Published in (2009) "Explanatory Fictions" in *Fictions in Science: Philosophical Essays on Modeling and Idealization*, M. Suárez (ed.), Routledge, pp. 91-109.

Explanatory Fictions

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> "They [semiclassical theorists] do not describe an electron as a point particle moving in real time along a classical orbit. Nevertheless, the fiction can be productive, permitting one to talk about classical orbits *as if* they were real." (Kleppner and Delos 2001, p. 606)

1. Introduction

The above epigraph sounds like it could have been taken from a page of Hans Vaihinger's ([1911] 1952) book on fictionalism, *The Philosophy of 'As If'*.[1] Instead , however, it comes from a recent article in a physics journal, coauthored by an experimental physicist at MIT (Dan Kleppner) and a theoretical physicist at the College of William and Mary (John Delos). Like Vaihinger, these physicists are defending the view that some fictions have a legitimate role to play in science, and also like Vaihinger they are defending these fictions on pragmatic grounds. Kleppner and Delos are concerned specifically with an area of research known as semiclassical mechanics, and the remarkable fertility of using fictional classical electron orbits to describe the quantum spectra of atoms placed in strong external fields. A striking feature of this research is that these fictional orbits are not simply functioning as calculational devices, but also seem to be playing a central role in the received scientific explanation of these phenomena. While there is a growing recognition that fictions have some legitimate role to play in scientific theorizing, one function that is traditionally denied to fictions is that they can *explain*. Even Vaihinger, who argues for the pervasiveness of fictions in science, rejects the view that there are explanatory fictions.

In what follows, I defend the view that, in some cases, fictions can genuinely explain. I begin, in Section 2, by situating my approach to fictions in the context of Vaihinger's classic account, then, in Section 3, turn to the concrete case of fictional classical orbits explaining quantum spectra in modern semiclassical mechanics. In Section 4, I introduce a new philosophical account of scientific explanation that I argue is capable of making sense of the explanatory power of fictions. Finally, I conclude in Section 5 by relating this case study and the new philosophical account of scientific explanation back to Vaihinger's account of fictions.

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[1] The revival of interest in Vaihinger's account of fictions is largely owed to Arthur Fine (1993), reprinted in this volume.

2. Beyond Vaihinger's Account of Fictions

In his comprehensive account of fictions in human thought, Vaihinger identifies four key features that he takes to characterize fictions.[2] The first and most salient characteristic of fictions is their deviation from reality. That is, fictions involve what he calls a "contradiction" with reality. He introduces an elaborate taxonomy of different kinds of fictions based on the various ways in which this deviation from reality can take place (e.g., neglective fictions, heuristic fictions, and abstract generalizations) as well as based on the various subject matters in which fictions can be employed (e.g., juristic fictions, ethical fictions, mathematical fictions and scientific fictions). In defining a fiction as almost any deviation from reality, however, Vaihinger's account does not clearly distinguish among discarded theories, models, idealizations, abstractions, and what we might today more narrowly call fictions. While there may be no hard and fast distinctions between these various things that Vaihinger groups together as 'fictions,' when it comes to assessing their epistemic status, such distinctions might nonetheless turn out to be important. So, for example, although phlogiston and frictionless planes are both "fictions" on Vaihinger's account, we might want to say that the latter is an idealization that can be subject to something like what Ernan McMullin (1985) calls a de-idealization analysis. Hence, there is an important sense in which reasoning based on frictionless planes has a different epistemic status and involves a different sort of justification than reasoning on the basis of a genuinely fictional entity of a discarded theory, such as in the case of phlogiston.

The second essential characteristic of fictions that Vaihinger identifies is that they are ultimately to be eliminated. He writes, "the fiction is a mere auxiliary construct, a circuitous approach, a scaffolding afterwards to be demolished" (Vaihinger [1911] 1952, p. 88). Elsewhere he notes that a fiction "only falsifies reality with the object of discovering the truth" (Vaihinger [1911] 1952, p. 80). That is, the proper aim and function of fictions is to prepare the road to truth—not be a permanent stand in for truth. One might have assumed that a recognition of the pervasiveness of fictions in science would lead Vaihinger to embrace some form of antirealism, such as instrumentalism. This second condition that he places on fictions, however, reveals that this is not the case; Vaihinger's view is in many respects closer to a form of scientific realism. Indeed he is quite optimistic that as our experience becomes richer and our scientific methods refined, these various fictions can and will be eliminated.

There are two important questions to separate here. The first question is whether fictions are eliminable from scientific practice. One might reasonably argue that all scientific representation involves some sort of idealization, abstraction, or fictionalization of the target system. This is not to say, of course, that scientists cannot recognize some scientific models as being more or less idealized or fictionalized than others. Hence, pace Vaihinger, fictions are not ultimately to be eliminated, but rather they are a permanent feature of science. There is, however, another possible interpretation of Vaihinger here, and that is that he did not mean that all fictions are eliminable from science, but only that

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[2] More precisely these four characteristics together define what Vaihinger calls "scientific semi-fictions"—the type of fiction most relevant to the discussion here.

any *given* fiction will someday be eliminated. While I think this view is more defensible, it is not clear that even this more restricted claim will always be the case. As I shall suggest below, there may be some fictions in science that, because of their great utility and fertility, scientists may decide to always keep "on the books," even though there may be less fictionalized hypotheses available for that same phenomenon.

Even if we grant that fictions and idealizations are a pervasive and permanent feature of science, it is a second, distinct question, whether such an ineliminability of fictions necessarily undermines scientific realism. In other words, just because a scientific theory or model involves a fictionalization, does that mean that it cannot give us genuine insight into the way the world is? Although I cannot defend these theses in generality here, my own view is that the answer to both of these questions is no: all scientific representation *does* involve some idealization or fictionalization, and this does not in and of itself render scientific realism untenable.

The third key characteristic of fictions that Vaihinger identifies is that there should be an "express awareness that the fiction is just a fiction" (p. 98). That is, when scientists deploy a fiction, they do so knowing full well that it is a false representation. I think this is actually a very import feature of scientific fictions. Much of our discomfort with the idea that there are fictions in science stems from our concern that fictions will necessarily lead scientists astray and render science subjective and arbitrary. As I think Vaihinger rightly recognized, however, this need not be the case. As long as scientists deploy a fiction with the full knowledge that it is just a fiction, then it is much less likely that they will be misled by it. Furthermore, the recognition that some concept is a fiction, need not make science subjective at all; indeed the fiction being deployed can be objectively recognized as a fiction by the scientific community as a whole. Finally the use of fictions also need not render science arbitrary. As Vaihinger repeatedly emphasizes, scientific fictions are constrained by their utility and expediency.

Vaihinger makes a very helpful distinction between a fiction and an hypothesis. With an hypothesis, the scientist is not yet sure whether it is an accurate representation of the object, system or process of interest. As Vaihinger explains "An hypotheses is directed toward reality, i.e. the ideational construct contained in it claims, or hopes, to coincide with some perception in the future. It submits its reality to the test and demands *verification*" (p. 85; emphasis original). By contrast, fictions make no claim to truth; rather than being subject to verification, Vaihinger argues they should only be subject to *justification*.[3] That is, fictions are to be judged by their utility and expediency.

This emphasis on the pragmatic function of fictions becomes Vaihinger's fourth key characteristic, which separates out those fictions that deserve the label "scientific" from those that do not. He writes,

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[3] Although Vaihinger sees a clear logical distinction between fiction and hypotheses, as well as noting that they lay out very different methodologies, he does recognize that in the actual history of science this distinction may be difficult to draw. There may be cases, for example, where the scientist making the assertion is unsure of whether it is to be properly thought of as an hypothesis or fiction when it is first introduced. In such a case Vaihinger notes that methodologically it is best to assume that it is a hypothesis, and in that way not block the road to verification by prematurely declaring it a fiction.

> Where there is no expediency the fiction is unscientific. . . . [Hume's] idea of the 'fiction of thought' was that of a merely subjective fancy, while ours . . . includes the idea of its utility. This is really the kernel of our position, which distinguishes it fundamentally from previous views. (p. 99)

Fictions have a legitimate role to play in science in so far as they are useful in furthering the aims and goals of science. Although Vaihinger does not spell this out in any great detail, his approach does suggest that there may be a variety of ways in which fictions can be pragmatically useful. For example, some fictions may be useful as proto-theories, other fictions useful as calculational devices, and still other fictions useful in generating predictions.

One function that Vaihinger clearly denies to fictions, however, is that they can explain. Drawing on his distinction between hypotheses and fictions Vaihinger writes, "The hypothesis results in real explanation, the fiction induces only an illusion of understanding" (p. *xv*). The reason, he explains, is that "[E]very fiction has, strictly speaking, only a practical object in science, for it does not create real knowledge" (p. 88). In other words, explanation and understanding are not to be counted among the ends of science for which fictions can be expedient, precisely because explanation requires having genuine insight into the way the world is, and fictions are incapable of giving us this sort of insight. So, for example, although Descartes vortex model of the solar system might make us feel like we have understood why all the planets move in the same direction around the sun, this understanding is illusory, and no genuine explanation of planets' motion has been given.

The view that Vaihinger is expressing here regarding the explanatory impotence of fictions is a wide-spread and intuitively plausible one. Nonetheless, I think it is mistaken. While it is certainly not the case that all fictions can explain, I believe that some fictions can give us genuine insight into the way the world is, hence be genuinely explanatory and yield real understanding. I shall call this (proper) subset of fictions *explanatory fictions*, and distinguish it from what we might call *mere* fictions. In the next section I shall show that it is just such an explanatory fiction that Kleppner and Delos (2001) are calling attention to in the quotation given as the epigraph of this paper.

3. The Case of Classical Orbits and Quantum Spectra

Although it was known from the early 19th century that different elements (such as hydrogen, helium, sodium) can absorb and emit light only at a specific set of frequencies yielding a "signature" line spectra, the first successful explanation of these spectral lines for the simplest element, hydrogen, did not occur until Niels Bohr introduced his planetary model of the atom in a trilogy of papers published in 1913. Bohr proposed that the atom consists of a dense nucleus, where most of the mass of the atom is concentrated, and the electrons orbit this nucleus in a discrete series of allowed concentric rings, known as stationary states. The state is "stationary" in the sense that when the electron is traveling along one of these orbits its energy does not change. Instead, the atom can only gain (or lose) energy when the electron jumps from one allowed orbit to another. Bohr was able to show for the hydrogen atom that each spectral line corresponds to a particular jump of the electron from one allowed orbit to another.

Moreover, Bohr proposed in his famous "correspondence principle" that *which* quantum jumps between stationary states were allowed was determined by the nature of the classical motion of the electron along the relevant orbital trajectory.[4]

When Bohr first proposed his model of the atom, he introduced the idea that electrons in atoms follow classical trajectories as a *hypothesis* in Vaihinger's sense. In other words, he took this to potentially be a literal description of the behavior of electrons in an atom. Despite the remarkable successes of Bohr's model in explaining the hydrogen spectrum as well as many other quantum phenomena, by the end of that decade there was mounting empirical evidence that this hypothesis that electrons in atoms are following definite trajectories was problematic. Not only did Bohr's old quantum theory have difficulty in calculating the energies of more complicated elements such as helium, but it also seemed unable to account for the fact that when atoms are placed in a magnetic field, the individual spectral lines are split into a complex multiplet of lines known as the "anomalous Zeeman effect." Right before the overthrow of Bohr's old quantum theory, and its replacement by modern quantum mechanics, Wolfgang Pauli wrote,

> How deep the failure of known theoretical principles is, appears most clearly in the multiplet structure of spectra. . . . One cannot do justice to the simplicity of these regularities within the framework of the usual principles of the [old] quantum theory. It even seems that one must renounce the practice of attributing to the electrons in the stationary states trajectories that are uniquely defined in the sense of ordinary kinematics. (Pauli [1925] 1926, p. 167; quoted in Darrigol 1992, pp. 181-182)

However, even before the rejection and replacement of Bohr's model of the atom by the new quantum theory in 1925, the idea that electrons are following definite trajectories in atoms had already begun to be transformed by the scientific community from a hypothesis to a useful *fiction*. For example, in a 1920 article published in *Nature*, Norman Campbell wrote of Bohr's model of the atom,

> Nor is it [the assumption that electrons are *not* moving] physically impossible if we accept Bohr's principle of 'correspondence,' which has been so astoundingly successful in explaining the Stark effect [splitting of spectral lines in an electric field] and in predicting the number of components in lines of the hydrogen and helium spectra. According to that principle, the intensity and polarisation of components can be predicted by the application of classical dynamics to certain assumed orbits, although it must be assumed at the same time that the electrons are *not* moving in those orbits. If intensity and polarisation can be predicted from orbits that are wholly fictitious, why not energy? (Campbell 1920, p. 408)

Campbell here is defending the practical utility of the idea of electrons moving in classical trajectories in atoms, even though he believes that these trajectories must be regarded as "wholly fictitious."

Even Bohr, as early as 1919, seemed no longer to view the motion of electrons in concentric stationary states as a literal description. Indeed he wrote to a colleague that

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[4] For an explication of Bohr's much misunderstood correspondence principle, see Bokulich (2008a), Chapter 4, Section 2.

year “I am quite prepared, or rather more than prepared, to give up all ideas of electronic arrangements in rings” (Bohr to O.W. Richardson, 25 December 1919; quoted in Heilbron 1967, p. 478). It is interesting, however, that he rejected Campbell’s label of “wholly fictitious” as a correct description of the status of these classical electron orbits. In a reply to Campbell also published in *Nature* Bohr writes,

> I naturally agree that the principle of correspondence, like all other notions of the [old] quantum theory, is of a somewhat formal character. But, on the other hand, the fact that it has been possible to establish an intimate connection between the spectrum emitted by an atomic system, deduced . . . on the assumption of a certain type of motion of the particles in the atom . . . appears to me to afford an argument in favour of the reality of the assumptions of the spectral theory of a kind scarcely compatible with Dr. Campbell’s suggestions. (Bohr 1921, pp. 1-2)

In other words, although Bohr takes the classical electron orbits to be only a “formal description,” he does think that they nonetheless give real insight into the structure and behavior of atoms, and hence are not properly thought of as *wholly* fictional.

With the advent of the new quantum theory, the idea that electrons are actually following definite classical trajectories in atoms would be entirely eliminated. Indeed Heisenberg’s uncertainty principle, introduced in 1927, would show that quantum particles, such as electrons, cannot have a precise position and a precise momentum at the same time, as would be required by the classical notion of a trajectory. Rather than having either a static or moving position, the electron is now more properly thought of as a cloud of probability density around the nucleus of the atom. Surprisingly, however, the introduction of modern quantum mechanics did not in fact mark the end of this history of describing electrons in atoms as following definite classical trajectories. Ironically it was the discovery of a new generation of “anomalous” spectral data in the late 1960s that would lead to the reintroduction of the notion of classical electron trajectories in atoms—though this time with the express recognition that these classical trajectories were nothing more than useful fictions.

Although the behavior of ordinary atoms in relatively weak external magnetic and electric fields is well-understood, when one examines the behavior of highly excited atoms (known as Rydberg atoms), in very strong external fields surprising new phenomena occur.[5] In a series of experiments beginning in 1969, William Garton and

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[5] The spectroscopic data and Zeeman effects that most of us are familiar with (including the anomalous and Paschen-Bach effects), take place in the regime in which the external magnetic field is relatively weak compared with the electrostatic Coulomb field of the atom. If, however, the magnetic field strength is increased so that it is comparable to the Coulomb field, then a diversity of new phenomena occur, collectively known as the quadratic Zeeman effect. The quadratic Zeeman effect is so named because the Hamiltonian of an atom such as hydrogen in a magnetic field has two terms involving the magnetic field, B: one that is linear in B and one quadratic in B (that is, it has B^2). For a sufficiently weak magnetic field, one can ignore the quadratic term in the Hamiltonian and only the linear term is important; if however the magnetic field is very strong, then the quadratic term cannot be neglected. Atoms in strong magnetic fields are often referred to as diamagnetic atoms.

Frank Tomkins at the Argonne National Laboratory examined the spectra of highly excited barium atoms in a strong magnetic field. When the magnetic field was off, these Rydberg atoms behaved as expected: as the energy of the photons being used to excite the atom increased, there were a series of peaks at the energies which the barium atom could absorb the photons; and when the ionization energy was reached (that is, the energy at which the outer electron is torn off leaving a positive ion), there were no more peaks in the absorption spectrum, corresponding to the fact that the barium atom could no longer absorb any photons. However, when they applied a strong magnetic field to these barium atoms and repeated this procedure, a surprising phenomenon occurred: the barium atoms continued to yield absorption peaks long after the ionization energy had been reached and passed (see Fig. 1).

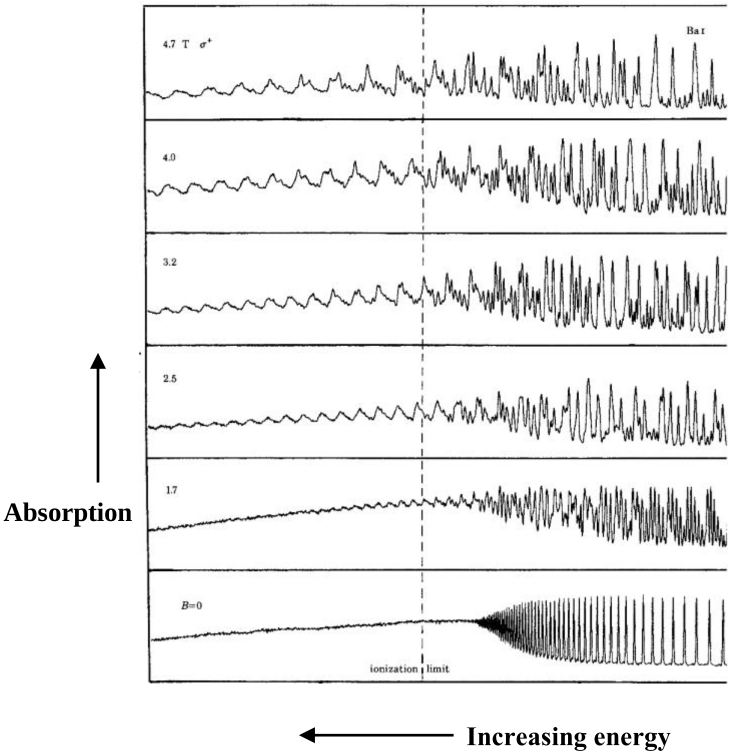

Figure 1: The absorption spectrum of barium. Higher energies are to the left, and the vertical dashed line is the ionization threshold, energies above which the barium atom ionizes. The bottom row is the spectrum with no magnetic field (B=0), and the subsequent rows above that are the spectra with a magnetic field at 1.7, 2.5, 3.2, 4.0, 4.7 Tesla, respectively. Note the surprising oscillations in the absorption spectrum above the ionization limit when a strong magnetic field is present. (Adapted from Lu et al. 1978, Figure 1).

These oscillations in the spectrum were later named "quasi-Landau" resonances, and were shown to have a spacing independent of the particular type of atom. Remarkably, even almost twenty years after these quasi-Landau resonances were first discovered, a full theoretical explanation of them remained an outstanding problem.

The situation was further exacerbated by the fact that experimentalists were continuing to find new resonances above the ionization limit. For example, higher resolution experiments on a hydrogen atom in a strong magnetic field, performed by Karl Welge's group in Bielefeld in the mid-1980s, revealed many more types of resonances in the absorption spectrum (Main et al. 1986; Holle et al. 1988).[6] Furthermore, these new resonances seemed to have lost the regularity of the quasi-Landau resonances discovered earlier. Instead, this new high resolution spectral data exhibited a complex irregular pattern of lines. By the end of the 1980s, Kleppner and his colleagues write,

> A Rydberg atom in a strong magnetic field challenges quantum mechanics because it is one of the simplest experimentally realizable systems for which there is no general solution. . . . We believe that an explanation of these long-lived resonances poses a critical test for atomic theory and must be part of any comprehensive explanation of the connection between quantum mechanics and classical chaos." (Welch et al. 1989, p. 1975)

Once again the Zeeman effect was yielding anomalous spectra, whose explanation seemed to require the development of a new theoretical framework. Although modern quantum mechanics was never in doubt, the mesoscopic nature of Rydberg atoms suggested that an adequate theoretical explanation of these resonance phenomena would require not only quantum mechanics, but concepts from classical chaos as well.

An important step towards explaining these resonances was made by the Bielefeld group in a subsequent paper. They realized that if one takes the Fourier transform of the complex and irregular looking spectra, an orderly set of strong peaks emerges in the time domain.

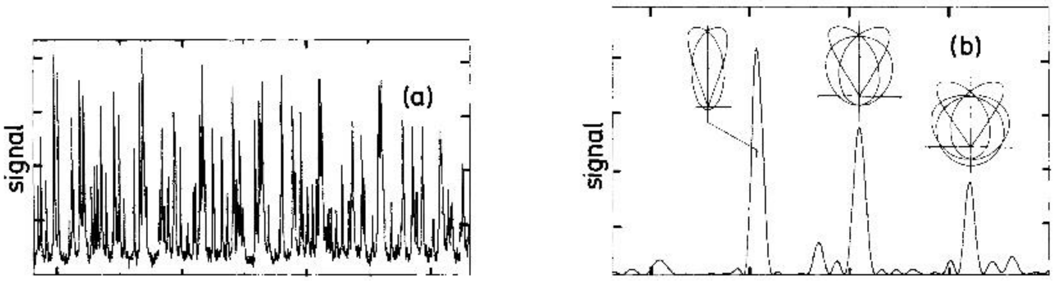

Figure 2: (a) The irregular looking scaled energy spectrum for a hydrogen atom near the ionization limit in a strong magnetic field (b) The Fourier transform of this same spectrum into the time domain. The particular classical closed orbit corresponding to each of these well-defined peaks is superimposed. (Holle et al. 1988, Fig. 1; courtesy of J. Main)

This resulting "recurrence spectrum" revealed that the positions of these peaks in the time domain were precisely at the periods, or transit times, of the classically allowed closed orbits for the electron moving in the combined Coulomb and magnetic fields; that is, each peak in the quantum spectrum corresponds to a different closed classical trajectory. They write,

> Though those experiments [by Welge's group in the 1980s] suggested the existence of even more resonances their structure and significance remained fully obscure. In this work we have discovered the resonances to form a series of strikingly simple and regular organization, not previously anticipated or predicted.

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[6] Even these higher resolution experiments were still of finite resolution, not resolving individual energy levels. What Figure 2 below shows is the average absorption curve as a function of energy.

> . . . *The regular type resonances can be physically rationalized and explained by classical periodic orbits of the electron on closed trajectories* starting at and returning to the proton as origin with an orbital recurrence-time T characteristic for each v-type resonance. (Main et al. 1986, pp. 2789-2790; emphasis added)[7]

Note that the explanation being offered for these anomalous resonances and their regular organization makes explicit appeal to the fictional assumption that these Rydberg electrons, instead of behaving quantum mechanically, are following definite classical trajectories.

The appeal to fictional classical trajectories in explaining these quantum spectra is not an isolated mis-statement by Karl Welge and his colleagues; rather it became the foundation for the received scientific explanation developed two years later by Delos and his student Meng-Li Du (Delos and Du 1988; Du and Delos 1988). The correspondence between each of the positions of these peaks in the spectrum and the transit times of the electron on a particular classical closed trajectory, suggested that a semiclassical approach such as Gutzwiller's periodic orbit theory, would be the key to explaining the experimental data (Gutzwiller 1971; 1990). Their theory, which is known as "closed orbit theory," is similar to Gutzwiller's in that it is grounded in a semiclassical approximation to the Green's function, though instead of using periodic orbits, Delos's theory makes use of closed orbits, namely, those orbits that are launched from, and return to, the vicinity of the nucleus.

In order to use closed orbit theory, one must first use classical mechanics to calculate the allowed orbits of a charged classical particle moving under the action of the combined Coulomb and magnetic field. These closed orbits can exhibit a variety of loops and zig-zags before returning to the nucleus. It turns out that, of all the possible allowed closed orbits of an electron in such a field, only about sixty-five orbits are relevant to explaining the quantum spectrum (Du and Delos 1988, p. 1906). Which orbits are relevant, and how they explain the anomalous spectra as Du and Delos claim above, has been summarized in an intuitive way as follows:

> Delos's insight was to realize that interpreting the departing and arriving electron as a wave meant that its outgoing and incoming portions will inevitably display the symptoms of interference. . . . [T]he survival of some of these quantum mechanical waves and the canceling out of others result in only certain trajectories' being allowed for the electron. . . . Once Delos established that only some trajectories are produced, he had effectively explained the new mechanism that caused the mysterious ripples [in the absorption spectrum above the ionization limit]. The Rydberg electron is allowed to continue to absorb energy, so long as that energy is precisely of an amount that will propel the electron to the next trajectory allowed by the interference pattern. (von Baeyer 1995, p. 108)

It is worth emphasizing again that this explanation of the anomalous resonances in the spectra is not a purely quantum explanation, deducing the spectrum directly from the

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[7] 'v-type' just refers to the clearly observable strong peaks in the Fourier-transformed spectra, with each peak being labeled with an integer, v.

Schrödinger equation.[8] Rather, it involves a careful blending of quantum and classical ideas: on the one hand the Rydberg electron is thought of quantum mechanically as a wave exhibiting the phenomenon of interference, while also being thought of fictionally as a particle following specific classical closed-orbit trajectories.

Despite the unorthodox hybridization of classical and quantum ideas in this explanation, closed orbit theory has proven to be strikingly successful empirically. With these classical closed orbits, one can predict the wavelength, amplitude and phase of these resonances to within a few percent, and, furthermore, the predictions of this theory have proven to be in very close agreement with the data generated by numerous subsequent experiments on the absorption spectra of hydrogen, helium and lithium atoms in strong magnetic fields.[9] This striking success of closed orbit theory shows that even in atomic physics, which is clearly under the purview of quantum mechanics, it is classical mechanics as developed through modern semiclassics that is proving to be the appropriate theoretical framework for explaining many of these quantum phenomena.

Closed orbit theory can not only explain the particular details of the experimental spectra, but can also explain why the earlier, lower resolution data of Tomkins and Garton yielded a very orderly series of oscillations, while the later, higher resolution data of Welge and colleagues revealed a wildly irregular series of oscillations. The explanation, once again, rests on a thorough mixing of classical and quantum ideas—specifically, a mixing of the quantum uncertainty principle with the fact that classical chaos is a long-time ($t\rightarrow\infty$) phenomenon that, on short time scales, can still look orderly. Because the low resolution experiments involved only a rough determination of the energy, only the short-time classical dynamics is relevant to the spectrum. The high-resolution experiments, by contrast, involved a more precise determination of energy, and hence the longer time dynamics of the classical system is relevant. Since the long time dynamics of a classical electron in a strong magnetic field is chaotic, this complexity manifests itself in the spectra.[10]

Ten years after closed orbit theory was introduced, Delos, Kleppner, and colleagues showed that, not only can classical mechanics be used to generate the quantum spectrum, but, even more surprisingly, the experimental quantum spectrum can be used to reconstruct the classical trajectories of the electron. As we have been emphasizing, part of the reason this is surprising is that electrons do not, in fact, follow classical trajectories at all—they are fictions. Recognizing this tension, they write, "We present here the results of a new study in which semiclassical methods are used to reconstruct a trajectory from experimental spectroscopic data. When we speak of the 'classical trajectory of an electron,' we mean, of course, the path the electron would follow if it obeyed the laws of classical mechanics" (Haggerty et al. 1998, p. 1592). While the previous experiments could be used to establish the actions, stabilities, and periods of the closed orbits, they could not be used to determine the orbits themselves, that is, the electron positions as a function of time. In this paper, however they show how "by doing spectroscopy in an

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[8] For a more technical discussion of the closed orbit theory explanation of the spectra see Bokulich (2008a), Chapter 5, Section 4.

[9] See Granger (2001), Chapter 1 for a review.

[10] See Du and Delos (1998) for a more detailed explanation, as well as a picture of a typical chaotic trajectory of Rydberg electron in a Coulomb and diamagnetic field.

oscillating field, we gain new information that allows us to reconstruct a trajectory directly—without measuring the wave function and without relying on detailed knowledge of the static Hamiltonian" (Haggerty et al. 1998, p. 1592).

Their experiment involves examining the spectrum of a highly excited (that is, Rydberg) lithium atom in an electric field (a phenomenon known as the Stark effect). While the behavior of a hydrogen atom in an electric field is regular, the behavior of a lithium atom in an electric field can be chaotic. Using an extension of closed orbit theory, they were able to show that an oscillating electric field reduces the strength of the recurrences in the spectrum, that is, the heights of the peaks, in a manner that depends on the Fourier transform of the classical electron orbits in the static electric field. Hence, by experimentally measuring the Fourier transform of the motion for a range of frequencies, one can then take the inverse Fourier transform, and obtain information about the electron's orbits. Using this technique they were able to successfully reconstruct from the experimental *quantum* spectra, two *classical* closed orbits of an electron in an electric field (these orbits are referred to as the "2/3" and "3/4" orbits, and are pictured in Figure 3 below).

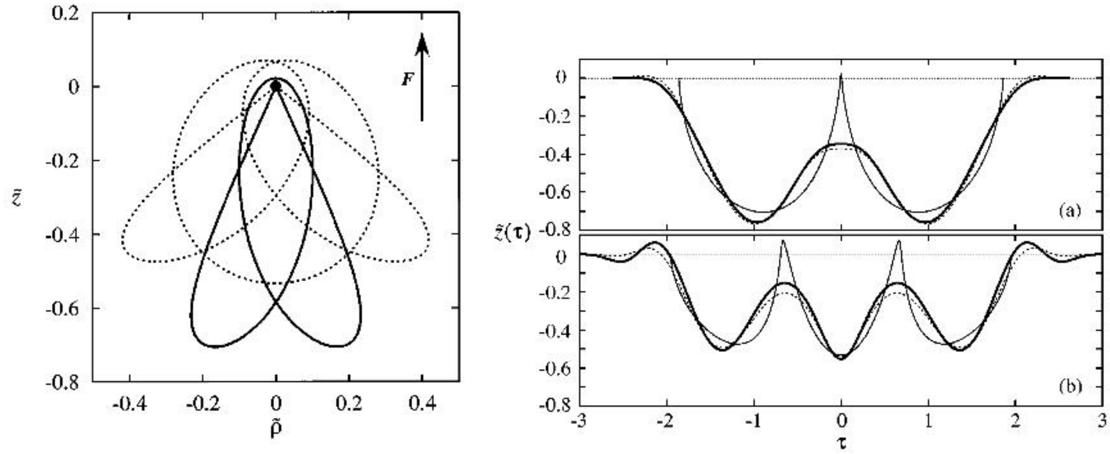

Figure 3: On the left: Two classical closed orbits of a Rydberg electron in an electric field; the solid line is the "2/3" orbit and the dotted line is the "3/4" orbit. The dark dot represents the nucleus of the atom. On the right: (a) the "2/3" orbit and (b) the "3/4" orbit. The light solid lines show the exact classical trajectories and the heavy lines are the experimentally reconstructed trajectories. Since the experimental frequency range is limited, the exact trajectories filtered through the experimental frequency window have also been included as the dashed lines for comparison. (From Haggerty et al. 1998, Figure 2 and Figure 4; courtesy of D. Kleppner).

They conclude, "Our experiment produces accurate, albeit low-resolution, pictures of classical trajectories important to the Stark spectrum of lithium" (Haggerty et al. 1998, p. 1595). Although they used the Stark spectrum of Rydberg lithium in this experiment, their method of extracting classical trajectories from quantum spectra can be applied to a variety of other systems. They emphasize that the limits to resolving these trajectories that they encountered are experimental, not fundamental, being many orders of magnitude away from the limits imposed by Heisenberg's uncertainty principle. Hence, although these experiments are deriving pictures of classical trajectories from quantum spectra, these trajectories in no way undermine the uncertainty principle.

Underlying these experiments is the following pressing but unspoken question: given that classical mechanics is *not* true, and that electrons in atoms do *not* actually follow definite trajectories, how can one legitimately speak of experimentally measuring such trajectories from a quantum spectrum at all? In a more recent paper, Kleppner and Delos (2001) tackle head on this question of the reality of these trajectories. They write,

> Because of the power of the concept of periodic orbits, one might question as to what extent they 'really exist' in the atom. Insight into this question was provided by a recent experiment at MIT, in which recurrence spectroscopy was used to measure the time dependence of an electron's motion along one of the closed orbits. To put it more precisely, recurrence spectroscopy was used to measure the time dependence of the fictitious classical trajectory that can be used as a calculational device to construct the quantum propagator. And to put it less precisely, recurrence spectroscopy showed how an electron in an atomic system would move in space and time if it obeyed classical physics. (Kleppner and Delos 2001, p. 606)

After discussing their experiment in more detail, however, Kleppner and Delos seem tempted by the view that these electron trajectories are more than mere fictions or calculational devices: They write, "These results lead us to question whether a trajectory should be described as truly 'fictitious' if one can measure its detailed properties" (Kleppner and Delos 2001, p. 610). The full realist claim, that electrons in atoms *really are* following these definite classical trajectories, would amount to a rejection of modern quantum mechanics and a violation of Heisenberg's uncertainty principle; and this is not something that semiclassical theorists, Kleppner and Delos included, intend to do.[11]

Nonetheless, I think Kleppner and Delos are groping toward an important point here: Not all fictions are on par. Even if we bracket those fictions that are useless for science and restrict our attention to those fictions that, as Vaihinger says, justify themselves by their utility and expediency, there are still some important distinctions to be made. On the one hand there are fictions in the sense of what Kleppner and Delos call "mere calculational tools." An example of such "mere fictions" might be Ptolemaic astronomy with its epicycles. While Ptolemaic astronomy might be a useful calculational tool for some navigators and surveyors, no one thinks that these fictions are giving any real insight into the way the world is or offering any explanations. On the other hand,

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[11] There are, of course, consistent interpretations of quantum mechanics, such as Bohm's hidden variable theory, in which electrons do follow definite trajectories. Typically, however, these Bohmian trajectories are not the trajectories of classical mechanics. As an empirically equivalent formulation of quantum mechanics, Bohm's theory would nonetheless be able to account for any experimental results just as the standard interpretation. For those who are interested in Bohmian mechanics, a Bohmian approach to these diamagnetic Rydberg spectra has been carried out by Alexandre Matzkin, who concludes "*Individual* BB [deBroglie-Bohm] trajectories do not possess these periodicities and cannot account for the quantum recurrences. These recurrences can however be explained by BB theory by considering the *ensemble* of trajectories . . . although none of the trajectories of the ensemble are periodic, rendering unclear the dynamical origin of the classical periodicities" (Matzkin 2006, p. 1; emphasis original).

however, there are some fictions in science that go beyond being simply calculational devices. These fictions, by contrast, do give some genuine insight into the way the world is and do seem to have some genuine explanatory power. I have called these latter sort of fictions *explanatory fictions*. As an example of an explanatory fiction, Kleppner and Delos cite rays of light. They write, " When one sees the sharp shadows of buildings in a city, it seems difficult to insist that light-rays are merely calculational tools that provide approximations to the full solution of the wave equation" (Kleppner and Delos 2001, p. 610). While they can certainly be used as calculational tools, these latter sort of fictions also carry explanatory force, and correctly capture in their fictional representation real features of the phenomena under investigation.

4. How Fictions Can Explain

The chief obstacle to admitting the existence of explanatory fictions is that it is difficult to imagine how a fiction could possibly explain. Indeed on the two most widely received philosophical accounts of scientific explanation—Carl Hempel's deductive-nomological (D-N) account and Wesley Salmon's causal-mechanical account—fictions cannot explain at all. According to Hempel (1965), a scientific explanation is essentially a deductive argument, where the phenomenon to be explained—the "explanandum"—is shown to be the deductive consequence of premises describing the relevant law or laws of nature and any relevant initial conditions (these premises that do the explaining are collectively referred to as the "explanans"). In order to count as a genuine scientific explanation, one of the further conditions that Hempel imposes is what he calls the "empirical condition of adequacy," by which he means specifically that the sentences constituting the explanans must be entirely true (Hempel 1965, p. 248). This is not "empirical adequacy" in our modern parlance, but rather a condition of *Truth*—with a capital "T." Hempel makes it quite clear that is insufficient for the explanans to be merely "highly confirmed by all of the relevant evidence available" (ibid). Given this strict requirement of truth, it is clear that fictions cannot be explanatory on this account of scientific explanation.

The second most widely received account of scientific explanation, Salmon's causal-mechanical account, also rules out the possibility that fictions can explain. On Salmon's (1984) account, to explain a phenomenon or event is to describe the causal-mechanical processes that led up to, or constitute, that phenomenon or event. Salmon draws a sharp distinction between genuine causal processes, which are physical processes capable of "transmitting a mark," and what he calls "psuedo-processes," which cannot. Only the former can lead to genuine scientific explanations. Thus on Salmon's account, like Hempel's, it seems that fictions cannot genuinely explain. Fictional entities and fictional processes do not meet the requirements of a genuine physical processes capable of transmitting a mark. Put more simply, a fiction A cannot be the cause of some phenomenon B—and hence explain B—if A does not exist.

The question, then, is whether we should dismiss the explanation being offered by Delos and colleagues of the resonances in the quantum spectra in term of fictional closed orbits as no explanation at all since it does not fit our preconceived philosophical ideas about scientific explanation? The answer, I believe, is no. Although the closed orbit explanation of the spectra is neither entirely true, as required by Hempel's account, nor can the fictional orbits be properly thought of as the *cause* of the oscillations, as required by Salmon's account, we should take the actual explanatory practices of scientists seriously, and nonetheless recognize it as a distinctive form of scientific explanation. Elsewhere (Bokulich 2008a) I have developed an alternative account of scientific explanation, which I call "model explanations," that can not only make sense of the explanatory power of idealized scientific models, but, as I shall argue next, can also correctly describe the sort of explanation that is being offered in the present case by closed orbit theory.

Model explanations can be characterized by the following three core features. First, the explanans must make essential reference to a scientific model, and that model (as I believe is the case with all models) involves some idealization and/or fictionalization of the system it represents. Second, that model is taken to explain the explanandum by showing that the pattern of counterfactual dependence in the model is isomorphic in the relevant respects to the pattern of counterfactual dependence in the target system.[12] Following James Woodward (2003), this pattern of counterfactual dependence can be explicated in terms of what he calls "what-if-things-had-been-different questions," or w-questions for short. That is, "the explanation must enable us to see what sort of difference it would have made for the explanandum if the factors cited in the explanans had been different in various possible ways" (Woodward 2003, p. 11). While I think that Woodward's approach is largely right, where I part company with his view is in his construal of this counterfactual dependence along strictly manipulationist or interventionist lines. It is this manipulationist construal that restricts Woodward's account to purely *causal* explanations, and as I argued above, I think it is a mistake to construe all scientific explanation as a species of causal explanation. The third condition that a model explanation must satisfy is that there must be what I call a further justificatory step. Very broadly, we can understand this justificatory step as specifying what the domain of applicability of the model is, and showing that the phenomenon in the real world to be explained falls within that domain. Although the details of this justificatory step will depend on the details of the particular model in question, it typically proceeds either from the ground up, via something like a de-idealization analysis of the model (McMullin 1985), or top down via an over-arching theory that justifies the modeling of that domain of phenomena by that idealized model.

It turns out that there are a variety of different subspecies of model explanations (Bokulich 2008a); hence even after one has identified a particular scientific explanation as a model explanation, there still remains the question of what *type* of model explanation it is. Determining the type of model explanation requires articulating what might be called the source of this counterfactual dependence. The type of model explanation that I believe is most relevant to closed orbit theory (and semiclassical explanations more generally—see also Bokulich 2008b) is what I have called *structural model explanations*. Following Peter Railton (1980, Section II.7) and R.I.G. Hughes (1989), a *structural explanation* is one in which the explanandum is explained by showing how the (typically mathematical) structure of the theory itself limits what sorts of objects, properties, states, or behaviors are admissible within the framework of that theory, and then showing that

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[12] The notion of an isomorphism is perhaps too strong, in that it implies a more precise formal relation than is intended here.

the explanandum is in fact a consequence of that structure.[13] A *structural model explanation*, then, is one in which, not only does the explanandum exhibit a pattern of counterfactual dependence on the elements represented in the model, but in addition, this dependence is a consequence of the structural features of the theory (or theories) employed in the model.

Applying this framework to the example presented in the previous section, I ague that classical closed orbits—despite being fictions—are able to genuinely explain the oscillations in the absorption spectrum in the sense that they provide a structural model explanation of this phenomenon. First, the closed classical orbits are the fictional elements that make up the semiclassical model of the quantum spectra. Second, there is a pattern of counterfactual dependence of the oscillations in the spectrum on the various features of these closed orbits (i.e., their actions, stabilities, periods, etc.). Moreover, this counterfactual dependence allows one to correctly answer a wide range of what-if-things-had-been-different questions. For example, one can say exactly how the oscillations peaks would have been different if the closed trajectories had been altered in various sorts of ways. Third there is a "top-down" justificatory step provided by closed orbit theory, which specifies precisely how these classical trajectories can be legitimately used to model the quantum phenomena.[14] The justification provided by closed orbit theory and the wide range of w-questions that closed orbit theory can correctly answer together suggest that these classical trajectories—despite their fictional status—are nonetheless giving us real insight into the structure of the quantum phenomena. In other words, although these classical trajectories are also useful calculational tools, they are not *mere fictions*. In so far as these closed orbits are giving us genuine insight into the structure of the quantum dynamics, they are *explanatory fictions*.

5. Conclusion

It is instructive to examine to what extent the present example of fictional classical orbits in quantum spectra fits with Vaihinger's account, and to what extent it suggests his account needs to be modified. The first key characteristic of fictions that Vaihinger identifies, namely their contradiction with reality, is maintained. The Rydberg electron in an atom is simply not following one of these classical closed orbit trajectories. In this sense, the fiction of closed orbits does involve a contradiction with reality. Vaihinger's second criterion, that the fiction is introduced only as a scaffolding to be eliminated, seems to require some modification. There is a straightforward sense in which classical trajectories in atoms have *already* been eliminated and replaced by the correct probabilistic description in terms of modern quantum mechanics. Indeed we saw

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[13] This definition of structural explanation is my own, and is not exactly identical to the definitions given by other defenders of structural explanations, such as Railton, Hughes and Rob Clifton. Nonetheless I think this definition better describes the concrete examples of structural explanations that these philosophers give, and is preferable given the notion of 'model' being used here. For further discussion of structural explanations and some examples see Bokulich (2008a, Chapter 6, Section 5).

[14] The technical details of this justificatory step are reviewed in Bokulich (2008a), Equations 5.9 – 5.14.

in the brief history at the beginning of Section 3 that Bohr had initially introduced classical trajectories in atoms as an hypothesis; by the early 1920s it had been transformed from an hypothesis to a useful fiction, and by the end of that decade the fiction, which had indeed been a fruitful scaffolding, had been eliminated in favor of what we would call the true description of the behavior of electrons in atoms. Yet even some thirty years after these electron trajectories were eliminated and replaced, the fiction was reintroduced again. The justification for the reintroduction of fictional electron trajectories was precisely their great fertility and explanatory power. Vaihinger's account does not seem to recognize this possibility, that some fictions may remain a part of the tool box of science even after the true description has been found. Hence not all fictions are introduced to be eliminated.

Vaihinger's third key characteristic of fictions, namely that the scientists expressly recognize that the fiction is just a fiction, is certainly maintained. At no point do semiclassical theorists, such as Delos and Kleppner, really think that the electron in the atom is following such a trajectory. As I mentioned earlier I think this third condition is an important factor in the legitimate use of fictions in science. Semiclassical theorists deploy closed orbit theory always with the express recognition that the trajectories being invoked are fictional. At no point are they rejecting modern quantum mechanics, which denies the existence of such trajectories. Nonetheless, simply recognizing the fictional status of a posit, does not mean that all such fictional posits are on par, as Vaihinger seems to suggest. Even if we restrict ourselves, as Vaihinger does in his fourth key characteristic, to those scientific fictions that are expedient, there are still some important distinctions to be made—it is not the case that even all *expedient* fictions are on par. In particular, I have argued that we need to distinguish between those fictions that are mere calculational tools, and those fictions that carry some explanatory force.

Vaihinger denied that there could be such explanatory fictions because he believed that fictions could not generate real knowledge. Using the example of classical trajectories in quantum spectra, I have tried to show that this assumption is mistaken. Some fictions can capture in their fictional representation real patterns of structural dependencies in the world, and hence generate real knowledge and be genuinely explanatory. It is noteworthy that semiclassical approaches to quantum phenomena, such as closed orbit theory, are not primarily valued as calculational tools. Indeed, in some cases the semiclassical calculations are just as complicated—if not more so—than their full quantum counterparts. Instead semiclassical approaches are valued because they provide an unparalleled level of physical insight into the structure of the quantum phenomena—a level of understanding that is difficult to extract from the full quantum solutions, even in those rare cases where the quantum solutions are available.

I suggested that one the chief obstacles to admitting the explanatory power of fictions is the lack of a philosophical framework for understanding how it is that fictions could explain. As I showed earlier, both of the current orthodox accounts of scientific explanation rule out the possibility of explanatory fictions. In response I outlined a new account of scientific explanation that I called structural model explanations, and argued that this account provides us with a way of making sense of how it is that some fictions—despite their ontological status—nonetheless deserve the epithet *explanatory*.

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