Should Friends and Frenemies of Understanding be Friends? Discussing de Regt

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In typical philosophical exchanges, one perfunctorily commends one's interlocutor before engaging in the intellectual equivalent of gladiatorial melee. Philosophical discussions about scientific understanding aren't special in this regard. Some, like me, hold that understanding is derivative of explanation and knowledge. Call us the *frenemies* of understanding. Under the thrall of this gladiatorial paradigm, we frenemies have sometimes taken too much glee in deflating the ambitions of the *friends* of understanding, who like my interlocutor, Henk de Regt, hold that something about understanding eludes explanation and knowledge.

These exercises in antagonism are tired, so I aim to disrupt them. I propose an alliance between these seemingly contradictory positions. Building bridges rather than burning them is the surer path to philosophical progress. Hence, the frenemy and friend of understanding should be friends![1] If you still pine for

[1] In this essay, I only look at de Regt's and my views; future work should forge connections with other friends and frenemies of understanding.

philosophical blood sport, then consider this a battle against debates that have outlived their usefulness.

1. Friends and Frenemies

Friendship between friends and frenemies requires mutual understanding about understanding. So, let's begin by discussing my and de Regt's views.

1.1. My Frenemy Account

In my book (Khalifa 2017), I presented my frenemy view—the Explanation-Knowledge-Science (EKS) Model of understanding—as a traditional philosophical analysis, replete with necessary and sufficient conditions. However, as part of my kinder, gentler approach, I will be breezier here. I subscribe to three core principles, the first of which is the *Explanatory Floor*:

> Understanding why *P* requires possession of a correct explanation of why *P*.

The Floor's underlying intuition is simple. It seems odd to understand why *P* while lacking a correct answer to the question, "Why *P*?" For instance, the person who lacks a correct answer to the question "Why do apples fall from trees?" doesn't understand why apples fall from trees. Since explanations are answers to why-questions, the Floor appears platitudinous.

But what does it mean to "possess" an explanation? Since the Floor only concerns what understanding *requires*, explanatory possession is any representation of an explanation with a mind-to-world direction of fit.[2] So, understanding requires something like accepting or believing an explanation, but it doesn't require desiring or coveting an explanation. This still leaves the delicate issue of when an explanation is "correct." As we shall see below, this is one place where friends and frenemies might join forces.

My remaining principles allow understanding to rise above the Floor. Suppose that you can correctly identify two causes of a fire, and I can only identify one of those causes. *Ceteris paribus*, you understand why the fire occurred better than I do. This intuition is enshrined in the EKS Model's *Nexus Principle*:

> Understanding why *P* improves in proportion to the amount of correct explanatory information about *P* (= *P's* explanatory nexus) in one's possession.

However, two agents who possess the same explanatory information may nevertheless differ in understanding because of their abilities to use that information in illuminating ways. The last of my principles, *The Scientific Knowledge Principle,* does justice to this intuition:

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[2] Khalifa (2017) restricted this only to belief. For reasons presented in Khalifa and Millson (2020), I have come to think this too restrictive. Moreover, Khalifa (2017) characterized the Floor as an account of "minimal understanding." I thank Federica Malfatti for spotting some infelicities with that formulation.

> Understanding why *P* improves as one's possession of explanatory information about *P* bears greater resemblance to scientific knowledge of why *P*.

Now, for this last principle to have teeth, I owe you an account of the relevant kind of scientific knowledge. Behold its teeth!

> *S* has scientific knowledge of why *P* if and only if there is some *Q* such that *S*'s belief that *Q explains why P* is the safe result of *S's* scientific explanatory evaluation (SEEing).

The core notions here are safety and SEEing. Safety is an epistemological concept that requires an agent's belief to not easily have been false given the way in which it was formed (Pritchard 2009). SEEing then describes the way beliefs in an explanation should be formed to promote understanding. SEEing consists of three phases:

1. *Considering* plausible potential explanations of why *P*;
2. *Comparing* those explanations using the best available methods and evidence; and
3. Undertaking *commitments* to these explanations on the basis of these comparisons. Paradigmatically, commitment entails that one believes only

those plausible potential explanations that are decisive "winners" at the phase of comparison.

Thus, scientific knowledge of an explanation is achieved when one's commitment to an explanation could not easily have been false given the ways in which that explanation was compared to other potential explanations of the phenomenon.

So, when it comes to sentences of the form "*S* understands why *P*," the EKS Model suggests that in addition to the Floor's requirements, understanding requires having a "sufficiently good grasp" of the "relevant" explanatory information. Context determines "sufficiency" and "relevance." The Nexus and Scientific Knowledge Principles determine the sense of "good grasp."

As should be clear, my recipe for understanding consists of the two basic frenemy ingredients—explanation and knowledge. Give or take some niceties, the EKS Model suggests that we know a bunch of stuff, and we just happen to call the *explanatory* stuff that we know "understanding." So, frenemies take understanding to be a philosophical dish best served bland.

1.2. de Regt's Friendly Account

By contrast, de Regt's palate is subtler than mine. When he discusses understanding, he savors little hints of grasping and delights in morsels of skillful intellection. Since these goodies seem to go beyond knowledge and explanation, he is a *friend* of understanding. More precisely, de Regt offers the following Criterion of Understanding Phenomena:

> **CUP:** A phenomenon P is understood scientifically if and only if there is an explanation of P that is based on an intelligible theory T and conforms to the basic epistemic values of empirical adequacy and internal consistency (De Regt 2017, 92).

Here, de Regt (2017, 38) construes empirical adequacy and consistency as values because "there may be variation in how these values are ranked and applied in specific cases." He takes them to be "basic" because every scientific explanation must exhibit these two values to some degree.

As might be expected, frenemies like me are not hostile to empirical adequacy and consistency. However, de Regt's master concept is that of intelligibility, which appears to be less at home behind frenemy lines. De Regt offers two complementary characterizations of intelligibility. First, he offers a "macro-level" characterization, intended to apply to science as a whole:

> *Intelligibility*: the value that scientists attribute to the cluster of qualities of a theory (in one or more of its representations) that facilitate the use of the theory (De Regt 2017, 40).

Here, the theoretical "qualities" denote a wide variety of explanatory desiderata, including simplicity, scope, familiarity, causation, unification, mechanism, and visualizability. Moreover, “using” a theory is exercising the requisite judgment and skills needed to construct and evaluate explanatory models of the phenomena.

Different scientists place greater value on different clusters of qualities. Scientists' skills determine which of these clusters furnish intelligibility in a given context. However, in this skeletal form, one might worry that the contextualist catchall is a short path to triviality. De Regt artfully blunts this worry with a “meso-level” account of intelligibility,[3] the Criterion for the Intelligibility of Theories:

> **CIT$_1$**: A scientific theory *T* (in one or more of its representations) is intelligible for scientists (in context *C*) if they can recognize the qualitatively characteristic consequences of *T* without performing exact calculations (De Regt 2017, 102).

Although de Regt offers no other criteria of intelligibility, he presents CIT$_1$ as providing only sufficient conditions (and subscripts it) to suggest that theories can be intelligible in other ways (De Regt 2017, 102, n.15). Moreover, CIT$_1$ appears intuitive—when scientists can effortlessly engage in qualitative reasoning with a theory, they have a “feel” for the theory. We frequently associate this “feel” for a theory with intelligibility and understanding.

[3] Meso-level standards of intelligibility characterize a particular scientific community's norms regarding understanding. De Regt also describes individual or micro-level standards of intelligibility, but these do not figure prominently in his account.

Hence, like me, de Regt accords a prominent role to explanation in understanding. However, whereas I appeal to the frenemy concept of scientific knowledge to flesh out understanding's remainder, he appeals to the friendly concept of intelligibility. With these details in hand, grab your popcorn and enjoy the show. Remember, however, that this isn't a tale of a valiant hero defeating a dreaded rival—but rather a story of a budding friendship.

2. Consideration

Let's consider a case of understanding—in the service of understanding a case of (explanatory) consideration.[4] The late 1960s marked the dawn of the Standard Model in particle physics. Quarky ideas appeared in theoretical papers and on dusty chalkboards, but experimental evidence was scarce. The tides began to turn when experimenters at Stanford and at MIT discovered unexpectedly high cross-sections in deep inelastic electron-proton scattering experiments, suggesting a more rigidly structured proton than previously conjectured. In other corners of Palo Alto, theorist James Bjorken had been developing a model of hadronic structure. He asked his colleagues to plot two curves from the data of their already-interesting results to test his model. To their delight, his model correctly predicted these "Bjorken scaling curves," as they are now called.

However, there was one problem. Few of the experimenters *understood* Bjorken's model. Their exasperated cries fell on the ears of one Richard Feynman,

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[4] See Khalifa (2017, Ch. 2) for further discussion.

who in August 1968 expanded on Bjorken's original explanation—the latter being heavily steeped in then-esoteric current-algebraic notation. Feynman's core idea is that hadrons are composed of hard, point-like entities that he called "partons" (and which are now called "quarks.") Mathematically speaking, Bjorken and Feynman's models are near equivalents, yet only the latter greatly facilitated experimenters in designing tests to compare this explanation to other explanations of Bjorken scaling.

Importantly, Feynman's *consideration* of the explanation of Bjorken scaling in terms of electron-parton interactions should be distinguished from the experiments done to *compare* that explanation to its rivals. This much the EKS Model gets right. However, my description of explanatory consideration has been paper-thin. De Regt's view has resources for a far richer account of explanatory consideration than mine. Our first opportunity for bridge-building arises.

2.1. Friendly Considerations

I propose the following:

> A *potential* explanation *E* of phenomenon *P deserves consideration (in SEEing)* if and only if:
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> (a) *E* is based on an intelligible theory *T*, and
>
> (b) *E* conforms to the basic epistemic values of empirical adequacy and internal consistency.

This differs from de Regt's CUP in being a criterion of *considering potential explanations* rather than one of *understanding phenomena*. However, taken at face value, this formulation undersells one of de Regt's key insights: intelligibility, empirical adequacy, and internal consistency all involve *value judgments*. After describing these judgments, I discuss their importance to explanatory consideration.

Recall that de Regt regards consistency and empirical adequacy as "basic values" in explanatory inquiry, meaning that any acceptable explanation must exhibit *some* degree of both. However, scientists have to make value judgments about the degree, relative weight, and precise interpretation they attach to both these basic values and the explanatory desiderata that figure in intelligibility. We can get a feel for these kind of judgments when we turn to de Regt's (2020, 923) most precise characterization of empirical adequacy, which he borrows from Bhakthavatsalam and Cartwright (2017, 446):

> …a theory (or model or set of scientific claims) is empirically adequate when the claims it makes about empirical phenomena – or at least the bulk of these claims, or the central ones – are correct, or approximately correct enough.

Even if we grant an explanation's "central claim" about an empirical phenomenon is its explanandum, scientists still must make judgments about what counts as "a bulk of claims" and "approximately correct enough." To prevent crackpot explanations from deserving consideration, I will assume that these judgments must be made by scientists who are competent and well-informed (or "skilled" in the de Regtian dialect) about the phenomenon to be understood. Consistency and the various explanatory virtues that inform intelligibility (simplicity, unification, visualizability, etc.) admit of similar treatments.

De Regt's emphasis on expert judgments doesn't merely *describe* the inner workings of consideration that I so crudely black-boxed. It also highlights two of explanatory consideration's *normative* aspects. First, thinking of consistency and empirical plausibility as "basic values" to be balanced against intelligibility captures a kind of flexibility that seems appropriate at the phase of consideration. In this early phase of explanatory inquiry, scientists may tolerate some inconsistencies or incorrect predictions at an explanation's periphery if these inaccuracies are incidental to the phenomenon to be understood and the explanation appears especially promising. For instance, in considering Feynman's explanation, physicists discounted the absence of any empirical evidence for fractionally charged particles. Moreover, intelligibility—particularly the macro-level conception's "cluster" of theoretical qualities—is a good indicator of explanatory promise (Nyrup 2015).

Tolerance toward potential explanations with different bundles of consistency, empirical plausibility, and intelligibility is especially laudable at the phase of consideration. For instance, physicists' understanding of proton structure was enhanced by considering both Bjorken and Feynman's explanations. Moreover, scientists can be unjustified in accepting an explanation because they have failed to consider a deserving explanation. Failing to control for a confounding variable or overlooking auxiliary hypotheses about malfunctioning instruments are paradigmatic examples. In short, "underconsideration" makes SEEing less safe (Khalifa 2017, Ch. 7). The Scientific Knowledge Principle thereby indicates that understanding improves, in part, by more comprehensive consideration. Consequently, using de Regt's friendly account to unpack consideration isn't merely *consistent* with my frenemy account—it *promotes* my preferred brand of understanding.

Synthesizing our two views highlights a second interesting facet of scientific practice, which has received meager philosophical attention. I call this *explanatory staging*: the representing of a plausible potential explanation so as to make it amenable to comparison with other plausible potential explanations of the same phenomenon, typically through empirical testing.

The effortless qualitative reasoning that de Regt prizes (in CIT_1) promotes explanatory staging. For instance, while formally identical to Feynman's model, Bjorken's current algebraic model wasn't "staged" nearly as well, as evidenced by experimenters' greater facility with Feynman's model. The Stanford-MIT experimenters were highly proficient at designing scattering experiments involving electrons and *protons*. By piggybacking off Feynman's qualitative reasoning, they could represent Bjorken scaling as involving another kind of scattering experiment: one involving electrons and *partons*. This, in turn, allowed them to design further experiments by which to test the Bjorken-Feynman Frankenstein or "parton model" (core ideas of which would later be incorporated into the Standard Model). Bjorken's largely quantitative reasoning didn't generate these analogies with their previous experiments.

By leaning on de Regt's work, I hope to have set the stage for future adventures in explanatory staging. Our story of friendship, however, must turn to its next act—what scientists must do to advance their understanding *after* they have considered deserving candidates for explanations.

3. Comparison

To that end, observe that Bjorken and Feynman were not the only theorists to propose potential explanations of Bjorken scaling. Moreover, while these two theorists' explanations were quite similar, other explanations posited radically different hadronic structure. Sakurai's vector meson dominance (VMD) model was among these competitors to the parton model.[5] If Sakurai's explanation of Bjorken scaling was right, then the parton model was wrong.

Like the parton model, Sakurai's model deserved consideration as a potential explanation of Bjorken scaling in the late 1960s. VMD models provided the most empirically successful and widely accepted explanations of scattering behavior in the high-energy physics community for most of the 1960s, so they appear empirically adequate and consistent in de Regt's more liberal sense.[6] Moreover, according to VMD models, ρ, ω, and ϕ mesons are the hadronic components of the

[5] The other main competitor was Arbanel et al.'s Regge exchange model. Most of my discussion of VMD has analogues to this model as well.
[6] Prominent works include Gell-Mann and Zachariasen (1961), Ross and Stodolsky (1966), Sakurai (1960, 1962), and Stodolsky and Sakurai (1963).

photon. This lends itself to qualitative reasoning—by using Feynman diagrams, for example. Most relevant to Bjorken scaling, Sakurai held that proton-electron interactions were mediated by the aforementioned "vector mesons" instead of Feynman's partons. Since Sakurai was the foremost VMD theorist at the time, he certainly was competent and well-informed. Moreover, even defenders of the parton model thought enough of the VMD model to test it. Hence, per the criteria forged by our first foray into friendship, VMD deserved consideration as an explanation of Bjorken scaling.

But understanding isn't simply an exercise in considering deserving contenders as explanations. Scientists march toward understanding by ascertaining which of these contenders is the champ. Sakurai's explanation of Bjorken scaling entails that the ratio of the proton's tendencies to absorb virtual photons longitudinally to its tendencies to absorb those photons transversely, is quite large (in between 1 and 10). By contrast, parton models entail that this ratio should be quite small (in between 0 and 1). In August 1969, Richard Taylor led a run of "crucial" experiments at the Stanford Linear Accelerator designed to adjudicate between the parton model and Sakurai's VMD. The results showed the lower ratio predicted by the parton model to be correct. This was widely seen as a reason to reject Sakurai's VMD model. Hence, by the time that Taylor's experimental results were reported, it was clear that the empirical evidence favored the parton model over Sakurai's model. Consequently, Sakurai's model fails to provide understanding of Bjorken scaling precisely because of its incongruence with Taylor's findings.

As noted above, de Regt discusses the *evaluation* of explanatory models. However, he doesn't emphasize that models can be evaluated in different ways at different phases of explanatory inquiry. For instance, one can judge that an explanation, such as Sakurai's, is good enough to *deserve consideration*, but then make a subsequent comparative evaluation that another explanation, such as Feynman's, *better explains* the phenomenon of interest.

It is unclear how de Regt's approach can capture this distinction. For instance, de Regt's (2020, 931) discussion of Maxwell and Boltzmann's disagreement about the latter's "dumbbell" model as one in which "neither…was irrational or unscientific: they only valued the empirical adequacy of the dumbbell model differently." This suggests that empirical adequacy is in the eye of the beholder. However, applying an analogous approach to Sakurai's case produces some counterintuitive results. Initially, Sakurai took the "main virtue" of his proposal as that of providing a clear experimental test by which to adjudicate between the parton and VMD models (Sakurai 1969, 981). Yet, in September 1969, Sakurai seemed to discount Taylor's findings, claiming that "we need better data," so he appears to have banished these results from his bulk (Riordan 1987, 164). Arguably, Sakurai merely changed *ad hoc* the degree to which he valued empirical adequacy to avoid refutation. However, this understandably struck most physicists at the time as a dubious way to achieve scientific understanding (Friedman 1991, 720, Riordan 1987, 165-166).

A less forgiving frenemy might conclude that de Regt's CUP runneth under: it fails to provide sufficient conditions for understanding phenomena. In the spirit of fostering friendship, this will not be my gambit. Rather, just as de Regt's account opened the black box in my account of consideration, I suggest that my account of explanatory comparison does the same for these aspects of his view. De Regt's account is consistent with, but does not entail, that different kinds of explanatory evaluation should be subject to different standards. Parts of my account provides de Regt with a framework for making these finer-grained evaluations.

3.1. Friendly Comparisons

While I haven't explicitly formulated my account of explanatory comparison using the language of empirical adequacy, it's no stretch to do so. When I have spoken of comparisons based on the best available evidence and methods, my examples consistently involve *empirical* evidence and methods apropos of *empirical* testing of explanatory hypotheses. With that in mind, I propose the following:

> An explanation *Q* of *P* is *empirically fit* in context *C* if and only if for all other explanations *Q** that also deserve consideration as an explanation of *P* in *C,* the judgment that *Q* better explains why P than Q* is unsafe given the best evidence and best methods available in *C*.

Intuitively, this says that an empirically fit explanation is at least "tied for first" when it comes to saving the phenomena needed for methodologically sound comparisons. Hence, multiple explanations of the same phenomenon can be empirically fit. However, unlike de Regt's account of empirical adequacy, empirical fitness is demanding enough to do justice to the idea that Sakurai's model fails to provide understanding, for there is at least one deserving competitor, the parton model, that can safely be judged to be better than Sakurai's explanation given that Taylor's experimental results were available.

Empirical fitness seems largely congenial to de Regt's approach to understanding. For instance, the proposed alliance between frenemy and friend shows how empirical thresholds change at different stops on the road to understanding. De Regt's account of judgment-based empirical adequacy is still vital in explanatory consideration, so it shouldn't be abandoned. However, it must be supplemented with the notion of empirical fitness if it is to avoid unduly awarding understanding to scientists who are mistaken about the phenomena they ought to save—as Sakurai was. Moreover, because an explanation can be empirically fit while still failing to accurately depict the unobservable, de Regt isn't being saddled with the unpleasant task of saving the noumena. All of this resonates with de Regt's strong empiricist scruples.

The proposed synthesis of our views also refines de Regt's contextualism about understanding in three useful ways. First, we can think of explanatory consideration and explanatory comparison as two kinds of contexts in which scientists have different objectives. When scientists are considering explanations, their main goal is to avoid overlooking any deserving candidate, which favors relatively permissive standards. When they are comparing explanations, their main goal is to determine which of these explanations is the best, which favors more restrictive standards. Since these pull in seemingly opposite directions, these two contexts will involve different epistemic standards.

Second, empirical fitness more precisely identifies understanding's context-sensitive and context-invariant elements. Understanding-providing explanations must be empirically fit; that is a context-invariant requirement. However, how that requirement is realized depends on (a) which explanations are deserving of consideration and (b) which methods and evidence are available. I would call these two features part of a scientist's "context," though they are determined by scientific communities; not by individual scientists. For instance, what has been circulated within the relevant scientific community at a given time determines which methods and evidence are available. Disciplinary context determines what count as the *best* methods and evidence. In the case of Bjorken scaling, these context-infused standards involve different plausible potential explanations making competing predictions about what will happen in highly controlled experiments. However, in other contexts, such as the social sciences, explanatory comparisons frequently lack these features (Khalifa 2019). Since de Regt does not explicitly identify (a) and (b) as context-sensitive determinants of understanding—much less make them communal rather than individual—this seems like a fruitful elaboration of his view.

Third, stocking the contextualist cupboard in this way licenses more nuanced evaluations about a model's capacity to provide understanding. For instance, subsequent revisions to Sakurai's work have resulted in so-called "generalized" VMD models that have had empirical successes as effective descriptions of quantum chromodynamical models at low energies. Indeed, some recent VMD models even seem to fare much better than their predecessors in explaining deep inelastic scattering, which is precisely where Sakurai's explanation of Bjorken scaling faltered.[7] So, it makes little sense to throw out the entire VMD enterprise. This is why comparisons are tied to a specific explanandum and a particular context. These benefits of generalized VMD approaches can all be acknowledged while still claiming that the explanation that *Sakurai offered in 1969* didn't provide understanding of *Bjorken scaling* because it was empirically unfit.

Empirical fitness also invokes safety. Safety is my frenemy calling card, for it yokes understanding to knowledge. Despite this, I am hopeful that de Regt won't rebuff my attempt at friendship. Shorn of epistemological jargon, this is simply a requirement that the evidence and methods used to adjudicate between different candidate explanations are reliable tools for this task. Suppose that Taylor's experiments were unsafe. Then they would have been so poorly designed that, had the relevant ratio been greater than 1, they would have still indicated that it is less than 1. Consequently, they would have been too unreliable to assist in explanatory

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[7] Ironically, Sakurai and Schildknecht (1972) first proposed generalized VMD to explain Bjorken scaling in light of Taylor's results.

comparison. Surely, this would diminish their capacity to advance physicists' understanding of Bjorken scaling.

So construed, I see no reason for de Regt to deny the requirement that our empirical evidence and methods should be safe in this way. Moreover, intelligibility—the "friendliest" feature of his account—remains completely untouched by empirical fitness, including its inheritance of safety from the epistemologists down the hall.

4. Commitment and Truth

Suppose that only one explanation exhibits empirical fitness after comparison. What happens next? As noted above, the EKS Model holds that there is a third phase, *commitment*, where scientists adopt the appropriate cognitive attitude toward the different explanations they have considered and compared. The key question is whether scientists have, at this point, a correct explanation. If so, then they have cleared the Explanatory Floor. If not, then they are (at best) on the right track to understanding, but still have work to do.

Thus, much hinges on how we define explanatory correctness. Some will decree that empirical fitness is enough. De Regt's CUP suggests as much.[8] Others will note that empirically fit explanations can be horrifically inaccurate, and horrifically inaccurate explanations frequently generate *mis*understanding. De Regt has recently suggested that we differ on precisely this issue (De Regt 2020, 931, De Regt

[8] More precisely, empirically fit explanations deserve consideration (per Section 3.1). So, they will satisfy all of CUP's requirements (per Section 2.1).

and Höhl 2020). My dear friend is only half correct in this appraisal. Allow me to explain.

4.1. Setting the Record Straight

It's unclear to me whether de Regt has fully appreciated my flexibility on truth's role in understanding. *Mea culpa:* while I have called my view of truth's role in understanding "quasi-factivist," I've always been more "quasi" than "factivist." Yet only the latter is a battle cry in the understanding literature, so quite reasonably, I am frequently portrayed as opposed to self-described "non-factivists," such as de Regt. Let me atone for my bad marketing choices and show how we are closer than he seems to think. As noted above, I'm committed to:

> *The Explanatory Floor:* Understanding why *P* requires some *Q* such that *Q* correctly explains why *P*.

By Tarski's Convention T, we get:

> *Quasi-factivism*: Understanding why *P* requires some *Q* such that '*Q* correctly explains why *P*' is true.

Because of quasi-factivism's appeal to truth, I considered this view to be in the factivist family. More importantly, because frenemies hold that understanding is derivative of scientific knowledge of statements of the form '*Q* correctly explains why *P*,' it's possible for quasi-factivism to be frenemies' *only* truth-requirement on understanding. It is certainly *my* only truth-requirement.

But why am I not a full-throated factivist but merely a half-hearted *quasi*-factivist? Primarily because adding truth-*talk* to the Explanatory Floor doesn't mean that I have strengthened the Explanatory Floor's truth-*requirements*. Specifically, quasi-factivism does *not* entail the following:

> *Explanatory Realism:* For all *P* and *Q,* if '*Q* correctly explains why *P*' is true, then '*Q*' is approximately true.

As I see it, full-throated factivists must add this realist[9] codicil to quasi-factivism. Consequently, they must deny that any explanans that falls short of approximate truth provides understanding.

Nor, however, am I a full-throated *non*-factivist. Like their dreaded realist rivals, these firebrands also add something to the quasi-factivist core:

> *Explanatory Antirealism*: There are some *P* and *Q* such that '*Q* correctly explains why *P*' is true and '*Q*' is not approximately true.

[9] Hereafter, I will use "realism" and its cognates as shorthand for "explanatory realism."

Unlike factivists, non-factivists can accept that explanantia that fall short of approximate truth provide understanding.

I add neither realist nor antirealist addenda to the quasi-factivist core.[10] This is possible precisely because the Explanatory Floor plays no favorites with respect to realism and antirealism. This is clearly expressed in the EKS Model's official "theory" of explanation:

> For all *P* and *Q,* if '*Q* correctly explains why *P*' is true, then *Q* satisfies your ontological requirements (so long as they are reasonable).[11]

In the hopes of dispelling confusion, let's give my longstanding position a new label: explanatory *voluntarism*.[12] On this line, some ontological positions will be more in keeping with explanatory realism; others, with antirealism. A frenemy who is also a voluntarist only demands that you should adjust the Explanatory Floor to be consistent with whatever ontological requirements you've settled on. If you impose more restrictive ontological requirements on explanation, as realists are wont to do, then adopt a similarly restrictive notion of understanding. If you have more relaxed ontological requirements on explanation, then adopt a similarly liberal notion of understanding. In short, your choices about explanation's truth-requirements

[10] This, of course, echoes Fine (1986).
[11] See, e.g., Khalifa (2017, 7).
[12] Chakravartty (2017) and van Fraassen's (2002) kindred positions suggest fruitful explications of ontological requirements' "reasonableness," e.g., your requirements must be probabilistically coherent and must not undermine your epistemic goals.

should dictate your attributions of understanding. Indeed, as Section 4.3 illustrates, explanatory correctness is often overdetermined by both realist and antirealist ontological requirements, so realists and antirealists will frequently agree about *when* understanding has been achieved even if they disagree as to *why*. In such cases, I suggest that theorists of understanding should tolerate different stances toward the realism issue.

I propose that we theorists of understanding cheerfully abandon the word "factive," even when it is prefixed by a "non-" or a "quasi-." For those of us who stand firmly on the Explanatory Floor, positions such as realism, antirealism, and voluntarism more precisely characterize our differences than the dreaded F-word. Crucially, De Regt (2017, 23) stands firmly on the Explanatory Floor. Consider his pithy equation:

> understanding a phenomenon = having an adequate explanation of the phenomenon.

Moreover, I think he has suffered from similar ambiguities as I have. On one and the same page, De Regt (2017, 131) expresses a commitment to antirealism, when he writes that "we can have genuine understanding of phenomena on the basis of theories and models that defy realistic interpretation" and echoes the kind of quietism characteristic of voluntarism when he also writes, "understanding and realistic representation are independent."

As a final gesture toward friendship, I suggest that we both become voluntarists. To that end, I'll show how I can accommodate explanatory antirealism and de Regt can accommodate realism without upsetting the synthesis effected in the Sections 2 and 3. As such, intelligibility and safe SEEing—our most distinctively friendly and frenemy elements, respectively—remain intact.

4.2. Frenemy Antirealism

There has been little doubt that frenemies of understanding can be explanatory realists. However, it is simply mistaken to assume that frenemies cannot be antirealists. The basis for this judgment appears to be that frenemies take understanding to be derivative of knowledge, and since knowledge requires truth, frenemies must be realists. As the preceding lays bare, and as argued elsewhere (Khalifa 2011, 2017, Ch. 6), this argument is flawed. In a nutshell: antirealism entails that in some cases, '*Q explains why P*' is true(!) even though '*Q*' is false. To deny this modest truth-requirement is to fall off the Explanatory Floor. So, it is consistent with antirealism that in these cases, a scientist still knows that *Q explains why P* (though she does not know that *Q*). Since this is also compatible with the Nexus and Scientific Knowledge Principles, antirealists can also be frenemies of understanding.

4.3. Friendly Realism

As noted above, de Regt leans toward antirealism, but has suggested that, like me, he would like to put issues concerning realism to the side when discussing understanding. My goal here is to recast de Regt's antirealist stance as one of many permissible sets of ontological requirements. Realist ontological requirements that leave his account of intelligibility unsullied are also permissible.

To do that, I want to assuage De Regt's chief reservation about realism: that its truth-requirements on explanation will incorrectly disqualify perfectly good instances of scientific understanding. To that end, I begin by noting that de Regt's target is sometimes unclear. Is it the mild-mannered explanatory realism that only requires explanantia to be approximately true? Or is it "bogeyman realism," the position that every detail of an understanding-providing explanation must perfectly mirror reality's every nook and cranny to a divine level of resolution? Consider his admission that "while… there is a role for truth, this is not unqualified, objective truth but truth relative to a particular context and a particular purpose" (De Regt 2017, 136). I see plenty here to upset bogeyman realists, but no genuine friction with explanatory realism. Worrall's (1989) classic essay ushered in waves of realist positions that are "selective" in the posits that warrant ontological commitment. Hence, these positions aren't "unqualified" about truth's role in explanation. Similarly, some realists have countenanced significant context-sensitivity through a contrastive theory of why-questions (Lipton 2004). Generally, realists can comfortably claim that context and purposes play a central role in determining which truths are *relevant* or *significant* (Kitcher 2001). If this is all that de Regt means by the ways in which truth is "relative to a context," then all he's done is raise the hackles of realists who aren't bogeymen. Moreover, he wouldn't even disagree with the modest realists who ought to have been his genuine foils!

However, if de Regt really wants to put up a fight—not with me, but with the realists he's parodied—he may double down in two ways. First, on a more ambitious antirealist reading of his remarks, context and purposes don't simply make certain truths *relevant*, but instead make certain statements *true*. However, this incipient pragmatist theory of truth comes at a high cost: his claim that theories that are "strictly speaking false" are still "useful… in certain contexts" (De Regt 2017, 131) becomes inconsistent. Moreover, nothing in de Regt's work suggests such an exotic theory of truth, so I think this interpretation sits uneasily with his position.

Alternatively, de Regt may keep the debate going by making this modest realist thesis his foil. After all, if we read his CUP literally, an explanation that was *massively* false about *every* unobservable it posited could still provide understanding, so long as it was intelligible, consistent, and empirically adequate. Hence, it would seem that anyone who required correct explanations to be *approximately* true about *some* of their posited unobservables—i.e., even the realists who are *not* bogeymen—would be too restrictive for his tastes.

Yet, so far as I can tell, de Regt's putative examples of antirealist understanding all leave interpretive wiggle room for this modest sort of realism. For example, all of the explanations offered for Bjorken scaling in the late 1960s assumed that electrons interact with hadrons.[13] But realists can say even more than this and still operate within the confines of only saying that some parts of an explanation are approximately true. Consider a structural realist's take on the parton model's success. Bjorken's model yielded a novel prediction of his eponymous scaling curves. Structural realists regard novel predictions as evidence that Bjorken's mathematics latched onto a physical structure. Finally, they will note that Feynman's model piggybacked off of Bjorken's model. Consequently, parts of Feynman's model are isomorphic to the physical structures that Bjorken's model captured.

Crucially, realists can say all of this while still tolerating significant falsehoods in *some* parts of the parton model. Once again, this is essentially the "selective realist" approach which has been *en vogue* for over three decades. For instance, Feynman's qualitative reasoning about partons more or less depicts them classically, as particles colliding in billiard-ball fashion. Yet we know that they are denizens of the quantum realm. However, the aforementioned suggestion—that the parton model is partially isomorphic to a physical (quantum) structure—is entirely consistent with its classical bits being false.

Once again, my point here isn't that realism is correct and de Regt's antirealism is incorrect. It is simply that realists shouldn't lose sleep over de Regt's

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[13] Similarly, scientific realists have long sought to show that de Regt's go-to example for explanatory antirealism, Newtonian mechanics, approximates relativistic and quantum mechanics under suitably circumscribed boundary conditions. So-called correspondence results are their weapon of choice. I don't claim that these arguments are refutations of de Regt; only that he hasn't given us any reason to think that his antirealist interpretation of Newton is better than these realist interpretations. Consistent with my voluntarism, I take this as evidence that realists can enlist Newtonian mechanics as a repository of understanding just as ably as antirealists.

arguments. As a voluntarist, I also think that the converse is true. De Regt should be unfazed by realist interpretations of his favorite episodes in the history of science. For instance, he's well within his rights to claim that the parton model provides understanding of Bjorken scaling simply because it's empirically fit. Realists can heap similar praise upon the parton model, while adding that, e.g., it is isomorphic to a physical structure. So, both parties will claim that the parton model provided understanding of Bjorken scaling. This accords well with voluntarism: in these sorts of cases, the choice between realism and antirealism makes no difference to understanding, so take your pick! More generally, I find relitigating the entire realism debate simply to reach similar verdicts about understanding is not time well spent.

5. Conclusion

With this, our story of friendship comes to a curious close. If asked how explanatory inquiry must proceed in order to furnish understanding, it appears that de Regt and I can—and probably should—point toward the same things. Both of us ought to acknowledge that the skillful construction of explanatory models predicated upon expert judgments about which bundles of consistency, empirical adequacy, and intelligibility warrant consideration should be followed up by careful empirical comparisons to determine which of these models rises to the top. The remaining issue is how much the fruits of these labors must latch onto unobserved reality. On this point, I have suggested that de Regt and my friendship could be cemented by a more thoroughgoing commitment to a voluntarism with which we have both only flirted. So, the ball is in de Regt's court. He can either decline some of these invitations or cement the friendship by embracing voluntarism.

In closing, I note that the friendship discussed here is more accurately (but less colorfully) described as a synthesis of philosophical views. There is—I hope!—no *personal* animus between Henk de Regt and me. Ever since our first correspondences over a decade ago, Henk has been generous and kind. I am fortunate to call him my friend.

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