Atomic Discourse in
The Feynman Lectures on Physics

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Introduction

Like all other complex human endeavors, science requires compromises amongst its competing ideals. These compromises can be made in numerous ways, and science is capable of simultaneously sustaining many such resolutions. The central focus of this study is the compromises that are actually made in modern scientific discourse concerning atoms, particularly those that are due to the failure of classical conceptions to provide an adequate account of the structure of matter, and the simultaneous failure of quantum mechanical conceptions to provide an intuitively satisfying picture of atomic processes. In order to further focus this study we have chosen to concentrate entirely on a single text: The Feynman Lectures on Physics. This is a brilliant work by a brilliant physicist which is already well known to both scientists and philosophers. It will represent for us a tractable microcosm of the larger macrocosm of scientific discourse. It is our claim that what we will find in this text is not simply idiosyncratic to Feynman, but is, instead, generic to much of current scientific discourse. The reader can easily test this hypothesis for himself.

Feynman’s treatment of quantum mechanics does seem to provide an intuitively satisfying picture of atomic processes. Unfortunately, this is accomplished at the expense of clarity. Feynman is well aware of this and gives the following warning and apology before presenting his most careful discussion of the fundamental principles of quantum mechanics.
Because atomic behavior is so unlike ordinary experience, it is very difficult to get used to and it appears peculiar and mysterious to everyone—both to the novice and to the experienced physicist. Even the experts do not understand it the way they would like to, and it is perfectly reasonable that they should not, because all of direct, human experience and of human intuition applies to large objects. We know how large objects will act, but things on a small scale just do not act that way. So we have to learn about them in a sort of abstract or imaginative fashion and not by connection with our direct experience.

In this chapter we shall tackle immediately the basic element of the mysterious behavior in its most strange form. We choose to examine a phenomenon which is impossible, absolutely impossible, to explain in any classical way, and which has in it the heart of quantum mechanics. In reality, it contains the only mystery. We cannot explain the mystery in the sense of "explaining" how it works. We will tell you how it works. In telling you how it works we will have told you about the basic peculiarities of all quantum mechanics.

(Lectures, I, 37-1,2)

These excuses for the obscurity of his presentation of quantum mechanics are unconvincing. The same kind of excuses would have been given for the peculiar and mysterious aspects of relativity physics had physicists rejected Minkowski's space-time approach as too mathematical and abstract, and instead continued to use Lorentz's approach. Just as a clear presentation of relativity physics requires the use of generalized geometry, a clear presentation of quantum physics requires the use of generalized probability theory. And this requires that time be spent in gaining a clear understanding of its conceptual structure. There are no mysteries remaining in quantum behavior once such a clear understanding has been obtained; just as one who understands differential geometry is not led to wonder about a fourth spatial dimension because three-dimensional space is curved. But one gets
the general impression that for Feynman in particular the mystery he sees in quantum mechanics is one of its special charms.

Feynman's most careful statement of the central mystery of quantum behavior occurs in his discussion of the two-slit experiment.

"Well," you say, "what about Proposition A? It is true, or is it not true, that the electron either goes through hole 1; or it goes through hole 2?" The only answer that can be given is that we have found from experiment that there is a certain special way that we have to think in order that we do not get into inconsistencies. What we must say (to avoid making wrong predictions) is the following. If one looks at the holes or, more accurately, if one has a piece of apparatus which is capable of determining whether the electrons go through hole 1 or hole 2, then one can say that it goes either through hole 1 or hole 2. But, when one does not try to tell which way the electron goes, when there is nothing in the experiment to disturb the electrons, then one may not say that an electron goes either through hole 1 or hole 2. If one does say that, and starts to make any deductions from the statement, he will make errors in the analysis. This is the logical tightrope on which we must walk if we wish to describe nature successfully.

(Lectures, I, 37-9)

This paragraph implies that electrons aren't objects at all, that the notion of the location of an electron is meaningless whenever we make no attempt to measure it. And this is exactly what Feynman says in a very similar discussion in his classic paper "Space-Time Approach to Non-Relativistic Quantum Mechanics" (Rev. Mod. Phys. 20,367 (1948); see p. 369). Thus, a position measurement doesn't detect the location of an independently existing object, but instead creates a location result. Those situations that would classically be described as containing
electrons would have to be quantum mechanically redescribed as supporting various probabilistic dispositions for responding to probing in various ways. This provides what we will call (and consider to be) the correct, careful, coherent approach to quantum mechanics. It leads to a radically non-classical view of the relationship of microphysics to macrophysics, one which many physicists find unpleasant. While this correct quantum mechanical worldview is stated in The Feynman Lectures, it is totally quarantined to the above paragraph. But this quarantining is not idiosyncratic to The Feynman Lectures; similar isolation occurs in scientific discourse in general.

Instead, a different approach to the interpretation of quantum mechanics, one that is less coherent, but much more intuitively satisfying, pervades the entire Feynman Lectures. It is that electrons most definitely are independently existing objects, though quantum mechanical objects instead of classical objects, that they do have a location at every moment, though in a quantum mechanical sense instead of in a classical sense. And this is exactly what Feynmen implies in a discussion of the uncertainty principle.

We can form an image of the hydrogen atom by imagining a "cloud" whose density is proportional to the probability density for observing the electron. A sample of such a cloud is shown in Fig. 6-11. Thus our best "picture" of a hydrogen atom is a nucleus surrounded by an "electron cloud" (although we really mean a "probability cloud"). The electron is there somewhere, but nature permits us to know only the chance of finding it at any particular place.

(Lectures, I, 6-11)

In Quantum Mechanics and Path Integrals (see pp. 13 and 14), Feynman
very explicitly chooses the intuitive approach over the careful approach.

Considering how close Feynman's intuitive approach is to the point of view of hidden variable theorists, one might wonder why he isn't more sympathetic to their program. This is because Feynman believes that quantum mechanics already is the right kind of theory, namely, an abstract mathematical algorithm which successfully allows us to anticipate nature—even if only her probabilities. He makes this point about the nature of mature physical theories many times in his Lectures (see, for instance, I, 4-1 and 7-9; and II, 1-9, 10). One of the things Feynman is attempting to do in his Lectures is to point out to his students promising areas for future research. But he also wants to steer them away from what he considers to be dead ends. And he considers the hidden variable theorists to be simply misguided. From the point of view of the careful approach to quantum mechanics, nature supports certain probabilistic dispositions which the quantum mechanical algorithm allow us to anticipate. Nothing further is required, except possibly heuristic aids. These aids, if they're needed at all, should definitely not be allowed to tamper with the successful abstract mathematics. But Feynman's intuitive approach is somewhat different. He very clearly rejects the notion of nature supporting an objective probabilistic disposition. That the notion of an objective disposition presents no problem for Feynman is clear from his discussion of the electro-magnetic field in volume II. In particular, he states:
First, we must extend, somewhat, our ideas of the electric and magnetic vectors, E and B. We have defined them in terms of the forces that are felt by a charge. We wish now to speak of electric and magnetic fields at a point even when there is no charge present. We are saying, in effect, that since there are forces "acting on" the charge, there is still "something" there when the charge is removed. If a charge located at the point \((x,y,z)\) at the time \(t\) feels the force \(\mathbf{F}\) given by Eq. (1.1) we associate the vectors \(\mathbf{E}\) and \(\mathbf{B}\) with the point in space \((x,y,z)\). We may think of \(\mathbf{E}(x,y,z,t)\) and \(\mathbf{B}(x,y,z,t)\) as giving the forces that would be experienced at the time of \(t\) by a charge located at \((x,y,z)\), with the condition that placing the charge there did not disturb the positions or motions of all the other charges responsible for the fields.

(Lectures, II, 1-3)

But Feynman takes a very subjectivistic approach towards probabilities. This is shown in the following quote.

We have also not considered how we should treat the case of a "coin" or some similar "chancy" object (say a stone that always lands in either of two positions) that we have good reason to believe should have a different probability for heads and tails. We have defined \(P(H) = \frac{N_H}{N}\). How shall we know what to expect for \(N_H\)? In some cases, the best we can do is to observe the number of heads contained in large numbers of tosses. For want of anything better, we must set \(\langle N_H \rangle = N_H\) (observed). (How could we expect anything else?) We must understand, however, that in such a case a different experiment, or a different observer, might conclude that \(P(H)\) was different. We would expect, however, that the various answers should agree within the deviation \(1/2\sqrt{N}\) [if \(P(H)\) is near one-half]. An experimental physicist usually says that an "experimentally determined" probability has an "error," and writes

\[
P(H) = \frac{N_H}{N} \pm \frac{1}{2\sqrt{N}}
\]  

(6.14)
There is an implication in such an expression that there is a "true" or "correct" probability which could be computed if we knew enough, and that the observation may be in "error" due to a fluctuation. There is, however, no way to make such thinking logically consistent. It is probably better to realize that the probability concept is in a sense subjective, that it is always based on uncertain knowledge, and that its quantitative evaluation is subject to change as we obtain more information.

(Lectures, I, 6-7)

Strangely enough, Feynman also gives the correct arguments in favor of an objective notion of probability two chapters later in his discussion of motion.

Many physicists think that measurement is the only definition of anything. Obviously, then, we should use the instrument that measures the speed-the speedometer-and say, "Look, lady, your speedometer reads 60." So she says, "My speedometer is broken and didn't read at all." Does that mean the car is standing still? We believe that there is something to measure before we build the speedometer. Only then can we say, for example, "The speedometer isn't working right," or "the speedometer is broken." That would be a meaningless sentence if the velocity has no meaning independent of the speedometer. So we have in our minds, obviously, an idea that is independent of the speedometer, and the speedometer is meant only to measure this idea.

(Lectures, I, 8-3)

In fact, the notion of instantaneous velocity shares many similarities with the notion of objective probability. Instantaneous velocity is a quantitative measure of a disposition a particle has to change its location. Furthermore, since its value is defined via a limit as \( \Delta t \to 0 \), one has the same kinds of problems in experimentally ascertainment its value as one has in the case of probabilities.

So, Feynman could have treated probability objectively, and
such an approach is necessary in a careful treatment of quantum mechanics. But, instead, Feynman's intuitive approach involves treating probability subjectively and the electron objectively. This undermines to some extent his criticism of hidden variables theorists. But Feynman might well argue that we're just splitting hairs and making too much of such heuristic aids as the notion of an electron, while, in what is really important, namely, the correct anticipation of experimental results, we're in entire agreement on how to use the quantum mechanical algorithms. (He makes this kind of point very clearly in Lectures, II, 20-9, 10 in a discussion on scientific imagination, and again at the end of his book Photon-Hadron Interactions (pp. 269-270) in a discussion on the reality of partons.)

Still, since the intuitive approach to quantum mechanics permeates our entire modern scientific worldview, we believe it is incumbent upon us to obtain as reasonably clear a view of both its strengths and its weaknesses as is feasible. This is especially so since most serious studies of the foundations of quantum mechanics either ignore the intuitive approach entirely, or else dismiss it out of hand as incoherent. That there is a certain amount of incoherence in the intuitive approach is undeniable. Yet the incoherence seems not to be so great as to be a serious hindrance in practice. This is especially the case in very complex situations where the intuitive approach functions admirably. Under such complex circumstances, the slight incoherence in the intuitive approach is simply overwhelmed by the massive incoherence in the rest of the situation, while the
intuitive approach still retains its advantages. The trade-offs seem to be well worthwhile, except in specifically foundational studies.

An appearance of great ontological unity is another fringe benefit that the intuitive approach gives to modern science. Much of modern science consists of studying the properties and interactions of photons, electrons, atoms and molecules and how these phenomena relate to other more macroscopic phenomena. When one switches to the careful approach to quantum mechanics, this illusion of unity is shattered. Quantum probability theory no more unifies microscopic phenomena than the use of numbers unifies the phenomena of vibrating strings with the phenomena of the population of a country, or the use of calculus unifies the motion of the planets with the motion of the economy. This lack of unity pokes its head up especially in discussions of holes, excitons, etc. (see, in particular, Lectures, III, 13-9, 10 and 14-7). These models are more clearly understood as direct applications of quantum probability theory than as coming in principle from an underlying theory of electrons. (Recall the change that has occurred in our attitude towards positrons since Dirac originally introduced them as holes.) But unity is an important ideal for most scientists. It is what is motivating and inspiring theoretical physicists today onwards towards grand unification and quantum geometrodynamics. Our own preference is for the pluralistic universe over the unified universe. So, this fringe benefit of ontological unity that is achieved by the intuitive approach is less significant to us, as
we prefer the richness (and clarity) of the careful approach.

The desire for unity sometimes leads the physicist to a
certain lack of candor or even self-delusion concerning the precise
relationships between his various theories. This is particularly
ture concerning the relationship between microscopic and macroscopic
physics. Physicists generally believe that only microscopic physics
is truly fundamental and that all the rest of phenomena should be
derivable, at least in principle, from fundamental physics. But
the actual situation is not that simple. Macroscopic phenomena
can be successfully described directly using, for example, thermo-
dynamics. But there is no known rigorous connection between microscopic
mechanical models and this macroscopic description. By introducing
the intermediate theories called kinetic theory and statistical mechanics,
one can successfully relate these theories to thermodynamics by what
Gibbs called thermodynamic analogies. But this still leaves a gap
between mechanics and statistical mechanics. This gap is filled using
heuristic arguments that cannot be made rigorous, though they do
yield useful results. What one really has is four different theories
which are only vaguely related. Feynman introduces his chapters
on these topics with an overview and warning of the complexity
of this subject matter (see Lectures, I, 39-1,2). However, in his
actual discussions, Feynman gives the misleading impression that
much more has been rigorously derived than is in fact the case.
This is surprising, as Feynman is usually quite candid about the
precise nature of his arguments. (For a wonderful example of
Feynman's candor and "anythinggoes" attitude, the reader should
examine Feynman's classical derivations of diamagnetism and par-
amagnetism in Lectures, II, 34.)
The main body of this study naturally divides itself into two parts. In part I we will analyze classical atomic discourse. Classical atomic discourse still makes up the bulk of modern atomic discourse. In §§1 and 2 we will criticize this discourse from the point of view of the careful approach to quantum mechanics. We will see how this quantum mechanical worldview leads to a radically non-classical view of the relationship of micro- to macro- physics. In particular, we will see how many of the very satisfying classical explanations - such as those of pressure, temperature, and sight - totally evaporate upon passage to quantum mechanics. In §3 we will be concerned with problems that arise entirely within classical discourse when one tries to relate micro- and macro- physics. In particular, we will be concerned with trying to obtain a clear view of the relationships between the four theories: mechanics, kinetic theory, statistical mechanics, and thermodynamics. We will also be concerned with understanding the nature of discourse in kinetic theory. Kinetic theory - as usually practiced - provides a very interesting example for methodological analysis.

In part II of this study we will analyze quantum atomic discourse. In §4 we will consider some of the informal previews of quantum mechanics that Feynman has scattered through volumes I and II of his Lectures. In §5 we will analyze the basic principles of Feynman's intuitive approach to quantum mechanics, in particular, his conception of an event being able to occur in several alternative ways. In §6 we will analyze a number of applications of quantum mechanics from both the careful and intuitive points of view. We will examine the pros and cons of both approaches.
Part I. Classical Atomic Discourse

§1. Matter is made of atoms

If, in some cataclysm, all of scientific knowledge were to be destroyed, and only one sentence passed on to the next generations of creatures, what statement would contain the most information in the fewest words? I believe it is the atomic hypothesis (or the atomic fact, or whatever you wish to call it) that all things are made up of atoms—little particles that move around in perpetual motion, attracting each other when they are a little distance apart, but repelling upon being squeezed into one another. In that one sentence, you will see, there is an enormous amount of information about the world, if just a little imagination and thinking are applied.

(Lectures, I, 1-2)

After making some general remarks concerning the nature of science and the problems of pedagogy, Feynman launches into his lectures proper with the above quote. This quote provides a very familiar picture with an ancient pedigree going all the way back to the Greeks. On the other hand, many of the problems with this point of view were well understood by the end of the nineteenth century. Attempts to resolve these problems led to quantum mechanics, which requires a radically different viewpoint. One can definitely say that twentieth century science has shown that the above viewpoint, while often an adequate or useful approximation, is certainly false. But the situation is actually quite complex.

At the turn of the century atoms were merely hypothetical elements useful (possibly) for simplifying theory and coordinating macroscopic phenomena. But this is no longer the case. One now has a plethora of fairly direct microscopic phenomena: electron tracks, pictures of wiggling uranium atoms and
the atoms on the tip of a needle, the substructures
of a proton, etc. These phenomena are not hypothetical.
In fact, many of the idealized pictures Feynman draws in chapter 1
should be close to actual pictures which will someday (probably
soon) be obtainable. If this is the case, then how can the view-
point be false? The error occurs in the extremely natural tran-
sition from "If we look at it very closely we see..." to "all
things are made of atoms." In quantum mechanics one can (at
least in principle, and sometimes in practice) successfully cor-
relate phenomena, but one runs into serious difficulties if one
tries to talk not only about atomic phenomena, but also about
atomic objects. On the other hand, something important is
gained by describing an experimental situation as dealing with
atoms instead of in a more impoverished language. By saying that
he's dealing with atoms the scientist situates the experiment in
a very broad context of previous theories and experiments. This
allows him to successfully anticipate how the situation would
respond to various forms of probing. It is possible that the huge
range of phenomena and theories that are currently held together
by the scientist under the umbrella of atomic theory will in the
future bifurcate and be seen as totally unrelated situations. Much
of the seeming unity of modern science would then disappear.
On the other hand, it seems more likely that modern science
will continue to preserve its unity at the expense of its clarity.

Let us try to be more specific about the changes the classical
viewpoint undergoes in its passage to quantum mechanics (done
carefully). Consider the following.

The water keeps its volume; it does not fall apart, because of the attraction of the molecules for each other.

Now the jiggling motion is what we represent as heat: when we increase the temperature, we increase the motion.

So many of them are hitting the top piston all the time that to keep it from being patiently knocked out of the tank by this continuous banging, we shall have to hold the piston down by a certain force, which we call the pressure (really, the pressure times the area is the force).

*(Lectures, I, 1-3)*

All these quotes are relating micro- and macro- phenomena, but there is a tendency to take the microphenomena more seriously. It is the microphenomena that cause and explain the macrophenomena; they aren't simply correlated in this classical viewpoint. Even while staying within a purely classical framework, this whole approach becomes very problematical when one contemplates the possible infinite hierarchy of submicroscopic phenomena. Further problems occur if one tries to do careful computations. In most mathematical models, both classical and quantum mechanical, one can only relate the macroscopic observables pressure and temperature to parameters occurring in a Gibbs state. Thus, one needs to use statistical mechanics and not just ordinary mechanics. In these models nothing moves, there are no trajectory observables. Thus, in these models temperature and pressure are not being related to the motion of atoms. (Recall, for instance, the Ising model.) None of the standard quantum mechanical models, such as
the Schrödinger theory, has any trajectory observables. One must introduce ad hoc arguments - such as reduction of the wave packet - to get anything resembling trajectories out of these models.

One can thus see that the intuitively satisfying explanations of pressure and temperature given in the above quotes don't survive in (existing) careful models. Yet, if carefully reworded, they would accurately describe actual correlations between micro- and macro- phenomena. No wonder they haven't been given up. In fact, the continued use of these pictures represents an intelligent case of "eating your cake and having it too." They provide intuitively satisfying explanations which are quantitatively adequate most of the time. When they fail, one must be more careful and use more abstract mathematical models. But these mathematical models-especially the quantum mechanical ones-have none of the satisfying intuitive appeal that the classical pictures have. Thus, a rational compromise is reached in which both approaches occur. Modern scientific thought has to be understood in terms of intellectual ecology, with mixed strategies predominating. This is a far cry from traditional natural philosophy with its goal of a single unified coherent world view. But the traditional goal has not been consciously given up either. The scientist still strives for - or at least gives lip service to-the ideal of a unified science. Feynman constantly makes the distinction between fundamental and phenomenological physics. (See in particular, Lectures, I, 2.) Fundamental physics is our
deepest, most precise physics. The only reason for using any other physics is that fundamental physics often yields computationally intractable descriptions. One is reminded of the situation in chess, where knowing the rules of the game is just the beginning of becoming a decent chess player. Knowledge of the von Neumann strategy is worthless in this situation because it is computationally intractable even if it is theoretically perfect. Feynman considers all non-fundamental physics as arising for the same type of reasons that chess strategies arise. While this is an interesting analogy, which is sometimes appropriate, we don't think it does justice to the complexity of our current thought. The relationships between our current theories are quite difficult to state, and calling some of them fundamental and others phenomenological is often no aid at all. For instance, is statistical mechanics fundamental or phenomenological? What about fluid mechanics?

Returning to the text, we find another traditional distinction, the primary-secondary distinction, in a particularly blunt form. Feynman is discussing evaporation of water in a closed container and states:

So we see that what looks like a dead, uninteresting thing—a glass of water with a cover, that has been sitting there for perhaps twenty years—really contains a dynamic and interesting phenomenon which is going on all the time. To our eyes, our crude eyes, nothing is changing, but if we could see it a billion times magnified, we would see that from its own point of view it is always changing: molecules are leaving the surface, molecules are coming back. (Lectures, I, 1-5)

The key phrases here are "really contains" and "from its own point of view." Both points of view
are human points of view useful for different purposes. There is no good reason (except a physicist's hubris) for giving ontological priority to the magnified as opposed to unmagnified viewpoint. A better strategy is simply to accept all viewpoints on their own terms while trying to relate them to other viewpoints. One problem with trying to give molecules ontological priority can be seen from the following.

In passing, we mention that the concept of a molecule of a substance is only approximate and exists only for a certain class of substances. It is clear in the case of water that the three atoms are actually stuck together. It is not so clear in the case of sodium chloride in the solid. There is just an arrangement of sodium and chlorine ions in a cubic pattern. There is no natural way to group them as "molecules of salt."

(Lectures, I, 1-6)

The point here is well taken; but the same point can be made for the concept of atoms from the point of view of electrons, and for the concept of protons from the point of view of quarks, etc. Unless one believes that this abyss eventually terminates, one should drop the primary-secondary and fundamental-phenomenological distinctions. Even if it did terminate, these distinctions would still be misleading. For example, there is a basic description of chess, and an ultra-fast hypothetical computer could play the von Neumann strategy without ever introducing ordinary human ways of looking at the board. But this would not in any way falsify the
ordinary human viewpoints. This type of situation occurs in the relationship of chemistry to the Schrodinger equation. In principle all you should have to do is solve the Schrodinger equation to explain chemistry. But, in practice, the Schrodinger equation is not much more useful in chemistry than is the von Neumann strategy in chess; it provides some insight, but not much else. The same sort of thing happens in nuclear physics when one tries to understand large nuclei; a description in term of quarks is intractible. In practice, one uses many different approaches whose precise relationships are quite opaque. There is no more reason to suppose that the actual theories of theoretical chemistry are any more contained in the Schrodinger theory, than to suppose that Alekhine's strategy is contained in von Neumann's analysis of chess.

The idea that fundamental theories contain-at least in principle-higher level points of view is stated very baldly by Feynman at the close of his first chapter.

Everything is made of atoms. That is the key hypothesis. The most important hypothesis in all of biology, for example, is that everything that animals do, atoms do. In other words, there is nothing that living things do that cannot be understood from the point of view that they are made of atoms acting according to the laws of physics. This was not known from the beginning: it took some experimenting and theorizing to suggest this hypothesis, but now it is accepted, and it is the most useful theory for producing new ideas in the field of biology.

If a piece of steel or a piece of salt, consisting of atoms one next to the other, can have
such interesting properties; if water—which is nothing but these little blobs, mile upon mile of the same thing over the earth—can form waves and foam, and make rushing noises and strange patterns as it runs over cement; if all of this, all the life of a stream of water, can be nothing but a pile of atoms, How much more is possible? If instead of arranging the atoms in some definite pattern, again and again repeated, on and on, or even forming little lumps of complexity like the odor of violets, we make an arrangement which is always different from place to place, with different kinds of atoms arranged in many ways, continually changing, not repeating, how much more marvelously is it possible that this thing might behave? Is it possible that that "thing" walking back and forth in front of you, talking to you, is a great glob of these atoms in a very complex arrangement, such that the sheer complexity of it staggers the imagination as to what it can do? When we say we are a pile of atoms, we do not mean we are merely a pile of atoms, because a pile of atoms which is not repeated from one to the other might well have the possibilities which you see before you in the mirror.

(Lectures, I, 1-9)

This must be the most enthusiastic statement of classical atomism ever made. One cannot detect in it any hints of the quantum revolution which shook classical physics, or of the Einstein-Bohr debates with which Feynman is certainly familiar. Clearly, this classical atomic worldview is being sustained because it provides such an intuitively satisfying and lucid description of the world which is empirically adequate for many purposes and—possibly most importantly of all—serves as a strong rallying call for physicists. In practice, molecular biology makes very little use of atomic physics. If one surveys Watson's Molecular Biology of the Gene, one finds lots of modern chemistry, but no modern physics. Watson gets his chemistry from Pauling's The Nature of the Chemical Bond, and Pauling
is very explicit (see pp. 215-220) about chemistry's essential independence of physics. Considering that atomic physics is not much help even in chemistry, it is ludicrous to suppose that it contains— even in principle—the strange patterns that water makes as it runs over cement, or what I see when I look in a mirror. On the other hand, there is no denying that it is a powerful perspective whose important applications increase daily. In the next section we will look at one of these important applications: the physicist's explanation of sight.
§2. The Mechanism of Seeing

First, light is, of course, familiar to everybody, and has been familiar since time immemorial. Now one problem is, by what process do we see light? There have been many theories, but it finally settled down to one, which is that there is something which enters the eye—which bounces off objects into the eye. We have heard that idea so long that we accept it, and it is almost impossible for us to realize that very intelligent men have proposed contrary theories—that something comes out of the eye and feels for the object, for example.

(Lectures, I, 26–2)

"... that there is something which enters the eye—which bounces off objects into the eye" is one of those classical viewpoints which don't survive the passage to a careful approach to quantum mechanics. In such a careful approach, photons and electrons aren't things. Instead, we can use a perspective which yields photon and electron phenomena, which may then be correlated with the phenomena in other perspectives. The utility of attempting to determine such correlations varies. As Feynman puts it:

In discussing the sense of sight, we have to realize that (outside of a gallery of modern art!) one does not see random spots of color or spots of light. When we look at an object we see a man or a thing; in other words, the brain interprets what we see.

(Lectures, I, 36–1)

Attempting to correlate what one ordinarily sees, such as tables and chairs, to a description in terms of photon and electron phenomena
is certainly a hopeless task. On the other hand, certain aspects of our visual experience can be successfully related to the physicist's description of the visual system. This is particularly true of such visual pathologies as nearsightedness and colorblindness. In this section we will first discuss some aspects of the physicist's description of the visual system. We will then discuss what light the physicist's description sheds on our visual experiences.

So what does happen when we switch on a light? From the ordinary point of view the answer is clear: we can see better, we can better avoid walking into obstacles, etc. But the physicist desires a different kind of answer. For instance, Newton thought of light as a stream of very small particles that bounced off of objects and then entered the eye and excited the nervous system. On the other hand, Maxwell thought of light as disturbances or waves in the electromagnetic field. The electromagnetic field itself characterized at least some of the response characteristics of that classical all-pervading substance, the ether. From a careful modern point of view, the correct sort of thing to say is that turning on the light changes certain response characteristics of the region surrounding the light. Photon detectors will behave differently when the light is on instead of off. The occurrence of objects in the region will also affect the response characteristics of the photon detectors. But there are no photons. Photon phenomena exist, they are created by photon detectors, but photon detectors are not detecting independently existing photons.
So how does an organism respond to light? Clearly, the physicist responds, it needs photon detectors, i.e., some special molecules that respond appropriately to photons. But, from the careful quantum mechanical point of view, molecules aren't objects either. Instead, we have various detectors and probes whose output might read "detected a molecule of type B in an excited state." (More realistically, one will simply obtain a graph with various "resonance bumps" on it. (See Feynman's discussion of resonance in nature in Lectures, I, 23-7, 8, 9.) Much of current theoretical physics (and chemistry) consists essentially of a "calculus of bumps"; given that certain bumps occur in certain data, what bumps should be expected to occur in other data.) In any case, the physicist is led to study the retina at the molecular level looking for the appropriate molecules. In this he is successful. Feynman discusses the structure of rod cells in Lectures, I, 36-6. There are two interesting things to notice about his discussion. The first is its essentially classical atomic language; the second is the way in which it interrelates a number of different theories and viewpoints. In particular, it interrelates the following: structures seen in an electron micrograph, chemical substances, chemical bond theory, electron behavior, theories of light and its absorption by electrons in molecules, human biology and visual experience. Such an explanation cannot be given a euclidean form. The successful unification of diverse phenomena into a euclidean theory, such as Maxwell's electro-magnetic theory, is an ongoing process, which, while always expanding, can be expected to
remain a small proportion of total scientific discourse.

Some of the color phenomena the physicist's approach helps us to comprehend include such striking effects as the colors seen on thin films and in rainbows, the lack of color in low intensity light and in peripheral vision, and the rules of color mixing. Some of the difficulties in understanding color vision are described by Feynman as follows

Color is not a question of the physics of the light itself. Color is a sensation, and the sensation for different colors is different in different circumstances. For instance, if we have a pink light, made by superimposing crossing beams of white light and red light (all we can make with white and red is pink obviously), we may show that white light may appear blue. If we place an object in the beams, it casts two shadows - one illuminated by the white light alone and the other by the red. For most people the "white" shadow of an object looks blue, but if we keep expanding this shadow until it covers the entire screen, we see that it suddenly appears white not blue! We can get other effects of the same nature by mixing red, yellow, and white light. Red, yellow, and white light can produce only orangey yellows, and so on. So if we mix such lights roughly equally, we get only orange light. Nevertheless, by casting different kinds of shadows in the light, with various overlaps of colors, one gets quite a series of beautiful colors which are not in the light themselves (that is only orange), but in our sensations. We clearly see many different colors that are quite unlike the "physical" ones in the beam. It is very important to appreciate that a retina is already "thinking" about the light; it is comparing what it sees in one region with what it sees in another, although not consciously.

(Lectures, I, 35-10)

So, it turns out that not only is it hopeless to try to go from the physicist's description of light in terms of photons to our
ordinary experiences of tables and chairs, but even attempting to predict when a person will see a blue patch is extremely difficult. Furthermore, this classical mode of thought seduces the physicist into saying that the blue patch isn't really there; it's only in our heads; it's not objective but subjective. After all, his photon detectors don't detect any "blue" photons. But other instruments will probably soon be available which could detect the blue patch, (for instance, the visual system of a robot). Switching to describing light via these more sophisticated instruments would yield an objective description of light which better correlates with our visual experiences. The physicist's approach, on the other hand, will require not only a theory of light and of the eye, but also of the visual cortex and other parts of the brain. Such understanding is not to be expected shortly.

In these first two sections we have stressed the changes that are required in going from a classical to a careful quantum mechanical mode of discourse. In the next section we examine some problems that occur entirely within the classical context.
§3. Properties of Matter

We can discuss matter only in a most elementary way; it is much too complicated a subject to analyze directly from its specific basic laws, which are none other than the laws of mechanics and electricity. But these are a bit too far away from the properties we wish to study; it takes too many steps to get from Newton's laws to the properties of matter, and these steps are, in themselves, fairly complicated. (Lectures, I, 39-1)

In this section we will explore the relationships between mechanics, kinetic theory, statistical mechanics and thermodynamics. Throughout his discussion Feynman implies—as in the above quote—that matter is fundamentally described by mechanics and that any deviation from this fundamental description is due to the fact that we can't handle the mechanical description because of our computational limitations. Furthermore, it is implied that the alternative descriptions that one uses to describe matter can be derived—though painfully—from mechanics. Moreover, the text actually gives such derivations; but they don't turn out to be so painful after all. The catch is, that while the derivations are quite convincing on a superficial level, they don't stand up to careful scrutiny. Worse still, it isn't just a question of logical gaps, but instead assumptions are made which are in flat violation of mechanics. This problem was clearly recognized in the nineteenth century and little progress has been made since that time in clarifying the situation. Kinetic theory works pretty well empirically, but its precise relationship to mechanics is still an open question.
We will be concerned here with the structure of the heuristic arguments Feynman actually gives for bridging the gap between mechanics and kinetic theory and with the location of their weak points. Even should some mathematician eventually provide a rigorous discussion of the relationship between mechanics and kinetic theory, we don't expect that such a discussion would displace ones of the type that Feynman gives. This is because a rigorous discussion would probably be very difficult and require a great deal of mathematical sophistication, while Feynman's discussion is very simple and yields useful results, even if one has had to sacrifice a bit on one's logic. The clash between our ideals of simplicity and logic can be rationally resolved by easing up a bit on one's logical demands. While ideally one would also like to know what happens rigorously, practice will involve compromises.

The reader is directed to Feynman's first discussion of kinetic theory (Lectures, I, 39-3, 4). Certainly few freshman would ever notice any problem at all in this lucid, highly convincing argument. The basic problem is in the notion of thermal equilibrium, a notion totally alien to mechanics. Feynman says that there are \( n = N/V \) atoms in each unit volume. But, in a mechanical system, no matter what sub-volume one chooses, the number of atoms in it may vary from 0 to \( N \) as time goes on. To even show that most of the time the density is approximately \( n \) is very difficult and sometimes even false. The same type of criticism applies to the average \( \langle v^2 \rangle \). So, it is not at all clear that one can even define the notion of the pressure of a collection of atoms starting from just mechanics. Some sort of averaging process is required, and this averaging process may not be deducible merely from mechanics. Even if it were so deducible the arguments would likely be quite subtle. The most
interesting point is that while Feynman is surely aware of these
problems, he doesn't bother to point them out until much later
(ch. 46). The student needs to learn to give the kind of argument
Feynman has just given, for, after all, it does work — it
gives important results. Becoming a physicist involves
learning just such heuristics.

The same sort of difficulties occur in Feynman's discussion of
temperature (see Lectures, I, 39-6, 7, 8, 9), and then again later
in chapter 41 when he discusses radiation (see Lectures, I, 41-3).
In chapter 43 Feynman alerts the student to the subtlety of the argu-
ments in kinetic theory as follows.

To get the correct numerical coefficient in Eq.
(43.13), which is correct as given, takes some care.
Without intending to confuse, we should still point
out that the arguments have a subtlety which can be
appreciated only by a careful and detailed study. To
illustrate that there are difficulties, in spite of
appearances, we shall make over again the argument
which led to Eq. (43.13) in a reasonable but erroneous
way (and the way one will find in many textbooks!).

We might have said: The mean time between
collisions is $\tau$. After a collision the particle
starts out with a random velocity, but it picks up
an additional velocity between collisions, which is
equal to the acceleration times the time. Since it
takes the time $\tau$ to arrive at the next collision it
gets there with the velocity $(F/m)\tau$. At the beginning
of the collision it had zero velocity. So between
the two collisions it has, on the average, a velocity
one-half of the final velocity, so the mean drift
velocity if $\frac{1}{2}F\tau/m$. (Wrong!) This result is wrong and
the result in Eq. (43.13) is right, although the
arguments may sound equally satisfactory. The reason
the second result is wrong is somewhat subtle, and has
to do with the following. . .

(Lectures, I, 43-5)
The careful and detailed study which Feynman recommends would reveal that the mean time between collisions, $\tau$, is not well defined in mechanics. If one is too careful, all of the derivations of kinetic theory from mechanics collapse. The moral is to be careful just the right amount. Kinetic theory is an example of a scientific theory that just slightly deviates from a euclidean structure. As such, it provides a very interesting example from a methodological perspective. Most scientific theories, especially those outside physics, have a highly non-euclidean structure.

Of course, kinetic theory can be given a euclidean structure by, for instance, simply taking the Boltzmann equation as an axiom. Similarly, statistical mechanics can be given a euclidean form by converting Boltzmann's law and Maxwell's velocity distribution into the axiom that the probability of different conditions of energy is given by $e^{-\text{energy}/kT}$. One can then totally liberate statistical mechanics from its subservience to mechanics by considering the use of probability to be fundamental and not just due to our ignorance of the actual mechanical state. This requires dropping the notion of particle trajectories and switching from velocity to momentum as the basic observable. One thus obtains a reformulation of classical statistical mechanics as an independent science. This formulation of classical statistical mechanics is incongruous with classical mechanics, but it is the natural formulation from the point of view of quantum statistical mechanics and its correspondence limit. One thus has two different ways of interpreting classical statistical mechanics depending upon whether
one views it as an approximation to classical mechanics or as an approximation to quantum statistical mechanics. Either way it has empirical problems as a model of actual gases. As was already known to Maxwell, it gives grossly incorrect results for the specific heats of gases at low temperatures. To get the specific heats right one needs to switch to quantum statistical mechanics (see Feynman's discussion in Lectures, I, 40-8, 9, 10,).

Feynman introduces his chapters on thermodynamics as follows.

So far we have been discussing the properties of matter from the atomic point of view, trying to understand roughly what will happen if we suppose that things are made of atoms obeying certain laws. However, there are a number of relationships among the properties of substances which can be worked out without consideration of the detailed structure of the materials. The determination of the relationships among the various properties of materials, without knowing their internal structure, is the subject of thermodynamics. Historically, thermodynamics was developed before an understanding of the internal structure of matter was achieved.

(Lectures, I 44-1)

While we're certainly ready to grant that scientists have gained a greatly enriched view of the "internal" structure of matter over the past century, we're less sure about the extent to which these atomic viewpoints are a secure foundation for thermodynamics or macroscopic physics in general. Feynman follows the above quote with the following.

To give an example: we know from the kinetic theory that the pressure of a gas is caused by
molecular bombardment, and we know that if we heat a gas, so that the bombardment increases, the pressure must increase. Conversely, if the piston in a container of the gas is moved inward against the force of bombardment, the energy of the molecules bombarding the piston will increase, and consequently the temperature will increase. So, on the one hand, if we increase the temperature at a given volume, we increase the pressure. On the other hand, if we compress the gas, we will find that the temperature will rise. From the kinetic theory, one can derive a quantitative relationship between these two effects, but instinctively one might guess that they are related in some necessary fashion which is independent of the details of the collisions.

(Lectures, I, 44-1)

As we've repeatedly pointed out, these classical viewpoints totally collapse in the passage to quantum mechanics. There has been a move towards presenting thermodynamics from an atomic perspective. (Compare, for instance, the text by Zemansky, Heat and Thermodynamics, with the text by Dickerson, Molecular Thermodynamics.) This is an unfortunate development, as thermodynamics is a much more general science than is atomic theory. Thermodynamics was successfully applied to radiation by Planck at the end of the nineteenth century and has recently been applied by Hawking to the geometry of spacetime. On the other hand, relating thermodynamics to statistical mechanics often yields deeper insights—a move taken by both Planck and Hawking. In general, one can go—at least in principle—from a Gibb's state in a statistical mechanical model to a thermodynamical potential in the associated thermodynamical model. The parameters occurring in the thermodynamical potential will then be given by functions of the parameters occurring in the Gibb's state. So, one at least has a reduction in the number of independent parameters
needed to describe the system. Furthermore, it is usually anticipated that at least some of the parameters occurring in the Gibb's state (such as the charge and mass of an electron) will have an applicability over a large range of models.

The following diagram summarizes the relationships between the four theories we've been considering:

Mechanics -- → Kinetic Theory -- → Statistical Mechanics -- → Thermodynamics

Mechanics, statistical mechanics, and thermodynamics can all be given clear euclidean formulations, and the association of a thermodynamical model with a statistical mechanical model is also conceptually straightforward, though usually computationally quite difficult. Kinetic theory (as usually practiced) is the non-euclidean break in this otherwise euclidean chain.
Part II. Quantum Atomic Discourse

In the last chapter we described how in quantum mechanics the angular momentum of a thing does not have an arbitrary direction, but its component along a given axis can take on only certain equally spaced, discrete values. It is a shocking and peculiar thing. You may think that perhaps we should not go into such things until your minds are more advanced and ready to accept this kind of an idea. Actually, your minds will never become more advanced in the sense of being able to accept such a thing easily. There isn't any descriptive way of making it intelligible that isn't so subtle and advanced in its own form that it is more complicated than the thing you were trying to explain. The behavior of matter on a small scale—as we have remarked many times—is different from anything that you are used to and is very strange indeed. As we proceed with classical physics, it is a good idea to try to get a growing acquaintance with the behavior of things on a small scale, at first as a kind of experience without any deep understanding. Understanding of these matters comes very slowly, if at all. Of course, one does get better able to know what is going to happen in a quantum-mechanical situation—if that is what understanding means—but one never gets a comfortable feeling that these quantum-mechanical rules are "natural." Of course they are, but they are not natural to our own experience at an ordinary level. We should explain that the attitude that we are going to take with regard to this rule about angular momentum is quite different from many of the other things we have talked about. We are not going to try to "explain" it, but we must at least tell you what happens; it would be dishonest to describe the magnetic properties of materials without mentioning the fact that the classical description of magnetism—of angular momentum and magnetic moments—is incorrect.

One of the most shocking and disturbing features about quantum mechanics is that if you take the angular momentum along any particular axis you find that it is always an integer or half-integer times $\hbar$. This is so no matter which axis you take. The subtleties involved in that curious fact—that you can take any other axis and find that the component for it is also locked to the same set of values—we will leave to a later chapter, when you will experience the delight of seeing how this apparent paradox is ultimately resolved.

(Lectures, II, 35-1)
In part I we have analyzed and criticized classical atomic discourse. In this second part of our study we turn our attention to quantum atomic discourse. In §4 we will analyze Feynman's very first discussion of quantum physics in his Lectures. This discussion occurs in I, 2, and already contains all the ambiguities of his approach. In §§5 and 6 we will analyze Feynman's systematic presentation of quantum mechanics which he gives in Lectures, III. §5 will be concerned with Feynman's approach to the fundamental ideas of quantum mechanics; while §6 will deal with the analysis of several important applications of quantum mechanics. In particular, in §6 we will want to see how Feynman's approach functions in practice.

§4. Previews of Quantum Physics

In Feynman's first discussion of quantum physics (Lectures, I, 2-6, 7, 8) he almost immediately warns the student of the difficulties of understanding microphysics.

Instead, it was discovered that things on a small scale behave nothing like things on a large scale. That is what makes physics difficult—and very interesting. It is hard because the way things behave on a small scale is so "unnatural"; we have no direct experience with it. Here things behave like nothing we know of, so that it is impossible to describe this behavior in any other than analytic ways. It is difficult, and takes a lot of imagination.

(Lectures, I, 2-6)
This kind of warning occurs repeatedly throughout the Lectures. As we've mentioned in our Introduction, we don't accept his excuses for the difficulties of understanding quantum mechanics. The difficulties arise, instead, because of a decision on his part to talk about micro-objects instead of objective probabilistic dispositions.

Feynman's very next passage exemplifies these confusions.

Quantum mechanics has many aspects. In the first place, the idea that a particle has a definite location and a definite speed is no longer allowed; that is wrong. To give an example of how wrong classical physics is, there is a rule in quantum mechanics that says that one cannot know both where something is and how fast it is moving.  

(Lectures, I, 2-6)

The second sentence implies that quantum mechanics requires a change in ontology: particles no longer have a definite location and a definite speed. On the other hand, the third sentence seems to imply that only an epistemological change is required: one cannot know both where something is and how fast it is moving. These are very different ideas. But Feynman doesn't believe that it is fruitful to make too sharp a distinction between epistemology and ontology. His opinion on this point comes out clearly in his discussion of scientific imagination (Lectures, II, 20-9, 10, 11), in particular, in the following passages.
... When I talk about the fields swishing through space, I have a terrible confusion between the symbols I use to describe the objects and the objects themselves...

... What I realized now is that when I talk about the electromagnetic field in space, I see some kind of a superposition of all the diagrams which I've ever seen drawn about them...

... We are unfortunately limited to abstractions, to using instruments to detect the field, to using mathematical symbols to describe the field, etc. But nevertheless, in some sense the fields are real, because after we are all finished fiddling around with mathematical equations—with or without making pictures and drawings to visualize the thing—we can still make the instruments detect the signals from Mariner II and find out about galaxies a billion miles away, and so on...

(Lectures, II, 20-10)

Feynman would presumably defend his presentation of microphysics by saying that the advantages gained in talking and thinking about micro-objects far outweigh the disadvantages accrued because of the resulting conceptual confusion. We don't see any way of objectively weighing the pros and cons of Feynman's approach. All we are aiming at here is a clear view of these benefits and losses. The fact that the intuitive approach has been sustained for a long time is prima facie evidence that it has a lot going for it.

Feynman next states the Heisenberg uncertainty relation, $\Delta x \cdot \Delta p \geq \hbar/2\pi$, and then uses it to explain the size of atoms:

What keeps the electrons from simply falling in? This principle: if they were in the nucleus, we would
know their position precisely, and the uncertainty principle would then require that they have a very large (but uncertain) momentum, i.e., a very large kinetic energy. With this energy they would break away from the nucleus. They make a compromise: they leave themselves a little room for this uncertainty and then jiggle with a certain amount of minimum motion in accordance with this rule.

(Lectures, I, 2-6)

Here, he seems to be drawing an ontological conclusion about the size of atoms from an epistemological principle concerning what we can know. This type of argument occurs frequently in current scientific discourse.

Feynman's next passage also contains the same type of confusions.

Another most interesting change in the ideas and philosophy of science brought about by quantum mechanics is this: it is not possible to predict exactly what will happen in any circumstance. For example, it is possible to arrange an atom which is ready to emit light, and we can measure when it has emitted light by picking up a photon particle, which we shall describe shortly. We cannot, however, predict when it is going to emit the light or, with several atoms, which one is going to. You may say that this is because there are some internal "wheels" which we have not looked at closely enough. No, there are no internal wheels; nature, as we understand it today, behaves in such a way that it is fundamentally impossible to make a precise prediction of exactly what will happen in a given experiment.

(Lectures, I, 2-6)

This passage would be fine if Feynman accepted the notion of nature supporting an objective probabilistic disposition. But such a notion he clearly rejects (see our Introduction). Thus, he often seems to imply that nature is engaged in an unbreakable conspiracy to keep us
from knowing what she's really up to. This strange worldview comes out even more clearly in Feynman's later discussion of the uncertainty principle.

The uncertainty principle describes an inherent fuzziness that must exist in any attempt to describe nature. Our most precise description of nature must be in terms of probabilities. There are some people who do not like this way of describing nature. They feel somehow that if they could only tell what is really going on with a particle, they could know its speed and position simultaneously. In the early days of the development of quantum mechanics, Einstein was quite worried about this problem. He used to shake his head and say, "But surely God does not throw dice in determining how electrons should go!" He worried about that problem for a long time and he probably never really reconciled himself to the fact that this is the best description of nature that one can give. There are still one or two physicists who are working on the problem who have an intuitive conviction that it is possible somehow to describe the world in a different way and that all of this uncertainty about the way things are can be removed. No one has yet been successful.

The necessary uncertainty in our specification of the position of a particle becomes most important when we wish to describe the structure of atoms. In the hydrogen atom, which has a nucleus of one proton with one electron outside of the nucleus, the uncertainty in the position of the electron is as large as the atom itself! We cannot, therefore, properly speak of the electron moving in some "orbit" around the proton. The most we can say is that there is a certain chance \( p(r) \Delta V \), of observing the electron in an element of volume \( \Delta V \) at the distance \( r \) from the proton. The probability density \( p(r) \) is given by quantum mechanics.

For an undisturbed hydrogen atom \( p(r) = A e^{-r^2/a^2} \), which is a bell-shaped function like that in Fig.6-8.

The number \( a \) is the "typical" radius, where the function is decreasing rapidly. Since there is a small probability of finding the electron at distances from the nucleus much greater than \( a \), we may think of \( a \) as "the radius of the atom," about \( 10^{-10} \) meter.

We can form an image of the hydrogen atom by imagining a "cloud" whose density is proportional to the probability density for observing the electron. A
sample of such a cloud is shown in Fig. 6-11. Thus our best "picture" of a hydrogen atom is a nucleus surrounded by an "electron cloud" (although we really mean a "probability cloud"). The electron is there somewhere, but nature permits us to know only the chance of finding it at any particular place.

In its efforts to learn as much as possible about nature, modern physics has found that certain things can never be "known" with certainty. Much of our knowledge must always remain uncertain. The most we can know is in terms of probabilities.

(Lectures, I, 6-10, 11)

Feynman's statement that "The electron is there somewhere, but nature permits us to know only the chance of finding it at any particular place" is totally incorrect from the point of view of the careful approach to quantum mechanics. But it forms the core of his alternative intuitive approach. Feynman is definitely aware that he is making a decision against the careful approach (see his book Quantum Mechanics and Path Integrals, pp. 13 and 14).

These problems aren't limited to Feynman's informal discussions of quantum mechanics in volumes I and II of his Lectures, but also pervade his more serious discussions in volume III, to which we will now turn.
§5. The Fundamental Principles of Quantum Mechanics

In this section we begin our analysis of Feynman's systematic discussion of quantum mechanics in the third volume of his Lectures. Feynman immediately prepares us for the worst by making his usual statements concerning the peculiar and mysterious aspects of quantum behavior (see Lectures, III, 1-1 and 3-1). Chapter 1 contains a detailed discussion of the two-slit experiment, first with bullets, and then with water waves, and then "with electrons." (We've put quotes around this phrase because it's exactly the use of such phrases that causes much of the difficulty of comprehending quantum phenomena.) Feynman's description of the two-slit experiment with electrons begins as follows.

Now we imagine a similar experiment with electrons. It is shown diagrammatically in Fig. 1-3. We make an electron gun which consists of a tungsten wire heated by an electric current and surrounded by a metal box with a hole in it. If the wire is at a negative voltage with respect to the box, electrons emitted by the wire will be accelerated toward the walls and some will pass through
the hole. All the electrons which come out of the gun will have (nearly) the same energy. In front of the gun is again a wall (just a thin metal plate) with two holes in it. Beyond the wall is another plate which will serve as a "backstop." In front of the backstop we place a movable detector. The detector might be a geiger counter or, perhaps better, an electron multiplier, which is connected to a loudspeaker.

![Interference experiment](image)

(Lectures, III, 1-4)

This description makes essential use of the classical notion of an electron as a small charged object. The result of the experiment is that the probability "of the arrival of an electron" is given by the interference curve (c) in Fig. 1-3. The central difficulty in comprehending quantum mechanics is that the classical notion of an electron as an object leads one to expect that \( P_{12} = P_1 + P_2 \), which turns out to be false. Various hidden variables theories are capable of obtaining the correct distribution for \( P_{12} \), but are unsatisfactory for other reasons. One wants to be able to treat an electron as an object in order to provide a rationale for holding together the constellation of phenomena called electron phenomena. Also, such an approach has heuristic value in setting up the mathematics to describe various situations. Feynman's solution to this
dilemma is to tacitly introduce the notion of a quantum mechanical object. Feynman summarizes the basic principles of quantum mechanics as follows.

(1) The probability of an event in an ideal experiment is given by the square of the absolute value of a complex number \( \phi \) which is called the probability amplitude:

\[
P = \text{probability}, \\
\phi = \text{probability amplitude}, \\
\mid \phi \mid^2
\]

(2) When an event can occur in several alternative ways, the probability amplitude for the event is the sum of the probability amplitudes for each way considered separately. There is interference:

\[
\phi = \phi_1 + \phi_2, \\
P = \mid \phi_1 + \phi_2 \mid^2.
\]

(3) If an experiment is performed which is capable of determining whether one or another alternative is actually taken, the probability of the event is the sum of the probabilities for each alternative. The interference is lost:

\[
P = P_1 + P_2
\]

(Lectures, III, 1-10)

The problem in understanding Feynman's approach is to comprehend exactly what it means for an event to be able to occur in several alternative ways. This is a problem because if a measurement is not made to determine which alternative actually occurs, then one cannot say that one alternative or the other is really occurring (see Lectures, III, 1-9 and 18-9). What Feynman does in practice is to introduce in effect a new quantum mechanical notion of
interfering alternatives, i.e. a new quantum mechanical understanding of the logical connective "or." (See Quantum Mechanics and Path Integrals, pp. 13 and 14, for an explicit statement of this point.) Thus, instead of taking the careful approach and viewing quantum mechanics as requiring a new form of generalized probability theory with its associated new ontology, Feynman retains to a great extent the old, comfortable, classical ontology and merely changes his logic. His approach has a great deal of heuristic power; but this heuristic power can be easily assimilated into the careful approach without taking the interfering alternatives seriously. But Feynman often does take them quite seriously. This will be clearly seen in the next section in our discussions of specific examples - especially the first one.
§6. Applications of Quantum Mechanics

In this section we are going to look at a number of specific applications of quantum mechanics from both the intuitive and careful points of view. This will allow us to see and compare how both approaches function in practice.

Example 1. The Bohm – Aharonov Effect

That Feynman is prone to attribute some kind of physical reality to unseen, interfering trajectories comes out most clearly in his discussion of the Bohm – Aharonov effect (Lectures, II, 15-7-14). The main issue in that discussion is whether the vector potential is a "real" field—in the sense of being useful for avoiding the notion of action at a distance— or merely a useful mathematical device. In classical theory it is usually viewed as merely a useful mathematical device for computing the magnetic field, but Feynman argues that in quantum mechanics it has greater physical significance. His main assumption occurs in the following passage.

Now we would like to state the law that for quantum mechanics replaces the force law $F = qvxB$. It will be the law that determines the behavior of quantum-mechanical particles in an electromagnetic field. Since what happens is determined by amplitudes, the law must tell us how the magnetic influences affect the amplitudes; we are no longer dealing with the acceleration of a particle. The law is the following: the phase of the amplitude to arrive via any trajectory is changed by the presence of a magnetic field by an amount
equal to the integral of the vector potential along
the whole trajectory times the charge of the par-
ticle over Planck's constant.

\[ \text{Magnetic change in phase} = \frac{q}{\hbar} \int_{\text{trajectory}} A \cdot ds. \quad (15.29) \]

(Lectures, II, 15-9)

From this assumption it easily follows that if one alters the two-
slit experiment by including a magnetic field localized to a small
region between the two slits, then there will be a shift in the re-
sulting interference pattern. This shift occurs even if the pro-
bability of the particle passing directly through the region of
non-vanishing magnetic field is essentially zero. From this Feynman
concludes that in quantum mechanics the vector potential is a real
field and not merely a useful mathematical device.

The conclusion is somewhat different when one takes the point
of view of the careful approach to quantum mechanics. In the careful
approach the trajectories that Feynman is talking about are not them-
selves real, but merely a useful mathematical device for determining
the interference pattern. Thus, they can in no way add reality to
the vector potential. The main correct point is that it is the vector
potential and not the magnetic field that occurs in the new law
(15.29). But it is natural to consider a useful mathematical device
as having "physical" significance in order to help justify its utility.
Also, taking such a physical point of view is often a useful mathe-
matical heuristic. This comes out clearly in Feynman's applications
of path integrals to statistical mechanics, (see his book Statistical
Mechanics), where he presents a physical view point which is clearly
not intended to be taken literally. The reader should also examine
Feynman's outlines of his path integral approach to quantum mechanics and its relationship to classical mechanics, (Lectures, I 26-8 and II, 19-9).

Example 2. Scattering Experiments

In Lectures, III, 3-3 Feynman discusses the scattering of neutrons by a crystal. His basic assumption is that low-energy neutrons are scattered by the nuclei of the atoms in the crystal without leaving any record of the scattering process except in the case of scattering with spin flip. This assumption readily leads to an adequate qualitative understanding of the empirical results of such scattering experiments. The only problematic aspect of his discussion is the exact status of the scattering process itself. This is because if one couldn't determine even in principle off of which nucleus the neutron scattered, then in the careful approach one couldn't say that it actually scattered off of any of the nuclei at all. But the success of the heuristic of thinking about quantum mechanical scattering processes inevitably leads to taking such processes seriously. Feynman's intuitive language is much more comfortable and suggestive in cases like this than is the language one would have to use if one were dogmatically following the careful approach. The careful approach requires that one not take seriously (except as heuristic devices) most of the entities with which physicists currently populate the world.

In his next section (3-4) Feynman discusses the scattering of identical particles. His heuristic here is that one must add or
subtract the amplitudes for alternative processes in which the two identical particles simply exchange roles. This heuristic leads to the correct empirical results, but still leaves one somewhat dissatisfied concerning one's insight into the exact difference between the cases of distinguishable and indistinguishable particles. The use of the word particle here is what causes the confusion. If one switches to the careful approach and only talks seriously about the results of the experiments, then one clearly sees that scattering experiments with distinguishable particles have a different space of results than do scattering experiments with identical particles. It is this difference in the space of results that is of central importance in understanding the difference between the two cases. Still, Feynman's heuristics allow him to make more precise predictions and serve to interrelate variations of the experiments. One sees again that for clear understanding one must use the careful approach, but for most other purposes the intuitive approach is preferable. Thus, the best approach is a mixed approach using as appropriate either the careful or the intuitive approaches.

Example 3. Stern - Gerlach Apparatus

In chapter 5 Feynman introduces the full machinery of quantum mechanics in order "to describe a quantum mechanical phenomenon in a completely quantum mechanical way" (Lectures, III, 5-1). He goes on to promise that he intends "to talk about something new in a new language." Sure enough, the mathematics Feynman uses is completely quantum mechanical. But the informal language accompanying the mathematics is still largely classical. And it is this clash that is
responsible for the incoherence residing in his presentation. On the very next page after the above promises we find the following.

You can see then that the first apparatus has produced a beam of "purified" objects-atoms that get bent upward in the particular inhomogeneous field. The atoms, as they enter the original Stern-Gerlach apparatus, are of three "varieties," and the three kinds take different trajectories. By filtering out all but one of the varieties, we can make a beam whose future behavior in the same kind of apparatus is determined and predictable. We will call this a filtered beam, or a polarized beam, or a beam in which the atoms all are known to be in a definite state.

(Lectures, III, 5-2)

And later we find the even more problematic passage:

In other words, as careful as we have been to make sure that we have the atoms in a definite condition, the fact of the matter is that if it goes through an apparatus which is tilted at a different angle it has, so to speak, to "reorient" itself - which it does, don't forget, by luck.

(Lectures, III, 5-56)

The conceptual difficulties of comprehending the functioning of Stern-Gerlach apparatus from these essentially classical points of view were already pointed out by Einstein and Ehrenfest right after the original Stern-Gerlach experiments in the early 1920's. These difficulties are eventually "resolved" by Feynman by simply appealing once more to "the old, deep mystery of quantum mechanics - the interference of amplitudes" (Lectures, III, 5-10). In the analysis of the functioning of Stern-Gerlach apparatus, the incoherence accompanying the intuitive approach is not compensated for by any redeeming advantages.
Example 4. **Barrier Penetration**

In Lectures, III, 7-7, 8 Feynman discusses the phenomenon of quantum mechanical penetration of a barrier. In particular, in discussing the explanation of α-particle decay of a uranium nucleus he makes the following statement.

> How is it then that in radioactive decay we find α-particles which started out inside the nucleus coming out with the energy E? Because they start out with the energy E inside the nucleus and "leak" through the potential barrier.  
> *(Lectures, III, 7-8)*

The picture suggested by this language is that of an essentially classical particle whose dynamics is somewhat unclassical in that it can "leak" through a potential barrier. This is exactly the appropriate language to use for describing the behavior of particles in such hidden variables theories as those of Bohm or Nelson. But Feynman repeatedly rejects the hidden variables approach to quantum mechanics. If an α-particle is described quantum mechanically by an amplitude as in Fig. 7-6(b), p. 7-8, then, in the careful approach one should not say that the α-particle "started out inside the nucleus." In the careful approach the α-particle isn't anywhere. Feynman knows this; he made this point clearly in his careful discussion of the two-slit experiment. But this point is so unpleasant that it is avoided whenever possible. *The Feynman Lectures* essentially uses language that is very similar to that used by hidden variables theorists. But Feynman's intuitive approach should
not be classified with the other hidden variables approaches. His motivation is entirely different. He totally accepts the standard quantum mechanical algorithms, but wants to supplement them with physically suggestive and heuristically powerful informal language which, on occasion, he takes rather lightly. This example of barrier penetration is a case where Feynman's language is certainly more satisfying than is the language of the careful approach. (See, also, Lectures, III, 8-12).

Example 5. Molecular Structure

In chapter 10 Feynman gives a very interesting discussion of the hydrogen molecular ion. He first defines the molecule as follows: "A positively ionized hydrogen molecule consists of two protons with one electron worming its way around them" (10-2). This is again an essentially classical picture; but is not meant to be taken too seriously. He summarizes his results concerning this ion as follows:

In our theory of the $H^+$ ion we have discovered an explanation for the mechanism by which an electron shared by the two protons provides, in effect, an attractive force between the two protons which can be present even when the protons are at large distances. The attractive force comes from the reduced energy of the system due to the possibility of the electron jumping from one proton to the other. (Lectures, III 10-5)

These electron jumps occur neither in "the real world," nor in the quantum mechanical mathematics. They occur only in Feynman's (and most other scientists') informal discourse. Feynman says something like this on page 10-4:
This kind of chemical binding is also often called "quantum mechanical resonance" (by analogy with the two coupled pendulums we have described before). But that really sounds more mysterious than it is, it's only a "resonance" if you start out by making a poor choice for your base states— as we did also! If you picked the state $|\text{II}\rangle$, you would have the lowest energy state—that's all.

(Lectures, III, 10-4)

But Feynman interprets the lowest energy state $|\text{II}\rangle$ as describing a jumping electron. Let us take a closer look at the empirical situation. Suppose that we have position measuring devices for electrons and protons, and that we can measure the positions of the protons "without disturbing" the electrons. (This is at least theoretically possible.) If we have a situation with just two protons, then we can observe them repelling one another, e.g., we might obtain the following picture:

A:

[Diagram of two protons repelling]

If we have a situation with two protons and one electron, then we might observe something like the following:

B:

[Diagram of a proton and an electron]
Here, the electron (in blue) binds to one of the protons and there is a small dipole attraction between this neutral object and the other proton. If we have a situation with two protons and one electron, but only observe the protons and not the electron in a way that does not "disturb" the electron, then we might obtain the following picture:

C:

This case would be the formation of $\text{H}_2^+$. Note that the electron is not observed in this case, though if we were to look for it we would find it near one of the protons.

Now the problem is how to talk intelligibly about the difference between case A and case C. In case A there are no electrons around, neither visible nor invisible nor even "virtual." In case C we might like to say that there is an invisible electron there binding the two protons together; and this kind of statement is fine in the context of such hidden variable theories as those of Bohm or Nelson. But if one rejects hidden variables theories, then one can't say that the electron is really near one of the protons but just unseen (recall Feynman's discussion of the two-slit experiment). Instead, in the careful approach one must say that there is a probabilistic disposition such that if you were to look you would find the electron near one of the protons with equal probability. But this disposition is not just subjective. It isn't simply that you don't know where the electron is and are forced out of ignorance to make probability statements. The objective existence of this disposition shows up
clearly in this case via the binding of the two protons; when the disposition is not there, the protons repel. So the disposition is "really there" and one might say - if one wanted to use causal language - that the disposition binds the two protons together. But most physicists prefer a different, more colorful language. They say that the electron is there "virtually" and the constant exchange of this virtual electron is what binds the protons together. This use of virtual language allows them to obtain the intuitive advantages of the hidden variables theories, while simultaneously rejecting the hidden variables mathematics. This language of virtual processes and resonances is powerful, comfortable, and suggestive; its only problem is that it is somewhat misleading as to what is actually "out there" in the real world and what is only a useful picture on paper. The user often has difficulty keeping this distinction straight. But, possibly, this distinction isn't all that important anyway. (The reader should also examine Feynman's discussions of nuclear forces (10-2) and the benzene molecule (10-4) for two other characteristic applications of the use of the language of virtual processes and resonances.)

Feynman's discussion of the hydrogen molecular ion also provides beautiful examples of "mixed" arguments (10-3, 5). The correction in the energy due to the electrostatic interaction of the two protons or of one proton with the other neutral atom is an essentially classical argument. In particular, the hydrogen atom is being treated as a classical neutral object. Of course, one could try to treat this situation purely quantum mechanically, but that would be much more difficult. (One would have to solve directly for the stationary
solutions of the Schrödinger equation for an electron in the fields of two protons a fixed distance $D$ apart.) In any case, once one obtains the interaction energy as a function of $D$ (the interproton distance), one determines the equilibrium interproton spacing for this molecule. The success of such mixed arguments is one of the forces preventing a purely quantum mechanical (i.e., careful) mode of discourse from supplanting the current mixed discourse. One of the main advantages of Feynman's intuitive approach is that it renders less painful the scientist's constant shifting back and forth from classical to quantum mechanical perspectives.

**Example 6. Electric Current and The Hall Effect**

One of the most popular of scientific explanations is that electric current flowing in a wire is due to the drift of conduction electrons in the wire. Feynman gives just such a description (*Lectures*, II, 13-7). But in *Lectures*, III, 13-1 we learn that the actual explanation for why metals conduct electricity is much more subtle. Feynman first points out that from the classical point of view one wouldn't expect metals to be good conductors because the atoms are packed so close together that the conduction electrons would have a very small mean free path. The resolution of this mystery requires quantum mechanics. The conduction electron's amplitudes go "pip-pip-pip from one atom to the next, working its way through the crystal" (*Lectures*, III, 13-7). So, a current in a wire is not due to a flow of electrons in a classical sense, but, instead, it is due to a flow of probability amplitude.
One of the striking things about Feynman's discussions is that in setting up his models his discourse is almost entirely classical in character. The only non-classical elements of his discussions are the use of probability amplitudes and transition amplitudes instead of probabilities and transition probabilities. Until he explicitly introduces these amplitudes one cannot tell whether the models are going to be classical or quantum mechanical.

On page 13-7 Feynman shows how to relate the above quantum mechanical picture to a more traditional classical picture. The main thing to notice is that in the resulting classical picture, the classical particles which seem to flow through the crystal will have properties (e.g., its mass $m_{\text{eff}}$) that depend on the crystal. In principle, these properties could be related back to the fundamental properties of an electron and the nature of the crystal. In practice, such a derivation is intractable. One thus ends up working with a great variety of non-fundamental particles and their empirical properties. Thus, the goal of a unified treatment is restricted in practice to the fact that the same kind of mathematics keeps arising. This point readily emerges from Feynman's discussion of holes and excitons on page 13-9.

It turns out that even the above quantum mechanical explanation isn't adequate for explaining the flow of electric current in all metals. On page 14-7 Feynman discusses an experiment, the Hall effect, which is capable of determining the sign of the carrier of electric current. And this experiment shows that in some metals - such as beryllium - the "objects" responsible for the conduction have
a positive charge. These objects are interpreted to be "holes," since the physicists consider the only relatively free objects in these substances to be electrons. As Feynman puts it: "...it is ultimately the electrons in the crystal which do the moving..." (14-7). This statement of Feynman's shows clearly the extent to which he has retained an essentially classical ontology. From the careful point of view, one might treat these positive charge carriers directly without trying to reduce them to electrons (a task which is in any case intractable). Just such a change in perspective has occurred in the physicist's understanding of positrons. But to admit such a move in this context is to accept the pluralistic universe, i.e. a variety of irreducible descriptions, and that is still not acceptable to most physicists.
Conclusion: No Ideal is Inviolable

Our main goal in this work has been to explore and explicate the tensions and compromises involved in actual scientific practice as they are exemplified in The Feynman Lectures. Our specific concern has been with the worldview of atomic physics, and the problems caused by the physicist’s distaste for the careful approach to quantum mechanics. Feynman’s intuitive approach allows the physicist to preserve much of his comfortable classical ontology, even if it is at the expense of introducing a new quantum mechanical logic (a new quantum mechanical notion of the logical connective "or"). We thus see that even the ideals of clarity and consistency associated with the basic concept of an exact science can be violated.