Back to Kant! QBism, Phenomenology, and Reality from Invariants*

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Abstract As it stands, QBism faces two problems, an epistemic and a semantic one: That it is unclear how, on QBism, an agent can coherently abduce the existence of others and an external, mind-independent world, and that it is unclear how talk of that world even becomes meaningful within the QBist framework. I will here go into elements of phenomenology that could potentially help in solving these problems, but also into what I see as their limitations within 'phenomenology proper'. I will then go back to Kant, in whose writings some of these phenomenological ideas are rooted, and make a big leap forward, in suggesting a broadly 'neo-Kantian' constructivism that I believe evades both problems.

1 Introduction: Routes to QBism and the road ahead

QBism is all about personal matters, so let me begin with a bit from my own personal story. When I first became seriously interested in Quantum Theory (QT), I was close to finishing a master's degree in philosophy, so I already had a fair bit of philosophy under my belt. Given this critical training, I had a hard time taking everything that physics textbooks were suggesting quite seriously. For instance, why would everyone make such a fuzz about the double slit experiment? Couldn't the physical setup simply alter the behaviour of tiny bits of matter in such ways that they would distribute as observed? Why should we assume them to follow straight-line trajectories anyways?

Looking for answers, I first stumbled upon Landé's early attempts to provide QT with new foundations, and later became attracted to the de Broglie-Bohm theory. As a long-time fan of the popular science fiction show *Star Trek: The Next Generation*, I also became fascinated with the Everett interpretation, which was the basis for the episode "Parallels". However, I should soon discover that each of these had serious problems,

*This is a preprint of a paper forthcoming in Berghofer, P. & Wiltsche, H. A., *Phenomenology and QBism: New Approaches to Quantum Mechanics*, Routledge.

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like being in conflict with relativity, or being unable to recover the Born rule in any sensible way—not to mention the difficulties associated with objective collapse views.

My story actually *begins* a little differently though. As an undergraduate, I had started out as a kind of sceptical epistemologist, interested in the limits of what can be known. However, I found no compelling reason to not take metaphysics at least seriously and so became interested in trope theory, the metaphysical theory that ultimately everything breaks down into particular properties (this red, that hardness...). This process culminated in my supervisor, a trained chemist, telling me that my ideas about objects being nothing but particular properties 'meeting up' wouldn't work: the Pauli principle, he told me, implies certain properties for a whole system of electrons that are not reducible to the electrons' individual properties. This was my first serious encounter with QT and there was something quite remarkable here: *entanglement*.

But a function of the form $\alpha\psi_a \otimes \psi_b + \beta\psi_b \otimes \psi_a$ didn't look very 'ontic' to me. Rather, it seemed to say something like: "Either electron 1 is in state *a* and electron 2 is in state *b*, or vice versa".[1] I was hence relieved to find that Spekkens (2007) had created a toy model which seemed to allow one to view the quantum state as (broadly) 'epistemic', i.e., something characterizing the experimenter's knowledge, information, convictions, etc. I was then very much disappointed to see that this model could not reproduce violations of Bell-type inequalities. Apparently, no (serious) epistemic model could!

So Schrödinger (1935, 555; orig. emph) seemed to have it right: Entanglement, unlike superposition, non-commutativity, or uncertainty, was not just *some* feature of QT; it was "*the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought." But if one's interpretation was to be (broadly) epistemic, meaning that the quantum state was *not* a representation of the goings on in a radically mind-independent reality, and there was apparently also no *other* way to refer in an empirically adequate way to what goes on between two misaligned Stern-Gerlach magnets—then how should one think about reality, the quantum state, and the relation between the two *at all*?

It was through the popular-level writings of N. David Mermin, who reminded me that "there is [...] a split[...] between the world in which an agent lives and her experience of that world" (Mermin, 2012, 8), that I realised it was time to reverse my metaphysical turn. I also realised that *QBism*, next to positions like those of Healey (2017) and Friederich (2015), was "by far the most interesting game in town." (Mermin, 2012, 9) So this is my personal 'route to QBism', i.e., the sequence of steps that led to me becoming interested in it.

Famously, QBism has itself followed an interesting route to its present development (cf. Stacey, 2019). When Chris Fuchs and Rüdiger Schack first proved Dutch Book theorems for quantum states (Caves et al., 2002a) together with Carlton Caves (Fuchs's PhD supervisor), as well as a de Finetti-style representation theorem (Caves et al., 2002b), the whole project was still executed under the name 'Quantum Bayesianism'. Fuchs and Schack then, however, took the whole thing into a philosophically more

[1] Notably, the tensor-product/conjunction correspondence *is* presupposed in recent arguments such Frauchiger and Renner (2018); I make this explicit in Boge (2019).

radical direction, in turn changing the name to 'Quantum Brunoism' (after Bruno de Finetti's subjectivist interpretation if probability), or 'Quantum Bettabilitarianism' (as the world, at least, allows us to bet on it), or simply 'QBism' (not an acronym for anything). This philosophical radicality certainly wasn't lessened when N. D. Mermin joined the QBist ranks and Fuchs et al. (2014) discarded the "intuition that correlations in the experiences of agents in widely separated regions ought to find their explanation in correlations in conditions prevailing in those regions." They claimed:

> The variable λ [encountered in derivations of Bell-type inequalities – FJB] is nothing more than a version of the discredited EPR elements of reality. For a QBist the nonexistence of such objective facts-on-the-ground as λ no more implies nonlocality than does the nonexistence of elements of reality in the original EPR argument.

Today, QBists' main focus is on what they call the *Urgleichung*. This is a particular representation of the Born rule, which reads

$$Q(j) = (d+1)\sum_{i=1}^{d^2} P(i)R(j|i) - 1, \tag{1}$$

where we recognize the usual law of total probability if we replace the $d+1$ by 1 and remove the -1. Most notably, there is no mention of state vectors or operators here at all, and so the notorious measurement problem – that we do not know any Lorentz-invariant interpretation of the transition $\alpha_1 |a_1\rangle + \alpha_2 |a_2\rangle + \ldots \mapsto |a_j\rangle$, especially when the state is entangled, or any coherent way to circumvent the assumption of such a step – *vanishes*: If quantum 'states' are basically probability assignments, then a transition of the aforementioned form is no more mysterious than a probability update.

This version of the Born rule follows if one is in possession of a 'SIC': a symmetric informationally complete positive operator valued measure; something known to exist for a large number of Hilbert space dimensions d, but presently not for arbitrary dimensionality (DeBrota et al., 2020). More precisely, a 'MIC' (minimal informationally complete positive operator valued measure) is a positive operator valued measure (POVM) $\{E_i\}_{1\leq i\leq d^2}$ where the E_i are linearly independent, and form a basis of the space $\mathcal{L}(\mathcal{H}_d)$ of bounded operators on Hilbert space $\mathcal{H}_d$ of dimension d. Furthermore, any density operator (positive trace-one operator) ρ on $\mathcal{H}_d$ can be expressed as a linear combination of the E_i, which allows to characterize ρ completely in terms of the probabilities it generates via the Hilbert-Schmidt inner product. A MIC wose elements are defined by $E_i = \Pi_i/d$, with the Π_i projections satisfying $\mathrm{tr}\left[\Pi_i\Pi_j\right] = (d\delta_{ij}+1)/(d+1)$ is called a SIC.

But assuming SICs exist in all dimensions, what is the meaning of the Urgleichung? First of all, note that already the expressibility of ρ in terms of probabilities generated by inner products with MICs has some interesting implications:

> the mapping $\rho \mapsto (p(1), \ldots, p(d^2))$, although injective, cannot be surjective; only some probability distributions in the simplex are valid for represent-

> ing quantum states [...]. If quantum states are nothing more than probability distributions, a significant part of understanding quantum mechanics is understanding what restrictions there are on the set of valid distributions. (Fuchs and Schack, 2013, 1698; notation adapted)

In other words: QT generally *constrains* the credences we can entertain. Furthermore, according to Fuchs and Schack (2013), the difference between the law of total probability and the Urgleichung is contained in the fact that the measurement with outcomes i remains *counterfactual*, i.e., that the bet in which i is an outcome has been called off, whereas this is not the case in Dutch book arguments for the law of total probability.

So QBism tells us that we only need to figure out the right ways to look at these probabilities, and then everything will fall into place. Problems solved, at least tentatively, right? Alas, if only this were true!

2 Challenges for QBism

2.1 Prelude: What's the explanandum?

Let's assume that the Urgleichung is indeed best construed as representing a situation where the bet for the events conditioned on has been called off. It may then be true that it would be "irrational in some situations" to bet in accordance with the law of total probability (Fuchs and Schack, 2013, 1697). But this does not even touch on the question as to why it would be *rational* to bet *in accordance with* the Urgleichung.

It seems that the discrepancy between the law of total probability and the Urgleichung points us to something 'out there'; something which 'makes it so' that we have to constrain our credences in a different way when faced with the sort of counterfactual dependency explored by Fuchs and Schack (2013). And it must be this 'something out there' which provides the reason why we should bet differently. QBists agree:

> Now, if you accept that the Born rule is an extra normative rule, you might ask me, "Why that rule; why not some other way of relating the probabilities?" When you ask me that question, I answer, "Because that's the way the world is." There is something about the world that has led us all to adopt this as the best adapted method for living in our world. (C. Fuchs, as cited in Crease and Sares, 2021, 14)

However, there is something important that, to my mind, QBism *neglects*: That this divergence from the law of total probability is ultimately forced upon us by calibrating credences on observed *frequencies*.

Consider, e.g., the form of the quantum de Finetti theorem (Caves et al., 2002b):

$$\rho^{(N)} = \int \mathrm{d}\rho \, \varrho(\rho) \rho^{\otimes N}, \tag{2}$$

where $\rho^{\otimes N}$ is an N-fold tensor product of the same state, $\rho^{(N)}$ is 'exchangable' in the sense that it is permutation symmetric and extenable as $\rho^{(N)} = \mathrm{Tr}_M \rho^{(N+M)}$, and $\mathrm{d}\rho \, \varrho(\rho)$

defines a probability measure over density operators. The gist of this theorem is that, when different agents with non-extreme priors update this state Bayesian-style on a growing number of measurements on the N measured systems, they will converge on a common state, regardless otherwise of the shape of their priors. Hence, the quantum-tomographical notion of an 'unknown state', which is then found out, can be *replaced* by that of a state *agreed upon* after several measurements. This is an important result, mirroring de Finetti's semantic replacement of 'unknown' probabilities. However (capital 'H'), *what* quantum state these agents will agree upon *will be determined by the distribution of outcomes they observe*.

Furthermore, while some singular observations may strike us as profoundly significant, we are more likely to discard them as illusions, misconceptions, or coincidences than unusual frequencies of certain types of events. It is those which we consider the 'scientific phenomena' to be accounted for (Massimi, 2007; van Fraassen, 1991). Experimental results which inevitably appealed to observed frequencies indeed stand at the very *inception* of QT: It was the fact that spectral lines always occurred, as predicted, 'Breit-Wigner distributed' within a very narrow interval around fixed relative distances to one another that convinced physicists of the formalism's utility back in the day, and it is the agreement to eight significant digits between the experimental *average* for the anomalous dipole moment and the Q(F)T prediction that convinces us of it today. Hence, relative frequencies are something quite important: They guide us both in shaping our credences and in collecting scientific evidence.

Here is what I take to be the most important set of relative frequencies not well accounted for by QBism. Recall from the introduction that I followed Schrödinger in considering quantum entanglement and the correlations it implies to be *the* characteristic feature of QT. Superpositions, uncertainty, etc. could all be interpreted as expressions of ignorance (Bartlett et al., 2012; Spekkens, 2007), to be surpassed by a future theory that makes better descriptions available. That this would be the case also for entanglement was certainly the hope of Einstein et al. (1935). But things did not turn out in this way—entanglement reflects the 'true quantumness' (Jennings and Leifer, 2016). Yet the remarkable correlations it implies tend to be downplayed in QBism:

> Correlations are just a special case of more general probability assignments. To explain a correlation is therefore no different than to explain a probability assignment. [...] [This] remain[s] unchanged in the case that the correlations $p(x, y|a, b)$ implied by the prior state and measurement operators violate a Bell inequality. (Fuchs and Schack, 2014, 5)

Let's consider the QBist rendering of the usual Alice-Bob story in a little more detail. Alice and Bob sit at space-like distance to one another, rotating, at agreed upon times, their Stern-Gerlach magnets at will to one of two arbitrary positions, resulting in three possible angles between them. In this way, they elicit experiences they each call 'spin up' or 'spin down', meaning visual impressions of dots on the upper or lower half of a screen (relative to the orientation of the magnet), respectively. They write down a table which codifies their experiences and then get together and compare. To their surprise, they find a remarkable number of coincidences between up and down in their respective tables, especially whenever there was no misalignment between both magnets. After some error-correction, they even find this correlation to be *perfect*.

Smart as they are, Alice and Bob realize that it would be very difficult to supplant a causal model for this correlation: The settings were chosen at such points in time that whatever locally caused the dots on the screen (say, invisible 'particles' transmitted from 'the source'), together with their own interventions, could not have interacted causally, at least not at (sub)luminal speeds.

Now, admittedly on QBism,

> quantum mechanics explains why the agent should expect the measured frequencies to lie in a certain range, but does not provide an explanation for the particular numbers the agent obtains in a given realization of the data table. (Fuchs and Schack, 2014, 5)

But we were never *interested* in explaining what frequencies agents should *expect* in the first place, were we? The puzzles associated with QT are the dots successively building up fringes on the screen in the double-slit experiment, the changing count rates in particle detectors in delayed choice-experiments, or the surprising relative frequencies with which Alice and Bob find coinciding values. If one hasn't explained *these*, one has arguably not explained *anything*. This concern I share with John Earman:[2]

> The [QBist] story explains why both [Bob] and Alice *expect*, with degree of belief one, to find anticorrelated spins, but [...] does not explain why the measured spins *are in fact* anticorrelated. (Earman, 2019, 418; orig. emph.)

Replace 'spins' by 'values on the two lists' and 'are in fact' by 'are experienced to be', and this objection should get even the most immutable QBist nervous: If Alice, a firm QBist, seeks an explanation for the correlated entries on the two lists, but wants to avoid invoking the "discredited EPR elements of reality", she should actually consider her interaction with *Bob* to be the cause of the observed correlations between table entries. Worse yet, Alice might justifiably take the whole situation to be a hoax concocted, and never resolved, by Bob and the team of scientists setting up the experiment—something akin to a conspiracy theory. Holding Bob causally responsible like this might seem ludicrously incompatible with other conceptions Alice entertains about him, based on her past experience. So how is Alice to regard this situation, if she does not simply want to shut up and bet on it?[3]

[2]In all fairness, I should note that Earman's paper otherwise misrepresents QBism in several ways (cf. Fuchs and Stacey, 2020), culminating in the fact that he claims QBists call themselves 'QBians'.

[3]Like Mermin (1989, 10), who almost certainly coined the original slogan (cf. Mermin, 2004), I hence urge to "rather celebrate the strangeness of quantum theory than deny it". However, I don't find it "*foolish* [...] to demand an explanation for the correlations beyond that offered by the quantum theory" (ibid., 11; my emph.), for I'm hesitant to say that QT *delivers* an explanation (cf. Boge, 2022).

2.2 With apologies to Chris Fuchs: QBism and solipsism

Let's step back a moment and recall that, according to QBism, QT is "a single user theory" (Fuchs and Schack, 2014, p. 3). Like Norsen (2016), I believe that this makes for a connection to *solipsism*, though that connection is more subtle than Norsen claims.

Unlike Norsen, I am not claiming that QBism *is* a kind of 'FAPP' solipsism, i.e., that it is practically indistinguishable from solipsism. It is true that QBism discards the representational character of our currently most successful theory while also dismissing other "objective facts-on-the-ground", as could be represented by additional variables λ. And it is also true that, by the standards of most philosophers, a position like this would count as *anti-realist*.[4] However, Fuchs in essence urges that this should *not* be mistaken as a kind of *metaphysical* anti-realism, and so not as (substantive) solipsism:

> We do [...] hold evidence for an independent world [...] external to ourselves [...] because we find ourselves getting *unpredictable kicks* (from the world) all the time. (Fuchs, 2002, 11; emph. added)

This reason for not becoming an idealist or solipsist of sorts is very similar to d'Espagnat's:

> We sometimes build up quite beautifully rational theories that experiments falsify. Something says no. This something cannot be 'us.' There must be something else than just 'us.' (d'Espagnat, 1995, 314)

In my own words: the *recalcitrance* of experience provides a wonderful reason for postulating (or abducing) the existence of other agents and material entities.

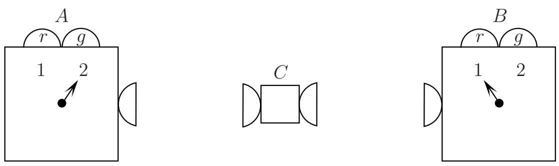

Figure 1: Mermin contraption

A	*B*	*A*	*B*	⋯
$1, r$	$1, r$	$1, g$	$2, r$	⋯
$2, g$	$1, g$	$2, g$	$2, g$	⋯
⋮	⋮	⋮	⋮	⋮

Table 1: Hypothetical protocol

Clearly, one must be careful not to mistake this reason for *postulating* the *existence* of 'others' and a 'mind-independent reality' with definite *knowledge* of their respective *constitutions*. And this is an element certainly also present in Fuchs's *participatory* realism (inherited from Wheeler), according to which "reality is *more* than any third-person perspective can capture" (Fuchs, 2017, 113; original emphasis). Nevertheless, it should thus be clear that the mere rejection of additional variables λ and the representational status of QT does not imply solipsism, so long as 'solipsism' is understood either as the *metaphysical* position that reality is a figment of one single mind, or the *epistemological* position that reality beyond that one single mind is unknowable.

What I do find to be correct, however, is that QBism is properly described by the term "*methodological solipsism*", for it "amounts [...] to an application of the form and

[4]See Lewis (2019); see also Glick (2021) for an assessment of the relation between QBism and realism.

method of solipsism" even if "not to an acknowledgment of its central thesis" (Carnap, 2003, 102; orig. emph.). What I do find to be correct as well is that the combination of QBism's focus on (guided) subjective credences, its appeal to recalcitrance as the ground for abducing an external reality, and the *neglect* of the 'stronger than classical'-correlations as a relevant datum makes it hard to see how QBism has any *advantage* over 'actual' solipsism. For our strongest reasons for *not* being solipsists lie exactly in our success with stipulating further variables (like λ) in other circumstances—say, *your* consciousness as creating *my* impressions of talking to some*one*, or the lawn 'out there' as creating my present impression of green. Hence, rejecting the hidden variables λ too easily out of hand yields a *slippery slope* towards solipsism.

To see this more clearly, consider a *single* scientist, using what Jarrett (2009) coined the 'Mermin contraption' (after Mermin, 1981), and writing down a protocol as in Tab. 1. Here, A and B refer to the 'measuring devices' placed on two diametrically opposite sides of C (the 'source'). They each have two settings, $1, 2$, and a red (r) and green (g) light on top of them. At certain times, lights flash on each of the two devices simultaneously, and the nob on each of A and B may or may not switch automatically just shortly before the lights flash, with no discernible correlation between both nobs.[5]

After watching a very long sequence of some thousands of flashes and writing down settings and lights flashed, our scientist notices that the frequency of joint occurrences of r and g on A and B, respectively, is somewhere near $(2+\sqrt{2})/8$ among the runs in which A was set to 1 and B was set to either 1 or 2, and equally when A was set to 2 and B to 1, but somewhere near $(2-\sqrt{2})/8$ when both are set to 2. However, for each setting, the occurrences of either r or g on either A or B individually settle down around $1/2$. Consequently, she notices the following empirical correlation between the frequencies, f, of flashing lights for given joint settings on both devices:

$$\begin{aligned} f(A=g\wedge B=r|a\wedge b) &\approx f(A=r\wedge B=g|a\wedge b) < f(A=g|a)f(B=r|b) \\ &\approx f(A=r|a)f(B=g|b) \qquad \text{for } a=b=2, \qquad (3) \\ f(A=g\wedge B=r|a\wedge b) &\approx f(A=r\wedge B=g|a\wedge b) > f(A=g|a)f(B=r|b) \\ &\approx f(A=r|a)f(B=g|b) \qquad \text{else}, \qquad (4) \end{aligned}$$

where a denotes A's setting and b denotes B's.

The scientist of course realizes immediately that if these visual experiences would be caused by some state of an external reality, λ, – maybe featuring also the states of two invisible particles being emitted from C – this would *predict* an observable correlation between the flashes. So without crunching many numbers, she may assume that a probabilistic model of the following form could help explain the correlation:

$$P(A\wedge B|a\wedge b\wedge\lambda) = P(A|a\wedge\lambda)P(B|b\wedge\lambda) \qquad (5)$$

Our scientist cannot be a convinced QBist: QBism not only denies the representa-

[5]We can imagine this to be brought about by further devices hooked up to A and B respectively.

tional status of QT, but also the suitability of further representational variables λ. But, as I will show, rejecting the possibility of such a causal model tout court also makes for a subtle connection to solipsism.

Compare the above situation to that contemplated by Reichenbach (1938), which he utilized to provide a *refutation* of solipsism. Reichenbach offered a philosophical thought experiment, reminiscent of Plato's cave, that delivered "a beautiful analogy for the problem of the external world." (Sober, 2011, p. 20) In this thought experiment, mankind is confined to a cube on whose outside walls shadows appear. The shadows displayed on two adjacent walls of the cube exhibit astonishing correlations, and the story's hero, 'Copernicus', comes up with the following explanation: There are objects outside the cube, and two matching shadows are just images caused as common effects by some mechanism involving one single object. That's why they are so remarkably correlated! And indeed, Copernicus is right: Outside the cube, there are birds flying around and a single bird's shadow is simultaneously projected onto two adjacent walls by a cleverly contrived system of lights and mirrors.

The appeal of the story as an argument against solipsism is that the cube represents an individual's confinement to her own immediate experience. But in contrast to any conceivable solipsistic hypothesis about this experience, an outside world-hypothesis could *predict* the correlations met with in experience, and so has the higher evidential support:[6] The correlations exhibited between, say, our auditory and visual experiences can be predicted on the assumption that they are both simultaneously caused by the states of an external reality, but typically not from mental states alone. In Sober's words: "It's the external world that is doing the work, stupid" (Sober, 2011, p. 18).

More concretely (cf. Sober, 2011, §7, §8), closing my eyes and making the visual impression, W, of waves crashing on the beach disappear does not make my auditory impression, S, of the crashing-sound go away. Similarly, shutting my ears eliminates W but not S. Hence, it doesn't seem that W and S are related as cause and effect.

This empirical correlation between W and S is extraordinarily robust, and so doesn't seem 'spurious'. And it also doesn't seem 'analytical', i.e., directly given by the *meanings* of W and S. Hence, this is a situation in which we might expect Reichenbach's *principle of the common cause* (PCC; Reichenbach, 1965) to apply;[7] that if X and Y are (robustly) correlated but logico-analytically independent, then either X causes Y, Y causes X, or X and Y are joint effects of a common cause λ that renders X and Y conditionally probabilistically independent ('screens them off').

Now the most obvious candidate common cause from within one's own mental world is certainly the *intention* to go to the beach. But this intention doesn't screen off W and S: I will experience their joint occurrence after having this intention *more often* than their individual occurrences in that very case. Hence,

[6] This is actually Sober's version of Reichenbach's argument, which avoids problems associated with Reichenbach's original account, connected to frequentism and positivism.

[7] Sober (2001) actually holds the PCC to be restricted in its scope, offering an example of a seemingly robust correlation between Venetian sea-levels and British bread prices. I am not convinced that this (and similar) correlation(s) cannot be explained in terms of further causal structure with a screener, or that interesting 'classical' correlations that cannot be explained causally are ever robust (Boge, 2021b).

> the solipsist must concede that [...] *something* is going on in the production of my wavy visual and auditory sensations besides my intending to go to the beach. [...] Suppose, for example, that right after I form the intention to go to the beach that I am rendered unconscious; the next thing I know, I am either experiencing wavy visual and auditory experiences, or I am experiencing neither. When I introspect, I find no further experiences that I can cite to explain this uncanny correlation of $[W]$ and $[S]$. (Sober, 2011, 18)

The inability to manipulate, say, S by intervening on W could still be the result of 'ham fisted' operations within one's mind. I.e., there might not be any proper ('surgical') *intervention* available, and so also no way to *exhibit* the causal dependency properly: Shutting my eyes might simultaneously eliminate and, *via* a different causal path, *create* S anew. But that is an obviously conspiratorial story, and so is not an attractive option for the solipsist: It features a causal connection that does not show up in the statistics, and so corresponds to what causal modlers call an 'unfaithful' causal model.

But now comes the saucy part. As pointed out above, our scientist abduces the existence of λ by appeal to the PCC, or even just from the fact that postulating λ predicts the correlation and so has higher evidential support than a solipsistic hypothesis. However, as is well known, she soon runs into trouble. If she also assumes that λ is causally unrelated to the setting (i.e., $P(\lambda|a \wedge b) = P(\lambda)$) and uses a numerical variable that maps g to $+1$ and r to -1 to compute expectation values, she easily derives the infamous CHSH inequality (Clauser et al., 1969). But plugging the values approximated by her noted frequencies into this inequality, she finds that this yields $2\sqrt{2} \leq 2$.

The scientist might now ponder whether the two flashes are not related as cause and effect. But similarly to the waves/sound case, she will quickly find that option to be undermined, by the observation that there are no conceivable interventions she could possibly undertake to alter the statistics in A by manipulating B, or *vice versa*; at bottom because the situation at A and B is perfectly symmetric (Friederich, 2015, p. 132). She could also come up with weirder causal stories, such as the latent variable λ being influenced by, or influencing, the settings (an option known as 'superdeterminism'), or maybe even that there are causal influences going back in time.

But being a skilled causal modler, she will soon figure out that all these options are possible only on pains of *stipulating causal relations that have no manifestation in the frequency data* (Wood and Spekkens, 2015). Hence, while in the W/S case, the realist *can* point to some state of an external reality as facilitating the common cause and the solipsist is forced to embrace unfaithful causal models, for the single-scientist Mermin contraption *neither* can offer a causal story without invoking conspirational, unfaithful models. So the solipsist might counter: "Maybe it's *not* the external world that's doing the work after all, stupid!"

Now, someone who, like a QBist, uses QT as a mere calculus for adjusting expectations about future experiences should thus raise *neither* question: that as to why some correlations are screenable and some aren't, nor that as to why, more generally, *some experiences seem like 'unpredictable kicks' whereas others seem 'fairly regular'*. For, if *anything* counts as 'recalcitrant' in the relevant sense, it is the correlations exhibited by the

Mermin contraption: They were totally unexpected, whence the infamous EPR paper "came down [...] as a bolt from the blue" on Bohr and Rosenfeld (1967). And they are radically out of our control—remember: no possibility to intervene locally on the remote outcome which is strongly correlated but, like the local one, apparently random. Hence, if we stick to recalcitrant experiences as evidence of a mind-independent world, these correlations should serve as such evidence if anything does.

In sum, if QBism *selectively* uses recalcitrant experiences as evidence of an external world it has no advantage over actual solipsism, since it offers no coherent basis for abducing reality from experience. I will call this the *epistemic problem* (EP) of QBism.

2.3 Wallace's challenge for non-representationalism

There is a second, maybe more profound problem lingering in the constructivist elements of QBism; much as the late Wittgenstein's *meaning* skepticism was judged to be more profound than knowledge skepticism by Kripke (1982, 60). For how, exactly, is "everything any of us knows about the world [...] constructed out of his or her individual private experience" (Fuchs et al., 2014, 753), if this involves, say, talk of "'particles' [...] that come to Alice and Bob from a common source" (ibid., 752)?

The problem I am hinting at is, of course, a version of a problem well-known to philosophers: The problem, associated with all constructivist or reductionist empiricisms, of first identifying certain 'purely observational' concepts and then showing how all other concepts that do not directly refer to experience can be *defined* in terms of these. *Prima facie*, that is a necessary step in showing how the world is 'constructed out of experience'; but as is well known, it is a formidable task. I will call this the *semantic problem* (SP) of QBism.

I am obviously not the first one to raise this concern. The following objection (or, let's say, observation) has recently been voiced by Wallace (2020, 91–2; orig. emph.):

> the central idea in the logical-positivist and logical-empiricist pictures of science [was to] make a principled distinction between the 'observation language' in which our observations are described, and the 'theory language' in which the non-observational parts of our scientific theories are stated. [...] Non-representationalist strategies at least seem to be committed to making the same division, whether the analogue of the 'observation language' is Copenhagen's use of classical mechanics, or pragmatism's 'non-quantum' description, or QBism's appeal to direct experience. The problem with these approaches [...] is not that making such a distinction is *unreasonable* or *illegitimate*, but that—at least at present—we do not know how to do it.

Wallace's criticism is not specific to QBism; a lengthy passage is devoted to Healey's pragmatism. However, Wallace *is* more specific as to where, precisely, he sees the troubles arise for *all* these approaches. Consider the following fictional dialogue concerning the meaning of 'quark-gluon plasma', presented by Wallace (2020, 20) as a means for showing the semantic difficulties faced by non-representationalist approaches:[8]

Q_1: What's the quark-gluon plasma?

A_1: It's the state of a quantum-chromodynamics (QCD) system above a certain temperature, at which a phase transition occurs to a state where the fermionic elementary excitations are associated to the quark field rather than to colourneutral products of that field.

Q_2: Slow down. What's 'temperature' in QCD?

A_2: A quantum system, including a field-theoretic system, is at (canonical) thermal equilibrium when its quantum state is

$$\rho(\beta) \propto \exp\left(-\beta \hat{H}\right), \tag{6}$$

where $\hat{H}$ is the Hamiltonian and β is a real number. For a system at thermal equilibrium—or that is reasonably close to thermal equilibrium—its temperature T is given by $\beta = 1/k_B T$.

Q_3: And what's an 'elementary excitation'?

A_3: Generally in quantum field theory, we can analyse systems in states reasonably close to the thermal equilibrium state as gases of weakly interacting particles. Those weakly interacting particles are the elementary excitations.

Q_4: 'Particles' as in classical point particles?

A_4: Not really. 'Particles' as in subsystems whose Hilbert space bears an irreducible representation of the Poincaré group, at least in the interaction-free limit.

Q_5: So the quark-gluon plasma is associated with one sort of particle, colder systems with another. Shouldn't I be able to say what the 'particles' are once-and-for-all?

A_5: Not in quantum field theory: the optimal choice of particle depends on the state of the system. Hot systems are described most naturally in terms of quarks, colder systems, in terms of protons and neutrons.

Q_6: Can't I just think of a proton or neutron as an agglomeration of three quarks?

A_6: Only heuristically. The more precise way to explain the relation between the protons and quarks is at the field level: the proton is associated with a certain triple product of the quark field.

Q_7: How is a particle supposed to be associated with a field?

A_7: If a quantum system is in thermal-equilibrium state $\rho(\beta)$, the 'two-point function' of that system with respect to field $\hat{\phi}(x)$ and that state is

$$G_2(x-y;\phi,\beta) = \mathrm{Tr}(\rho(\beta)\hat{\phi}(x)\hat{\phi}(y)) \tag{7}$$

If the Fourier transform of that state has a pole, there's a particle associated with it.

Q_8: That's a weird postulate.

[8] I have numbered questions and answers for reasons to become clear below.

A_8: It's not a postulate; it's something you derive, by looking at the dynamics of states obtained by excitations of the thermal-equilibrium state. Where there's a pole, there's a subspace of states which can be interpreted as superpositions of singly localized excitations and which is preserved under the dynamics.

The point Wallace is trying to make here is that he has

> *not the faintest idea* how to make sense of any of this without taking the quantum state of the QCD system, and its dynamical evolution under the Schrödinger equation, as representational. (Wallace, 2020, 90; orig. emph.)

After all, "[e]ven the claim that the system has temperature T is a claim about its state." The point, then, is that a *high-level* concept, such as 'quantum state' is arguably fundamental for the semantic content of the entire dialog, and any reductionist or even constructivist approach seems to be doomed to failure. Let's call this *Wallace's challenge*.

What could a non-representationalist about the state respond? I believe a dialogue between the representationalist, providing the answers, and a more skeptical inquirer would go a little differently:

A_2: A quantum system, including a field-theoretic system, is at (canonical) thermal equilibrium when its quantum state is

$$\rho(\beta) \propto \exp\bigl(-\beta \hat{H}\bigr), \tag{8}$$

where $\hat{H}$ is the Hamiltonian and β is a real number. For a system at thermal equilibrium—or that is reasonably close to thermal equilibrium—its temperature T is given by $\beta = 1/k_B T$.

Q_{2a}: Remind me what a 'quantum state' is?

A_{2a}: It' s a positive trace-one operator on a suitable Hilbert space.

Q_{2b}: Yes, I recall. But that's an abstract mathematical definition. What does it represent, physically?

A_{2b}: It represents the state of the system.

Q_{2c}: O...k, but then why would it *have* these mathematical properties? Or maybe more importantly: Why should it take on the form you just showed me when the system is in an equilibrium? Frankly, what's a 'thermal equilibrium' anyways?

A_{2c}: It's complicated. Quantum theoretically, thermal equilibrium pertains to a system which has just this sort of state. This is how you define it. A little more illuminatingly, you could maybe say that the defining property is that the energy remains constant within the system.

Q_{2d}: And whence the form?

A_{2d}: Well originally, the thermal equilibrium of a system was defined by the property that its temperature is homogeneous across the system and constant in time. Maxwell and Boltzmann then came up with negative exponential distributions for systems in thermal equilibrium, and...

Q_{2e}: Wait, besides the fact that you just used 'temperature' in your explanation, it seems that you are telling me now that the form of the state ultimately gets its justification from a correspondence with a probability distribution. Is that correct?

A_{2e}: No. These were merely historical remarks. I mean, it's true that von Neumann did call it an 'analog' for Boltzmann's distribution, when he introduced his famous entropy formula. But you can derive this form of the density operator by requiring unit normalization and that the expectation value of the Hamiltonian be constant.

Q_{2f}: But didn't I just hear 'normalization to unity' and 'expectation value' in your formal explication? And aren't these still hallmarks of a probability distribution? ... I'm starting to get terribly confused here. Let's set this issue back for the moment. [*continues with* Q_3]

The point of this exercise is not to discredit Wallace's challenge altogether. In a way, I believe it to be quite serious. However, the first part of this extended dialogue is useful for rebutting what I would like to call the *first level* challenge: That the quantum *state* has to be taken as representational.

As the extended considerations tickled out by the skeptical inquirer show, it is far from clear that we have to take the state as representational just because it figures importantly in the explication of other concepts. By the same token, we would otherwise have to take a classical probability density as (directly) representational. But while the choice and definition of a probability density may elucidate the concepts one entertains about the system in question and *why* one apportions one's credences in a certain way (say: maximizing entropy), this doesn't mean that the density itself *represents* the state of the system. Same thing with density *operators*: that something is *important* for understanding something *else* does not mean that the first thing is representational.

However, what I would like to call the *second level* challenge arguably persists: That in order for the whole dialogue to make sense, *something* about the formalism has to be taken as at least *tentatively* representational, and not in any obviously reducible or deconstructable way. In other words, it would mean throwing out the baby with the bathwater to conclude, from the observation that the quantum state has much in common with a probability distribution or density, that *no* concept connected to the quantum formalism, and not in any obvious way to direct sense experience, is at least *intended* as a representation *sui generis* of the goings on in a mind-independent reality.

I believe that a defence of *state*-non-representationalism – one that takes to heart many of the messages of QBism while avoiding EP and SP – is possible on the grounds of this distinction. I will offer my own one in Sect. 4. For now, however, let me first establish a connection to *phenomemology* – which is, after all, the defining philosophy of this volume.

3 Help from phenomenology?

I suppose I should have qualified my personal story even a little further: When I became interested in QT, I had a fair amount of *analytic* philosophy under my belt. In contrast, my knowledge of continental brands, such as phenomenology, was – and still is – rather limited. Hence, what follows is almost certainly a caricature of actual phenomenology. Following, however, Gallagher and Zahavi (2008, 28; orig. emph.), we may take the heart of phenomenology to be a certain methodology that can be specified in terms of four basic steps:

(1) The *epoché* or suspension of the natural attitude.

(2) The *phenomenological reduction*, which attends to the correlation between the object of experience and the experience itself.

(3) The *eidetic variation*, which keys in on the essential or invariant aspects of this correlation.

(4) *Intersubjective corroboration*, which is concerned with replication and the degree to which the discovered structures are universal or at least sharable.

The 'natural attitude' here means that "Reality is assumed to be out there, waiting to be discovered and investigated. And [that] the aim of science is to acquire a strict and objectively valid knowledge about this given realm." (ibid., 22) But practicing the epoché does not mean engaging in radical skepticism or metaphysical anti-realism:

> the epoché entails a change of attitude towards reality, and not an exclusion of reality. The only thing that is excluded as a result of the epoché is [...] the naïvety of simply taking the world for granted, thereby ignoring the contribution of consciousness. (ibid., 23)

The aim of the phenomenological reduction, on the other hand,

> is to analyse the correlational interdependence between specific structures of subjectivity and specific modes of appearance or givenness. [O]nce we adopt the phenomenological attitude, we are no longer primarily interested in what things are [...] but rather in how they appear, and thus as correlates of our experience. (ibid., 25)

Hence, these first two steps are, in a sense, preparatory: They set the stage of investigating 'things' as 'things-for-us', rather than 'things-out-there'. What is arguably the most important step, then, is the eidetic variation. It is the key to tickling out

> what Plato called the *eidos* or *essence* of things. [...] If the object that I am examining happens to be a book, what features of it can I imaginatively vary without destroying the fact that it is a book. I can change the colour and

> design of the cover; I can imaginatively subtract from the number of pages, or add to them; I can change the size and weight of the book; I can vary the binding. [...] [T]he core set of properties that resist change [...] constitute the essence, the 'what makes a book a book'. (ibid., 27; orig. emph.)

Finally,

> another tool at the phenomenologist's disposal [...] is simply the fact that phenomenologists do not have to do their phenomenological analyses alone. Descriptions allow for intersubjective corroboration. And again, the quest for invariant, essential structures of experience is not narrowly tied to the peculiarities of my own experience. (ibid., 28)

In sum, we find out the essence of something by first retreating from our inclination of thinking in terms of things being simply 'found out' by science, by then focusing on what things are *to us*, by abstracting away as much as possible, and by then comparing what's left (the purported 'eidos') with what others may have found.

Admittedly, I see some fundamental problems associated with this method. Before I turn to a critique, however, let me first point out in what ways it could be of help to QBism and also express my sympathies for parts of it.

Recall that I had claimed the two main problems of QBism to be a want of a coherent basis for abducing reality from experience (EP) and the want of a coherent basis for constructing complex concepts, as appealed to by QBists in explicating their own position, from their acclaimed foundation in experience (SP). Now if phenomenology contains the central realization that experience comes *pre-structured*, i.e., that some properties of the objects we encounter are *essentially* attached to them and cannot even be 'stripped away in thought', then this might allow one to circumvent the pertinent problems. For instance, Nagel (2000, 346), and Ladyman (2000, 2010) following her, argue that:

> To make the kind of epistemic use of experience that empiricism demands, we need at least the capacity to sort out its deliverances from other products of the mind [...] and this sorting task is [...] a rational enterprise [...] that demands substantive a priori knowledge for its execution.

Hence, insofar as the eidetic variation is such a source of substantive a priori knowledge, it might help circumvent these problems, and so help QBism solve the SP. Here is Merleau-Ponty (1945, xxx):

> if I am able to speak about "dreams" and "reality," to wonder about the distinction between the imaginary and the real, and to throw the "real" into doubt, this is because I have in fact drawn this distinction prior to the analysis, because I have an experience of the real as well as one of the imaginary.

Furthermore, phenomenology could give rise to what might be seen as a *dis*solution, rather than a solution, of the EP:

> To believe in [...] a pure third-person perspective is to succumb to an *objectivist illusion*. [...] It is a view that *we* can adopt of the world. It is a perspective founded upon a first-person perspective, or to be more precise, it emerges out of the encounter between at least two first-person perspectives; that is, it involves intersubjectivity. (Gallagher and Zahavi, 2008, 40; orig. emph.)

Hence, phenomenologist methodology seems to offer a way to bypass any abductive, inferential step: 'The world' is, in a sense, directly given, in the way it appears to 'us'. And by reflecting on experience in the ways suggested by phenomenology, and corroborating results by intersubjective exchange, we can get a clear view of *that* world we so experience. Claims to any 'world beyond', however, are thus effectively rendered moot.

Now I am deeply sympathetic to taking a step back from 'normal' or 'natural' modes of thinking – including, say, the Husserlian rejection of a hypostatization of mathematical concepts as directly indicative of 'the real' (see Gurwitsch, 1974, 44 ff.) –, to reflecting deeply on one's own consciousness, and even to using variational methods in sorting out what may count as 'essential' or maybe 'objective'. However (capital 'H'), I see various problems associated with the ways phenomenologists suggest to proceed from these initial steps, and what they believe these can establish.

For instance, take the idea of intersubjective corroboration. The first kind of problem I see here is that *you*, dear reader, are an object to me. Don't take that personally: It's just meant as an epistemological claim. If you are in physical pain or suffering a terrible loss, I may feel compassion for you however strongly. But that does not mean that I actually feel what you feel. These are just the feelings I experience in relation to my auditive, visual, olfactory, and maybe even haptic experiences I have of you otherwise. These feelings may incline me to think of you as a very special object; one that has an 'inner world', very much like my own. *But that doesn't change the fact that, for me, you are among the objects constructed out of experience.*

The question thus arises why I should prefer my experiences of *that* sort of object over my experiences of *other* objects. At the level of fundamental epistemology I see no compelling reason—especially when taking into account how I can feel compassion *even for a car*. In general, it seems to me that the status of *objects* in phenomenology is all but clear. Here is how Berghofer and Wiltsche (2020, 14; orig. emph.) phrase the relevant point:

> The main question [...] concerns the relationship between consciousness and the external world: Does transcendental phenomenology only imply that the *meaning* or *sense* of the intended objects is constituted by consciousness? Or does transcendental phenomenology advance the more radical claim that the objects themselves are constituted by consciousness and that, consequently, there is no reality beyond the phenomena?

Frankly, I have no clue as to what the most natural answer to be gathered from the writings of Husserl or Merleau-Ponty would be. But I find it telling that different Husserl scholars – and presumably different phenomenologists in general – have come up with remarkably different answers:

> First, there are those who understand Husserl's [...] as a purely methodological endeavor that is consistent with both metaphysical realism and metaphysical idealism [...]. Second, there are those who argue that [it] inevitably culminates in a form of metaphysical idealism [...]. Third and finally, some commentators argue that transcendental phenomenology [...] can be considered [...] a rejection of metaphysical realism [...] without thereby collapsing into some sort of metaphysical idealism[.] (ibid.)

Let us zoom in very briefly on the third alternative, as defended e.g. by Zahavi (2017). Zahavi (2017, 186) argues that Husserl's phenomenology

> is an explication of the sense that the world has for all of us "prior to any philosophizing" [...]. Indeed, there is nothing wrong with the natural attitude and with our natural realism; what Husserl takes exception to is the philosophical absolutizing of the world that we find in metaphysical realism

However, contrast this with the following two observations:

> Husserl is adamant in rejecting the notion of an inaccessible and ungraspable *Ding an sich* as unintelligible and nonsensical [...]. To posit a hidden world that systematically eludes experiential access and justification, and to designate that world as the really real reality, would for Husserl involve an abuse of the term 'reality'[.] (ibid., 69)

> The phenomenological credo 'To the things themselves' calls for us to let our experience guide our theories. We should pay attention to the way in which reality is experientially manifest. (ibid., 151)

The problem I see associated with this rejection of the (Kantian) 'Ding an sich' and the claim to the 'things themselves' as being experientially manifest is this: If there is no thing independent of my experience, whose mere *thought* is the Kantian notion of a 'Ding an sich' (Allison, 2004, 3.I), then either phenomenology *does* collapse into an idealism of the Berkeley-variety, or it verges on something incomprehensible: Claiming that the (philosopher's beloved) chair-as-experienced is the chair itself while simultaneously denying that it also has an 'an sich'-ness, i.e., an existence completely independent of my (modes of) experiencing it, must mean that the chair *pops in and out of existence whenever I turn towards / away from it*—worse than Einstein's bed![9]

This leaves the other two options (pure methodologism or Berkeleyanism), the former of which might seem promising for *science*. However, I honestly doubt that 'letting

[9]Recall that Einstein explained his discomfort with the Dirac-von Neumann interpretation of QT to Putnam (2005, 624) as follows: "Look, I don't believe that when I am not in my bedroom my bed spreads out all over the room, and whenever I open the door and come in it jumps into the corner."

experience guide our theories' does justice to actual scientific practice, when the latter's success is measured by (use-)novel predictions and the production of new phenomena and technologies. In particular, I doubt that the phenomenological method could have brought us to QT: "Nobody has ever understood what the hell Heisenberg was [...] smoking [...] when he invented matrix mechanics." (Susskind, 2008, 15:19–15:31)[10]

So apparently, there is something other than attention to intersubjectively communicable experiences going on in theory construction, especially in modern physics.[11] But it is equally unclear to me whether phenomenology can help ground talk of scientific *entities* such as "'particles' [...] that come to Alice and Bob from a common source".[12]

Take a quark. Exercising the epoché, I should maybe not take the QCD Lagrangian, the collective evidence from colliders, or even the fact that I can somehow associate particles with isolated mass shells as providing a picture of the quark being 'discovered out there'. I am more than happy with that. The next step, the phenomenological reduction, might mean attending to the displays of apparent 'tracks' in computer-generated images of detectors, and how they can be traced back to a certain interaction point and matched up with certain types of interaction. But I'm beginning to feel that this is hardly what phenomenologists have in mind. Furthermore, when it comes to intersubjective corroboration, I am completely lost: Why would it help to compare the result of the foregoing process with the results of similar processes as undergone by others, in an effort to sort out what a quark, essentially, is?[13]

Quite certainly, a lot more could be said here and there will be more charitable ways of reading phenomenology. Ryckman (2007, Ch. 5–6), for instance,[14] carefully argues for a definite impact of phenomenology on Weyl's thinking and demonstrates the influence of phenomenological ideas such as 'Wesensschau' on Weyl's development of his geometry. Nevertheless, even in this case, several things remain unclear to me.

First, Weyl's approach is displayed by Ryckman (2007, 144) as an exercise in "regional ontology", which presupposes the (local) acceptance of the natural attitude. It remains unclear to me in how far this approach really trades on basic phenomenological ideas and cannot be largely detached from them. Second, it is not clear to me in how far Weyl's approach even succeeds, given his somewhat *ad hoc* response to the Einstein-Pauli 'prehistory' objection (discussed at some length in Ryckman, 2007, § 4.2.4 ff.), of an effective, dynamical washing out of history-induced effects on atomic spectra. Einstein's elevating the apparent approximate invariance of atomic spectra to something motivating a general physical principle (see Giovanelli, 2014, 27) might be seen as a superior move, and, I believe, is somewhat consistent with the epistemological position I

[10]Cf. Heisenberg's autobiography or Rovelli (2021) for the details.

[11]My skepticism here is nurtured also by certain passages from the relevant phenomenological literature, such as Gurwitsch's (1974, 59) remark that his own thorough analysis of Husserl's approach to physics results in "no more than sketchy hints for a phenomenological theory of the natural sciences", or Wiltsche's (2021, 468) concession that "Husserl's most noteworthy engagement with physics is a rather general analysis of the early modern mechanics of Galileo Galilei". An oft-cited counterexample is Weyl's development of a purported unified field theory, to which I will turn briefly below.

[12]See French, this volume, for similar doubts about phenomenology's scope.

[13]I have here bracketed the eidetic variation for reasons to become clear below.

[14]See also Wiltsche (2021) or, more generally, the other contributions to this volume.

advertise below. Third, it is not clear just how close Weyl's attachment to phenomenology really is, as pointed out, e.g., by Bernard and Lobo (2019) or Sieroka (2019), and underscored by Weyl's self-admitted interest in thinkers such as Fichte, Plato, Hume and others.[15] Finally, even if geometry was a field in which the phenomenological method could be put to good use, it remains unclear to me whether the same is true of quantum physics—wherein "Evidenz" and "Anschaulichkeit" (Ryckman, 2007, § 5.4.1; § 6.3.1) clearly become touchy subjects (however, see French, 2020, in this connection).

Hence, I believe the concerns raised in this section do point to some serious challenges for phenomenology, wherefore I now turn to a philosophical stance that, to me, seems more clearly capable of circumventing these or similar concerns.

4 Back to Kant! (...and then a big leap forward)

When the 19th century 'neo-Kantians', such as Otto Liebmann, took issue with German idealism's reception of Kant, they coined a notion that was later paraphrased as *Back to Kant!* (cf. Ollig, 2017, 9 ff.) It is interesting to realize, in this context, that "despite all kinds of [...] differences" the basic approach and methodology of phenomenology is also "firmly situated within a certain Kantian or post-Kantian framework". For it takes to heart

> the realization that our cognitive apprehension of reality is more than a mere mirroring of a pre-existing world. Rather, a philosophical analysis of reality, a reflection on what conditions something must satisfy in order to count as 'real', should not ignore the contribution of consciousness. (Gallagher and Zahavi, 2008, 23–4)

As I have argued above, there are several respects in which phenomenlogy employs this Kantian heritage in a way that I find objectionable. I will hence follow the neo-Kantians' call and take inspirations more directly from Kant (though maybe also not too stringently).

Now Kant (CPR, A158/B197) was famously concerned with "conditions of the possibility of experience in general" that would "at the same time" be "conditions of the possibility of the objects of experience themselves, and thus possess objective validity in a synthetical judgment *a priori*." In the course of sorting these out, he declared space and time "pure forms of our sensibility", and objects to be "representations [...] which [...] are connected and determinable [...] in space and time [...] according to laws of the unity of experience" (A494/B522). Hence, in a Kantian view, objects are (involuntarily) *constructed*, or constituted, out of experience by the mind according to a fixed scheme.

However, thus declaring space and time pure forms of sensibility also misled Kant into endowing the principles of *Euclidean* geometry (the only geometry he knew of)[16] with the status of a synthetic a priori (A47/B64), and this move became untenable

[15] I owe thanks to Erhard Scholz for pointing me to this literature.

[16] This is but almost right; cf. Cuffaro (2012).

with the rise of non-Euclidean geometries in the 19th century (Friedman, 1999, 6). However, Reichenbach (1920, 46) pointed out that "the notion of an a priori has two distinct meanings in Kant. Firstly, it means something like 'apodictically valid', 'valid for all times', and secondly it means 'constitutive for the concept of an object'." (my translation—FJB)

Several authors (d'Espagnat, 2011; Friedman, 1999, 2001; Mittelstaedt, 2009; Reichenbach, 1920) have hence suggested to dispose of the first meaning while keeping the second intact. One might then sort out what Reichenbach called the 'axioms of coordination' of a given theory Θ, which contrast with 'axioms of connection'. The former ones "must be laid down antecedently to ensure [...] empirical well-definedness in the first place" (Friedman, 1999, 61), and so provide "structurally and functionally [...] that without which the rest of a theory would lack content" (Howard, 2010, 337)—or in yet other words, that which is merely *constitutively* a priori. The latter ones are "empirical laws in the usual sense involving terms and concepts that are already sufficiently well defined." (Friedman, 1999, 61)

In the version endorsed by Friedman (1999, 66), what is constitutively a priori may be sorted out by determining invariants of a given theory under a relevant group of transformations. Such invariants are also sometimes called *symmetries* (though sometimes this name is also given rather to the transformation-group), where "the symmetry of a 'something' (a figure, an equation,...) is defined in terms of its invariance with respect to a specified transformation group, its symmetry group." (Castellani, 2003, 322)[17]

The attentive reader will have already noted a vague similarity between the general description of symmetries and the eidetic variation. In the eidetic variation, the task is to sort out the essence of a given something, and this is done by removing as many contingencies as possible. More abstractly, all possible conditions under which a given something can be viewed are considered, and that which is unchanging under this variation of perspective or circumstance is then considered the eidos. But on the same level of abstraction, nothing else really happens in theoretical considerations of symmetries in modern physics.

To see how this more clearly, let's once more resume the dialog that establishes Wallace's challenge and extend it even a little further:

Q_7: How is a particle supposed to be associated with a field?

A_7: If a quantum system is in thermal-equilibrium state $\rho(\beta)$, the 'two-point function' of that system with respect to field $\hat{\phi}(x)$ and that state is

$$G_2(x - y; \phi, \beta) = \mathrm{Tr}(\rho(\beta)\hat{\phi}(x)\hat{\phi}(y)) \tag{9}$$

If the Fourier transform of that state has a pole, there's a particle associated with it.

Q_8: That's a weird postulate.

[17]Nothing depends on using only *group theory* for inspecting symmetries though(e.g. Dardashti, 2019; Guay and Hepburn, 2009). It rather requires some well-defined, structured set of transformations.

A_8: It's not a postulate; it's something you derive, by looking at the dynamics of states obtained by excitations of the thermal-equilibrium state. Where there's a pole, there's a subspace of states which can be interpreted as superpositions of singly localized excitations and which is preserved under the dynamics.

Q_{8a}: Wait, but didn't you say the particle was a subsystems whose Hilbert space bears, in the interaction free limit, an irreducible representation of the Poincaré group?

A_{8a}: Yes, this subspace bears the irreducible representation.

Q_{8b}: But isn't *that* a postulate: that the irreducible representation you retrieve somehow identifies the particle?

A_{8b}: Well, no. Wigner actually *showed* that there are two invariants under the Poincaré group, which, for a massive particle, are mass and spin.

Q_{8c}: But that doesn't 'show' that a particle *has* the properties corresponding to these invariants, does it? For otherwise we would appear to be moving in circles. So I take it that this is the postulate then: That a particle can be identified through that whichever remains invariant under the transformations specified by the Poincaré group?

A_{8c}: *mumbles* In a way... I guess, but... *mumbles*

To unpack this dialog a little, recall some details.[18] As is well known, Wigner (1931) first showed that symmetry transformations in QT are represented by unitary or antiunitary operators. Focusing on the proper orthochronous (inhomogeneous) Lorentz group ($|\Lambda| = 1$ and $\Lambda^0{}_0 \geq 1$), which is a connected Lie group and can be combined with parity and time inversion to reproduce any element of the whole Lorentz group, one can use a continuity argument (the connection to the unit element) to argue that any operator representing an element of that group must be unitary rather than antiunitary.

Focusing, further, on infinitesimal transformations $\Lambda^\mu{}_\nu = \delta^\mu{}_\nu + \omega^\mu{}_\nu$ with additional infinitesimal translation ϵ^μ, it is then possible to show that the group of unitaries representing these has generators $\boldsymbol{J}^{\rho\sigma}$ and $\boldsymbol{P}^\rho$ whose commutation relations exhibit the relevant Lie algebra structure. Furthermore, defining all states of definite momentum p^μ in terms of states of a fixed reference momentum k^ν (e.g. that defining the system's rest-frame, or, for massless systems, one where its three-momentum lies along the z-axis), it is possible to induce a representation of the whole group from what is known as the 'little group', the subgroup of transformations $\boldsymbol{W}^\mu{}_\nu$ that leave k^ν unchanged.

After sorting out the physically interpretable cases ($p^2 \leq 0$ and $p^0 > 0$), it becomes possible to classify massive representations according to their (continuous) mass value and their spin-quantum number j and massless representations by the remaining three-momentum component and the helicity, *because these are invariants of the little group*.

This doesn't tell us anything about QFT and poles yet. To make the connection, first note that the trace functional $\mathrm{Tr}(\rho(\beta)\hat{\phi}(x)\hat{\phi}(y))$ computes the expectation value of $\hat{\phi}(x)\hat{\phi}(y)$ w.r.t. $\rho(\beta)$. In what is called the 'thermofield dynamics'-formalism (e.g. Khanna et al., 2009), one can seek out a 'thermal vacuum state' $|0(\beta)\rangle$ such that this

[18] E.g. Peskin and Schroeder (1995); Weinberg (1995). I assume a metric with signature $(-+++)$.

can be recast in the more homely notation $\langle 0(\beta)|\hat{\phi}(x)\hat{\phi}(y)|0(\beta)\rangle$, where we can vividly see the 'excitation' of the equilibrium by means of the operators. This induces the need to introduce additional 'thermal operators' and changes the Lie algebra generating the unitary representation of the ('thermal') Poincaré group, but we can ignore this here.

Assuming, for simplicity, also that $\hat{\phi}(x)$ is a scalar operator and using $|\Omega\rangle$ as the vacuum state of a generic interacting theory, we recall that the time-ordered version of $\langle\Omega|\hat{\phi}(x)\hat{\phi}(y)|\Omega\rangle$ can be rewritten as

$$\langle\Omega|T[\hat{\phi}(x)\hat{\phi}(y)]|\Omega\rangle = \sum_{\lambda}\int \frac{\mathrm{d}^4 p}{(2\pi)^4}\frac{i}{p^2+m_{\lambda}^2-i\varepsilon}e^{-ip(x-y)}|\,\langle\Omega|\hat{\phi}(0)|\lambda_0\rangle\,|^2, \tag{10}$$

where $|\lambda_0\rangle$ is an energy eigenstate in the rest-frame ($\boldsymbol{p}=0$).

The Fourier-transform of the first term, which, up to normalization and choice of vacuum, corresponds to the interaction-free limit, is thus proportional to $1/(p^2+m^2-i\varepsilon)$. For $\varepsilon \to 0$, it has a pole at $p^2 = -m^2$, which coincides with the particle being 'on shell' and satisfying the relativistic energy-momentum relation. Hence, we exactly retrieve something which has a space of states associated with it that can be expanded in terms of momentum and labeled by mass and spin.[19] Furthermore, we can immediately also see in what sense that space is 'preserved under the dynamics' by realizing that the infinitesimal generators of the unitary representation of the Poincaré group commute with the energy operator $\boldsymbol{P}^0$, which generates the dynamics. Hence, it makes no difference as to whether we 'move around' in the space first and then let the dynamics run or *vice versa*; the reachable space remains the same.

Two things are crucial here: (a) The group itself has two invariants, which are spin and mass (or three-momentum and helicity), and (b) the dynamics has an invariant, which is the entire (sub)space bearing the group's representation. But this really gives us quite a lot: a particle of a given *type* can, up to considerations of charge, be identified by the unchanging values of mass and spin, and the particular particle itself can be identified as that whichever is described by the ('non-Boolean', contextual set of) assertions allowed by (projections onto) the dynamically unchanging subspace. Hence, invariants can be used, exactly, for sorting out what a particle *is*.

I'm neither the first to deliver such an analysis nor the first to associate it with Kant (e.g. Auyang, 1995; Falkenburg, 2007; Mittelstaedt, 1978). What I would hence like to do here is (i) draw the red line from Kant to Friedman, with a few added details (cf. Boge, 2021a); (ii) draw the line between Kant and phenomenology more clearly, with an eye on the constitutive use of *theoretical* symmetries; and (iii) draw conclusions as to how a Kantian line of reasoning might help QBism defend itself against EP and SP.

4.1 From Kant to Friedman (and beyond)

It is interesting to first expound on how the use of invariants in constituting objects is, indeed, essentially Kantian. This fact is mentioned almost in passing by several Kant

[19]'Superpositions of singly localized excitations' essentially means that the representation is not co-diagonal with a spacetime one.

scholars (e.g. Schrader, 1951, 520; Allison, 2015, 340; Rosenberg, 2005, 250), but given the weight I will put on it here, it may be helpful to look more deeply into the way in which Kant himself deploys invariance-based arguments

Let's begin with the transcendental aesthetic. Recall that Kant called "all cognition *transcendental* that is occupied not so much with objects but rather with our *a priori* concepts of objects in general." (CPR, A11–2/B25; orig. emph.) The task of the transcendental aesthetic, construed as "a science of all principles of *a priori* sensibility" (A21/B35), was to sort out the "mere form of sensibility in the mind" (ibid.), where by 'form' Kant means "that which allows the manifold of appearance to be intuited as ordered in certain relations" (A20/B34). In the end, Kant famously comes down with two such principles, namely space and time; but what really interests us here is the *way* in which he arrives at them:

> In the transcendental aesthetic we will [...] first isolate sensibility by separating off everything that the understanding thinks through its concepts, so that nothing but empirical intuition remains. Second, we will then detach from the latter everything that belongs to sensation, so that nothing remains except pure intuition and the mere form of appearances, which is the only thing that sensibility can make available *a priori*.

Hence, in order to isolate what constitutes the pure (i.e., content-free) form of sensibility, it is necessary to detach it from any particular context of understanding as well as from any particular sensation. However, thus detaching it from all particular sensations, one realizes that spatiality itself remains throughout all possible sensations:

> in order for certain sensations to be related to something outside me (i.e., to something in another place in space from that in which I find myself), thus in order for me to represent them as outside one another, thus not merely as different but as in different places, the representation of space must already be their ground. [...] One can never represent that there is no space, although one can very well think that there are no objects to be encountered in it. (A23–4/B38–9)

In other words: regardless of its content and regardless of how this content is ordered, we cannot 'imagine space away', even if we can at least *think* an endpoint to this variation in content wherein it is entirely empty (cf. Mohanty, 1991, 262). Hence, space (or spatiality) is an invariant of this variational process, and so must be part of what constitutes an object of sensation.

A similar reasoning chain is encountered in the transcendental analytic, when Kant offers his 'deduction' of transcendental consciousness:

> it is this one consciousness that unifies the manifold that has been successively intuited, and then also reproduced, into one representation. This consciousness may often only be weak, so that we connect it with the generation

> of the representation only in the effect, but not in the act itself, i.e., immediately; but regardless of these differences one consciousness must always be found; even if it lacks conspicuous clarity, and without that concepts, and with them cognition of objects, would be entirely impossible. (A104)

Here Kant tells us that, no matter how much we transmute the *content* of experience, the fact that this experience is *conscious* cannot be altered. That *one* consciousness to which all these experiences are attached is the invariant of all experiences.

There are several things to note here, the first being that many a philosopher of physics will certainly object to the connection between Kant and the use of symmetries in modern physics I am trying to promote here because, say, abstract symmetries like local gauge invariance do not fit the bill. I am not convinced that this is true, and I consider myself in good company with this (e.g. Falkenburg, 2007; Janas et al., 2022; Lyre, 2009; Mittelstaedt, 1978, 2009). To my mind, the crucial observation is Kant's apparent, illegitimate slide from constituting a *manifest* image to considering the foundations of that image as also necessarily being the foundations of any possible *scientific* image.

With 'synthetic' replaced by 'constitutive', we can detach the 'apodictic validity' from the constitutive elements of the manifest image so constituted, and see how a specifically scientific image – which is preferable for some but not *all* purposes – can be constituted by means of much more abstract and less intuitive symmetries.

That being said, the second thing to note is that both the *generation* and constitutive *utilization* of these more abstract symmetries can be guided by decidedly pragmatic elements, as I have argued at some length in Boge (2021a). To illustrate this very briefly, refer once more to the modified Wallace-dialog considered in this section. I had the skeptical inquirer claim, in Q_{8c}, that it is a postulate that a particle can be identified through that whichever remains invariant under the Poincaré group. But did we not see mass and spin *follow*, by a symmetry-based argument, as two 'natural' identifying properties qua Poincaré invariants in relativistic QFT?

In a way, this is correct, but why should one even *use* the Poincaré group in the first place? Why a *unitary* representation thereof? Ultimately, this is justified by empirical success, and the limitations of this very success may ultimately justify the move to some working quantization of general covariance. But neither Poincaré invariance nor unitarity were *directly suggested* to us by evidence.

I already mentioned above how "[n]obody has ever understood what the hell Heisenberg was [...] smoking [...] when he invented matrix mechanics." (Susskind, 2008, 15:19–15:31) But of course the use of Hermitian matrices, first introduced as tables of numbers by Heisenberg, is ultimately the root of the unitary evolution in QT. These weren't *forced* upon Heisenberg by experience, but he somehow managed to get his head around the idea by appealing to a mix of values, preferences, purposes, etc. Similarly, Einstein wasn't forced to embrace the principles of special relativity by sheer evidence: Lorentz's ether program, in which the Poincaré transformations or their divergence from the Galilei transformations would have an *empirical* meaning, was still relatively respectable at the time. And Einstein was at best vaguely aware of the Michelson-

Moreley experiment: He based his ideas on a desire for a theory that *uniformly applies* to both mechanics and electrodynamics in a coherent fashion (Zahar, 1989, Chapt. 3).

The point is that any 'Kantianism' able to cope with this sort of development in modern physics must be a kind of *pragmaticized* Kantianism. Such a position has been attributed to Bohr by Folse (1994, 121–2), and described as follows:

> Pragmatized Kantians defend their claims to knowledge through appeal to the pragmatic virtues of the categories under which the content of experience is subsumed. [...] Bohr's work in philosophy is in effect simply this: a campaign to revise the limits of application of key concepts in the physicist's synthesis of the experiences which form the empirical basis of our knowledge of the atomic domain.

However, above I pointed out that there is a further sense in which pragmatic elements enter into constitutive efforts; namely in *comprehending* certain symmetries *as* being constitutive. Repeating what has become my favourite example, the *scaling invariance* of certain cross sections can be used to *define* what is meant by the 'pointlikeness' of elementary particles (Drell and Zachariasen, 1961; Falkenburg, 2007).

Quite often, particles are introduced simply as 'bumps in the field', based on the fact that the free propagator corresponds to something which (almost) satisfies the relativistic energy-momentum relation, and that its Fourier transform can be formally read as the probability of 'finding something at x', given that there 'was something at y'. Hence, when $|\Omega\rangle$ is naïvely considered the 'state of a field', the structure of these results looks suspiciously as if QFT was telling us to expect "a disturbance in the field to propagate from [y to x]." (Zee, 2010, p. 24) However, not only do several results (Halvorson and Clifton, 2002; Malament, 1996) imply tight limitations on the localizability of this 'disturbance'; any alternative in terms of 'properties of the field', wherein $|\Omega\rangle$ is literally thought of as a 'state', obviously runs into the notorious measurement problem.

A slightly less problematic phrasing has particles be 'pointlike' in the sense that "we construct interaction Hamiltonians by multiplying the relevant fields at exactly the same spacetime point." (Duncan, 2012, 164) However, that phrasing still encourages the problematic interpretation of particles as properties of fields. The only *coherent* way to make sense of this I know of is that 'pointlike' really means 'structureless', and that in the sense of 'not being breakable into finer pieces' (Falkenburg, 2007). However, the very definition of pointlikeness is thus an experimental condition: that of a scattering cross section being (approximately) *scaling invariant* under conditions where the scattered particles can be considered 'essentially free'. For, this tells us that what it means for entities to be 'pointlike' is to scatter off of one another in the same ways, no matter how hard they are smashed together. This 'hardness' is quantified by the interaction scale Q^2, and for protons scattered at a Q^2 where they can be construed as 'collections of almost free quarks and gluons', this is supported by evidence.

Now scaling invariance is certainly nothing like Poincaré invariance: the former is usually construed as a *fundamental* symmetry of the theory, the latter somehow as an emergent property exhibited under constrained conditions.[20] In Boge (2021a), however, I have argued that scaling invariance can be seen as the symmetry of a *sub-theory* of the Standard Model, namely, its *scattering theory*. In brief, the idea is as follows.

Many of the steps necessary for deriving a certain cross section are in no way sufficiently constrained by the fundamental assumptions defining the Standard Model; so it makes sense to take the 'theory' in 'scattering theory' seriously. Nevertheless, the shape a cross section can take on is constrained also by the principles underpinning the Standard Model, whence it makes sense to call its scattering theory a *sub*-theory (Boge, 2021a, for more details). In this way, however, scaling invariance can have a non-accidental, even though also in a sense non-fundamental, status; and being non-accidental, it can thus also function as a constitutive principle induced by the *application* of a more encompassing theory to a certain problem-set (scattering scenarios).

A second crucial observation here is that scaling invariance is always only *approximate*. For partons, this could be expected even with little knowledge of QFT, as presumably anybody has heard about 'quark confinement'. And if quarks and gluons must be considered constituents of hadrons, any 'probe' will have to interact with the whole hadron (thus indicating substructures through scaling-non-invariant scattering behaviour). However, even an electron is best thought of as only ever *asymptotically* free; for otherwise one's theory is necessarily interaction-free (cf. Bain, 2000).

Thus, if we think that scaling invariance tells us to constitute quarks as structureless, an approximate, non-fundamental symmetry can have a constitutive function. Furthermore, this symmetry would become exact when the theory would be literally free. It is interesting to note, in that respect, that for most practical purposes, any cross section involving hadrons can usually be expressed as a weighted sum of cross sections on the parton level, i.e., as defined in terms of definite patron momenta ('free parton wavefunctions'). This has only been 'proven' (in the physicist's sense of the word) for a number of selected cases, but the resulting 'factorisation' of cross sections involving hadrons into parton-level cross sections is applied ubiquitously for high energetic scattering events with hadronic scatterers, on account of heuristic arguments for generalizability (e.g. Schwartz, 2014, Sect. 32.5).

When provable, this factorisation bears the hallmarks of a *decoherence theorem* (also Schwartz, 2014, 674): The final form nicely separates the non-elementary cross section into a probability-weighted sum over elementary cross sections (for leptons and partons), and so suggests a basis of partonic rather than hadronic (momentum) states. Since cross sections include a matrix element squared, this is formally analogous to computing with a mixed state over different parton momenta and flavours. Furthermore, since the interaction scale will be determined by the four-momenta of the scatterers (the electron and the proton) and thus increases as both or one of them is accelerated in the lab-frame, the result becomes more and more valid as Q^2 increases.

What is interesting about this in the present context is that we can see another *approximate symmetry* at work here, namely the dynamical invariance of the preferred

[20] I have been confronted with this objection for the first time by Dean Rickles at the Stellenbosch Institute for Advanced Study, and I should thank David Glick for helping me sort out the right response to it.

basis (the pure parton states) under increasing acceleration. Quite generally, decoherence can be described by the condition $UA \approx AU$, where A is a (preferred) system-observable, and U the interaction between system and environment. Multiplying both sides from the left with $U^{\dagger}$, we see that this corresponds to $A \approx U^{\dagger}AU$; so A remains approximately unchanged when the time is evolved according to U.

Decoherence usually has the consequence that 'classical' observables, such as position and momentum, are dynamically singled out—and both simultaneously, to a respectable degree. Hence, decoherence is often equated with an 'emergence of classicality' (Joos et al., 2003; Schlosshauer, 2007). However, as a general assessment, this seems inappropriate for two reasons: (a) In the scattering case considered here, the preferred states which are used in the 'dynamically emergent mixture' are plane-wave states (momentum eigenstates), and so not very classical (perfectly indefinite in position). Hence, decoherence at best *often* sanctions a classical treatment. And (b), a state-non-representationalist obviously cannot interpret the transition to an approximately diagonal density matrix as a dynamical process in which the interference between terms in a superposition become suppressed, and a classical trajectory (or maybe a multitude of neatly separated 'worlds') literally 'emerges' in consequence.

I suggest a more general view of decoherence as a *bridging principle*: It sometimes specifies under what conditions we can treat physical systems the way we always have (as buzzing around in space); sometimes under what conditions we can treat protons as mere collections of quarks and gluons with well-defined speeds; and sometimes maybe yet other things. But unless we buy into a many-worlds interpretation – and there are pretty good reasons for abstaining from this (Boge, 2016, 2018) – this ability to think in terms of trajectories or fixed momenta for elementary particles is *only* given if the content of the quantum state is considered inherently *probabilistic*: These theorems do not deliver a "selection step" (Fuchs and Schack, 2012, 246).

In at least some agreement with Healey (2012), I suggest that decoherence theorems urge us to distribute our credences in a certain way, namely across the different values of the magnitudes that are approximate dynamical invariants under the evolution considered. Certainly, decoherence theorems do not exist for all contexts in which we may want to assign credences in accordance with one density matrix or another. However, when they do exist, they tell us that, relative to the model of the dynamics we have chosen, certain quantities come out as those across whose values we *should* distribute our credences, and hence as in a certain sense *objective*.

To sum up: In an extension of the Friedmanian program, I suggest to pay attention not only to fundamental and exact symmetries, but also to symmetries that become important in certain contexts of application, as well as certain approximate symmetries. The former ones can be seen as fundamental, or at least constitutive, symmetries of *sub-theories* which arise in the context of applying a theory to a problem set and have a life of their own; the latter ones as bridging principles between two different theories, such as hadron and parton-level, or even quantum and 'classical' theories. This I see as the coherent execution of at least part of the Kantian program, wherein invariant elements occupy a crucial role in determining what is *objectively fixed*. However, in contrast to (what I take to be) Kant's original doctrine, I concede that *scientific* objectivity is determined far more *opportunistically* – relative to a sufficiently large but not boundaryless *problem set* – and so not in the same sense transcendental.

4.2 Kant vs. Phenomenology

As already indicated above, there is an obvious connection between Kant's method of reasoning and the eidetic variation: Clarke (2014, 268) describes Kant's method in the transcendental aesthetic as "a precursor to Husserl's method of eidetic variation", and similar parallels are drawn by Wiesing (2014, 64) or Mohanty (1991). The question thus arises what the defining differences are.

I have highlighted the difficulties I see associated with phenomenology, and I believe that the most important difference – for my purposes at least – lies in the status of synthetic a priori judgments. For instance, Gallagher (1972) argues that Kant employs the synthetic a priori to refer to necessary *forms* of experience only; and thus to *structural* knowledge about the way things are bound to appear to us (also Ladyman, 2020, Sect. 3.1). The particular necessary structures he thought he could derive, among other things, from the shape of the existing mathematics and mathematical physics of his time (Gallagher, 1972, 342; Friedman, 2001, 10). But as I have argued above, (a) this makes for an important *continuity* to the project I am undertaking here, and (b) the *necessity* can be removed on the pains of a loss of certainty alone. By contrast,

> Husserl is holding for [...] a necessity which is based upon insight into essential connections between the *content* of subject and predicate. In this sense, the insight into necessity, far from being a formal condition for the experience of objects, is rendered possible *through* the experience of certain objects. (Gallagher, 1972, 343; orig. emph.)

So whereas Neo-Kantians in the Reichenbach-Friedmanian tradition essentially suggest to significantly *weaken* the synthetic a priori, while simultaneously retaining its connection to scientific and mathematical theories, Husserl, and presumably most phenomenologists following him, urge to *strengthen* it into an all-pervading guide to impermutable conceptual structures. But it was the extraordinary genius of certain scientists to *let go of* certain apparently impermutable conceptual structures which made possible the scientific revolutions that gave us modern science and technology.

Hence, unlike phenomenology, I suggest to let go of certainty and embrace progress. Nevertheless, insight into conceptual structures that we *create* on the way in this need-driven, flexible, opportunistic process can be generated in ways that are rooted in Kant and compatible with at least part of the phenomenological project as I understand it.

An important objection might cross one's mind at this point: Did I not say that one major problem of phenomenology was the ineffable connection between consciousness and reality therein, and isn't every form of Kantianism haunted by that same problem? Frankly, didn't Husserl reject the Ding and sich due to its very ineffability, as evidenced by the notorious 'problem of affection' in the Kant-literature? For 'affection' appears to be a causal notion and we are thus, apparently, (i) either only affected by appearances (so Kant's position collapses into Berkeley's), or (ii) causation fulfills a dual role and can reach out into a 'nuomenal world' (in defiance of Kant's view of causation as a form of experience), or (iii) that there are two distinct kinds of causation ('nuomenal' and 'phenomenal' causation); a kind of silly fix. However, I believe that the thorough Kant exegesis of Henry Allison (2004) has led to a satisfying solution to this problem:

> [T]he Kantian theory of sensibility not only requires that something "affect" or be "given to" the mind; it also maintains that this something becomes part of the content of human cognition [...] only as the result of *being subjected* to the apriori forms of human sensibility (space and time). [...] The point is [...] that, insofar as [the spatiotemporal objects of human experience] are to function in a transcendental account as material *conditions* of human cognition, they cannot, without contradiction, *be taken under* their empirical description. (Allison, 2004, 67–8; emph. added)

The key to (dis)solving the problem of affection is hence to read Kant as an epistemologist, not a metaphysician in the modern sense of the word: All talk of 'things in themselves' and 'affection' is intended as an analysis of what it means for sensibility to be receptive and for cognition to be discursive (to require concepts and sensible intuitions). Beyond that, Kant can simply *remain silent* about the status of 'super-empirical entities' or their relation to us (Allison, 2004, 73). It is unclear to me whether such a move is available to phenomenology, with its focus on the 'things themselves'.

4.3 What's to gain for QBism (or state non-representationalism more generally)

Let us recall that I had identified two basic problems that I see associated with QBism:

(EP) By *selectively* using recalcitrant experiences as grounds for postulating a 'world out there', QBism undermines its own basis for doing so.

(SP) By putting 'naked' experiences at center stage, QBism undermines its own basis for employing 'higher level' concepts.

As for SP, recall also that I argued that phenomenology offers a source of "substantive a priori knowledge" (Nagel, 2000, 346), and hence could help QBism circumvent SP. However, the same, I believe, is true of the Neo-Kantianism I am advocating here, even if the amount of a priori knowledge is significantly sparser and more detached from everyday-life modes of thinking. All we need to circumvent the SP is a creative contribution of the mind in terms of not directly experience-related concepts. Hence, the invariants contained in theories of modern physics may do this job just as well—even if our vision of what we take the world to be when viewed through the lens of the given theory may thereby end up being equally sparse.

To see this more clearly, refer once more to Wallace's second level challenge; that a dialog on such things as the quark-gluon plasma becomes incomprehensible if we do not consider concepts connected to the quantum formalism as at least intended as representations. We had seen above how a symmetry-based argument can, in the interaction-free limit, identify a particle as that whichever is characterized by spin and mass (or three-momentum). However, more generally speaking, we can also always associate a preferred basis to a given system, relative to a given situation, whose projections represent the properties it has in that situation, with the basis either singled out by decoherence or in some other way (e.g., by considerations of preparation and measurement). Since unitaries only leave the whole 'non-Boolean lattice' invariant, however, the system is that whichever carries the whole collection of properties; not something characterized by a select set of continuously evolving properties (Mittelstaedt, 2009; see similarly Janas et al., 2022).

Now QBists may object that this view is too narrow: POVMs as a generalization of PVMs (projector valued measures) have established themselves as representations of properties in a quantum context. While true, I believe this is not an objection: Obviously, POVMs too have symmetries, and these determine the possible situations the system identified by the collection of properties represented by the POVM can be in (cf. Decker and Grassl, 2007, for an example). Furthermore, singling out the symmetries of a *collection* of POVMs, with elements from different POVMs *not* jointly resolving the unity, one may similarly generalize the treatment in terms of non-Boolean *lattices* sketched above. So I see no principled differences between PVMs and (general) POVMs.

So much for the SP. The treatment of the EP I consider far more interesting. For first of all, how is *abduction* at all related to the constitutive Kantian project I have bought into here? Usually, abduction is used in the context of stronger realist positions; say in the form of no miracles-style arguments: This and that theory has unrivaled success; if that success was due to a correspondence between (crucial aspects of) the theory and a mind-independent reality, it would be expected; therefore, it probably is due to that.

However, that is certainly not what the Kantian can have in mind (if and) when she talks of an 'abduction base' for constructing reality. A very first point to note are the scare quotes I have employed when I have considered Kant's 'deductions' above. For surely, the arguments Kant deploys in the Critique are not stringent deductive arguments in the modern sense of 'deduction':

> Jurists, when they speak of entitlements and claims, distinguish in a legal matter between the questions about what is lawful (*quid juris*) and that which concerns the fact (*quid facti*), and since they demand proof of both, they call the first, that which is to establish the entitlement or the legal claim, the *deduction*. (Kant, 1998, A84/B116)

This establishment of what is lawful in court is clearly similarly uncertain (and thus non-deductive, in the modern sense) as is the establishment of facts. For otherwise, there could not be precedents which determine the future understanding of what is lawful according to a set of laws. What jurists seek out in 'deductions' is the best *interpretation* of the existing laws. However, that is clearly not what Kant is after.

Lyre (2009, 493), indeed, analyses Kant's reasoning as *abductive*: "the transcendental argument structure [...] should perhaps [...] be reconstructed as an inference to the best explanation: the existence and validity of [preconditions of experience] PE is the most plausible explanation for [experience] E." Hence, transcencendental arguments deliver a preferred *explanation* for the structure of particular *experiences*. However: "it is Kant's special claim that synthetic judgments a priori are accompanied by necessity and generality. But it is exactly this demand, which should better be weakened in view of a modern revised use of transcendental arguments[.]" (ibid.)

Now, with the a priori already constitutive, handling the EP by abducing certain preconditions of experience doesn't seem sufficient; for in the original Kantian line of argument, these were always maximally *general* preconditions. Hence, at face value, the sort of abductive argument given by Kant, when viewed under the terms of a relativized, constitutive a priori, has to do rather with the discovery of *theoretical frameworks*.

I suggest that two further realizations are important for making sense of how to deploy 'transcendental-style' reasoning in order to arrive at a solution to EP. First, several scholars identify Neo-Kantianism as an anti-realist position, because it seems to "reject[...] the metaphysical dimension of realism" (Chakravartty, 2017, Sect. 4.1). According to the epistemological reading I have sided with here, that is not right in the sense that Neo-Kantianism implies a rejection of the metaphysical thesis that there exists a mind-independent reality. That would be idealism of the Berkeley-variety. Rather, the claim must be that questions as to the mind-independent reality of x are answerable only by reference to science's presupposing x's reality. And science is, in fact, not pursued as a mere exploration of the mind. So in other words, Neo-Kantianism does *not* commit to the metaphysical thesis of the *non-existence* of 'nuomenal' entities; it rejects the very *question* as to their 'really, really real' existence as unanswerable.

This has clarified the sense in which Neo-Kantianism is 'anti-realist'. Second, however, it is crucial to realize that "to requests for explanation [...] realists typically attach an objective validity which anti-realists cannot grant." (van Fraassen, 1980, 13) Hence, when we have settled for a certain theory (say, the Standard Model) with certain constitutive principles, and then infer the existence of *particular* entities (say, Higgs bosons) from observations that accord with its principles, *then this cannot be understood by the Neo-Kantian as an inference to the really real reality of those entities*. I am suggesting, in other words, that the Neo-Kantian can happily indulge in abductive practices, all the while being aware that she thereby engages, in fallible, revisable ways, in the *construction* of an empirical reality in accord with the principles of an accepted theory.

The solution of the EP should now become obvious: The basis for *what* to abduce, in this constructive sense, is delivered by the subsumption of experience under the fundamental concepts of the theory accepted at the time of the inference; or, if necessary, in a process in which the fundamental conceptual structures underlying that theory are revised. Thus, the pragamaticized Neo-Kantian can embrace the existence of 'pointlike' entities with spin and mass that carry experimentally measured properties without committing either to their 'super-empirical' existence, their picturability as spinning solid spheres, or the fact that in the future she will still embrace them.

Let me here finally reassess the problem raised by the correlations encountered by Alice and Bob, when they compare their protocols of an experiment on the Mermin contraption. In particular, consider the spin singlet $|\chi\rangle = 2^{-1/2}(|\uparrow\rangle\,|\downarrow\rangle - |\downarrow\rangle\,|\uparrow\rangle)$, recently used for acclaimed loophole-free violations of Bell-inequalities (Hensen et al., 2015): Any unitary which models the two particles involved here as traveling from a common source towards two detection devices without affecting the spin-part will, in virtue of the invariance of the norm under unitaries ($|U\,|\chi\rangle\,|^2 = |\,|\chi\rangle\,|^2$), leave the perfect anti-correlation $p(\uparrow_{\text{one side}} \mid \downarrow_{\text{other side}}) = 1$ intact. Hence, we can identify it as an objective property of the two-particle system on the Neo-Kantian analysis.

In addition, the state is *rotation invariant*, so if both spins are measured along the same axis, the (anti-)correlation is perfect *regardless of the axis of measurement*. This fact may be spoiled in a theory that models the whole situation in a spacetime curved under the influence of gravity, because the agreed upon axis of measurement is not invariant under parallel transport (von Borzeszkowski and Mensky, 2000). But this just means that, with the change of conceptual system, the symmetry involved in rotation invariance is watered down to a merely approximate one. Hence, while the *correlation* is still constitutive of the two-particle system, its *perfectness* is only so in a limiting case.

Given the problems I had outlined with causal models of the situation above, what is the status of this relation between both particles? A fruitful view has been offered by Salmon (1984) and Gebharter and Retzlaff (2020), as that of an extra-causal, *nomological* relation. Similarly Mermin (1990, 184) observes that "some physicists today might regard [the Aspect experiments] as no more than an extremely complicated confirmation of Malus's classical law"; or more generally, of *angular momentum conservation*.[21]

Caveat emptor: I firstly do not think that we thus retrieve a satisfying *explanation* of the correlations. At best, one recovers a kind of deductive-nomological explanation which is clearly in conflict with our intuitive requirements on 'explanation'. Secondly, one must not mistake this proposal as providing a metaphysical story in the sense that the 'law intervenes and makes it so' that the two particles correlate. Rather, so long as we commit to representing our credences by a singlet (or relevantly like) state, we commit ourselves to an *image* in which the 'pair of particles' always comes out with opposing values when measured along the same axis. This may be dissatisfying for a metaphysician, and certainly flies in the face our desire to know and understand. But it can be acceptable to a Kantian *epistemologist* as much as 'affection' and 'things in themselves' can be.

[21] This is ironic, insofar as Kant attributed a synthetic a priori status to causal closure (e.g. CPR, B134). However, if we follow Cassirer (1956) in equating causation with *law-likeness* – a move objectionable on certain grounds, but presumably consistent with Kant's thinking – then, given the above considerations on 'pointlikeness' and merely contextual attributability of properties, the main message of QT could indeed be taken to be that the *res* it treats of does not "possess a substantial thinglike being, a being immediately describable by analogy with ordinary perceivable objects." (ibid., 150)

5 Conclusions

I have argued here that QBism, or state-non-representationalism more generally, could profit from a Neo-Kantian philosophy of science. The reason is that this allows for a solid, comprehensible abduction-basis and a solid framework for a non-reductionist semantics, thus doing justice to actual physical practice. The suggestion should actually not come as a big surprise, since a connection has been made before (e.g. Chalmers, 2014), and since not only phenomenology, the philosophy currently 'flirted with' by QBism, has its roots in Kant, but also QBisms's 'old love' *pragmatism* (cf. CP, 5.452).

Furthermore, with theoretical symmetries occupying a central role, it is possible to not only hold fast to the beautiful non-representationalist solution to the measurement problem, but to also to reserve a righteous place for "*the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought." On the other hand, the scientific image that results from this move is a highly deprecated desert-landscape in which a lot of things we would like to ask and say must be relegated to the questions that do not have a possible answer, or to the claims that do not refer to any state of affairs, respectively. As a corollary of my treatment, QT hence most forcefully reminds us of the difficult and notorious "split[...] between the world in which an agent lives and her experience of that world" (Mermin, 2012, 8).

Acknowledgements I have profited from discussions with other contributors to this volume, including (in alphabetical order by last name) Philipp Berghofer, Michel Bitbol, Steven French, Jacques Pienaar, Thomas Ryckman, Harald Wiltsche, and Hervé Zwirn, as well as from comments by members of the Wuppertal Philosophy of Physics reading group, including (same order) Radin Dardashti, Oliver Passon, Erhard Scholz, and Marij van Strien. While writing this paper, I was employed with the DFG research unit *The Epistemology of the LHC* (grant FOR 2063), so I acknowledge the funding.

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