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What Helium Teaches Us about the Success and Failure of Dynamical Theories

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Abstract

Philosophers have typically explained predictive success through some sort of referential relationship between theoretical terms (entities, properties, structures) and objects of reality. Through a case study of the successes and failures of Bohr's atomic model, in particular as it pertains to helium, I argue that more complicated relations are needed to adequately explain both the successes and failures of a dynamical theory. Bohr's problem was not a matter of "failure of reference", i.e. Bohr's energy levels not lining up with the actual energy levels, but rather a matter of "failure of tracking", which is a matter of incompatibility between the theoretical state assignments and the underlying *transitions* among the states. Unlike reference, which is a *static* notion, tracking is irreducibly *dynamic*: it is a matter of cooperation between how the states are assigned and how they evolve. As a result of this, unlike reference which tends to be *global* and *referentially transparent*, tracking is *local* and *referentially opaque*.

1 Introduction

Bohr's model of atomic structure, first proposed in 1913, was the most successful theory of its kind. It not only explained many known phenomena at the time, but also provided the foundation on which Heisenberg and others built modern quantum mechanics. In particular, it was the specific *failures* of Bohr's theory that directly led to the advent of matrix mechanics.

One of the major episodes in this saga has to do with the element helium. It is well-known that Bohr's atomic theory was successful in dealing with the hydrogen atom and other single-electron systems, but failed to account for helium (e.g. OpenStax 2016, §6.2; Uppal 2006, 426). What is less clear from the historical literature is what exactly was wrong with Bohr's model when it came to helium. In the following, I will argue that the problem cannot be cashed out in terms of *referential success or failure*. Rather the issue is a matter of "failure of tracking". I shall define tracking below in terms of a precise form of compatibility between the theoretical state assignments and the underlying *transitions* among the true states of the system. Unlike referential success, which relies on a *static*, *global*, and *referentially transparent* mapping between the theory and the world, tracking is irreducibly *dynamic*, *local*, and *referentially opaque*.

The general framework of this paper is that of *dynamical systems* theory. In this framework, every theory is cashed out in terms of a set of state assignments and accompanying transition rules among those states. Hamiltonian mechanics, for instance, assigns ordered pairs of position and momentum (x, p) as the state of the system, and encodes

the transition rules in the Hamiltonian function H. Diffusion theory assigns states of the form $(\rho(0,x), \rho(t,0), \rho(t,R))$, where ρ is the density of the diffusing substance and R some fixed distance, and encodes the transition rules in the diffusion function. Kaveh (2021a) argues that this framework can be applied quite generally in physics, but it has certainly become apparent that the dynamical systems framework is appropriate for the study of Bohr's theory. While Norton (2000) provides a strong case, this fact about the Bohr model was known as far back as the 1920s (see, e.g. Van Vleck 1926). In light of this, every use of the word "theory" below must be read as referring to a set of state assignment rules and their accompanying transition rules.

In the following, I will begin with some preliminaries on Bohr's atomic theory and then proceed to examine the issue of its success in the case of single-electron systems and its failure in the case of helium.

2 Preliminaries on Bohr's Atomic Theory

Bohr (1913) posited that an atom is analogous to a mini-planetary system, the nucleus being the "sun" and the electrons orbiting it being the "planets", but with one important restriction: of all the orbits that are classically permitted by Coulomb's inverse square law, the electron must occupy very specific energy levels, namely only those that satisfy the constraint

$$J = nh, \tag{1}$$

where J is the angular momentum of the electron, n is an integer known as the *principal quantum number*, which labels the discrete energy levels, and h is Planck's constant.

Bohr further posited that the electron does not emit radiation unless it "jumps" from one orbit to another, say from the nth orbit to the mth orbit, in which case the difference of energy will be released as radiation of frequency

$$\omega_{nm} = \frac{1}{\hbar}(E_n - E_m). \tag{2}$$

From these two posits, Bohr was able to recover the experimental results pertaining to spectral lines. The derivation is fairly simple: in any standard Kepler problem with the central force $F = -\frac{k}{r^2}$, one can show that

$$E = -\frac{k^2 m}{2J^2}, \tag{3}$$

where E is the energy and m the mass of the orbiting object and $E_\infty := 0$. Bohr's assumption of discrete J therefore leads to

$$E_n = -\frac{R}{n^2}, \tag{4}$$

where E_n is the binding energy of the electron and R is the Rydberg constant. Combining (4) and (2), Bohr was able to derive the Rydberg formula, which summarizes the

experimentally-known pattern of frequencies in the spectral lines of hydrogen:

$$\omega_{nm} = R(\frac{1}{m^2} - \frac{1}{n^2}).$$

While impressively simple, Bohr's derivation of the Rydberg formula is technically not a "novel prediction", given that Bohr constructed his theory to account for the hydrogen spectrum. However, Bohr was also able to make impressive, novel predictions about the spectral lines of once-ionized helium, twice-ionized lithium, and other single-electron ions both under normal conditions and in the presence of external fields. These successes reportedly prompted Einstein to say "the theory of Bohr must then be right" (see Pais 1991, 154)!

As explained above, the difference of energy between any pair of states is connected to empirical data through a spectral line of specific frequency given by equation 2 above. Thus, the successes of Bohr's theory stem from its assignment of discrete energy states to an electron orbiting a nucleus.[1] But what explains the alleged *failure* of Bohr's theory with respect to helium?

[1]See Norton 2000 for a detailed explanation of why the discrete energy states are the only "working posits" of Bohr's theory while the orbits play a heuristic role.

3 What is the Matter with Helium?

Since the success of Bohr's theory stemmed from its state assignments, one might wonder if the widely-reported failure of Bohr's theory in the case of helium was due to the fact that its calculated energy levels did not line up with (or "refer to") the actual energy levels of helium. In the following, I argue that this cannot be the answer and provide my alternative notion of *tracking* as a solution.

3.1 The two cases introduced

To set up the problem, we may compare Bohr's energy levels with the much more accurate ones derived from modern quantum mechanics. The two cases are shown below. Figure 1 illustrates the relationship between Bohr and Schrödinger states for a hydrogen atom.[2] In such contexts, Bohr states line up pretty nicely with Schrödinger states. I say "pretty nicely" because there is a *fine-structure* which, from the contemporary point of view, is due to spin-orbit interactions and relativistic corrections. This results in some finer separation that is not accounted for in Bohr's theory, which causes misalignment between the two sets of states.

Next consider a similar comparison between Bohr and Schrödinger states for an *orthohelium* atom, a neutral helium atom ($Z = 2$) in the spin-1 mode. This is shown in Figure 2 (note that fine structure has been suppressed in this figure).[3] Orthohelium is to be contrasted

[2]For precise values of the energy layers see Nanni 2015, 60.

[3]Figure based on Rohlf 1994, 267, Figure 9-11.

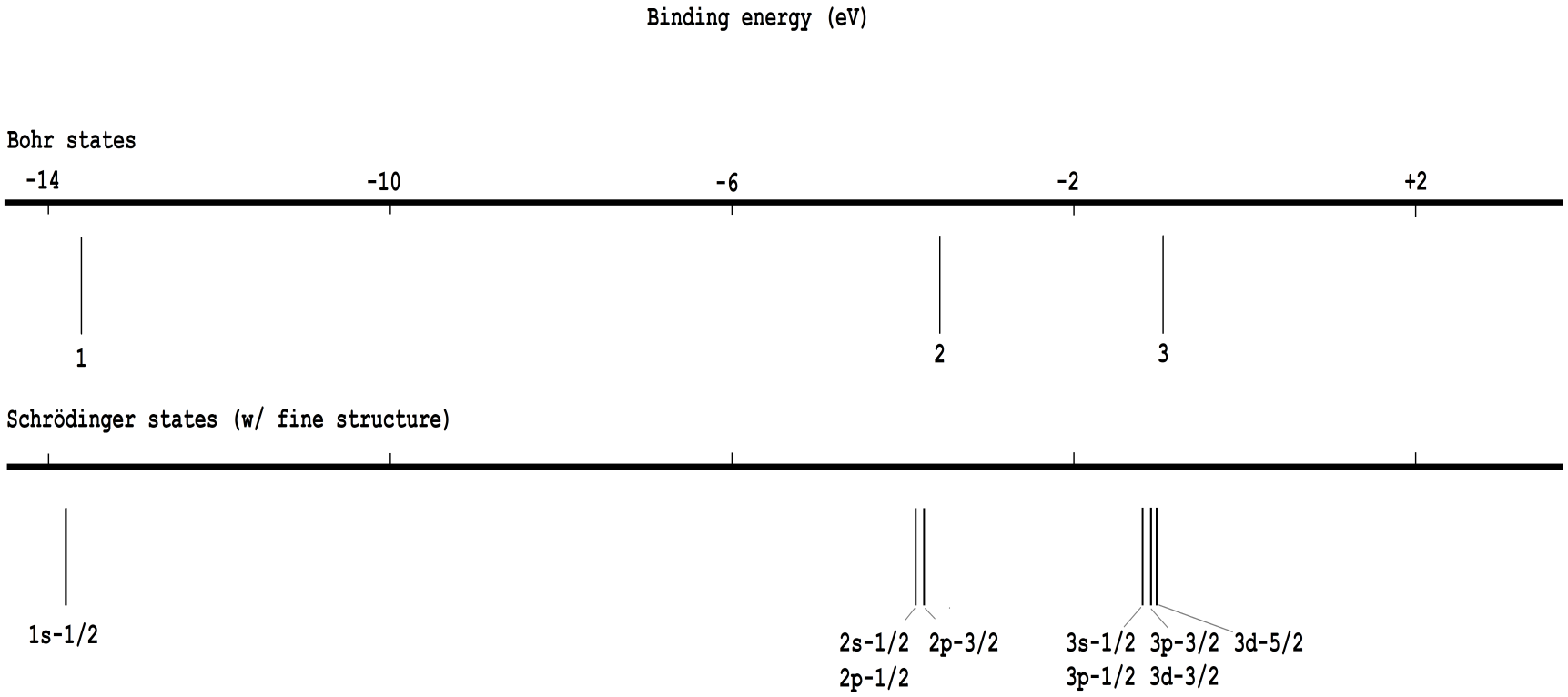

Figure 1. The state assignments of Bohr (top) and Schrödinger (bottom) for the first few layers of the hydrogen atom with exaggerated fine structure separations (not drawn to scale). The Bohr states approximate but do not match the Schrödinger states. Can a notion of "referential success" be defined on this basis?

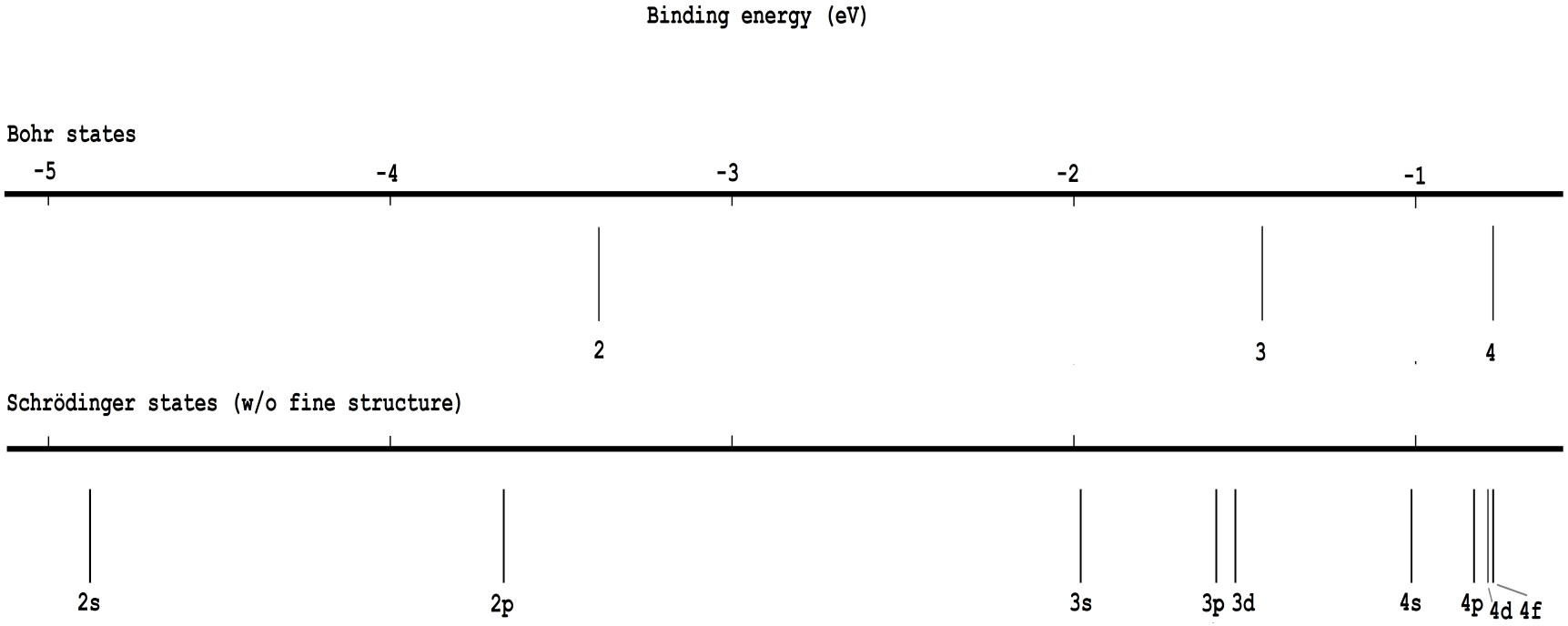

Figure 2. The state assignments of Bohr (top) and Schrödinger (bottom) for the first few layers of the orthohelium atom with fine structure suppressed (drawn to scale). The qualitative relation between the two sets of states in the helium case does not look much different from that of the hydrogen case. Why should we say that Bohr's states "refer" in the latter but not the former?

with *parahelium*, a neutral helium atom in the spin-0 mode. The main difference between the two is that unlike parahelium, the second electron in orthohelium is exiled to $n \geq 2$ and cannot decay to the ground state due to the exclusion principle. The figure for parahelium would therefore look quite similar, except $2s$ would be pushed 0.8 eV higher, and more importantly there would be a $1s$ state. One says that orthohelium's $2p$ is *meta-stable*. I shall differentiate orthohelium and parahelium states by writing subscripts o and p, respectively.

3.2 In search of a difference-maker

The crucial question is this: How is the case of neutral orthohelium different from the case of hydrogen or ionized helium? What difference could explain the fact that one is widely considered a confirmation of Bohr's model but the other a disconfirmation?

The common explanations in the physics literature are not particularly helpful. Sometimes it is said that Bohr's theory fails in the case of helium because it cannot take electron-electron interactions into account (OpenStax 2016, §6.2). This is clearly false: in fact, Bohr discusses such interactions in the second part of his famous trilogy (1913), in which he presented his model for the very first time. Or take Bohr's letter to *Nature* titled "Atomic Structure" in which Bohr explicitly considered electron-electron interactions, including the possibility of orbits penetrating below the lower shells (Bohr 1921; see also Darrigol 1992, 152-153). Other textbooks claim that Bohr's theory fails for multi-electron atoms and ions because in this theory the binding energy only depends on the principal quantum number n whereas experimentally it also depends on the azimuthal quantum number l which indicates angular

momentum, and/or other quantum numbers such as spin that are missing from Bohr's theory (Uppal 2006, 426). But this cannot be the difference-maker either, for two reasons: first of all, one can add further quantum numbers to Bohr's n, and this is indeed what was done by Sommerfeld and others immediately after Bohr's proposal. Secondly, as we saw above, in the case of hydrogen and ionized helium, too, the energy levels depend on angular momentum due to fine structure. There is therefore no meaningful difference between the two cases in terms of lack of further quantum numbers.

Let us see if we can find a meaningful difference between the two cases by examining the two figures. At first glance, the two figures look quite similar. Obviously the Bohr states do not perfectly match up with any of the Schrödinger states in either case. In both cases one sees more spectral lines than expected from Bohr's theory, and one sees them in wrong locations. For instance, a transition such as $3p_o \rightarrow 2s_o$ in orthohelium would release more energy, and thus a higher frequency, than $3s_o \rightarrow 2p_o$. Yet both of those transitions would map onto $3 \rightarrow 2$ for Bohr. This means, in terms of experimental data, that one sees more spectral lines with more (and different) colors than promised by Bohr's model. The same can be said about transitions among the fine structure states.

So, in both cases, Bohr's theory "lumps together" physically distinct states. To be sure, the approximations are quantitatively much better in the case of hydrogen. Indeed, the sub-layer separations in Figure 1 are orders of magnitude smaller than the average of their energy levels, while the separations in Figure 2 are comparable to the average energy values. However, the difference between the two cases could not simply be a quantitative matter. First of all, I have

only shown the states according to the original 1913 theory. If one were to add the azimuthal quantum number k to Bohr's theory (a la Sommerfeld), the quantitative agreement with data would significantly improve. Secondly, as explained in more detail below, quantitative discrepancies in the case of helium were considered resolvable at the time by most experts such as Kramers, Born, and Bohr himself. As late as 1922, a year before it was considered refuted, Born declared the Bohr model "the most probable configuration of helium" (Born 1922, 677). Thirdly and most importantly, as long as both deviations are considered statistically significant, the mere quantitative difference cannot explain why one case counts as success and the other as failure. At the end of the day, if the separations cannot be attributed to experimental error, the theory is strictly speaking false: in both cases Bohr's theory contradicts experimental data in that it has the wrong number of terms and has them at the wrong places. As Kragh explains:

> The observed fine structure could have presented a real problem to Bohr's theory... . However, it was not regarded as an anomaly and not allowed to impede the rapid acceptance of Bohr's theory. (Kragh 1985, 69)

As said above, the discrepancies in helium lines, despite their larger magnitude, were also not regarded as unforgivable anomalies for Bohr's theory. Yet as we shall see, further experiments on helium in the 1910 and 20s were indeed taken as the death knell of Bohr's theory. We are back at the question: why did physicists consider the first one a success and the second a failure?

3.3 Why referential success is not the difference-maker

In the most straightforward sense of the term, Bohr's state assignments *do* represent lumps of underlying states in *both* hydrogen and helium. In the strictest sense, on the other hand, the Bohr states do *not* represent any of the true states *whether for hydrogen or helium*. Thus, it seems, focusing on the static referential / representational relation alone will not give us a rich enough account to explain predictive success or failure of dynamical theories.

But let us not give up so quickly. Suppose there were some quantity in reality that Bohr's states successfully referred to / represented in the case of hydrogen, something that could be said to have generated the theory's success. What would this referent be? The most obvious candidate would be the principal quantum number n, which has been "retained" in subsequent theories as one member of a larger set of quantum numbers. Bohr's theory works as much as it does, the idea goes, because it successfully refers to the principal quantum number. But this victory would be short-lived for the proponent of referential success, because this move would make referential success incapable of explaining the failure of Bohr's theory. For if Bohr states refer to the principal quantum number in one case, they must do so *in both cases*, and of course in both cases n has been "retained" in the superseding theory. Thus, Bohr's theory would be referentially successful in both cases but predictively successful only in one, which means that referential success cannot explain predictive success.

On the flip side, both in the case of fine structure and in the case of meta-stable helium, the main cause of deviation from Bohr's theory (according to current theory) is the omission

of spin. In neither case does Bohr's theory represent spin in any way, yet it works for hydrogen and single-electron ions despite its failure to refer. Once again, referential success / failure cannot differentiate the two cases.

Generally speaking, the only option for the proponent of referential success is to say that, whatever Bohr's states refer to, they are referential in Figure 1 but not in Figure 2. One would have to say, for example, that Bohr's $n = 3$ state refers to $3s_{\frac{1}{2}}$, $3p_{\frac{1}{2}}$, $3p_{\frac{3}{2}}$, $3d_{\frac{3}{2}}$, and $3d_{\frac{5}{2}}$ energy states in a helium ion, but that the same state suddenly refers to *nothing* if the ion acquires a second electron and becomes neutral. This strikes me as a bizarre manner of speaking, but regardless, the question would then be: what would account for the fact that Bohr's states fail to refer in the case of orthohelium? The proponent of referential success must provide an *independent* reason for thinking so, or else this solution is utterly ad hoc. One should be able to determine, without knowing which case was considered a predictive success and which a failure, what (entity, property, or structure) the Bohr states refer to in the case of hydrogen-like systems, and then show that the same entity, property, or structure does not exist in orthohelium, and hence that Bohr's terms are non-referential in that case. As I just argued, this referent most likely cannot be the principal quantum number, for then the theory would be referential in both cases. The proponent of referential success must come up with an alternative referent that exists in hydrogen but is absent from orthohelium. I do not know what that alternative could be, and until one such alternative has been justified, I see little hope for referential success as an explanation of predictive success.

But let us assume one were able to pull off the claim that Bohr's states refer to something

in ionized helium but not in neutral helium. Far from helping the referentialist, this would ironically *weaken* the notion of referential success significantly. For it would then appear that whatever Bohr's theory is representing about hydrogen is a quite volatile and unstable entity (or property or structure or what have you), seeing as it immediately disappears upon the second electron's approach. As such, the success of Bohr's theory will not have been explained by its ability to refer to anything robust in the nature of the atom, but rather something quite flimsy and superficial.

Thus, I am inclined to conclude, whether the theory is successful is not determined by whether it represents anything in the system, at least not anything robust.

3.4 The difference-maker: cooperation with transition rules

As we saw, in both cases Bohr's theory "lumps together" what are in reality distinct states. It defines coarse-grain "equivalence classes" of underlying states, if you will. The question is what makes the coarse-graining acceptable in the case of hydrogen and ionized helium but unacceptable in the case of neutral helium. I claim that the two cases cannot be distinguished *unless one pays attention to the underlying transition rules*. In other words, my claim is that the equivalence classes cannot be justified merely in terms of the form of the vertical *mapping* between Schrödinger states and Bohr states, i.e. the accuracy with which Bohr states "line up against" or "approximate" or "represent" Schrödinger states. To determine which states can belong in the same equivalence class one must also study the manner in which those states *evolve*. Allow me to explain.

When the spectral lines of sources believed to contain helium were first studied, it was realized that upon constructing the "term series" (what Bohr later interpreted as energy levels) for the spectral lines, one obtains two separate sets of terms (energy levels), as it were belonging to two different elements. That is, one finds no transitions "across" the two sets of states. The two elements were given different but similar names. One was referred to simply as "helium" (later "orthohelium") and the other as "parahelium" or "parhelium" (Mehra and Rechenberg 1982, 398). What we now know as 2^3s was at this time considered the *ground state* of (ortho)helium. But soon it was realized that the two "elements" are really two excited states of the same element, now called helium. The experimental findings were thus reinterpreted as indicating a "no combination rule": orthohelium and parahelium states do not transition among themselves. For example, while both $2p_o \rightarrow 2s_o$ and $2p_p \rightarrow 2s_p$ happen quite frequently, there are hardly ever cross-transitions of form $2p_p \rightarrow 2s_o$ and the like. It was accepted that any adequate model of helium must entail the no combination rule.[4] I suggest that the problem with Bohr's theory in the case of helium was precisely that its state assignments were incompatible with the no combination rule.

Bohr and Kramers, building on Landé's work, proposed a model in which helium's two electrons could either occupy coplanar or crossed orbits, and hypothesized that the former corresponds to orthohelium and the latter to parahelium.[5] All seemed fine and good at first.

[4]Recent experimental findings indicate some rare transitions across para- and ortho-states (see NIST Helium Tables), but the fact that these have such extremely low amplitudes still needs to be accommodated in the theory.

[5]Edwin Kemble proposed a similar model in 1921. The model is sometimes called the

Frank and his colleagues believed that they had tested and (somewhat qualitatively) confirmed Bohr's model through a series of experiments on helium ionization (e.g. Franck and Einsporn 1920). To be sure, there were quantitative discrepancies between the experimental values of helium ionization energy and those predicted from the Bohr-style model, but this was not taken as a definitive refutation of Bohr's theory, as the issue was widely considered a matter of finding the right orbital configuration.[6] Bohr himself tried to explain away the quantitative discrepancies by arguing that the ground state of helium is unstable, which according to Bohr results in "indeterminacy" ("Unbestimmtheit") in its energy (Darrigol 1992, 176). Kramers similarly rationalized that the theoretically obtained energies should not be identified with ionization energies to begin with, because the latter depend not just on the final state, but on the *process* through which the second electron arrives at its bound orbit from infinity (Kramers 1923, 340-341).

So what was the real failure of Bohr's model? The answer lies in a set of state assignment rules that Bohr introduced in his 1918 "On the quantum theory of line-spectra" as a generalization and clarification of the old quantization rules $\oint pdq = nh$. One of the fundamental principles that Bohr (1918) drew on to assign stationary states was called the "Principle of Mechanical Transformability"[7]. Without getting into much detail, the principle

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Bohr-Kramers-Kemble model. See Kragh 2012, 253.

[6]See, e.g., Sommerfeld 1923 for an example of attempts at solving the ionization energy problem. See also Van Vleck 1926, 86 ff.; 105-106.

[7]Van Vleck calls it the "Principle of Continuous Formation" (Van Vleck 1926, 98). Bohr introduced this principle having been inspired by Ehrenfest's adiabatic theorem; see Navarro

states that given a quantum mechanically allowed stationary state, all and only those orbits are allowed that can be connected to said state through a continuous, multi-periodic, classical path (see Pérez 2009; Pérez and Valls 2015). This principle in general gives rise to several alternative families of orbits, each family closed under mechanical transformability, such that states in each family can transitions among themselves, but not across other families.

Now, the crucial point is this: Bohr and Kramers originally thought that their models of coplanar and crossed orbits belong to different families prescribed by the Principle of Mechanical Transformability. Since the two sets of states belong to alternative quantization schemes, they could not be realized in the same atom at the same time. At any given moment, the energy levels of the helium atom is quantized in accordance with either one or the other family of state assignments, but not both. Therefore, Bohr and Kramers inferred, we can deduce that orthohelium and parahelium states cannot transition among themselves, which was the desired result. In correspondence with Ehrenfest, Bohr spoke of this result as a crucial advantage of his and Kramers's model over Landé's (Mehra and Rechenberg 1982, 407).

However, trouble came in 1923, when Born and Heisenberg embarked on what Heisenberg described to Bohr as "a general investigation of all mechanically allowed orbits of excited helium". As Heisenberg explained, "if in the end the experimentally found terms are not included, then one knows that the mechanics is wrong." (Heisenberg to Bohr 2 Feb. 1923, quoted in Kragh 2012, 255) The results were devastating: in particular, Born and Heisenberg showed that the solution space is such that the principle of mechanical transformability cannot

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and Pérez 2006 for a discussion of the genesis of Ehrenfest's theorem.

be applied without serious trouble: insofar as stable, multi-periodic solutions are available, they are all mechanically transformable into each other (Born and Heisenberg 1923, 240). And since Born and Heisenberg also show that the only permissible solutions are coplanar and crossed orbits, this meant that the orthohelium and parahelium states of Bohr and Kramers belonged in the same family of orbits after all. As such, they are *realizable in one and the same system*, contrary to what Bohr and Kramers expected. Being realized alongside each other in the same atom means that analogous orbits, such as $2p_o$ and $2p_p$, would be in the same dynamical equivalence class (they would be "lumped together"), which in turn means that they must transition indistinguishably. Therefore, it turned out, Bohr's model could not explain the experimental fact that the orthohelium states never mix with the parahelium states. *This*, it seems, is what brought Bohr's atomic theory to its knees, as it appeared that Bohr's very method of state assignment was doomed. Bohr was the first to declare defeat:

> Born and Heisenberg's investigation may be particularly well suited to provide evidence of the fundamental failure of the laws of mechanics in describing the finer details of the motion of systems with several electrons. (Bohr 1923, 271)[8]

Thus, the mathematical algorithm through which Bohr's 1918 state assignments are prescribed is *incompatible with the underlying dynamics* of the system. This is a *qualitative* (as opposed to *quantitative*) failure for Bohr's theory. In the following, I will refer to this as a failure of *tracking*.

[8]This is not to deny that the Bohr-Sommerfeld theory faced other problems around the same time, not the least of which was the anomalous Zeeman effect (Kragh 2012, 314 ff.).

4 Properties of the Tracking Relation

In the following, I will present a generalized and formalized account of what I have been calling "(failure of) tracking", and proceed to discuss some of the main properties of the tracking relation that set it apart from referential success (failure).

4.1 Tracking defined

If the analysis above is correct, the difference-maker between the two cases is that the state assignments for helium lump together groups of underlying states that transition distinguishably according to the very same way of lumping them. Note the contrast with the case of hydrogen. The various hydrogen states that Bohr's theory lumps together are relatively well-behaved: for the most part, states that Bohr's theory lumps together transition to other states that the theory also recognizes as the same. The only exception to this is the $3 \rightarrow 1$ transition, which is only allowed for $3p \rightarrow 1s$ (suppressing fine structure) but not for the other $n = 3$ states. However, this would only be a problem for Bohr's theory if i) one were not allowed to add the azimuthal quantum number, and ii) one were able to prepare $3p$ separately from the other $n = 3$ states. Since neither condition was true at the time, the coarse-graining of various hydrogen states mostly respects the way the underlying states transition.

When states within the same theoretical equivalence class cannot be shown to transition distinguishably, I will say that the theoritcal states *track* the true states of the system within the constraints and resolution limits of the context. Kaveh (2021b) provides a detailed and formal

explanation of the notion of tracking and its properties, along with mathematical demonstrations of some of its applications. In the following, I will summarize some of these properties in largely informal terms.

> *Tracking:* A set of states S *tracks* another set of states S' iff there is a mapping from S' to S (inducing equivalence classes on S') such that members of each equivalence class defined on S' transition in a manner that is indistinguishable according to the same state assignment rules.

If, on the other hand, there are several states that the theory recognizes as being in the same equivalence class, but when one prepares them separately, each regularly transitions to final states that the theory itself recognizes as distinct, we say that the theory *fails to track*. This indicates that the initial states should not have been put in the same equivalence class, and therefore that the theory's state assignments are faulty.

Note that while tracking is defined as a relation between two dynamical theories (such as the Bohr and Schrödinger theories above), it can also be used as a relationship between a dynamical theory and fundamental *reality*. In such cases, we are assuming that the reality of the system also consists of certain states (the "true states") that form a certain dynamical system. If there is any discomfort about applying the notion of tracking in this way, we can simply posit a "true theory" which corresponds to the reality of the system in a perfect one-to-one manner, and consider the tracking relation as a relation between our theory of interest and the true theory. In this way, tracking is a genuine alternative to reference, which is

also a relationship between theoretical terms and the true objects of reality.

4.2 Tracking is referentially opaque

One might still be tempted to say that tracking is a form of referential success. After all, does tracking not imply that the higher-level states refer to those lower-level states that transition indistinguishably? In other words, are the theoretical terms not picking out certain natural groupings of the underlying states that evolve similarly? The answer is no.

First of all, while reference is a two-place relation, tracking is a three-place one. Reference takes a theoretical term and a true term, and returns either "yes" or "no" (corresponding to "refers" or "does not refer", respectively). Tracking, on the other hand, needs three inputs: a set of theoretical states, a set of true states, and a set of transition rules among the true states, in order to return "yes" or "no" (corresponding to "tracks" or "does not track", respectively).

Since tracking depends on the *interaction* between the underlying transitions and the mapping between the two sets of states, it cannot be reduced to any static or atomistic referential relationship. As such, there is nothing *natural* or *inherent* about the groupings, because the equivalence classes do not share any property that could be described purely at the level of truth and without reference to the higher-level theory. To see this, first note that successful state assignments are not unique. Consider a deterministic system with four true states $S_1, ..., S_4$ and let the transition rules be $S_1 \rightarrow S_3$ and $S_2 \rightarrow S_4$, and suppose the characteristic magnitudes of the two transitions are roughly equal. Now consider the following three ways of mapping ("lumping") the underlying states into equivalence classes of

theoretical states:

1) Assign $\{S_1, S_2\} \leftrightarrow S_T$, $\{S_3, S_4\} \leftrightarrow S'_T$

2) Assign $\{S_1, S_2\} \leftrightarrow S_T$, $\{S_3\} \leftrightarrow S'_T$, $\{S_4\} \leftrightarrow S''_T$

3) Assign $\{S_1\} \leftrightarrow S_T$, $\{S_2\} \leftrightarrow S'_T$, $\{S_3\} \leftrightarrow S''_T$, $\{S_4\} \leftrightarrow S'''_T$

It is easy to verify that the coarse-grainings in (1) and (3) result in a set of state assignments that track (co-transition), whereas the ones in (2) fail to track (co-transition). Now compare the assignment $\{S_1, S_2\} \leftrightarrow S_T$ in (1) to the same assignment in (2). Both assignments lump the same underlying states into the same equivalence class, yet one is successful and the other is not. Clearly, the success of (1) is not due to the fact that S_T "picks out a real property" of the system, or else the same assignment would not fail in (2). The proponent of referential success might try to attribute this to the fact that (2) fails to lump S_3 and S_4 together. Perhaps by failing to put the latter two states in the same equivalence class, (2) fails to represent an important property that they share and thus fails to predict. In other words, perhaps (2) is being too fine-grained for its level. But this cannot be right, because the same fine-grained assignment works perfectly well in (3). In short, the success and failure of state assignments cannot be decided in isolation. It is *the entire set of state assignments* that either tracks or fails to track *as a whole*. Tracking is *holistic*.

Thus, "the common attribute", if there is any, that is supposedly shared by all the states in the same class cannot be cashed out in terms of any properties that reside entirely at the level of true states. For this would require describing an inherent, theory-independent property that

S_1 and S_2 have in common, but no such property is forthcoming: without reference to the theoretical equivalence classes, there is no natural description of S_1 and S_2 that unites them together. In short, predictive success is not a matter of each "higher-level" state picking out an inherent property of some "lower-level" states, but a matter of how the entire set of coarse-grained state assignments cooperates with the dynamics. Tracking is *irreducibly theory-dependent*.

Similarly, "transitioning indistinguishably" cannot be cashed out without reference to the system's constraints and the observer's resolution limits. For instance, non-relativistic spin states are only well-defined for particles constrained to small velocities. Or consider how the Schrödinger theory only tracks the true states if one ignores hyper-fine structure, which is due to quantum field theory corrections. Nature does not recognize these "lumps"; they only make sense for an observer that has accepted particular constraints and resolution limit for their current purposes. Tracking is *context-dependent*.

Because of properties such as being holistic, theory-dependent, and context-dependent, tracking is *referentially opaque*. That is, simply because a set of theoretical states tracks the true states does not mean we can *read off* any part of the ontology of reality from any possible formulation of the theory.

What about structural representation? Can we not say that Bohr's theory represents part of the *structure* of the underlying dynamics? This might seem promising at first, because one of the many definitions of "structure" is *relational properties*, and it might seem that while tracking is not about individual theoretical states representing specific entities or properties, it

does have to do with the relations among the theoretical states "mirroring" the relations among the underlying states.

My response to this line of reasoning is to say, first of all, that I would not be surprised if out of the plethora of ways "structure" has been cashed out, some of them would fit the bill here. Once a concept is so flexible as "structure" is in philosophy of science, its successful application is hardly impressive. That being said, I am not sure that even the "relational properties" view of structure would apply here, because the only "relations" at play here are the transition rules. That is, one must define a relation that obtains between two states when one transitions to the other. But what about all the other relational properties that one could define among the underlying states? Take for instance the most obvious one: the relative differences in their characteristic magnitudes (e.g. difference of binding energy between various atomic states). As we have seen, Bohr's theory does not represent the "structure" of these relations, unless one abstracts a coarse-grained structure from it, which can only be defined by taking the transitions into account.

But there is an even bigger difference between tracking and structural similarity: the latter is *referentially transparent* whereas as argued above, the former is *referentially opaque*. A structural realist maintains that the structure of the theory *reflects* or *mirrors* that of (part of) reality, which would allow one to read back the structure of reality from the theory. Tracking, on the other hand, does not imply any sort of mirroring relationship between theory and reality.

4.3 Tracking is local

I argued above that one feature of tracking is context-dependence. Among other things, context-dependence also means that tracking is *local*. A dynamical theory tracks the true states only within a limited domain specified by concrete constraints and resolution limits. The constraints effectively pick out a subspace of the total state space within which the system is allowed to roam, and resolution limits blur the differences between different states within neighborhoods of a certain size. In the case of Bohr's theory, the constraint is that there can only be one electron in the system, and the resolution limit is that one ignores fine structure differences in energy due to spin. As such, constraints and resolution limits are essential to delineating the appropriate locality of a tracking relation.

That tracking is local is an advantage of my view, not a weakness, because success itself is clearly local: even our most successful theories have a domain beyond which they break down. Other explanations of success, such as reference and structural similarity, have a harder time explaining why the success of a given theory must be confined to a certain domain. For instance, if the theoretical terms successfully refer to objects of reality, then presumably they do so in all contexts, unless the object in question is an "emergent" entity which only exists in certain localities. Similarly, if the theory captures a true structure of nature, then it must do so in all contexts, unless the structure in question is an "effective" one that only emerges locally. Neither option seems desirable: on the one hand, claiming that the theory reflects reality in the same way in all contexts would raise the question of why the theory's success is limited to

certain contexts; on the other hand, claiming that the referents of the theory are only "emergent" or "effective" would concede to the antirealist that successful theories have no obvious connection to *fundamental* reality.

The locality of tracking means that even with a single pair of "higher-level" and "lower-level" theories, one might need to explain the success of the "higher-level" theory differently in different contexts. In other words, a given theory might manage to track another theory under multiple sets of constraints and resolution limits. The proof of the tracking relation would then have to be carried out separately for each case, for the equivalence classes that the "higher-level" theory imposes on the "lower-level" states would be different in each case. For instance, one can show that the discontinuous spin states of non-relativistic quantum mechanics track Dirac's fermionic states under a specific set of constraints and resolution limits. One can also show that the *continuous* states of non-relativistic quantum mechanics such as position, etc. also track Dirac's fermion states. But the derivations in the continuous and discontinuous cases will be different. Thus, there is no single way in which non-relativistic quantum mechanics tracks relativistic quantum mechanics; the relationship must be examined context by context (cf. Rosaler 2015, 56).

Conclusion:

Philosophers of science have typically explained predictive success through some sort of referential relationship between theoretical terms (entities, properties, structures) and objects of reality. The case study above suggests that more complicated relations are needed to adequately explain both the successes and failures of a dynamical theory. In particular, a

dynamical theory fails in its predictions if it fails to *track* the underlying states of the system within an appropriate locality. If I am right, the problem in these cases cannot be cashed out in terms of the "mapping" between theoretical states and true states; the problem rather lies in the way this mapping interacts with the inherent transition rules of the system. I have argued that unlike referential success, which is a *static*, *referentially transparent*, and *global* relationship between theory and reality, tracking is *dynamic*, *referentially opaque*, and *local*.

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