

# Rosenkranz on Quandary, Vagueness and Intuitionism

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Sven Rosenkranz provides a searching critique of a line of argument in my 2001 paper ‘On Being in a Quandary’ (henceforward ‘Quandary’) towards a broadly intuitionistic conception of the nature and logic of vagueness. I think his principal criticism scores some hits. In the three sections to follow I shall respectively outline what I take to be the essence of his criticism, propose a modification of my argument that I think can resist it, and then suggest a different, more economical route to the proposed intuitionistic distinctions for vague statements.

## 1.

Our concern is with the usual kind of Sorites series for a vague predicate  $F$ , with  $x'$  the immediate successor of any of its elements  $x$ . The argument on which Rosenkranz is focusing occurs at pp. 78 ff of ‘Quandary’. There it is suggested that whenever Evidential Constraint (EC) holds for atomic predications of  $F$  and their negations (I will term both kinds of statement ‘ $F$ -predications’), then any truth of the form,  $Fa \wedge \neg Fa'$ , will be knowable and hence that the conditional

$$(1) \exists x(Fx \wedge \neg Fx') \rightarrow \exists x \text{FeasK}(Fx \wedge \neg Fx')^1$$

will hold good (and be known if EC is.) My proposal was then that, since any sharp cut-off would lie in the region of quandary for  $F$ —the region of cases where we do not know that knowledge of the truth of an  $F$ -predication is so much as metaphysically possible—the consequent of (1) will also present as a quandary. We accordingly do not know that it holds nor therefore that (1)’s antecedent does. However, since the Sorites reasoning itself disproves the negation of the antecedent, we do

<sup>1</sup> As in ‘Quandary’ I write ‘FeasK[...]’ to express the feasibility of knowledge that [...]. I (and Rosenkranz) assume that the relevant range of  $F$ -predications may be so understood as to give rise to no good concern about ‘quantifying in’.

know that the double negation of (1)'s antecedent holds. So we have a solid motive to refuse Double Negation Elimination in this context. And once we do so, we are empowered to treat the Sorites as a harmless *reductio* of its major premiss, without the ensuing uncomfortable commitment, imposed by classical logic, to claiming that there is a sharp cut-off on the series in question.

It is of course essential to this strategy that the consequent of (1) is *no worse* than a quandary. For reason to *deny* the consequent would contrapose into reason to deny its antecedent—and so would motivate the major premiss for the paradox. That is the potential Achilles heel at which Rosenkranz is probing.

Let ' $KX(A)$ ' express that  $A$  is known by methods  $X$  and consider

$$(2) \neg \exists x(\text{FeasKM}(Fx \wedge \neg Fx')),$$

where  $M$  denotes our actual normal range of means for assessing  $F$ -predications. Rosenkranz claims that we have undefeated empirical evidence (I will call it the *Rosenkranz evidence*) for (2). Now suppose

$$(3) \text{Methods } M \text{ are } \textit{comprehensive}$$

—that any predication of  $F$  which can be known at all can be known by application of methods  $M$ . (2) and (3) together presumably entail the negation of the consequent of (1). and hence the negation of its antecedent,

$$(4) \neg \exists x(Fx \wedge \neg Fx')$$

But (4) of course engenders a Sorites paradox. So the friend of EC who acknowledges the Rosenkranz evidence must regard (3) as false.

That is certainly worrying if one reads it as a commitment to the usual (classical) negation of (3), namely

$$(5) \text{There are methods besides } M \text{ for competently assessing } F\text{-predications}$$

If that conclusion is allowed, then it appears that it is the defender of EC who is reduced to making a claim for which there is no evidence (assuming his evidence for EC did not already involve a defence of (5))

However, this reading is questionable in the present dialectical context. The original argument was that the combination of classical logic, EC and quandary leads to claims for which there is no warrant and that, if we want a logic which is conservative with respect to warrant,

then intuitionistic distinctions are required, in particular the distinction between a proposition and its double negation. The argument above, for its part, seems to show that EC, classical logic and the Rosenkranz evidence likewise leads to an unwarranted claim. So there is once again the option of rejection of classical logic and the enforcement of intuitionistic distinctions. If we take that option, we can rest on the intuitionistic negation of (3)

- (6) It is not the case that methods *M* are the only competent means for assessing *F*-predications

without commitment to the dubious existential claim made by its classical counterpart.

There are, however, concerns about a commitment, issuing in this way, even to (6). The position is that, in the presence of the Rosenkranz evidence, there are grounds for EC only if there are grounds for denying the comprehensiveness of existing methods for assessing *F*-predications. But it does not seem that, for the general run of vague predicates, we have any such grounds. What reason can anyone produce for supposing that the knowable heaps, bald men and adolescents are not exactly those which can be recognized as such on the basis of our standard grounds for applying such concepts? However if EC really does enforce the conditional (1), then—in the presence of grounds for (2)—we must have some such reason, or lack any sufficient reason for EC in the first place. Rosenkranz is suggesting that the situation is the latter.

The situation is actually yet more awkward, once account is taken of the kind of special motivation for EC envisaged in ‘Quandary’. Consider again the case of colour and take ‘*F*’ as ‘red’. In support of regarding EC as good for colour I wrote as follows:

It is a feature of the ordinary concept of colour that colours are *transparent* under suitable conditions of observation: that if a surface is red, it ... will appear as such when observed under suitable conditions; *mutatis mutandis* if it is not red. Colour properties have essentially to do with how things visually appear and their instantiations, when they are instantiated, may always in principle be detected by our finding that they do indeed present appropriate visual appearances. So, according to our ordinary thinking about colour ... EC is inescapable ...<sup>2</sup>

But if that is the motive, then the case for EC for simple colour predications is *exactly* a case for regarding all true such predications as recognizable by methods *M*—a case for the relevant version of (3). So the

<sup>2</sup> Wright 2001 (‘Quandary’) p. 72.

derivation of (1) from EC—however exactly it is envisaged as going (we will come to that in a minute)—will presumably go over into one of

$$(1)^M \exists x(Fx \wedge \neg Fx') \rightarrow \exists x(\text{FeasKM}(Fx \wedge \neg Fx'))$$

But now the Rosenkranz evidence will sustain a contraposition, and thereby reinstate a Sorites. Therefore the following appears enforced: if we really do have the Rosenkranz evidence, then the (a priori) motivation for EC, in so far as it is typified by the thought outlined for colour, must be regarded as defective. And in that case—since that was the motivation I offered—the ‘Quandary’ argument for intuitionistic revisions collapses.

Rosenkranz himself does not elaborate on the nature of the Rosenkranz evidence. But it would not be a good response to question whether we really do have any such evidence. It does seem clear that no-one with a normal training in and understanding of the general run of vague predicates ever feels confident about drawing a line in a Sorites series—so much is indeed a datum of the problem, a basic manifestation of vagueness itself. If belief is a necessary condition of knowledge, then evidence that it is not possible to move someone to hold an opinion of the form,  $Fa$  and  $\neg Fa'$ , on the basis of reliance of our ordinary methods of assessment of  $F$ -predications is evidence that it is not possible to acquire an item of knowledge of that form on the basis of just those methods either.

## 2.

A better reaction is rather to look again at the pedigree of the conditional (1). Rosenkranz himself observes that in order to derive (1) from the two ingredient conditionals of the relevant case of EC:

$$Fa \rightarrow \text{FeasK}(Fa)$$

$$\neg Fa \rightarrow \text{FeasK}(\neg Fa)$$

we need to presuppose that feasible knowledge is additive in the relevant cases, that the feasibility of knowledge that  $Fa$  and that  $\neg Fa'$  individually implies the feasibility of knowledge of their conjunction. He points out that additivity is not exceptionless: that it fails for example, with the conjuncts of Fitch sentences and will fail wherever feasible knowledge is disciplined by margins of error in the manner argued by Williamson and  $a$  and  $a'$  are sufficiently similar.<sup>3</sup> He might have added

<sup>3</sup> The basic reference is Williamson 1992.

that it also may fail in cases where the realization of knowledge involves significant opportunity costs. To be sure, none of those considerations seems germane to the type of case at hand. The conjunctions in question display nothing analogous to the knowledge-limitative character of the Fitch conjunctions, the relevant margin of error principle

$$\text{FeasK}(Fa) \rightarrow Fa'$$

is not available in the presence of EC,<sup>4</sup> and there do not seem to be relevant opportunity costs associated with the application of standard (casual observational) methods for assessing predications of the kind that concern us. But what Rosenkranz should also have observed is that the Rosenkranz evidence *itself* presents a powerful and relevant objection to additivity. If the evidence is that reliance on our actual methods for assessment of predications of  $F$  is impotent to generate conviction about the truth of a conjunction of the form,  $Fa \wedge \neg Fa'$ , then one who believes in EC for  $F$ -predications ought to suppose both that, if some such conjunction is true, then each of its conjuncts is individually knowable and that (there is no reason to think that) any such conjunction is knowable on the basis of actual methods. So there is no basis for additivity in the relevant context.

Rosenkranz's discussion leads him to the general conclusion that there is a tension, not between EC itself and the admission of quandary, but between our possession of compelling a priori warrant for EC while we simultaneously recognize that some of the statements within its scope present quandaries. However there is no reason to accept that general conclusion unless EC does indeed impose the conditional (1) — and that we have just seen reason to doubt. The required additivity step is suspect. Yet without conditional (1), does not my case for a distinction between the acceptability conditions of  $\exists x(Fx \wedge \neg Fx')$  and those of its double negation collapse anyway?

Well, not for that reason at any rate. There was never any essential need for conditional (1). Any conditional of the form

$$\exists x(Fx \wedge \neg Fx') \rightarrow A$$

for which EC does provide compelling reason and whose consequent presents a quandary will subserve the 'Quandary' argument. Consider for instance

$$(1)^* \quad \exists x(Fx \wedge \neg Fx') \rightarrow \exists x(\text{FeasK}(Fx) \wedge \text{FeasK}(\neg Fx'))$$

No question but that the two schematic EC conditionals:

<sup>4</sup> Since in conjunction, the two principles will obviously introduce Sorites-form paradoxes

$$Fa \rightarrow \text{FeasK}(Fa)$$

$$\neg Fa \rightarrow \text{FeasK}(\neg Fa)$$

entail (1)\*. But since any verifying instance,  $x$ , of the consequent of (1)\* must fall in the borderline region for  $F$ , the conception of that region as consisting in cases where  $F$ -predications make for quandary implies that we are in no position to claim that the two items of knowledge demanded by the consequent—knowledge of some such instance,  $a$ , that it is  $F$ , and knowledge of its immediate successor that it is not  $F$ —are feasible. Accordingly we are in no position to claim knowledge of the consequent of (1)\* itself not therefore, if knowledge of EC, and hence of (1)\* is assumed, knowledge of its antecedent. From there, the argument can proceed as before. The reliance on additivity, involved in the derivation of (1) from EC, was an unnecessary hostage to fortune.

Once again, this is no progress unless the consequent of (1)\* is indeed a quandary and not something which, like the consequent of (1), we have reason to *deny* given additional plausible assumptions—or anyway assumptions which we cannot reasonably claim grounds to reject. Do analogues of the problems that attended (1) in this regard also afflict (1)\*?

The assumption that posed the problems for (1) was assumption (3), that all true predications of  $F$  may be known by reliance on our existing methods  $M$ . It seemed clear—the Rosenkranz evidence is that—no conjunction of the form,  $Fa \wedge \neg Fa'$ , may be so known. So, on the assumption of EC, an arguably unwarranted denial of (3) seemed to be enforced. But the Rosenkranz evidence makes no similar trouble for (1)\* since it concerns our inability to know conjunctions and the consequent of (1)\* precisely makes no claim about that

Evidence that would make trouble for (1)\* in a similar way would, of course, be evidence that for any  $x$  and  $x'$ , there is no possibility that  $Fx$  is known and that  $\neg Fx'$  is known. Do we have any such evidence? According to the view of borderline cases proposed by 'Quandary', no-one presented with a borderline case of  $F$  can know that someone who takes a suitably qualified but nevertheless committal—positive or negative—view of it does so incorrectly. Borderline cases are cases where one's springs of opinion dry up—cases where one fails to come to a view despite enjoying every normal cognitive advantage relevant to a judgement of the kind concerned. But they are not cases where one recognizes that no knowledgeable verdict is possible. When, in a Sorites series, I reach a range of cases about which I am reluctant to give a verdict, it does not convict you of incompetence if you are not so reluctant

provided your willingness to take a view is appropriately qualified and it is wholly understandable to you that others may not share it. To regard a case as borderline is not to regard it as having a status inconsistent with both polar verdicts, but to find oneself unable to come to a polar verdict. Such an inability is consistent with recognizing that other, competent judges may, tentatively, feel able to do so without betraying their competence. For any borderline pair,  $a$  and  $a'$ , about which I myself am moved to no view, it may be, for all I can rule out, that to judge that  $Fa$ , or  $\neg Fa'$ , is to judge truly. And if such judgements are arrived at by careful exercise of an appropriate competence in what we think of as relevantly unimprovable circumstances, that is surely to grant that, if correct, they are knowledgeable. So viewing a case,  $a$ , as borderline, on this conception, does indeed fall short of non-exclusion of the feasibility of knowledge that  $Fa$  and of the feasibility of knowledge that  $\neg Fa'$ .

That conclusion still leaves a gap. Suppose  $a$  and  $a'$  are cases about which we have a 'drying of the springs of opinion'. What has just been suggested is that we cannot, in that case, discount the idea that the verdict,  $Fa$ , is true nor, if it is arrived at by someone competent in the right kind of way, that it is knowledgeable. Likewise for the verdict,  $\neg Fa'$ . But it is a further step to conclude that we cannot rule out that *both* those verdicts are knowledgeable—which is what regarding the consequent of (1)\* as a mere quandary. requires. For all that has been argued, it might yet be that a necessary condition for the knowledgability of the verdict,  $Fa$ , is precisely that there can be no knowledge of  $\neg Fa'$ .

This gap, however, was implicitly closed by the penultimate point in the line of thought outlined: the claim that if someone arrives at a true judgement about an  $F$ -predication by careful exercise of the normal competences in relevantly unimprovable circumstances, then his judgement is perforce knowledgeable. If this is accepted then to allow that while we cannot exclude the possibility that the judgement,  $\neg Fa'$ , is knowledgeable, we can exclude its being so in any case where there is also a knowledgeable judgement of  $Fa$ , is implicitly to grant that whenever  $\neg Fa'$  holds for some  $a$ , it cannot be that  $Fa$  is true. For if it were, someone might judge it to be so in the circumstances just granted to make that judgement knowledgeable. But then we have implicitly granted that whenever  $\neg Fa'$ ,  $\neg Fa$  holds also and so set the Sorites running again.

I propose, then, that the line of thought in 'Quandary' which attracts Rosenkranz's objection may be so modified, by replacing (1) by (1)\*, as to draw its sting.

## 3.

My principal concern in the parts of 'Quandary' which deal with vagueness was to explore the possibilities for an intuitionistic treatment whose motivation would be as close as possible to (what I regard as) the most powerful case for Intuitionism in mathematics. Let me therefore conclude by observing—which I did not do in 'Quandary'<sup>5</sup>—that actually the most basic case for suspension of classical logic where vague statements are concerned is independent of their subjection to Evidential Constraint.

First, note that *any* sufficient case, EC-driven or not, for agnosticism about Bivalence over the predications in a Sorites series is a reason for agnosticism about the existence of a sharp cut-off. The point is simply that *F*-ness monotonically decreases in such a series. So reason to believe in the existence of an  $\{Fx; \neg Fx'\}$  pairing would be reason to believe that all of *x*'s predecessors were *F* and all the successors of *x*' were not, and thereby to accept that Bivalence held over all the *F*-predications involved. Any sufficient case for agnosticism about Bivalence is therefore a sufficient case for agnosticism about the existence of cut-offs,—about the truth of the relevant instance of ' $\exists x(Fx \wedge \neg Fx')$ '—notwithstanding the elementary and unexceptionable *reductio* provided by the Sorites itself of such statements' negations.

Rosenkranz does not question that the recognition that a range of statements is both subject to EC and includes quandaries must rationally undercut an adherence to Bivalence as a known principle concerning them. The thrust of his intended criticisms is against the possibility of any such recognition—against the existence of any motivation for EC in the presence of quandary. However, while—*pace* Rosenkranz—I stick to it that (enough, basic) vague predications may be recognized to meet these twin conditions, the most basic cause for concern about Bivalence as applied to vague statements is just a direct corollary of what it commits us to, even without EC.

Critics of standard Epistemicism<sup>6</sup> have generally fastened onto its perceived hostages to semantic theory. If there really are the sharp boundaries to the application of vague expressions in which the Epistemicist believes, then each vague predicate, for example, is associated (in any given context of use) with an absolutely definite extension. Where is the theory that tells us what constitutes these associations? What makes it the case, for example, that my use of 'red' has the partic-

<sup>5</sup> But see, however, my 'Vagueness; a Fifth Column Approach' (Wright forthcoming 2003).

<sup>6</sup> For instance, Wright 1995 and Schiffer 1999.



ular sharply bounded extension that it does? Epistemicists often go pragmatist at this point and invoke the (alleged) advantages of classical logic as a ground to accept that there is indeed such an extension. But semantic facts must somehow be constituted in linguistic practice and the (alleged) pragmatic advantages of classical logic provide no clue as to how the association might, even in principle, have been set up by our practice. How can we reasonably believe in the existence of something for which we have no direct evidence and no adequate conception of how its existence is possible?

Timothy Williamson has tended to reply to this concern that the connections between language and the world are something of which we lack an adequate philosophical account in general, vagueness apart—that Epistemicism ‘has not been shown to be inconsistent with anything taught by the theory of reference’.<sup>7</sup> That may be. But the one reasonably clear model (or type of model) we have of how the determination of the extension of a predicate may not be transparent to those who fully understand that predicate—the model of lay natural kind terms like ‘water’ and ‘heat’ owing to Kripke and Putnam—seems to have no relevant bearing on vague expressions in general. I myself see no reason to expect, and every reason to doubt that we shall ever have a generally satisfactory semantics of natural language—especially a semantics of predication—which discharges Epistemicism’s debts. But let that opinion pass. The question is: can anyone at all, in our present state of information, justifiably take themselves to *know* that Bivalence is good for vague sentences? If it is, each vague expression is associated with a sharply bounded semantic value of the kind appropriate to it, a sharply bounded property, relation, function, or whatever. Grant that our so-far articulated philosophical understanding of the determination of semantic value does not put us in position to rule that out (even if we regard it as outlandish.) The question is: can anyone, even the most rampant Epistemicist, put her hand on her heart and say that she *knows* that such is indeed the situation—that the required semantic associations really are in place? Williamson’s defensive point was: well, you cannot rule it out. But we can grant that and still quite rightly be agnostic about the matter. And if we are, we should be agnostic about Bivalence in this context too. Such agnosticism is little more than proper intellectual modesty.

<sup>7</sup> At p. 43 of Williamson 1996.

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