freakishly unlikely at the outset. This raises the worry that, if the vase is never actually dropped, worlds that continue in the first way should be considered equally 'close' to the actual world as those that continue in the second way. So if our semantics for counterfactuals of the form 'If A, then B' requires *all* the closest A-worlds to be B-worlds, many ordinary counterfactuals, like 'If you were to drop the vase, it would break', will turn out to be false.

Of course, we encountered a version of this problem in section 1.5 above. And as we saw there, a number of responses have been proposed in the literature.⁴⁶ Some theorists hold that the truth of a counterfactual requires only a high enough proportion of the closest A-worlds to be B-worlds (See n. 29 above). Others hold out hope that there is some principled way of distinguishing 'remarkable' low-probability worlds from 'unremarkable' ones — in that case we can jettison remarkable worlds from the inner sphere (See n. 26 above). And still others recommend giving up on the attempt to analyse counterfactuals in terms of anything like qualitative similarity. For example, Hawthorne contends that one A-world is counterfactually 'haloed' and that's that.⁴⁷

⁴⁶ For more discussion, see Hawthorne 2005, Hájek 2007, Williams 2008.

⁴⁷ On his preferred view: 'for any possibility that P, and any world *w*, there is a unique closest world to *w* where P. I realize, of course, that this is to give up altogether on the Lewisian idea of analyzing counterfactual closeness in terms of similarity.' Instead, 'the closeness relation between worlds and the counterfactual operator on propositions form a family into which there is no entering reductive wedge' (p. 404). McDermott (1999) also appeals to

Nevertheless, suppose we adopt the type of indeterminism sketched above, and then bite the bullet when it comes to the consequence that most ordinary counterfactuals are false (see Hájek 2007). In that case, what should we say about (PROP)? The truth-conditions it assigns to ‘this vase is disposed to break if dropped’ require that for a high proportion of precise dropping scenarios, the vase is such that it would break if dropped in them. But on our present assumptions, the vast majority of precise dropping scenarios will fail to determine a single outcome, or even a single outcome closest to the actual world. (Our ‘C-cases’ are not quite centred worlds but situations that are specific enough to settle everything causally relevant to the initial conditions that can trigger the relevant disposition.)⁴⁸ And given our present assumption about the semantics of counterfactuals, it will follow that ‘the vase would break if dropped in C’ turns out to be false for the vast majority of the relevant cases C, it follows that no vase will be disposed to break if dropped: clearly a counterintuitive result.

In response to this concern, one might say: so much the worse for the combination of assumptions just sketched! Or perhaps: if the vast majority of our ordinary counterfactual beliefs are false, why not our beliefs about dispositions as well? But there are also more conciliatory things to say. For example, in our 2008 we considered changing ‘would’ to ‘would probably’ in (PROP), but we decided that this would not quite do the trick: ‘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’ (p. 78). We concluded that (PROP) itself—and not just the corresponding analysis of comparative ascriptions, (MORE)—must be revised to ‘consider weighted proportions of C-cases’ (p. 79).

To be clear, here is why it would not work to use a complex weighting with (MORE) while making do with ‘would probably’ in the case of (PROP). Consider two objects whose breaking when dropped is governed by indeterministic laws. One is such that, for 30% of initial dropping scenarios, its chance of breaking is 0.51. The other is such that, for 29% of such scenarios, its chance of breaking is 0.99. There is a greater proportion of dropping-cases such that the first would probably break if dropped in that case. But not only do we want to say that the

primitive counterfactual facts in his treatment of the relationship between counterfactuals and indeterminism.

⁴⁸ See Manley and Wasserman 2007, p. 72.

second object is *more* disposed to break if dropped than the first; we also want to allow that in some context the second meets the threshold for ‘is disposed to break if dropped’ and the first does not. This illustrates the fact that the gradable positive predicate must involve a context-dependent specification of a threshold along the very same scale that is set by the comparative. And this uniformity would be lost if we took precise chances into account only for the comparative.

How exactly should the idea of ‘weighted proportions’ be fleshed out? In our 2007, we wrote that ‘if the world is indeterministic ... the account would involve an aggregate value for the many objective chances that *x* would break’ (p. 73). The idea is that for each C-case there will be a range of counterfactuals sensitive to what the chances are that a given object will break, such as ‘*x* would very likely break if it were in C’. If we focus only on the most specific ones, like ‘there would be a 0.67 chance of *x* breaking’, we can weight each C-case by the corresponding chance of breakage before arriving at the relevant proportion. On this way of measuring C-cases, the second object described above counts as more disposed to break if dropped. And it will also meet whatever threshold is in play for the gradable predicate before the first object does.

Vetter suggests a simpler account that (a) uses centred worlds in place of our C-cases, and (b) understands the chance of a object’s breaking subsequent to a fully specified initial condition in terms of the *proportion* of worlds with that initial condition that go on to involve the object’s breaking. (Hence her treatment of ‘would probably’ directly in terms of proportions of worlds; see Vetter 2012, pp. 1180 and 1184.) On this view, the degree to which *x* has a disposition can simply be settled by the proportion of centred *x*-dropping worlds that are also *x*-breaking worlds—and the chances will already be taken into account.

The problem is that some—ourselves included—find it implausible to treat chances as fixed by an enumeration of worlds. To fix ideas, consider a complete specification of the initial segment of a simple world, centred on one electron. Suppose the laws are indeterministic, but there are only two fully specific continuations they allow: there is a 99% chance that an electron in that situation goes left, and a 1% chance that it goes right. Everything else is settled. Now suppose we ask whether a given electron *e* is disposed to go left if it were in this scenario. Taking the chances into account, we should be inclined to say ‘yes’. But on many accounts of the nature of possible worlds, there will only be two nomically possible worlds in which *e* is in this scenario—in one it goes left, and in the other it goes right. (This is

because on many views there is only one world for every complete description of things.)⁴⁹ Vetter's proposal would require us to insist that there must be at least 99 duplicate LEFT-worlds, and in particular 99 times as many as there are RIGHT-worlds.⁵⁰

Likewise, consider Vetter's case of the probabilistic reverse Achilles' heel. Vetter claims that 'this [centred worlds] reading of MW's proposal solves the problem of PRAs'. But this conclusion requires the assumption that, since for each precise dropping scenario there is 99% objective chance that the object breaks, there must be 99 times as many centred worlds in which the object is in that scenario and breaks as worlds in which it is in that scenario and does not break. But we do not think this follows. And at any rate we would not want to tie our analysis of dispositions to an account of chance in terms of the enumeration of worlds. For those who *do* like this picture of chance—and who also hold the assumptions that motivated the worry to begin with—the revision in section 2 of Vetter's reply should be a compelling option.⁵¹

3.2 Modal force and stimulus conditions

In her section 5, Vetter sets out to refashion (PROP) after a type of semantic treatment for modal expressions like 'possibly' and 'necessarily'. She takes as paradigmatic the following semantic clause for 'probably':

- (PR) '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 (p. 1184)⁵²

She then offers a schema for semantic clauses for modal expressions: they must specify a *modal base* (in this case a restricted set of worlds)

⁴⁹ For example, this is the case for many 'ersatz' views of possible worlds.

⁵⁰ The point easily applies to Vetter's case of the probabilistic reverse Achilles' heel. It is consistent with Vetter's description that for each C-case there are only two maximally specific outcomes.

⁵¹ One might prefer a variant of the 'weighted proportion' approach that uses centred worlds instead of C-cases. One could weight each centred world according to the chance, at the initial conditions, of the outcome that in fact obtains at that centred world. This would make improbable breakings as well as improbable failures to break count for less towards the overall assessment of proportion. But we do not see any special advantage of simplicity for such a view. The differences between such a position and our own will turn out to be pretty *recherché*, especially given that C-cases settle everything causally relevant to the manifestation of a disposition.

⁵² We find this particular semantics for 'probably' implausible—whether the relevant modality is metaphysical or epistemic—but for reasons that do not bear on the contrasts Vetter wants to illustrate.

and a *modal force* (in this case characterized by the determiner ‘most’). Turning to (PROP), Vetter recommends using centred worlds for the modal base, but wants to know what the *modal force* would be if (PROP) were pressed into this schema. She doubts that the phrase ‘a suitable proportion’ is very helpful in this respect: “‘Suitable’ cries out for supplementation ... : suitable for what?” (p. 1185). In addition, the threshold proportion required to satisfy a given dispositional predicate is highly context dependent, so the modal force invoked in the analysis would have to be extremely variable, covering virtually the entirely quantificational spectrum.⁵³ ‘But a quantifier that covers the entire quantificational spectrum is no quantifier at all, except in a trivial sense’ (p. 1187). For this reason, she ‘cannot even begin to see’ how any quantifier could correspond to ‘a suitable proportion’ (p. 1185).

To respond: we do not see why a semantic clause for dispositional predicates should be modeled after (PR) rather than after the standard semantic treatment of context-dependent gradable predicates. After all, the expressions at issue *are* context-dependent gradable predicates rather than modal sentential operators. Vetter is assuming that our account ought to be statable in the following form:

(MS) ‘N is disposed to M in C’ is true at a context *c* iff DET C-cases are such that N would M in them

where the modal base is identified with the set of C-cases and the modal force is characterized by some as-yet-to-be-identified determiner (DET) that can vary across the quantificational spectrum. But no such determiner is necessary to state the view. In (PROP), the phrase ‘a suitable proportion’ simply quantifies existentially over *proportions* that count in the context of utterance as suitably large — namely, those that are at least as large as a threshold proportion settled by context.⁵⁴ Here is a more perspicuous statement. For a given context *c* that determines a threshold proportion *p*:

(PROP*) ‘N is disposed to M in C’ is true at *c* iff there is a proportion of C-cases $\geq p$ in which N would M

⁵³ We agree that the context-dependence of the target sentence is not to be captured by simply by varying the domain of C-cases from one context to another, and so if we were to provide a semantics of the form given in (MS) below, the relevant determiner would itself have to be highly context-dependent.

⁵⁴ ‘How big a proportion is “suitable” will depend ... on the context of utterance ... We can think of the contexts as providing the standards for “fragility” by establishing a requisite proportion of C-cases in which an object would break, for example’ (2008, p. 76).

There is no need for ‘suitable’ in the metalanguage, or for variable quantification over C-cases.

In appealing to a contextually-supplied threshold, (PROP) implements the standard approach to context-dependent gradable predicates like ‘tall’ and ‘rich’. These ascribe properties that come in degrees along a scale, and are closely related to comparatives that describe an ordering between objects on that scale. Thus ‘*x* is taller than *y*’ orders objects by height without any contextual contribution, but ‘*x* is tall’ requires context to settle a threshold point along the height scale.⁵⁵ (And degree modifiers like ‘very’ can be treated as boosting the required height.) On our view, the degree to which an object is disposed to M in C corresponds to the proportion of C-cases in which it would M. With the resulting scale, the semantics of ‘more disposed to M in C’, ‘disposed to M in C’, and ‘very disposed to M in C’ can proceed in the standard way.

In short, we see no reason to press (PROP) into the form of (MS). But neither do we see any barrier to doing so: one could use a determiner that is sensitive to a contextual parameter of the sort just sketched. And arguably there are such determiners in English—for example, ‘many’.⁵⁶ (Note that the proportion of Fs that must be G to suffice for the truth of ‘Many Fs are G’ can vary widely: contrast ‘Many Americans were killed on 9/11’ with ‘Not many of his cousins are male’).⁵⁷ In fact, although Vetter expresses doubts that any determiner could stand in for ‘DET’, she herself glosses (PROP) using ‘many’ as the relevant determiner in several places (pp. 1175–7, 1181). This is not to claim that replacing ‘DET’ with ‘many’ would yield an instance of (MS) that would be true at every context. But given the radical context-dependence of ‘many’, we see no bar in principle to the existence of a determiner that could stand in for ‘DET’. We prefer

⁵⁵ See nn. 17 and 18 above for some of the relevant literature, and see our 2007 for an extended discussion of (PROP) and its relationship to the semantics of gradable predicates.

⁵⁶ Wasserman (2011) explores exactly this proposal.

⁵⁷ In a typical context where the first is uttered, it is enough that 1 in 100,000 Americans were killed; in a typical context where the second is uttered, it may not be enough even if 1 in 3 cousins are male. This variability occurs even in cases involving the same modal base: in plenty of contexts one can truly say ‘Not many Americans are atheists’ even though about two percent are. This sort of phenomenon makes it difficult to provide a satisfactory semantics for ‘many’ without making use of an additional contextual contribution, such as a threshold proportion or a ‘normal’ number. See, for example, Lappin 2000.

(PROP) in part because it seems worthwhile to avoid context-dependent expressions in the metalanguage.⁵⁸

Vetter concludes with a positive suggestion for how to fill out (MS) while avoiding variable modal force. We had argued in our paper that some dispositions have no stimulus conditions to speak of, and suggested that, for example, fragility is simply the disposition to break (p. 72). For such dispositions, we had said (p. 77), the range of C-cases will include all (nomologically) possible scenarios.⁵⁹ Vetter's idea is that in such cases, the range of C-cases is so large that the quantifier can be conceived of as simply (non-negligible) possibility. In fact, 'perhaps disposition ascriptions *in general* are to be understood simply in terms of their manifestation conditions'. In that case:

The 'cases' are now any (centred) worlds whatsoever, without any restriction apart from the general ones, ones, to wit, that N exist in them and 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. (p. 1188)

However, this view is subject to some significant problems.

First, consider an object that fails to satisfy a disposition ascription even though it is subject to a reverse-fink, a reverse-mask, or an Achilles' heel. Presumably Vetter's idea is that, since all nomologically possible cases are in the domain rather than simply stimulus cases, the *proportion* of cases in which such an object exhibits the manifestation behaviour will be too 'negligible' for the object to count as having the disposition. (As we argued in our previous papers, such cases cannot simply be ruled out of the relevant domain in context, and Vetter appears to accept this point.) But surely that will not always be the case. For example, there will be contexts in which an atom does not count as 'disposed to remain stable' even though it would remain stable in some much-higher-than-negligible proportion of nomologically possible situations.

Relatedly, we doubt such a view can properly handle the context-dependence of dispositional ascriptions. Vetter does not intend for the relevant domain to shift from one context of ascription another—she stipulates a general lack of restrictions beyond 'the general ones'.⁶⁰

⁵⁸ Contrast Vetter's gloss on her semantic clause for 'probably': 'What counts as probably true varies between contexts, and which proportion counts as most of the cases varies as well' (p. 1185).

⁵⁹ That is, those containin the object as it actually is intrinsically, at least at the outset of each scenario (2008, p. 76). One exception will involve stimulus conditions that specify intrinsic change in the object, like the disposition to laugh when drunk, predicated of a sober person.

Instead, perhaps context shifts the requisite proportion of cases, as on our view. But how is the result an improvement? If one were to focus only on the case of ‘fragile’, one might have thought that relevant proportions would all be close-to-negligible proportions of all possible cases. But as we have just seen, this cannot be right in general.

Third, what about cases where dispositional ascriptions have explicit stimulus conditions? It is hard to see how one could avoid using these conditions to restrict the modal domain. Note the truth-conditional difference, at a given context, between ‘Bob is disposed to be angry when drunk’ and ‘Bob is disposed to be drunk and angry’. If Bob virtually never gets drunk or angry, he might satisfy the first ascription but not the second. (In fact he might be highly disposed to sobriety precisely because he is aware of his disposition to be angry when drunk.) So we certainly cannot treat both ascriptions as simply requiring that in some uniformly low proportion of *all* cases, Bob is angry and drunk. The first ascription concerns the proportion of *drunken* cases in which Bob gets angry, making his customary sobriety irrelevant to the truth-conditions. Thus, at least some dispositions appear to have stimulus conditions that act as restrictions on the relevant modal domain. In other words, it cannot be that ‘disposition ascriptions *in general* are to be understood simply in terms of their manifestation conditions’ (p. 1188).

In short, while Vetter’s semantic proposal is appealing in its simplicity, it is not flexible enough to account for the full range of data involving disposition ascriptions.

⁶⁰ And anyway, the context-dependence of dispositional ascriptions cannot simply be a matter of shifting domains of C-cases. Suppose a thick glass G counts as ‘fragile’ in some ordinary context C₁ but not in some other ordinary context C₂. To handle this by shifting domains would require either that there are cases in which G breaks that we ignore in C₂ but not in C₁; or cases in which G does *not* break that we ignore in C₁ but not in C₂. But intuitively, it might be that in both contexts the very same cases are relevant to the degree to which a thing has the ascribed disposition, and the same cases are such that breakings in them would count as manifestations of the ascribed disposition. And this fact would be brought out by considering what cases would count towards an assessment, in each context, of the *degree* to which a thing had the disposition, as well as the resulting comparative judgements one would make regarding objects that differed only in whether they would break in a given kind of case. (See our 2007, pp. 73–4, where we argue that C-cases should include all ‘circumstances in which giving a response would count as a manifestation of that disposition’.)

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