

Author Meets Critics:
Jill North, *Physics, Structure and Reality*

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The following brief essays are write-ups of our presentations from the 2022 Eastern APA, at an Author Meets Critics event organized by Valia Allori and chaired by Phil Bricker. Although we don't intend to publish these pieces, we all felt that they might serve as a useful companion to North's book. The authors grant permission to cite these pieces if desired.

Structure and Quantum Statespaces

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Introduction

On the opening page of *Physics, Structure, and Reality*, Jill North writes

there is a certain notion of structure that is familiar (if often inexplicit) in physics and mathematics, and paying attention to structure in this sense . . . is important to figure out what physics, especially fundamental¹ physics, is saying about the world. (2021, 1)

North introduces the notion “structure” not through a definition meant to capture its extension, nor through a set of hard-and-fast rules for individuating structures. Rather she illustrates the notion by example and in action. North details its application to a range of central (including to the history of philosophy) cases. Documenting the insights those applications yield, she thereby makes a case for the utility (and past service!) of the notion.

North’s examples illustrate how to discern structure in, and identify differences in structure between, a variety of theory-formulations. They also illustrate how to apply two core epistemic principles governing inferences about structure. North regards these principles as (imperfect but nevertheless valuable) guides to what a piece of fundamental physics “is saying about the world.” North’s core epistemic principles are:

SUPPORT: “infer physical structure in the world from the mathematical structure presupposed by the laws” (53)—specifically, infer the structure “needed for the laws to even make sense” (57); and

¹North refrains from committing herself on the question of whether the epistemic principles she spells out should apply to non-fundamental theories (see eg 57).

MINIMIZE: “posit the *least* structure required for the fundamental laws” (60)

North develops her view in the register of scientific realism. A question that will be running in the background of my contribution is whether her view can be transposed, without significant loss, to a wider register. I’m especially interested in a range of antirealist stances (including mine!) that are also *anti-instrumentalist*, insofar as they take projects of interpreting physical theories, in North’s sense of “figur[ing] out what physics is saying about the world,” to promote key scientific aims.² For these anti-instrumentalist antirealists (AIARs),³ interpretive projects are necessary, but not sufficient, for a contentful realism about physics. Having figured out what a theory is saying, the AIARs contend, it’s an additional step, one from which they refrain, to a realism constituted by believing what the theory says. An AIAR, it seems, can take North’s principles to guide the interpretation of our best and most successful physical theories, while declining to join realists in believing those theories are getting things right.

A fault line may be lurking in this amiable truce. I suspect that North suspects that a “winning structure” will typically emerge from assessments based in her principles guiding inferences about structure. The winning structure is the one she takes the smart money to back as the interpretation that captures what the fundamental physics is saying about the world. North emphasizes that the bet isn’t a sure thing: she recognizes other interpretive considerations and their potential to serve as defeasers of structure-guided judgments about the best interpretation (see, for example, 6, 59). What drives me down the road to anti-instrumentalist antirealism is the thought that, for many of our best theories of physics, there is no winning interpretation — and so no clear candidate for what a realist believes when she believes those theories, and in particular no candidate for a belief that reaps significant

²North may prefer the formulation “formulating physical theories” to my “interpreting physical theories”. I’m going to treat formulation as a middle ground between theory and interpretation, where its mediation consists in inviting an interpretation as natural. Because I don’t see the invitation as a command, and because I take physical equivalence to be a relation that obtains between fully interpreted theories, I allow that formulations that invite different natural interpretations can nevertheless admit the same one. (I think this happens all the time when we use a formulation— for instance of Newtonian mechanics in terms of non-inertial frames, bedevilled by fictitious forces— for convenience, without thereby reconceiving the world to which we apply it.) While this seems, superficially at least, at odds with the central theses of Chapter 7, I *think* it’s just a difference in bookkeeping, whereby North’s books score formulations that invite different natural interpretations as different theories and my books don’t have a ledger entry committing a formulation to its natural interpretation.

³among whom I also number the Bas van Fraassen who wrote *The Scientific Image* and *Quantum Mechanics: An Empiricist View*

abductive support from the empirical successes of those theories. If it is a commitment or a consequence of North's picture of structure that winning interpretations will typically emerge, my favored route to antirealism is blocked. Conversely, if in enough significant and central cases, considerations of structure fail to single out a (even defeasibly) winning interpretation, North's account of structure reinforces my preferred route to AIAR!

A less sweeping project occupies the foreground of this contribution. If I understand North's notion of structure, I should be able to extend it to new cases. If it has distinctive payoffs in those cases, that strengthens the case for its utility. This contribution is an attempt at extension. The majority of the applications animating *PSR* concern classical physics and spacetime theories. Quantum theories make cameo appearances, some of them quite memorable. Here I want to bring them center stage. I attempt to extend the domain of application of the notion of structure to include quantum theories. I aim to likewise extend the scope of judgments concerning differences in structure, judgements that are input for the epistemic principle governing inferences about structure. In what follows, I'll try to suggest that if these extensions are apt, they have remarkable payoffs indeed, including (arguably!) payoffs that reach beyond the metaphysics of science to the interpretation of probability!

My extensions target quantum statespaces. This is fitting, because I take one of the signal innovations of *PSR* to be its pioneering efforts to extend patterns of reasoning, evolved in a natural habitat of debates over *spacetime* structure, to *state-space* structures. Another homage I'll attempt to pay to *PSR* is to, as far as possible, relieve my exposition of technical burdens that constitute irrelevant complications to (indeed, distractions from) the questions at hand.

Quantum Theory

Herewith an introduction to quantum theory and some statespaces it deploys — an introduction paying special attention to the matter of how those statespaces support physical magnitudes implicated in quantum mechanical laws.

Normal Quantum Statespaces

In textbook quantum mechanics, the *state* ϕ of a physical system corresponds to a *vector* in a vector space \mathcal{H} . (' \mathcal{H} ' because quantum theories use a variety of vector space known as a *Hilbert space*.) Physical magnitudes, also known as observables, pertaining to our system correspond to *self adjoint operators* on \mathcal{H} . The collection of physical magnitudes comes with an algebraic structure encapsulated by a gadget

called the *commutator bracket*, a map from ordered pairs of operators to the operator that is their commutator. We'll be focussing on a very simple physical system: a point particle moving in one linear dimension. In this context, the central commutator bracket is the one given by the *canonical commutation relation* between the position observable \hat{Q} and the momentum observable \hat{P} . Where \hat{I} is the identity operator:

$$[\hat{Q}, \hat{P}] = i\hbar\hat{I} \quad [1\text{D CCRs}]$$

Indeed, on one (but by no means the only) way to think about *what it is to be a quantum theory*, we haven't got a quantum theory of a particle moving in one dimension unless we have canonical observables \hat{Q}, \hat{P} obeying the CCRs.⁴

Those observables, along with the CCRs, reveal much of note about the theory. For instance, they tell us how to use the momentum observable to implement position translations, and how to use the position observable to implement momentum translations, where these two families of translations correspond to statespace symmetries.⁵

Rather than *determining* the value of a physical magnitude \hat{A} pertaining to the system, a quantum state ϕ typically defines a *probability distribution* over possible values. Usually, given a pair of quantum observables \hat{A} and \hat{B} , there's a tradeoff between ϕ 's capacity to predict \hat{A} 's values and its capacity to predict \hat{B} 's values. (This is related to Heisenberg's notorious uncertainty principle.) The commutator bracket also sets the terms of this tradeoff.

Once an Hamiltonian observable \hat{H} is specified, the Schrödinger equation determines how states change over time. In symbols, where $\phi(0)$ is a system's state at an

⁴The way of thinking is the Hamiltonian quantization recipe for obtaining a quantum theory by "quantizing" a classical one, where quantizing requires taking the classical theory's canonical poisson bracket relations, promoting them to commutation relations, and finding a collection of vector space operators satisfying those canonical commutation relations. That's just a start: the operators affording a representation of the CCRs can be used to define further observables (e.g. energy is momentum squared divided by mass), eventuating in an algebra of quantum observables. Cogniscenti will observe that I'm taking the algebra of observables pertaining to point particle to be the von Neumann algebra affiliated with the standard Schrödinger representation of the CCRs. This is to simplify exposition: as I let on in a later footnote, another way to make quantum mechanical sense of precise positions is to work with non-standard, indeed non-separable, representations of the Weyl relations (an integral relative of the CCRs), whose affiliated von Neumann algebras aren't isomorphic to the one presupposed here.

⁵Some details: acting on a wave function with the operator $e^{-i\hat{Q}b}$ shifts its momentum by b ; acting on a wave function with the operator $e^{-i\hat{P}a}$ shifts its position by a ; as unitary, the shift operators just constructed preserve the transition probabilities posited by the theory; identifying those with the theory's empirical content motivates the claim that unitary operators implement symmetries.

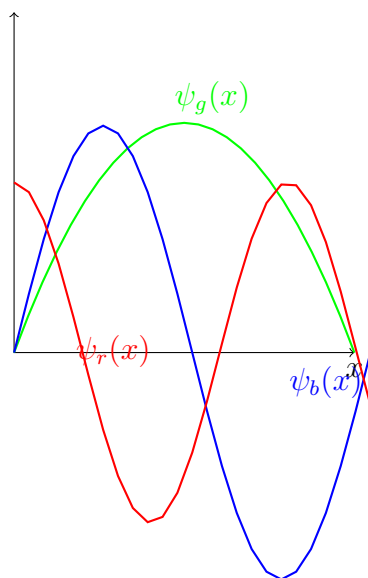
initial time $t = 0$, its state $\phi(t)$ at any other time t is given by

$$\phi(t) = e^{-i\hat{H}t}\phi(0) \quad [\text{SCHRÖDINGER EVOLUTION}]$$

Solutions $\phi(t)$ to Schrödinger's equation describe *continuous* evolution through Hilbert space: intuitively, as the difference between t and t' shrinks, so too does the difference between $\phi(t)$ and $\phi(t')$ shrink, *smoothly*, that is, in such a way that the evolution between an initial state and a final one passes through intermediate states at intermediate times. More technically, $\phi(t)$ is continuous in inner product norm.⁶ I'll illustrate this criterion of closeness in a moment. It's going to be crucial to what follows that *Schrödinger's dynamical law holds only if the evolution is continuous in this technical sense*—otherwise, there's no system Hamiltonian observable \hat{H} generating the history $\phi(t)$ of time-indexed states as [Schrödinger evolution] demands.

Examples: Position and Energy in 1d

Consider a point particle of mass m living its life on a line. In textbook QM, its instantaneous quantum state is a *wave function* $\psi(x)$, a curve associating a complex number with each point on the line, and thus with each possible exact position of the particle. The wave function $\psi(x)$ is also a vector, an element of the Hilbert space $L^2(\mathbb{R})$ of complex-valued square-integrable functions of the real numbers.⁷

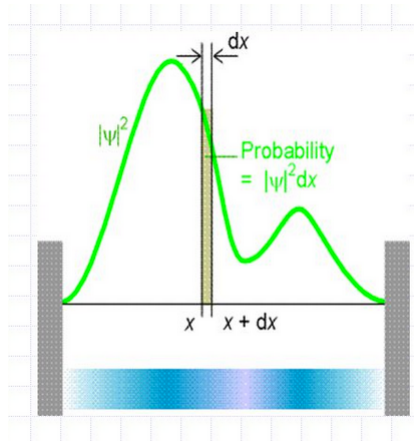


⁶That is, $\lim_{t \rightarrow 0} |\langle \phi(t) | \phi(0) \rangle| = 0$.

⁷ $\phi(x)$ is square-integrable iff $\int_{\mathbb{R}} \phi^*(x)\phi(x)dx$ exists and is finite. Square-integrability is a continuity property imposed to get the rest of the theory to work out.

Some wave functions

The wave function $\psi(x)$ defines a probability distribution over possible outcomes of *position* measurements performed on the particle. For instance the probability of finding the particle in a subinterval Δ of the line is related to the “area” confined by Δ and the curve $|\psi(x)|^2$.



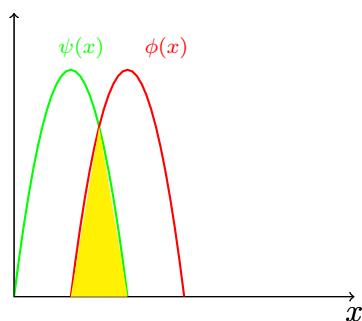
calculating probabilities: please consider dx an alias for Δ

The wave function $\psi(x)$ doesn't define probabilities only for position measurements. For any quantum observable we might measure on the system, $\psi(x)$ offers (via the Born Rule) a probability distribution over possible outcomes. The wave function $\psi(x)$ also defines *transition probabilities*, probabilities of “quantum jumps”, to other states.⁸ Consider the wave function state $\chi(x)$. The probability of a transition between $\psi(x)$ and $\chi(x)$ is related to the *inner product* (roughly, overlap) between the wave functions.⁹ This has the pleasing consequence that when $\psi(x)$ and $\chi(x)$ coincide, their inner product (and transition probability) is 1.

The inner product supplies the norm according to which Schrödinger's evolution is continuous: as t approaches t' , the “overlap” between $\phi(t)$ and $\phi(t')$, as gauged by their inner product, increases until, when $t = t'$, the wave functions coincide as well.

⁸Why we'd want to entertain such jumps is a long story. I'm invoking transition probabilities in an attempt to present the inner product norm as a gauge of how similar states are to one another.

⁹Where that inner product $\langle \chi(x) | \psi(x) \rangle$ is given by $\int_{\mathbb{R}} \chi^*(x) \psi(x) dx$. Observe that here is another place square integrability matters!



“overlap” between wave functions is gauged by their inner product

Imagine our particle confined to a box of length L . Inside it can move freely; it’s prevented from leaving by infinite potential barriers. We can set up and solve the Schrödinger equation to find the ground (lowest energy) state of the particle as well as each of its excited states. Wave functions $\psi_n(x)$ correspond to *energy* levels of the particle; $n = 0$ is the ground state, $n = 1$ the first excited state, and so on. With probability 1, a particle with wave function $\psi_n(x)$ has energy $\frac{n^2\pi^2\hbar^2}{2mL}$. But $\psi_n(x)$ does not assign the particle a determinate position—for any subinterval Δ of the box, $\psi_n(x)$ assigns a non-zero probability to the outcome of finding the particle in Δ . This illustrates the predictability tradeoff we described earlier: if the particle’s wave function allows us to predict its energy with certainty, the best it can do for position is to offer a non-trivial probability distribution over possible outcomes of position measurements.

Being There?

Whereas the textbook quantum formalism incorporates states (say of a point mass confined to an infinite square well) that correspond to *precise and definite energies*, it lacks states that correspond to *precise positions*. According to an interpretive stipulation that conveys the “conventional understanding” of QM, such a state would predict with certainty that the particle is located at a point λ in the box.

Why aren’t there precise position states in quantum mechanics? A rough explanation is that quantum states are *continuous* (square integrable) wave functions, and there just aren’t (square integrable) wave functions that amass their probability at a point.¹⁰ A somewhat less rough explanation that presupposes a little prior acquaintance with QM is: QM vector spaces are conventionally assumed to be *separable*.

¹⁰The scandalous “Dirac delta function” is not a function at all, and certainly not a vector in $L^2(\mathbb{R})$, and most certainly not a quantum state. It can be an ingredient in quantum calculations where it gets integrated against well-behaved functions to yield sensible answers.

Each has only as many distinct dimensions as there are natural numbers. But position can take *uncountably* many distinct values. QM’s ground rules require states corresponding to distinct values of an observable to be orthogonal. There aren’t enough dimensions in a separable Hilbert space to accommodate uncountably many orthogonal states.

Textbook QM sustains no notion of *being at a point*. It thereby stifles an aspiration that unites a significant subset of would-be realists about QM, including primitive ontologists: the aspiration to understand the theory in terms of an ontology of beings-there. But we needn’t limit our physical imagination to confines set by textbook QM.

Abnormal quantum state spaces

We have encountered quantum states in the guise of wave functions. One way to accommodate quantum mechanical *being at a point* is to generalize our notion of quantum states, so that wave functions are a special case, with other cases corresponding to systems occupying precise positions.

Gleason’s theorem assures us that for most quantum systems (including the particle on a line), there’s a one-to-one correspondence between quantum probability functions and quantum states. Let’s take that on board, and think of a quantum state realized by a wave function $\psi(x)$ in terms of the probabilities it assigns. For every subinterval Δ of the real line, $\psi(x)$ defines, via the area-under-the-curve (aka Born) rule, a probability that a position measurement performed on the system will yield an outcome in Δ . To generalize the notion of quantum state, we eliminate the middleman, and take a quantum state ω to *directly impose* a probability distribution on the collection of “quantum events.” A generic member of the space of quantum events has the form “measurement of observable \hat{A} yields outcome in interval Γ .” We’ll focus on quantum events realized by position measurement outcomes, but it’s important to remember that the probabilities assigned by a state have to respect the “logical structure” of quantum mechanics, which is reflected in how commutation relations organize the collection of quantum magnitudes.

So understood, a quantum state ω determines, for each subinterval Δ of the real line, a probability $\omega(\Delta)$ that a position measurement performed on a system in ω yields an outcome in Δ . In order for the collection of $\omega(\Delta)$ s to count as a probability assignment, it must satisfy the following two requirements.¹¹ First, a

¹¹Along with others, that pedants can supply on their own.

position measurement is bound to find the system *somewhere*. So ω must satisfy

$$\omega(\mathbb{R}) = 1 \quad (\textit{normalization})$$

We also demand that ω conform to other expectations anchored in the probability calculus. For instance, if Δ and Δ' are disjoint, so that the events of finding the system in Δ and finding it in Δ' are *mutually exclusive*, we should expect the probability of the *disjunction* “found in Δ or found in Δ' ” (equivalently, “found in $\Delta \cup \Delta'$ ”) to be the sum of the probabilities of the disjuncts.¹² So ω must satisfy:

$$\omega(\Delta \cup \Delta') = \omega(\Delta) + \omega(\Delta') \quad [\textit{FINITE ADDITIVITY}]$$

ω isn't a probability assignment unless it satisfies both normalization and finite additivity.

Now there's a further virtue we might demand of ω . This virtue is not only further but also supererogatory in the sense that ω can lack the virtue without thereby failing to define probabilities. This virtue is *countable additivity*. Let Δ_i be a (countably) infinite sequence of mutually disjoint subintervals. Countable additivity requires:

$$\omega(\cup_i \Delta_i) = \sum_i \omega(\Delta_i) \quad [\textit{COUNTABLE ADDITIVITY}]$$

Countable additivity extends the “special disjunction” property finite additivity imposes on finite disjunctions to disjunctions comprising infinitely many mutually exclusive disjuncts.

It is hotly debated whether countable additivity is a coherence requirement or a discretionary imposition on respectable probability functions—and if discretionary, whether the consequences of its imposition are unbearable. What matters for us and for now is that ω can define quantum probabilities without defining *countably additive* quantum probabilities. It happens that wave function states like $\psi(x)$ correspond to countably additive quantum probability assignments; let's introduce the term of art *normal states* for states with this feature. If we liberalize our notion of quantum state to accommodate probability assignments to quantum events that aren't *countably additive* probability assignments to quantum events—to introduce another (not so orthodox) term of art, probability assignments induced by *abnormal states*—we can accommodate as well a quantum mechanical notion of *being at a point*.

¹²Because the event structure is quantum mechanical, our yearning for the probabilities ω assigns to behave like classical probabilities is going to be disappointed *somewhere*. What we need to trigger the disappointment is to consider *multiple, non-commuting* observables.

Details, details (an aside)

Where ω is a state of a point particle living in the unit interval $[0, 1]$, and Δ is a subinterval of $[0, 1]$, $\omega(\Delta)$ gives the probability that a position measurement yields an outcome in Δ . Normalization obliges ω to assign probability 1 to the interval $[0, 1]$. A natural way to introduce a state with a precise location, say at a pointset $\{\lambda\} \subset [0, 1]$, would be to require $\omega(\lambda) = 1$. But this isn't going to work. If Δ and Δ' differ by a set of measure 0, they're the same quantum event. So if λ is a point, $\omega(\lambda)$ has to coincide with $\omega(\emptyset)$, which must be 0 due to normalization and finite additivity. The upshot is that we can't set $\omega(\lambda) = 1$

But here's what we can do (Halvorson 2001): Define a state ω_λ that "converges to λ ," as follows. Where Δ_i is a countable family of nested, shrinking subintervals including λ , $\omega_\lambda(\Gamma) = 1$ just in case for some i , $\Delta_i \subset \Gamma$. A particle in the state ω_λ has good claim to be precisely located at the point λ .

But note! ω_λ isn't countably additive (aka normal). (Here's an Eleatic illustration, in the form of a countable set of disjoint Δ_i , each of which ω_λ assigns probability 0 but whose union is the unit interval, which ω must assign probability 1. For simplicity, suppose $\lambda = \frac{1}{2}$. Let $\Delta_1 = (0, \frac{1}{4})$, $\Delta_2 = (\frac{3}{4}, 1)$, $\Delta_3 = (\frac{1}{4}, \frac{3}{8})$, $\Delta_4 = (\frac{5}{8}, \frac{3}{4}) \dots$)

Thanks to the offices of the GNS construction, we can realize abnormal states as Hilbert space vectors — just not vectors in the Hilbert space $L^2(\mathbb{R})$ housing the normal states familiar from textbook quantum mechanics. Rather, states like ω_λ are elements of a non-separable (=uncountably infinite dimensional) Hilbert space $\ell^2(\mathbb{R})$ of square summable maps from countable subsets of the reals to the complex numbers (see Halvorson 2001 for details). There's a dialectic about structure that shadows the one developed in the main text but focusses on the structures $L^2(\mathbb{R})$ and $\ell^2(\mathbb{R})$, rather than the structures of normal and abnormal states.

Quantum statespaces and structure

Enough background is in place to bring North's apparatus for thinking about structure to bear. Let \mathcal{S}_n be the quantum statespace consisting of textbook normal states of our particle, and \mathcal{S}_a be a quantum statespace comprising normal and abnormal states. Some states in \mathcal{S}_a correspond to probability functions violating countable additivity; no states in \mathcal{S}_n do. This closing section will briefly treat three questions:

1. Do \mathcal{S}_n and \mathcal{S}_a differ in structure in North's sense?
2. If they do, how do North's epistemic principles governing inferences about structure apply to choices between these quantum statespaces?

3. Does their application have consequences for questions about realism and anti-instrumentalist anti-realism raised in §1?

Prima facie,¹³ the answer to (1) is YES: One of North’s templates for hierarchical structure is “a higher-level structure is less general, a special case of a lower-level structure, satisfying further conditions” (50). \mathcal{S}_n and \mathcal{S}_a fit this template perfectly, with \mathcal{S}_n being the higher-level structure that results from imposing the further condition of countable additivity on the lower-level structure \mathcal{S}_a .

Answering question (2) takes a little more work.

Drifting

A particle in an abnormal state ω_λ has accomplished *being at a point*. Now let’s add motion to its list of accomplishments. An particle in initial state ω_λ moves if a time t later, it’s a distance r away—that is, in the state $\omega_{\lambda+r}$ that converges to the point $\lambda + r$:

$$\omega_\lambda \longrightarrow_t \omega_{\lambda+r}$$

A *normal state* (conventional wave function $\psi(x)$) can undergo a similar evolution, one that “shifts” the wave function a distance r in a time t :

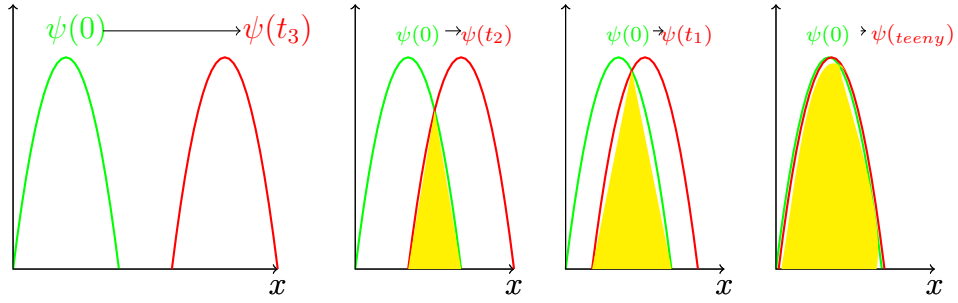
$$\psi(x) \longrightarrow_t \psi(x - r)$$

It is instructive to compare abnormal ω_λ ’s “drift” evolution with the drift evolution undergone by normal states. For simplicity, assume that the drift has unit velocity ($r = t$). Thus

$$\psi(x) \longrightarrow_t \psi(x - t)$$

To decrease notational clutter, we’ll call the state on the l.h.s., the initial state ψ_0 in what follows. The evolved state on the r.h.s. we’ll call ψ_t .

¹³There’s more work to be done here, but no space to do it here. North typically speaks of structure being “in the world,” and says it “concerns the invariant, description-independent features of quantities or facts, those that are the same regardless of choice of description” (32). I haven’t made a case that differences between \mathcal{S}_n and \mathcal{S}_a , differences between collections of ways quantum systems *might be*, are structural differences *in this sense*, differences in description-independent features of ways quantum systems *are* in the world. North’s chapter 4 models how to negotiate a transaction between statespace structures and structures in the world, a model that I believe can be adapted to the present case.



the continuity of drift evolution for a normal state: as t shrinks, the overlap between $\psi(0)$ and $\psi(t)$ grows smoothly

This evolution represented by normal drift is *continuous*: as $t \rightarrow 0$, $\psi_t \rightarrow \psi_0$ in inner product norm. Gloss: as t decreases, the “overlap” between ψ_t and ψ_0 increases until those wave functions coincide. Physical translation: as t decreases, the *transition probability* between ψ_t and ψ_0 approaches 1. And this continuity qualifies normal drift as an instance of *Schrödinger evolution*, with the system Hamiltonian H as *infinitesimal generator*. That is, $\psi_t = e^{-iHt}\psi_0$. (In the case of drift evolution, H is the momentum observable.)

The Hamiltonian H and the normal state ψ_0 are the gears the Schrödinger equation engages to *nominally bind* the instantaneous state of the particle to its states at other times. Normal drift is a variety of dynamical development constituting a continuum of nominally related events we can understand as the natural history of an enduring object.

By contrast, the abnormal drift evolution $\omega_\lambda \rightarrow_t \omega_{\lambda+t}$ is *discontinuous*: no matter how small t is, as long as it’s not 0, the transition probability between distinct exact position states ω_λ and $\omega_{\lambda+t}$ stubbornly remains 0.¹⁴ When $t = 0$, ω_λ and $\omega_{\lambda+t}$ coincide, and the transition probability leaps to 1. The earlier state jumps directly from being *maximally unlike* to being *maximally like* the later state, without passing through intervening stages of increasing resemblance.

It follows that the $\omega_\lambda \rightarrow_t \omega_{\lambda+t}$ abnormal drift evolution is *not an instance of Schrödinger evolution*. Schrödinger evolution is continuous and generated by the system Hamiltonian. Abnormal drift is not continuous; it therefore is generated by no system observable and eo ipso it’s not generated by the system Hamiltonian. This

¹⁴This can all be made precise by wheeling in the “position representation” mentioned in the next footnote (or by applying an algebraic definition of transition probability). Here’s a plausibility argument evoking that representation: as long as $t \neq 0$, ω_λ and $\omega_{\lambda+t}$ are distinct, point-valued eigenstates of a continuous position observable. As such, they’re orthogonal. Therefore their inner product is 0.

disruption of Schrödinger evolution is related to another striking difference between normal states and the abnormal state ω_λ : states connected by abnormal drift aren't states to which quantum mechanical notions of momentum or energy even apply, because they're states falling outside the domain of the drift Hamiltonian (which is just the canonical momentum operator).¹⁵ Please note that this is a stronger claim than: they're states whose momentum and energy quantum mechanics does not predict with certainty.

We've said enough to invoke North's first inference principle—infer the structure needed to support the mathematical laws. The structure needed for the dynamical law of Schrödinger evolution to make sense includes statespaces pierced by histories with continuity properties consistent with that law's fundamental posit, that a self-adjoint Hamiltonian observable generates dynamical evolution. North doesn't take a stand on whether the first inference principle applies to non-dynamical laws, but it's very tempting here to so apply it. If anything is a *kinematical* law, [1d CCRs] is. The structure needed for [1d CCRs] to make sense includes a well-defined momentum observable. The statespace \mathcal{S}_n of normal quantum states sustains that structure. The statespace \mathcal{S}_a including abnormal quantum states does not. Conclusion: North's epistemic principles license the inference that \mathcal{S}_n , but not \mathcal{S}_a , tells us about the structure of physical world.

Under some (not exactly mild) additional assumptions, this inference has repercussions outside of physics. One not exactly mild additional assumption is that quantum states are (or code) objective chances; the repercussion holds for probability theory construed as a theory of objective chance: probabilities must be countably additive. Another not exactly mild additional assumption imposes a chance-credence norm requiring subjective probabilities (credences) to share the additivity properties of objective probabilities; it has the repercussion that subjective probabilities also must be countably additive.

Conclusion?

Thanks to abnormal states like ω_λ , we can make quantum mechanical sense of being at a point. But assigning a system a state like ω_λ alienates it from other physical magnitudes — momentum, energy — critical to making sense of it as an enduring

¹⁵Helping myself to apparatus not explicated in the main text, I can follow another route to a similar conclusion: ω_λ is a normal state with respect to a representation of the Weyl form of the CCRs on the *non-separable* vector space $\ell^2(\mathbb{R})$ (Halvorson 2001). But in this *position representation*, the momentum observable is not well-defined, and the “at-at” evolution, though unitary, is not continuous.

object falling under quantum mechanical law. Or so I have tried to suggest, in an effort to show that North’s views about structure extend potently to quantum statespaces.

Yet I can imagine a decidedly different, equally potent, account of how to extend North’s analysis to quantum statespaces. This decidedly different account might be favored by primitive ontologists and others who follow John Bell in holding, that when it comes to figuring out what quantum theory is saying about the world, “What is essential is to be able to define the positions of things” (Bell 1987, 175). This decidedly different account selects quantum statespaces (for instance the nonseparable statespace $\ell^2(\mathbb{R})$ described in the previous) sustaining structures that enable the positions of things to be well-defined — even at the cost of making sense of momentum, energy, and so forth.¹⁶

I extended North’s views by following epistemic principles focused on structures supporting theoretical law, somewhat narrowly construed. This rival extension focuses on structures that (putatively) satisfy another criterion North discusses: the *directness criterion*, according to which “we should prefer formulations that more directly represent the physical world” (137). (Or, alternatively, the rival extension relies on a reading of the “structure needed even for the laws to make sense” version of the first epistemic principle heavily influenced by metaphysical, epistemological, and emotional commitments to the idea that making physical sense of anything requires a picture of stuff ornamenting spacetime.)

AIAR that I am, I am inclined to code the rival extensions as reflecting the sort of “underdetermination of structure by theory” that would add a Northern branch to my preferred road to anti-realism. But there are myriad ways to resist that coding, starting with rejecting as inapt my attempt to extend North’s notion of structure to quantum statespaces and running through many levels of and subtleties in North’s account. Supposing that I haven’t talked North out of her realism, it would be illuminating to hear what forms of resistance she’d recommend!

Acknowledgements

I am obliged to Dave Baker, Gordon Belot, John Earman, and Jill North for feedback on earlier versions of these comments —and very, very grateful to Jill for writing such a wonderful book!

¹⁶“But try, in contrast, to describe your immediate environment in terms of particles which have only momentum and not position. It is hard even to know where to begin” (Maudlin 1997,16).

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Suggestions for my Ally

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It's a cliché in commentaries of this sort to say that you enjoyed the book very much, but nonetheless: I *very* much enjoyed *Physics, Structure and Reality*. There is a special sort of joy that comes from seeing good arguments for views you agree with, and against views you disagree with. In this sense, I'm the ideal audience for this book, since Jill North and I share many premises and commitments in common.

I'd like to offer some advice on how to extend the project beyond the scope North sets for it in her book. She stipulates at the beginning that her inquiry concerns only the interpretation of theories under the assumption that they're completely fundamental theories of everything. This stipulation will likely be seen as illegitimate by some opponents of her approach. Fortunately, I think it's possible to generalize what North is up to beyond these limitations, and thereby answer a family of objections that I expect will arise out of the structural realist school of thought.

I'll begin with some questions for North about how we ought to draw the line between her views and those of some opponents, the adherents of "quotienting" or "sophistication" as an interpretive method. A look at the dialectic between North and these thinkers will quickly reveal why the issue of less-fundamental theories is so pressing.

1 North vs. the quotienters

North nicely articulates a commitment that she and I share: "[W]e should, other things being equal, prefer a formulation that most directly corresponds to the nature of the physical

world.” This is North’s notion of *structure*.

Those who disagree with North and myself on this count will tend to object that, while theories are capable of expressing truths or modeling reality, there’s no sense to be made of direct correspondence between reality and a formulation of theory (at least not a fine-grained correspondence). “Quotienting,” or “sophistication” as Neil Dewar calls it¹, is one way of filling in this claim. The quotienter asserts that theories have ineliminable representational redundancies, and at a certain point, nothing more can be said except that several different formal representations are equally good matches to reality.

I’m going to separate the quotienters into two camps. The hardcore quotienters are quietists about many questions in the metaphysics of physics, as exemplified by this firebrand quote from Ladyman and Ross (2007, 159):

According to [ontic structural realism], if one were asked to present the ontology of the world according to, for example, GR one would present the apparatus of differential geometry and the field equations and then go on to explain the topology and other characteristics of the particular model (or more accurately equivalence class of diffeomorphic models) of these equations that is thought to describe the actual world. There is nothing else to be said, and presenting an interpretation that allows us to visualize the whole structure in classical terms is just not an option.

This suggestion that “there is nothing else to be said” seems to extend to the question of how, exactly, the math corresponds to the real world, beyond noticing that certain distinctions draw in the math (between diffeomorphic solutions, in this case) are not representative of anything in physical reality. The method seems to be to present a theory in mathematical terms, together with its empirical content and a specification of which states are physically equivalent to which other states. Having done this, you’ve said everything that can be said about reality on the basis of the theory. It should be pretty clear why I called these guys hardcore.

For the softcore quotienter, my chosen exemplar is Neil Dewar (2019). For Dewar, physical equivalence does bottom out at stipulation and sometimes you can’t say more about why

¹Which is probably a better term, since it doesn’t overlap with a technical mathematical term—but I’ll follow North in sticking with quotienting.

two solutions are equivalent. But in practice, Dewar seems to try his damndest to say what equivalent models have in common (for instance, by interpreting electrostatics as a theory of scalar potential properties, but without trans-world facts about which value of the scalar potential is which). The method is analogous to sophisticated substantivalism—we don't let ourselves talk about the features that aren't shared in common by equivalent models, but that doesn't present some fairly heavy-duty metaphysical theorizing about the features they *do* share in common.

This brings us to the first question I have for North: what is the difference between her view and the softcore quotienter? I'm not sure if there is a difference between North and Dewar. Both think we can say a fair bit, in terms familiar from traditional metaphysics, about the picture of reality in physics. Both also agree that redundancy in representation isn't necessarily a problem here, and may be ineliminable. (North thinks this is true of coordinate systems, for example.)

In other words, neither North nor Dewar considers it realistic to describe *only* the real structure without also describing unreal structure. Thus neither aspires to provide a complete explanation of how it could be true, coherently and consistently, that only the real structure exists. (Contrast this with the ambitions of Sider in *Writing the Book of the World*, for example.)

Dewar is perhaps the most softcore of the quotienters, and has been accused of not being very consistent about his quotienting (Martens and Read, forthcoming). So maybe there isn't much daylight between him and North because he isn't really a quotienter at all! What about the quotienters that North spends the most time engaging with, David Wallace and Chris Timpson?

Wallace and Timpson see their spacetime state realism as one way of interpreting quantum theory, and as especially perspicuous in some sense, but also as equivalent to other versions such as wavefunction realism (in the special cases where the latter version exists). North doesn't agree with this, but one might ask what the principled difference of opinion here really is.

Imagine Wallace and Timpson asking: "You, Jill, are happy to recognize the usefulness of coordinates and to treat the differences between coordinate systems as a sort of redundancy, rather than treating coordinates as structure. Well, we consider the difference between Spacetime State Realism and Wavefunction Realism to be the equivalent of a choice of

coordinates.”

One highly relevant point that North makes on this score is her response that Wallace and Timpson are trying to have it both ways, since they also say at times that wavefunction realism is a misleading representation. But perhaps they could reply that some *coordinate* representations are misleading too. For example, non-inertial coordinates, or coordinate systems in general relativity that make horizons look like singularities (so-called “coordinate singularities”).

Wallace and Timpson could say that spacetime state realism is analogous to inertial coordinates, or non-singular coordinates, and wavefunction realism is like non-inertial or non-singular coordinates. Both give equivalent descriptions of the underlying stuff when properly understood, but we also learn something when we realize that one of them is a more illuminating representation in some sense.

2 Non-fundamental theories

Back to the hardcore quotienters. What are they thinking?? Their view is pretty extreme. What sort of advantages would they cite on its behalf, in response to North?

Among other things, I think they’d say that hardcore quotienting is the only way to interpret non-fundamental theories. The paradigm of ordinary metaphysics has been developed to ask and answer questions about rock-bottom building blocks of reality, stuff that is exactly true rather than approximating reality in some sense. The same goes, they would observe, for North’s view of structure. This makes it obscure how to interpret approximate, non-fundamental theories on North’s paradigm.

But approximate, non-fundamental theories are all we have at this stage of scientific history! So if North can’t say anything convincing about what we should conclude about our own reality from the mere approximate truth of e.g. electrodynamics—but can only say what we should conclude about reality’s structure on the false assumption that electrodynamics is perfectly fundamental—this is a problem. It means that North’s views can’t tell us anything about the nature of our own actual reality, at the present stage of science.

Set aside the question of whether the hardcore quotienters can do better. It’s still a problem for North if she can’t talk about the metaphysics of the real world, except in hypothetical terms. My goal for the rest of this little piece is to give North a hand by

gesturing at how her view can be extended to talk about the real world.

My idea is that in order to apply North's approach to non-fundamental theories, what's needed is (1) a slight adjustment of the goal-aim for highly fundamental structure rather than perfectly fundamental structure—plus (2) a workable notion of approximate truth.

Let's take (2) first. A satisfactory semantics for approximate truth hasn't yet been developed. But this is everyone's problem, since it's very clear that science can't operate without such a notion. Even Bas van Fraassen will need to be able to say that a good theory like quantum mechanics is *approximately* empirically adequate, i.e. that it's approximately true to say that it's empirically adequate. And I agree with Stathis Psillos that our intuitive concept is sharp enough to be useful (Psillos, 1999, 261-279). So let's take approximate truth on board as commitment and see where that gets us.

On (1), metaphysicians working on fundamentality (Lewis, Sider) have always thought that there must be degrees of fundamentality. But Lewis famously failed to analyze it. It seems to me that approximation can help.

Consider an idealized model of the physical goings-on within a certain room: an extended object moves through a spacetime volume R from one end of the room to the other. This model could be representing a number of possibilities—a solid object rolling from one end of the room to the other, or a wave in a string stretching from one end to the other. Or the model could be used to approximate a pressure wave moving through the air in the room, or a water wave if the room is filled with water. I submit that, regardless of which of these more fundamental descriptions is true, it is a good approximation to the truth to say that an object moves through R . In North's terms, it's a good approximation to the truth to say that the world's structure includes such an object.

In general, it seems right to me to say that the quantities treated as basic by a non-fundamental theory are approximately fundamental within its domain—that is, it's a good approximation to the truth to say that these quantities are perfectly fundamental structure. So within the domain of thermodynamics (systems with many degrees of freedom), temperature is an approximately fundamental quantity. You don't go very far wrong by treating temperature as one of the basic building blocks of these systems, although strictly speaking it isn't really basic. So I suggest that North think of real but non-fundamental structure as structure that is approximately fundamental.

So my suggested addition to North's approach is the following. If you want to learn

about the highly fundamental structure in some domain (ie, some subject matter: “What is going on in this room, to X degree of approximation?”) proceed as follows. Take your best theory of that domain/subject matter. If it’s not a fundamental theory, no problem—assume it’s fundamental anyway. Use North’s method as outlined in *PSR* to determine what it says about the structure of reality. Suppose it says “the structure is S.”

That theory is probably a good approximation to the truth about the domain. Insofar as it is a good approximation, it is an equally good approximation to say “S is the fundamental structure of reality.” Then you make one of two possible moves. Either (1) you say that what it is for S to be highly fundamental structure is for it to be a good approximation to say “S is the fundamental structure of reality,” or (2) you give some other analysis of degrees of fundamentality that justifies you in inferring “S is highly fundamental structure” from “it’s a good approximation to say that S is perfectly fundamental structure.”

The final thing to note is that this will require a degree of detente between North and the Quotienter. Because no one should want to treat *all* of the structure of the non-fundamental theory as highly fundamental. Some of it will range outside the domain of approximation. We (North and I) don’t want to say that GR is right about the structure at the Planck scale. Instead I think we want to adopt a limited form of quotienting: treat solutions of GR as equivalent (for interpretive purposes, when asking questions about our reality) if they say the same thing about GR’s domain of application (ie, at large enough scales).

As North rightly notes, a problem with quotienting is that it provides no explanation for the equivalence being posited. But in this case, that’s fine, because we expect that a more fundamental theory can provide the explanation. Thus we aren’t actually saying that there are facts about equivalence that are really unexplained, we’re just saying our non-fundamental theory isn’t sufficiently accurate to provide the explanation.

The bottom line, then, is that North should agree with the (softcore) quotienter, but only about non-fundamental theories. When it comes to truly fundamental theories, on the other hand, I’d urge North to go for the gold, go for the gusto. Aim at a theory that has no (semantic) representational artifacts, that says all and only true things about what reality’s structure is like, without appeal to or quantification over anything unreal. Maybe this isn’t possible for humans, but it’s the goal we should strive for, and we should get as close as we can.

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Comments on North, *Physics, Structure and Reality*

Wayne C. Myrvold

For Eastern APA Author Meets Critics session, January 19, 2022

Jill North's *Physics, Structure and Reality* is a rich and wonderful book, one that weaves together a number of threads related by the notion of structure as it is used in physics, and it will serve as a springboard for a number of discussions for years to come. In my remarks, I will focus on one thread in this tapestry, with an aim of continue the dialogue between North and those who hold a different view, with an eye to understand what they do and don't disagree on.

This thread has to do with spatiotemporal structure, and with inferences about it based on what North calls the "minimize structure" principle. This principle advises us to posit no more spacetime structure than is required by the dynamical laws. According to North, this principle yields "non-conclusive inferences" (p. 69). It merely tells us not to posit spacetime structure devoid of dynamical significance in the absence of reasons to do so. However, says North, "[t]here could be reasons to posit an absolute space structure in a world fundamentally governed by Newton's laws (as Newton himself thought) or an absolute simultaneity structure (as Lorentz thought)...." (p. 70).

One question I want to raise is whether we *can* posit spacetime structure that is devoid of dynamical significance. North takes as a "starting point" that we can, that spacetime structures can be specified independently of dynamical laws (p. 68, 141). Opposed to this is a view she attributes to Harvey Brown and David Albert, which she calls the "dynamical approach," on which talk of spatiotemporal structure codifies certain sorts of feature of the dynamical laws. This sort of view "denies that there is such thing as spatiotemporal structure that is presupposed by the laws, and can come apart from them" (p. 140).

North says that she doesn't have any conclusive argument against this sort of view, but notes that it involves rejection of one of her starting points, "the basic thought that the dynamical laws 'require the support of various space-time structures,' in Earman's phrase, so that the laws come apart from those structures." She notes that this starting pointing is in line with the usual ways of framing philosophical discussions.

This raises the question of whether the disagreement about this matter must remain in this state, that is, one having to do with starting points, with different starting points lead to different conclusions. This would be an unfortunate state of affairs. But it doesn't seem to me that we are stuck with it, in part because, for the authors cited (which include, in addition to Brown and Albert, also Brown & Pooley, Myrvold, and Knox), rejection of North's starting point is not *itself* a starting point, but, rather, the *result* of reflection on the role of spatiotemporal concepts in physics.

First, a point of agreement: It is not always obvious, given a formulation of dynamical laws, what spacetime structure is presupposed. Newton's first law of motion, as formulated by Newton, might sound like it invokes a distinction between rest and uniform motion, but, as they occur only in the disjunction, "rest or uniform motion," a distinction between them plays no role. It is even less

obvious that classical electromagnetism, as standardly formulated at the turn of the 20th century, does not depend on a notion of rest or motion with respect to the ether.¹

This is relevant to both the historical cases of Newton and of Lorentz. It would be anachronistic to ascribe to Newton a preference for what has come to be known as “Newtonian spacetime,” a spacetime with a distinguished state of rest, over Galilean spacetime, which lacks a distinguished state of rest, as the latter was not at the time an explicitly formulated option, nor were the conceptual resources in place to formulate it. In his Scholium on space and time, Newton seems to be considering only two options: a spacetime with an absolute notion of velocity, and a purely relational alternative on which only relative motions of objects have significance. He argues against the latter, and concludes in favour of the former.² We must admit that we simply do not know how Newton would have responded, had it been proposed to him that Galilean spacetime is (as Stein puts it), “the true structure of the space-time of Newtonian dynamics” (Stein 1967, p. 183; 1970, p. 267).

In a similar vein, those of us who agree with Einstein (1954, p. 281) that “[Lorentz’s] discovery may be expressed as: “physical space and the ether are only different terms for the same thing; fields are physical states of space” have to admit that Lorentz didn’t see it that way, and will have to say something like: *Einstein understood what Lorentz did better than Lorentz himself*.

Another point of agreement: shifts in our ideas about what the dynamical laws are that govern the world, and the accompanying shifts in conceptions of spacetime, do not happen as a result of conceptual analysis (p. 72).

The way I would put it is: empirical evidence informs our ideas about what dynamical laws govern the motion of objects, but, once we think that we know what those dynamical laws are, conceptual analysis is required to understand the spacetime structure associated with them. As already mentioned, it may not be immediately obvious whether or not a given set of dynamical laws requires a notion of absolute velocity. On this picture, evidence is evidence about spacetime structure only *via* being evidence about dynamics.

¹ Einstein, in the opening paragraphs of his famed 1905 paper on the electrodynamics of moving bodies, cites an example, involving a magnet and conducting loop in relative motion in which the usual theoretical treatment differs depending on whether the magnet or conducting loop are at rest with respect to the ether (specifically, the presence or absence of an electric field depends on whether the magnet or conductor is moving with respect to the ether). Understanding how, despite appearances, the concept of motion with respect to the ether is dispensable, involves seeing how it makes sense for presence or absence of an electric field to be frame-relative, and not an intrinsic feature of the physical situation. It takes some work to see this that this does, indeed, make sense!

² Howard Stein: “although he is clear that dynamics does not provide any way to distinguish motion from rest, he does not seem to have conceived of the philosophical possibility that *that distinction cannot be made at all*; that is to say, that the spatio-temporal framework of events does not intrinsically possess the structure of the Cartesian product $S \times T$, but a weaker structure. One easily understands why Newton should not have conceived of this possibility; even Poincaré, at the end of the nineteenth century, could express the view that if rotation is real then motion must be real, and if acceleration is real then velocity must be real” (Stein 1970a, 266–67).

Here's what North says about this (and here lies a potential locus of disagreement). The shifts in question "happen as a result of shifts in our evidence for what the world's spacetime structure is" (p. 72). This suggests that we could have evidence about what the world's spacetime structure is that goes directly, not via the route of being evidence about the dynamical laws. If this is right, it opens up the possibility of drawing conclusions about dynamics on the basis of antecedently drawn conclusions about spacetime structure, which would be unthinkable on the Brown-Albert view. There are other passages that suggest the same; *e.g.*, "one reason to think that Aristotle's physics is not the fundamental physics of our world is that we don't think the world has the requisite spatial structure" (p. 54).

So, one question I have is: is this what is meant? Could there be evidence about the spatiotemporal structure of the world (*e.g.*, that its structure is Aristotelian, or Newtonian, or Galilean, or Minkowski) that does not go via the route of being evidence about dynamics? If yes, how does it work?

This is important, because, if a case can be made for an affirmative answer to this question, the dynamical approach is dead in the water.

The minimize-structure rule says that no more spacetime structure should be posited than is required for the dynamical laws to make sense. North takes this rule to yield non-conclusive inferences. There might, she says, be other reasons that outweigh it, in which case we might very well have reason to posit spacetime structure with no dynamical significance. This is something that a proponent of the dynamical approach *must* reject; on this view, we *can't* posit spacetime structure with no dynamical significance.

Let me say why I think that is, in fact, true, that we can't posit spacetime structure with no dynamical significance. It's because structure with no dynamical significance *just doesn't count* as spatiotemporal structure.

An example might help make clearer what is meant by this. Consider a world that, like Aristotle's, is bound within a celestial sphere, but which, unlike Aristotle's, is governed by Newtonian physics, with forces of interaction between objects dependent on their relative distances. The celestial sphere that bounds the universe prevents objects from leaving it but otherwise does not act upon the objects within. The center of the sphere is a distinguished point, in being the sole point that is equidistant in all directions from the celestial sphere. But it is not a *dynamically* distinguished point.

I think that, in such a case, the spacetime is locally Galilean. The center point is singled out as the only point equidistant from all points on the celestial sphere, but, not being dynamically distinguished, a small patch containing that point has the same intrinsic spatial structure as a patch not containing that point. The two patches differ in their relation to the celestial sphere, but do not differ intrinsically.

To take another example: we can imagine an infinite universe with Aristotelian dynamics. In such a universe, there is a distinguished world line and a distinguished standard of rotation about that worldline with the following properties: (i) the natural state of motion of inanimate terrestrial objects (that is, objects containing preponderance of the element Earth) is to be at rest with respect to that worldline. Once at rest with respect to it, they will remain at rest unless acted upon, and (ii) that world line is the natural place of inanimate terrestrial objects; if moved away from it by some force, once released they will move towards it until that motion is resisted by some other object.

In that universe, the distinguished worldline, though it's not the center of any celestial sphere, *is* part of the spacetime structure of the world. In such a universe, spacetime is not Galilean.

Someone who holds that there can be spacetime structure without dynamical significance would (I think) deny that, in the first case, the point at the center of the celestial sphere is to be denied the status of being singled out in the spacetime structure *solely by virtue of not being dynamically distinguished*. On such a view (I think), there could be a difference in spacetime structure between a patch containing that point and one not containing that point, even though objects don't behave differently in those patches. And, perhaps, there could be two universes, with the same dynamics, one in which there is a difference in spacetime structure between a patch containing the center point and a patch that doesn't, and one in which there is no such difference.

My second question is: Is that right? If yes, I'd like to hear more about what it is that distinguishes spatiotemporal structure from other structure; it may be that therein lies the key point of disagreement between proponents of the dynamical approach and views, such as North's, more in line with the mainstream approach in the philosophical literature on space and time

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Replies to critics

Jill North

Eastern APA (online), January 2022

1. Ruetsche

According to my idea of structure and the principles governing our inferences about it, we should infer the structure required for or presupposed by the fundamental dynamical laws (for the laws to even make sense), and at the same time we should infer the least such structure. (This goes for both the mathematical structure required to formulate the laws, and the physical structure in a world governed by these laws.) I suggest that these rules or principles apply not just to a theory's spacetime structure, as familiarly done, but more broadly, including to a theory's statespace structure. I also suggest that we should be able to use these principles to figure out a theory's structure, both the mathematical structure in the formulation and physical structure in the world, for any candidate fundamental physical theory. (More or less, since these principles can be indecisive in various ways.)

Ruetsche wants to explore whether these ideas extend to quantum theories. Since our best fundamental physics is going to be a quantum theory, they had better so extend, if they are going to be of use. And in the first chapter of the book, I suggest that I limit my discussion to less highfalutin physics purely for reasons of simplicity.

As Ruetsche's discussion makes clear, however, quantum theory poses a challenge for my ideas, and especially for the realist attitude I favor. I don't argue for realism in the book so much as presuppose it, but I do believe that a realist attitude should be, if not a consequence of the arguments I give, at least compatible with them. I even go so far as to say that my discussion provides an indirect case for realism.

Yet the case of quantum theory is trickier than I let on. For in this case, Ruetsche points out, there are two different statespace structures one might infer for the dynamics: the standard, textbook, separable Hilbert

space (and concomitant probability assignment); and a non-standard, non-separable Hilbert space (ditto). She calls these, respectively, the “normal” statespace, S_n , which contains representations of “normal” quantum states—“normal” in the sense of: standard, textbook—and the “abnormal” statespace, S_a , with representations of “abnormal” quantum states.

She goes on to suggest that the structure principles are unable to reasonably or decisively choose between these two statespaces. If that is right, then the case of quantum theory supports her anti-instrumentalist antirealism (AIAR) over my realism about structure. For this is a case of the “underdetermination of structure by theory.” There are two different structures we might infer for the theory; there is no reason to think that (only) one of them is right; and so there is no reason to infer what (only) one of them says about the world. We have here a theory for which, even granting that a theory’s structure tells us about the world, we should refrain from believing what it tells us—Ruetsche’s AIAR.

I understand the following to be Ruetsche’s main conclusion: (1) This is a case for which there seems to be no “winning structure,” by the lights of my guiding principles, and so a case that supports Ruetsche’s AIAR over my realism about structure. (2) Even if there is a winning structure, this will be for reasons that both wildly overreach—yielding conclusions on things that go way beyond the metaphysics of science, such as the nature of probability—and also contravene the chief aim of the realist about quantum mechanics, which is to find an ontology of “being there”s. Either way, I am in trouble: when it comes to quantum theory, the structure principles will lead me either to an antirealism of the kind that Ruetsche favors, or to a realism that comes along with some awkward implications. As a more general conclusion, Ruetsche’s discussion suggests, there is no reason to think that the structure principles will generally be able to pinpoint “the” structure for a given theory, either the mathematical structure in the formalism or physical structure in the world: a problem for my realism about structure.

This is really interesting, and as Ruetsche rightly predicts, I want to avoid the antirealist conclusion if at all possible. So let me gesture at some ways I might do so—not in worked-out detail, but in a (to misuse a wonderful word from Ruetsche (2011)) gistified way. I will do this by considering her final three questions in turn.

(1) Do the two statespaces differ in structure in my sense?

Ruetsche offers a reason to think they do: their respective probability assignments satisfy different conditions. In particular, the probabilities assigned to states in the normal statespace satisfy countable additivity; the probabilities assigned to states in the abnormal statespace do not satisfy this further condition. So the abnormal statespace has *less structure* than the normal statespace in that it, or its probability assignment, satisfies fewer conditions. (For purposes of this discussion Ruetsche treats the statespace and its probability assignment as effectively the same, or at least as possessing the same structure (via Gleason's theorem plus "eliminating the middle man": p. 8).) And "satisfying fewer conditions" is one of the things I suggest we can use as a measure of relative amounts of structure. In particular: satisfying fewer conditions means possessing less structure. The minimize structure principle would then tell us to infer the abnormal statespace and its probability assignment.

I'm not sure though. When I talk about one structure's satisfying more conditions or constraints than another, I have in mind paradigm cases such as the difference between a bare set and a topological space. A topological space has more structure than its underlying set—more generally, a topological space has more structure than a set—in that we add certain constraints, the axioms concerning open and closed subsets, to a set to get a topological space. We introduce additional primitive notions ("open" versus "closed" set), which satisfy certain axioms, and define further notions in terms of them (continuity of curves, neighborhoods of points, etc.), in order to endow a set with topological structure—notions that simply do not apply or make sense at the level of a mere set structure. In this way we may say that a topological space is a "special kind" of set, one in which more notions are defined, additional facts and distinctions are countenanced; and that a topological structure presupposes a set structure.

The difference between the two quantum statespaces doesn't have the same feel. The difference in probability assignments is traceable directly to a difference in the underlying sets of the statespaces, in particular to a difference in their cardinality: one is a countable set of orthogonal vector states, the other uncountable. It is this difference that is responsible for

the probability assignment satisfying countable additivity in one case and not the other. And it isn't clear to me that this underlying difference counts as a difference in structure in the relevant sense. That is: it is a *difference*, perhaps even a difference in *structure*, but it is arguably not a difference in relative *amount* of structure. To say that a separable and non-separable Hilbert space differ in amount of structure—to say in particular that the former possesses more structure, as evidenced by the additional constraint satisfied by its probability assignment—would be like saying that a countable set has “more structure” than an uncountable set, which sounds wrong: it is not as though a countable set is a “special case” or “special type” of uncountable one, with additional notions specified, satisfying further conditions; as though it presupposes an uncountable set structure, in the same way that a topological space is a “special kind of set,” with structure that presupposes a set structure. Rather, it is simply a different kind of set, with a different “number” of elements (different cardinality), possessing a different basic set property. (They lie at the same “level of (set) structure.”) More specifically, adding the constraint of countable additivity to a probability assignment doesn't seem to amount to adding further primitive notions, nor defining additional concepts in terms of them, yielding additional facts and distinctions that presuppose the basic notions of the other probability assignment. The two probability assignments are simply different, but not different in amount of structure.

So in this case, the two statespaces, with their respective probability assignments, are indeed different, perhaps even different structures. But I'm not sure they have different amounts of structure—the kind of difference required for the minimize structure principle to gain purchase.

(2) At this point Ruetsche will say: but if there is no choice to be made on the basis of the statespaces' differing amounts of structure, then this is a case of “underdetermination of structure by theory” that supports her AIAR. For it remains the case that there are two different statespace structures we could infer for the quantum laws, hence two different kinds of things we could infer about the nature of the world. (For purposes of this discussion, Ruetsche grants the idea that a theory's statespace structure can tell us about the nature of physical reality.)

Hence her second question: what do the guiding epistemic principles

tell us to infer in this case? *Which* structure is required by the quantum laws (in particular if the minimize structure principle does not choose)?

Here Ruetsche offers an argument that Support, the principle to infer the structure needed to support the fundamental laws, will pick out the normal statespace as the one that is presupposed by the Schrödinger dynamics (as well as the canonical commutation relations, which she suggests, and I am inclined to agree, should also guide our inferences about structure, albeit they are not dynamical laws). Only normal states exhibit the continuous drift required to be an instance of Schrödinger evolution, with the Hamiltonian the infinitesimal generator of the time evolution. (Relatedly, only normal states allow us to make sense of features, like energy and momentum, essential to our ordinary conception of objects in motion.) The structure of the normal statespace is therefore the one that's required or presupposed by the fundamental dynamical laws, the Schrödinger equation in particular. Support then tells us that this is the statespace structure to infer, and so (according to my realism about structure) the one that should guide our inferences about the nature of the world.

However, there is a problem with this result, at least for the realist about quantum mechanics. For this statespace, the space of “normal states,” with the structure of a separable Hilbert space, does not countenance the notion of “being at a point,” of being at an exact location—a “being-there,” as Ruetsche calls it. Only the abnormal statespace, the space of “abnormal” states, which has the structure of a non-separable Hilbert space, supports such a notion. And yet all realists about quantum mechanics, from wavefunction realists to primitive ontologists, are after an ontology of “beings-there.” (Here too enters Ruetsche's further point that the inference to S_n wildly overreaches, effectively asserting that probabilities must be countably additive.)

Ruetsche has put her finger on a tension. For what the normal statespace, the one we are led to by being realists about structure and dutifully following the epistemic principles governing our inferences about it, seems to say about the nature of the physical world is not what realists about quantum theory generally want to be able to say. There is a tension between the structure principles, which pick out the normal statespace as what's needed to support the dynamics, and the realist's metaphysical

commitments, which—in accordance with the directness criterion—pick out the abnormal statespace as the one that’s able to directly represent a suitable ontology. (A tension further hardened by the fact that abnormal states abandon other things realists would seem to want, such as being able to make sense of moving objects having energy and momentum.) More generally, whereas I suggest that a theory’s metaphysical and mathematical aspects work in tandem to pinpoint a mathematical structure and “picture of the world,” in this case they seem to pull in different directions.

Once again, we seem pushed toward Ruetsche’s AIAR, on which we do not have to choose between the two statespaces and corresponding features of the world. Instead, we allow that there is no one structure, and corresponding features of the world, indicated by quantum theory. Support picks out one statespace structure, which indicates certain features of the world; a criterion of Directness picks out another statespace structure, indicating different features of the world; but this is not a problem, there is no genuine conflict that must be resolved, since we do not go on to believe what either of these structures tell us about the world. (Note that if the two statespaces differ in amount of structure in the way mentioned above—if S_n does have more structure than S_a —then what Minimize tells us to infer will happily coincide with the desire to support a notion of “being at a point.” Still the question remains whether Support also converges on this verdict. And if it doesn’t, then this is a case for which different structure principles pull in different directions—Minimize says to choose the abnormal statespace, Support says to choose the normal one—and so a case that, once again, supports her AIAR, on which we don’t need a definitive answer as to which structure to infer.)

Ruetsche says that all realists about quantum mechanics, primitive ontologists and wavefunction realists alike, are united in wanting to countenance an idea of “being at a point” that is only captured or directly represented by abnormal states. But I am not so sure about this. Or at least: it may depend. It may depend on one’s conception of quantum mechanics; on one’s particular theory of quantum mechanics; and on the notion of “being at a point” one has in mind.

Take the wavefunction realist, who says the fundamental physical ontology consists of a high-dimensional physical field that is directly represented by the mathematical wavefunction (and perhaps also a “mar-

velous particle,” in the case of the Bohmian). It doesn’t seem so odd for this kind of realist to think that the formalism of quantum mechanics doesn’t accommodate precise “being at a point” states, in the sense applicable to ordinary particles and their positions. For particles and their locations in ordinary space are not fundamental, on this view. What is fundamental—the various parts or amplitudes (and phase) of the wavefunction field (perhaps also the location of the marvelous particle)—will be precisely located at points in the fundamental space, the high-dimensional space of the wavefunction (and marvelous particle); in that sense, there is a (fundamental) ontology of “beings-there” that is captured in the formalism. Since ordinary particles in low-dimensional space aren’t fundamental, it may not be a problem, for the wavefunction realist, if *these* things don’t have states that correspond to “being located precisely at a point” represented by the formalism of the fundamental theory.

So perhaps in quantum theory (at least in its nonrelativistic guise!¹), the wavefunction realist can resist the pressure toward the abnormal, non-separable Hilbert space and go with the normal one that, if Ruetsche is right, the dynamics pick out, all the while maintaining a formalism that directly reflects an ontology of beings-there (at the fundamental level). In which case one’s metaphysical commitments plus the directness criterion plus the structure principles all converge on the same verdict, and we are “saved” from AIAR.

Or take the primitive ontologist, who typically advocates a version of quantum mechanics that has more to the dynamics than just Schrödinger evolution. It may be that Support, when applied to the structure for that dynamics, won’t conflict with the directness criterion.

Take the Bohmian primitive ontologist. The fundamental ontology comprises particles with definite, precise locations in a low-dimensional physical space: an ontology of (low-dimensional) beings-there that is stipulated from the outset. As Ruetsche suggests at the end of her comments, the Directness criterion then seems to guide us to the abnormal statespace. Although this seems to conflict with what Support tells us to infer, it seems

¹As Ruetsche (2011) discusses, a non-separable Hilbert space may be required for other quantum theories, e.g. quantum field theory—which yields the problem of unitarily inequivalent representations, and so a different kind of problem for the realist about structure, for quantum mechanics “beyond the ordinary”: more to think about!

to me that more work must be done to figure out exactly what structure is required by the dynamics in this case. On this view, the wavefunction, in its high-dimensional space, (somehow) guides the motions of the particles, in their low-dimensional space. The structure required for the dynamics should then include the structure for the Schrödinger equation as well as the structure required for the guidance equation. And it is not immediately clear that this will conflict with the structure we are led to by following the directness criterion. The guidance equation, after all, presupposes that particles have definite, precise positions, which evolve over time in a way governed by the wavefunction's evolution (even if the wavefunction does not represent the particles that way: on this theory there is more in the world than what's represented in the wavefunction).

Or take a GRW primitive ontologist. Here too it is stipulated from the outset that there is an ontology of stuff—"flashes" or a mass density field being the popular options—in low-dimensional space. When evaluating what Support dictates, we need to take into account the structure needed for the collapse dynamics as well as the Schrödinger equation, and it is not clear to me that this must be the normal statespace. The flashes, for instance, (are stipulated to) occur at precise locations in ordinary space. This may pick out a different structure as the one that's indicated by both Directness and Support, once we take into account the dynamics governing the flash events.

(When it comes to Everettian quantum mechanics—eschewed by (all?) primitive ontologists—it seems plausible that the verdicts of Support and Directness will converge; for the only fundamental dynamics is given by the Schrödinger equation, and the fundamental ontology is directly represented by the state vector or wavefunction. Not all Everettians will agree with this take on things though. David Wallace (Wallace and Timpson, 2010; Wallace, 2012), for one, seems to deny that different formalisms can differ in how directly they represent the physical world, even if one can be most "perspicuous.")

All of which is to say that in the case of quantum theory, my suspicion is that we need to have in hand a solution to the measurement problem, and an interpretation of it (a "picture of the world"), before we can figure out both the structure presupposed by the fundamental laws, and which mathematical representation is most direct—things that go beyond the

textbook quantum mechanics that Ruetsche focuses her sights on. This is because different solutions to the measurement problem contain different laws (they disagree on whether “the” structure required for the dynamics is only the structure required for Schrödinger evolution, for instance). And even within the context of a given solution, there are different possible pictures of the world for the mathematical formalism to be more or less directly about (a primitive ontologist versus wavefunction realist one, for instance). If so, then there may be no answer to the question, “what is the structure required by quantum theory?”, full stop; but only to, “what is the structure required by this or that version of quantum mechanics?”, to be investigated on a case by case basis, for each solution to the measurement problem and each interpretation or conception of that solution. AIAR does not (yet!) follow, in other words; for it may be that, for a given more fully worked-out theory, there will be a particular statespace structure picked out by the laws and one’s (meta)physical commitments.

(3) This brings me to Ruetsche’s third question: Does this case have consequences for the question of realism vs. anti-instrumentalist anti-realism? I agree that it is possible to maintain the latter position, even while endorsing the various ideas and principles concerning structure (which for purposes of this discussion Ruetsche endorses). But I also think it is possible to retain the thoroughgoing realism I prefer—though this will require more work, within the context of different solutions to the measurement problem, to show that this is the case. I appreciate being pushed to think about this! Which I will continue to do.

2. Baker

Baker presents himself as an ally, and suggests that I should go even further than I do in the book in my realism about structure. There are three main things I wish to discuss in response: (1) about the characterization of my realism about structure; (2) about Baker’s suggestion for how to understand Wallace and Timpson’s (2010) view; and (3) about the application of these ideas to nonfundamental physics. I will also ask two side questions along the way.

(1) In the first part of his comments, Baker questions whether there is

any meaningful difference between my view and that of the quotienter, especially what he calls the softcore quotienter. The quotienter presents the mathematical formalism of a physical theory and stipulates that, because of the various representational artifacts in the formalism, there will be mathematically equivalent representations or formulations; but the quotienter does not go on to say why they are equivalent, instead simply asserting that they are. Baker calls this kind of view “hardcore quotienting,” and distinguishes it from a “softcore” version, according to which we at least describe the respects in which the equivalent mathematical representations agree, though without explaining why they do.

(Here is my first side question: what exactly does the difference between hardcore and softcore quotienting amount to?)

Baker suggests that my own realism about structure is in the end not so very different from these quotienting views, especially of the softcore variety. The reason he gives is that both I and the quotienter are “willing to acknowledge that there are ineliminable representational artifacts.” In the book I recognize, indeed positively endorse, the usefulness of various representational artifacts we use all the time in physics, such as coordinate systems and reference frames and units of measure. I argue that we needn’t abandon our reasoning in terms of these things (contrary to the prevalent attitude in foundations of physics), so long as we are sufficiently careful in our reasoning—careful to keep track of which features are mere artifacts of the chosen descriptive device, and which are not. And I say all this while also saying that coordinate systems, reference frames, and so on, do not directly tell us about structure, and will involve some representational redundancy or inaccuracy.

Baker suggests that we should instead be hardcore structural realists, rejecting all forms of quotienting, at least when it comes to fundamental physics. As he puts it: “A truly fundamental theory should be taken totally at face value.”

(My second side question: what is the difference between Baker’s hardcore realism about structure and hardcore quotienting? Baker presents his view in opposition to quotienting. But the idea that we should take a theory’s mathematical formulation “totally at face value” is what motivates hardcore quotienters like Wallace and Timpson. Is Baker’s thought that, for a truly fundamental theory, there will effectively be no need to be a

quotienter, for there won't be any representational artifacts in its formulation? So that the disagreement between the hardcore structural realist and hardcore quotienter comes down to a disagreement over whether there will be any such artifacts in a truly fundamental theory?)

I think there is an important difference between my realism about structure and quotienting, even of the softcore variety, however. In the book, I emphasize the ways in which we can legitimately theorize about physics, even about *structure* in physics, while referencing coordinate systems or other kinds of representational artifacts. This may give the impression that I am all for formulations of physics, even fundamental physics, in terms of coordinate systems or reference frames and the like. But that's not quite right. For I also think there is a criterion of "directness" that is important to choosing the best formulation. And formulations that don't mention coordinate systems and other such representational artifacts are more direct, precisely because coordinate systems and the like are descriptive devices that we bring to bear, and will invariably involve some arbitrariness and "misleadingness." Direct formulations are preferable (other things being equal), especially when it comes to a fundamental theory, for they are more perspicuous, and so less apt to mislead us about the true nature of the world.

To put it another way, although I do think that we can reason well enough about physics in terms of coordinate systems and so on, I don't mean to suggest that there must be truly *ineliminable* descriptive artifacts: I do think it best, other things being equal, to do away with such things, and that a fundamental theory of everything plausibly should do so (though I don't argue for this, and explicitly remain neutral on it, in the book). But I *also* think it's important to keep in mind that the mere mention of coordinate systems does not mean that we are not, or that we cannot be, describing genuine structure—as the case of different coordinate systems for the Euclidean plane is meant to show: in this case, the fact that there are coordinate systems in which the metric takes a certain form suffices to characterize the plane's structure, it just does so indirectly.

So in all: I think that a direct, artifact-free formulation is in general preferable, for it is more perspicuous, less misleading; and more generally, I don't think there are bound to be truly ineliminable representational artifacts, especially when it comes to a fundamental theory. However,

I also think that an indirect formulation, one that does involve such artifacts, can be a good guide to the nature of the physical world. (In this latter respect I may seem to approach the quotienter; but I think the underlying nature of the world explains why the indirect formulation is a good indicator of the structure, which goes against the core of the quotienter attitude, softcore and hardcore alike.)

(2) In another part of his comments, Baker sticks up for the hardcore quotienter in one respect. He says that Wallace and Timpson treat the difference between a conception or representation of quantum theory in terms of spacetime state realism versus wavefunction realism as analogous to a difference in choice of coordinate system. Given this, Baker suggests, Wallace and Timpson have a ready response to my “trying to have it both ways” concern. This is the concern that, on the one hand, Wallace and Timpson want to say that no one description or representation, among the equivalent ones, is more correct or more accurate or closer to the truth than any other (as per their quotienting); while on the other hand, they also want to say that one description can be most perspicuous. This is what they say of spacetime-state realist over wavefunction realist conceptions of quantum theory in particular: wavefunction realism isn’t a less accurate depiction of reality, it doesn’t fail to represent the truth about reality, albeit it is a less perspicuous depiction. This seems like trying to have it both ways: it seems like saying that spacetime state realism both is, and is not, the most accurate depiction of quantum reality; that wavefunction realism both does, and does not, *misrepresent* that reality.

Baker suggests that in response, Wallace and Timpson can fall back on the analogy to different kinds of coordinate systems. We know that different coordinate systems can yield equivalent descriptions or representations, while at the same time, certain coordinate systems can yield more or less misleading representations than others: think of using inertial versus non-inertial coordinate systems for describing a Newtonian system, for example. Non-inertial coordinates provide misleading descriptions, even though (with enough care on our part) they yield fully equivalent descriptions to those of inertial coordinate systems. Wallace and Timpson can say that, analogously, wavefunction realism and spacetime state realism differ in how misleading their representations of physical reality

are (with wavefunction realism being the misleading one), even though they are fully equivalent, merely different representations of that reality.

I am not convinced that Baker's suggestion allows Wallace and Timpson to elude the charge of trying to have it both ways, however. Take the case of coordinate systems. In this case we can ask: why do non-inertial coordinates yield more misleading representations of Newtonian systems?, and there is an answer. It is because non-inertial coordinate systems provide less direct, therefore less accurate or perspicuous, depictions of the reality they are representing (even though inertial ones, qua coordinate systems, still provide somewhat indirect depictions); they are for that reason misleading guides to the nature of reality. The underlying reality in this case possesses an inertial structure, with inter-particle forces causing objects to depart from their inertial trajectories. Non-inertial frames misrepresent these things, misleadingly suggesting that objects can travel non-inertially in the absence of a net external force, for instance. The coordinates of inertial frames more directly or accurately or naturally represent the nature of a Newtonian world; they are closer to the truth about a Newtonian world; this is why they are less misleading.

Wallace and Timpson won't want to say this kind of thing: they don't want to say what it is about the nature of the world in virtue of which one description is more or less misleading. They don't point to the nature of the underlying reality that makes certain coordinate systems particularly well-suited or well-adapted to describing it. In their view, wavefunction realism isn't misleading *because* it inaccurately represents reality as consisting of a fundamental high-dimensional field; it is simply a less perspicuous representation, one that is (therefore?) misleading.

So we seem to be back to trying to have it both ways, saying that some descriptions both are and are not more misleading or accurate than others. Wallace and Timpson want to say that some descriptions are more misleading than others, without also saying that some are more misleading *because* they inaccurately represent certain features of the world. Again, this is different in the case of coordinate systems. The reason we think that different coordinate systems all yield equally legitimate descriptions—of a Newtonian world, say, or of the Euclidean plane—is that we can point to a coordinate-independent nature the different coordinate descriptions all agree on. This allows us to say that different

coordinate systems simply yield different ways of describing that nature; and more, that some coordinate systems are more well-adapted to, and are therefore less misleading about, it. It is only given the nature of the thing being described that we can say that certain coordinate systems are less misleading about it; that even though all coordinate systems in one sense yield equally legitimate, equivalent descriptions (in the way that non-inertial coordinate systems, properly understood, yield equivalent descriptions of Newtonian systems), in another sense, they aren't all *equally* legitimate, for they are not equally natural or well-suited to the nature of the reality they are depicting (in the way that non-inertial frames are not as well-adapted to Newtonian systems). All of this is said against a picture of the underlying reality which the different coordinate representations all are about. (As I put it in the book: both what is an allowable coordinate system, and what is a particularly natural or well-suited coordinate system, depends on the underlying structure.) It is hard to see how Wallace and Timpson can say these sorts of things we familiarly say about coordinate systems in the case of wavefunction realism versus spacetime state realism, without abandoning their quotienting.

(3) I appreciate Baker's pressing me on something I hedge throughout the book: what exactly to say about fundamental versus nonfundamental theories, and the extent to which my ideas apply to nonfundamental ones. I hedged this in the book pretty much in order to avoid having to say anything detailed about what I mean by "fundamental."

Baker tries to defend me from the objection that my approach is useless in practice, that it won't extend to "real-life" theories, since it only applies on the supposition that a given theory is fundamental. (In the book I leave it open whether what I say applies to nonfundamental theories; Baker suggests that it can't carry over as is, not without some further work.) Here he has some really interesting things to say about the notion of approximation, which I want to think about more.

One question that immediately comes to mind, though, is how bothered I should be by the objection Baker is trying to save me from. My initial reaction is to not be too worried about how this will work "in practice." I am not trying to tell practicing physicists, who are presumably not actively working with the fundamental theory, how to do their job

or think about the theories they use. Rather: I want to think about our theorizing in foundational discussions. And in this context, I'm not sure that it's so problematic if what I say primarily, or even only, applies to fundamental theories—to theories that we at least pretend for the sake of discussion are fundamental, with the hope, or expectation, that we can apply the lessons learned to any genuine candidate fundamental theory. (We can then leave it to the philosophers of science and metaphysicians to spell out how what one says about fundamental physics has ramifications for nonfundamental science.) So I wonder, and would like to hear: how bad is it for me to adopt this kind of attitude?

That said, I like what Baker is thinking in trying to extend these ideas to the nonfundamental, and I think I am pretty much on board with it all: that we aim for, as it were, the most fundamental structure we can get at (this is my initial gloss on Baker's "highly fundamental," which he spells out more at the end of his comments); and by way of using some notion of approximation. (I avoided these things in the book precisely because I did not want to touch issues involving notions of approximation! I am happy that Baker is willing and able to go there.) I think I agree with Baker's suggestions, especially the thought that this isn't objectionably quotienting. It may amount to a kind of (softcore) quotienting at the level of the nonfundamental, but only in the sense that we do not—cannot—say, at that level (in the language of the nonfundamental theory), why there is the relevant equivalence relation. There is an underlying explanation for the equivalence, in other words, albeit the explanation comes from a more fundamental theory. Again: more to think about.

3. Myrvold

A few thoughts in the short time I have left. I mostly want to get clearer on the opposing view that Myrvold is defending, the dynamical approach to spacetime. I'd like to better understand the idea that there can be no such thing as a spatiotemporal structure without dynamical significance—that, "Structure with no dynamical significance just doesn't count as spatiotemporal structure"—as well as the idea that there cannot be any evidence about spacetime structure "that does not go via the route of being evidence about the dynamics." I want to ask three questions about this.

(1) Take Myrvold’s celestial-sphere-world example. If I understand correctly, the claim is that there is a distinguished point, albeit not a spatiotemporal-structure-distinguished point. For the point is privileged or distinguished by virtue of the shape of the celestial sphere, not by the dynamics. And it is only if the dynamics privileges a location that it can be regarded as spatiotemporally privileged, that is, privileged by, or a part of, the spacetime structure. And so we should not—cannot—say that the otherwise-distinguished point is part of the spatiotemporal structure of the world in this case.

But then I wonder a bit how to make sense of this. Isn’t the distinguished point itself part of the spatial structure, and therefore effectively part of the spatiotemporal structure, even if this is due to the geometry of the celestial sphere? (In which case the local patch containing the point would differ in structure from one not containing that point.) Is the thought that because this is purely spatial structure, it is thereby not spatiotemporal? I suspect that is not the reason. Naively, the spatial structure is part of the spatiotemporal structure; it seems we can put, or translate, Myrvold’s characterization of the world into spacetime terms. So what am I missing? Am I misunderstanding what is meant by a “celestial sphere” and the geometry of such a thing? Or maybe it is important to distinguish between spatial structure and spatiotemporal structure more than I am doing? (Or maybe I misunderstand the idea that objects are prevented from leaving the sphere: this may be invoking dynamical facts. But in that case, it seems Myrvold would want to say that the privileged point is dynamically privileged.) To put it another way, Myrvold says that, on the dynamical approach, we cannot posit spacetime structure that lacks any dynamical significance. But isn’t that effectively what Myrvold is doing in spelling out the sphere example, when he says that the universe has a spherical geometry, which comes along with a privileged point, even though the privileged point isn’t “noticed” by the dynamics?

(2) Myrvold says that there can’t be evidence for a privileged point (more generally, of a spacetime structure) that does not go by way of evidence for the dynamics. A naive question: does this mean that we must actually observe objects approach the point in order to have evidence

that it is part of the spacetime structure? Part of what I am wondering is what Myrvold will say about Reichenbach-style cases, which suggest that there can be a spatial geometry that comes apart from the one the empirical evidence of bodies in motion would lead us to believe, because of the distorting effects of universal forces. Are such cases possible, on the dynamical spacetime view? (Reichenbach would seem to agree with me that the dynamics and the spacetime structure can come apart; though he thinks that we settle on a geometry by convention.)

(3) Take the historical theories of Lorentz and Newton. I'd like to better understand the dynamical-spacetime position on these. Does Myrvold think that a Galilean spacetime understanding of Newton's laws, and a Minkowski spacetime understanding of Lorentz's ether theory, are the only possible understandings of these two theories and their respective dynamical laws? (For a contrary view, see Bradley (2021).) Does he in particular think that a space-and-time conception of either theory is not possible; so that Lorentz and Newton were wrong about how they understood their own theories (even though we can't blame them for this, given the mathematics at the time)? And so, on this view, the reason we "prefer" Galilean-spacetime (over Newtonian-spacetime) Newtonian mechanics is that there is simply no other option! In particular, this is not due to any minimize-structure principle. Is this right?

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