

An Interpretive Introduction to Quantum Field Theory

# An Interpretive Introduction to Quantum Field Theory

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QUANTUM FIELD THEORY is a notoriously hard theory to learn. The best physics students do well with it, but many able students flounder, eventually resigning themselves to going through the motions with their problem sets to make it through the course. Among philosophers of physics, I have heard many colleagues express interest, only to learn a year or two later that they had somehow gotten involved in other things.

I too have found the subject extremely difficult, and after much effort, I have managed to understand only some of the basics. My impression is, however, that it should not be so hard to grasp at least the leading ideas of the subject. I suspect that the difficulty in getting started with quantum field theory is due to two stumbling blocks: Most presentations aim to get students started at solving problems, and the wealth of detailed physics makes the leading ideas harder to understand. This circumstance is compounded by what I see as a pedagogical tradition that has not moved as much as it might have beyond the historical circumstances in which the theory developed. If I had had to learn nonrelativistic quantum mechanics from Heisenberg's 1930 Chicago lectures, I don't think I would have ever gotten off home base. Today that subject has been made vastly more accessible. I feel there hasn't been as much progress in making the ideas of quantum field theory clear.

This book represents my attempt to understand the basic ideas and first moves in building up quantum field theory. I stress that I do not go very far into the subject. Indeed, the ideas presented here were all in place by 1950. I give only a one-paragraph sketch of the now-popular functional methods, and I do not deal at all with gauge theories, in particular the electro-weak theory and QCD (quantum chromodynamics) that comprise what practitioners call the "standard model," surely one of the greatest achievements of physics. Nonetheless, understanding what I have presented here is a prerequisite for understanding the more recent developments.

I hope that my presentation will serve two audiences. First, I write for other interested interpreters of physics who have been fascinated by the conceptual problems already present in nonrelativistic quantum mechanics but who have found quantum field theory too forbidding. Second, I hope the book will be useful to physics students who are VIII PREFACE

having trouble seeing through all the details to get an idea of how, in outline, it is all supposed to work. Students looking for this sort of instructional aid can skip chapters 2 and 5, which are largely devoted to interpretation. More generally, I hope this book will be read by those physics students who have an interest in the conceptual structure of their subject matter, by those who can make good use of relatively easy entrée into this subject, and by those who do not plan to learn the subject in any detail but want to get a quick view of leading ideas.

Although much of the detail of the physics has been stripped away to get at the basic ideas, I have worked hard to make the presentation responsible. Physics students who start with this book should not have to unlearn anything presented here if they go on to study the subject in more detail.

Beyond introducing readers to the most basic rudiments of quantum field theory, I also struggle, as a philosopher, to understand "what it all means." As an interpreter of science I see myself tackling a number of interconnected jobs: I hope to help clarify the ideas of the science as it presently exists, to compare and contrast these clarified ideas with other domains of our understanding, and finally to examine critically both present expositions of the science's ideas and on occasion the ideas themselves, wherever I can with my limited knowledge. Often these jobs call for stripping away much of the detail that constitutes good physics so that we can more easily see the conceptual structure. For example, for decades talk of "discarding infinities" in renormalization has seemed mysterious, even absurd. Many students and most interpreters of physics have been unable to see how renormalization works because all responsible presentations presuppose mastery of a lot of physics and lie buried under a mass of computational detail. When the detail is stripped away, it all turns out to be neither so very hard nor, on several plausible readings, at all mysterious. The last chapter, on renormalization, essentially a reprint of my "Infinite Renormalization" (see the Bibliography), can be read without reading the rest of the book. Indeed, one need have only a vague idea of what an integral is and otherwise not know any physics.

Here also is a sketch of some of the further bill of interpretive fare. Starting from a prequantum particle concept, I see what quantum theories take away: What is left of the idea of a particle? I then explore the relationship between the residue of the particle concept and a field-theoretic quantum description. Along the way I discuss common miscon-

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ceptions about quantum field theories, for example, concerning Feynman diagrams, so-called 'virtual particles', and problems about understanding the role of field-theoretic ideas. Many other interpretive problems are located in the basic framework but are left as "excercises for the reader."

The book assumes only that the reader knows the basics of conventional nonrelativistic quantum mechanics and presupposes no background in philosophy at all. In notation, boldface signifies 3-vectors, and a caret over a symbol signifies that the symbol denotes an operator. Throughout I use Dirac notation. Although Dirac notation can sometimes take a little skill in sure use to get correct answers, it is very easy to read. For those entirely unfamiliar with Dirac notation, I highly recommend ten minutes' work reading Merzbacher (1970), pp. 309–11, for a wonderfully clear and brief exposition at just the level needed here. Readers will find expositions in a great many other introductions to quantum mechanics.

Another matter of notation concerns the special use of quotation marks, which does not agree with usual editorial practice. In interpretive work it is important to respect what in philosophy we call the use/mention distinction. For example, I can contrast talk about my dog with talk about the word 'dog'. As I have done in the last sentence, in philosopy we use single quotation marks to turn a word into a name of that word: Thus I mention the word 'dog' when I use it to talk about my dog. The notational headache arises because quotation marks have several other uses, and common editorial practice disregards the use/mention distinction. In preparing the manuscript I have used the following conventions. When I mention, as opposed to use, a word or other symbol, I turn the word or symbol into a name of the word or symbol by surrounding it with single quotation marks. Because the resulting expression is a name of the word or symbol, punctuation marks go outside the closing quotation mark. However, I recognize two exceptions to this convention. When reporting words as someone has spoken or written them, I follow standard usage and use double quotation marks, with punctuation marks inside the closing quotation mark. When introducing a word or phrase for the first time I italicize it instead of quoting it. Finally I also use double quotation marks as "shudder quotes," to indicate to the reader that the word or phrase is being used in some special way: ironically, with a special meaning, or just to indicate that the discussion is calling the customary usage of the word into question.

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(Readers may test their grasp of these conventions by determining which is the convention I have *not* illustrated in this paragraph!)

Let me include a few words on the process of writing this book. Chapter 2 is an extensively rewritten version of a paper coauthored with Michael Redhead (1991). Half of the material in chapter 5 has appeared in Teller (1990), and chapter 7 includes only minor changes from Teller (1989). In ever would have succeeded in attaining the modest level of understanding of the subject represented here without the remarkably extensive and patient help I have received from Michael Redhead and Gordon Fleming. Writing this book was a collaborative effort. Much of the work involved my struggling to understand while Redhead and Fleming struggled to explain, to make suggestions, and to present a spectrum both of interpretive ideas and of clarification of the physics. Although I think that neither of them would have put this material together in anything like the way I have tried to do, I cannot stress enough how many details they have contributed and to what extent the ideas presented here have developed as a result of our dialogue.

With great unease I venture to mention the extensive help I have received from many others, unease because I know I will have neglected to mention some, and I want them also to know how much I value their assistance. Tim Maudlin and Jeremy Butterfield commented extensively on the entire manuscript, with immeasurable resulting improvement in the work. I also received more assistance than I can begin to detail from Jim Cushing, Andrew Elby, Carl Haefer, John Halpin, Pere Kosso, Martin Jones, Steven Leeds, Don Robinson, Alexander Rüger, Simon Saunders, Bas van Fraassen, Andrew Wayne, Bob Weingard, and Linda Wessels.

Finally, this work would never have been accomplished without the leave made possible by grants from the National Science Foundation. Much of the manuscript was put together in 1989 while holding grant no. SES 8706261. I did the work on renormalization, as well as a great deal of basic background training, four years earlier, under grant no. SES 8217092.

<sup>&</sup>lt;sup>1</sup>I am very grateful, respectively, to Foundations of Physics, Philosophical Topics, and Philosophy of Science for their permission to reprint and use material; and also to Michael Redhead for permission to use material from our joint article.

An Interpretive Introduction to Quantum Field Theory

## Preliminaries and Overview

QUANTUM FIELD THEORY CONSTITUTES physics' best description of the world in the small. By reputation, it's a strange theory. Among many items, we hear of "virtual particles" said to populate empty space and to mediate interactions. There is dark talk of mathematical black magic, of "throwing away infinities." Quantum field theory is also a hard theory to learn. Physics texts tend quickly to plunge students into the technical niceties of complex calculation. Many students and curious outsiders have a difficult time seeing what the theory seems to be saying about the world.

In this book I will try to improve our interpretive fix on quantum field theory. This job can't be done with metaphors, which are inevitably misleading, so I will present a technical outline of the most basic ideas. I will strip the outline of details so that we can begin to see the theory's profile, yet I will strive for accuracy so that those wishing to fill in the outline will not have to unlearn things learned here.

This chapter sets the scene. I will start with a few paragraphs about my prejudices on the nature and function of scientific theories, so that readers can see more clearly what kind of a project I have in mind when I talk about "interpretation." We'd also better have a few words about general issues in interpreting quantum theories. I can't be both brief and synoptic, but by getting my presumptions out on the table we will have fewer misunderstandings, and those holding other views can in many places make adjustments for themselves. Finally, this chapter will provide an overview of the project, a kind of plot synopsis that should help one to see how subplots fit into the larger story.

### THEORIES AND THEIR INTERPRETATION

An older view of theories took them to be composed of laws of unlimited generality and (for correct theories) unqualified truth. From these laws, including ones connecting "observational terms" with "theoretical terms," all specialized applications were supposed to follow by strict deduction.

There never have been, are not now, and most likely never will be interesting scientific theories fitting this description. For example, we know that both classical mechanics and the special theory of relativity give accurate descriptions only within restricted domains. The same goes for general relativity when quantum phenomena are brought into consideration. Yet these are examples of our sterling best in science.

More importantly, this older view of theories does not do justice to the practice of science. Rarely does one simply calculate from first principles. Instead, application involves a delicate process of tinkering to get a satisfactory fit with the facts. For example, often one will linearize, that is use only the linear terms in a power series expansion. Linear expressions are comparatively easy to work with, and often this ease is worth the cost in accuracy. The trick is to linearize the right expansions and to know the limits of the resulting approximation.

We describe these facts about theories and their application more faithfully by saying that science provides various kinds of models. For our purposes I don't need to take any very specific stand on what models are. But one approach is to view them as abstract objects embodying just those properties and relations which are of interest to us in the physical systems to be modeled. For example, the "simple harmonic oscillator" can be taken to be a "system" characterized by two variables, t and x, with t invariably interpreted as the time and satisfying the relation  $\frac{d^2x}{dt^2} = -cx$ , for constant c. This "simple harmonic oscillator" can be used to model various physical systems, such as oscillating pendulums and springs.

On this account of science, models are never expected to correspond exactly to physical systems. Instead we think of a model as a little toy universe, a limited way parts of the world might be, which is supposed to be similar to the ways those parts really are in certain respects and to certain degrees. The similarity is always limited to certain aspects of the situation, aspects either explicitly stated or made clear by the context. The similarity is also limited as to its degree. A real pendulum only approximates the simple harmonic oscillator model, and then only for small displacements. Usually a model is not exactly like the things modeled even in the aspects modeled. A model is useful only insofar as the failings of similarity are not of practical concern.

<sup>&</sup>lt;sup>1</sup>My outlined account of models and theories closely follows Giere (1988), ch. 3. Ideas similar in many respects can be found in Cartwright (1983).

This view makes sense of the practice of science. Laws are not eternal truths to be used only as premises in deductions. Instead they are like basic dress patterns, to be tailored to suit the idiosyncrasies of the different customers. Properly tailored laws work together to form a model. Theories, in turn, are collections of models that have been loosely grouped both according to phenomena to be modeled and according to common technical tools used in building the models. On this description of theories, their boundaries are not sharp, and grouping can be somewhat arbitrary.

This view also makes sense of the historical succession of theories. A new theory (for example, special relativity) is seen as an improvement over a former theory (for example, classical mechanics) if there is a significant domain of phenomena more accurately modeled by the new than by the old theory. Note that special relativity improves on classical mechanics in certain respects, while nonrelativistic quantum mechanics does so in others. We should expect no neat linear succession if the point of theories is to provide models similar to the things modeled in a variety of ways.

It's worth mentioning that this way of thinking about theories applies nicely to various prescientific, or extra-scientific, ways of thinking about the world. For example, quite apart from classical mechanics, we tend to think of physical objects as composed of some infinitely divisible material substance in which properties "inhere" and which provides or fixes the objects' identity. This description outlines a type of model that agrees, to various degrees, with the nature of physical objects in certain ways and not at all in others.

Indeed, this view of theories, as a kind of model of scientific theorizing, applies very nicely to itself.

Given this view of what theories are and how they work to describe things, what constitutes a theory's interpretation? I take an interpretation to be a relevant similarity relation hypothesized to hold between a model and the aspects of actual things that the model is intended to characterize. By extension, an interpretation of a theory is a programmatic sketch for filling out interpretations of the theory's constitutive models. Theories rarely come from the hands of their creators completely uninterpreted. At the very least theory builders have in mind, either explicitly or tacitly, experimental procedures for testing their

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theories. What then are we, as interpreters of science, doing when we "interpret" an already stated theory? We are elaborating in any way that clarifies, sharpens, or extends the similarity relation between a model (or models loosely thought of as comprising a theory) and the things described by the theory.

I intend this view of theories to be neutral on the issue of realism. To sketch the realism issue, let's try to draw a distinction between the things we see quite directly, such as chairs and volt meters, and things we detect only very indirectly, such as electrons and electric charges. Although there is latitude in how one might draw this distinction, for the moment take it as given. Most of us have no worries about the existence of things on the chairs and volt meter side of the distinction. But what sort of attitude should we take toward the apparent reference to "unobservable" things like electrons and their charges?

My sketched view of theories and their interpretation can be elaborated in different ways, with some elaboration for each of the responses to the realism question. For example, instrumentalism specifies that interpretation be limited to parts of theories that are supposed to correspond to "observable" things. Constructive empiricism softens this stand a little: say what seems plausible and useful about the "unobservables," but be agnostic about any literal reading of such parts of the similarity relations. Full-blown realism seeks to fill in the similarity relation for the terms putatively referring to "unobservable" as well as "observable" things and properties. Finally, one may reject the whole issue of realism, for example, on the ground that there is no reasonable distinction between the observable and unobservable, which the whole issue presupposes. 3

As this flexibility on the realism issue shows, I have provided no more than a first rough sketch of an attitude toward theories and their interpretation. I hope thereby to have said enough to show what I take myself to be doing when I try to contribute to the "interpretation" of quantum field theory. At the same time I hope to have said little enough so that readers of a wide range of persuasions can fill in consistently with their own, more detailed understanding of the nature of theories.

<sup>&</sup>lt;sup>2</sup>See van Fraassen (1980).

<sup>&</sup>lt;sup>3</sup>Arthur Fine presents a view similar to this, though on different grounds. See his "The Natural Ontological Attitude" and "And Not Antirealism Either" in *The Shaky Game* (1986).

## Two General Issues of Interpretation for Quantum Theories

Superposition is a hallmark of all quantum theories. But what are superpositions?

Consider a system, say a particle, and two properties, such as two values of a quantity for which the system can have values, for example, two values for spin in the z direction. Then there is a third property, formally described by forming the weighted sum of standard descriptions of the two original properties. In our example this will be a value for spin in some new direction. Although, in the first instance, superposition is a formal manipulation of mathematical descriptions, when the above relations hold I will also speak of the third property's being a superposition of the first two properties. The same kind of gloss goes for superposition of many or of a continuum of properties, or values for quantities.

How should such superposition of properties be understood? To explain my attitude, I need to say a few words about dispositions. Consider the relation between being fragile and actually being broken, or between being flammable and actually being in flames. Fragility and flammability are examples of dispositions, and being broken and being in flames are examples of what I will call a disposition's display property. A fragile glass is broken when struck by a rock. A flammable piece of paper ignites when touched with a lit match in the presence of oxygen. Being struck by a rock and being touched with a lit match in the presence of oxygen are examples of what I will call activating conditions. In general, by a disposition of an object I mean a state or property that involves, or perhaps is constituted by, the reliable manifestation of some further property, the display property, when activating conditions are in effect. 'Reliable manifestation' here does not mean 'actual manifestation': It means that manifestation can be reliably expected when activating conditions are in effect.

Note that the display property for a disposition may itself be a disposition. Litmus paper has the disposition to turn red in an acid bath, but being red itself is a disposition to appear in various ways in various circumstances.

I have left my characterization of dispositions nonspecific in many ways. For example, some hold that dispositions are actually specific physical properties, picked out with dispositional language that specifies the reliable connections of the property in question with other properties. On this view the fragility of a piece of glass is constituted by certain features of the molecular structure of the glass, features reliably connected with shattering on the application of an impulsive force. Other accounts of dispositions are also available. Again, I want not to take a more specific stand, hoping that I have said enough for what will follow while allowing holders of different views about dispositions to fill in as they see fit.

A pure conception of dispositions involves an unfailing display of the display property when the activating conditions are in effect. On this view, if a piece of fragile glass fails to shatter when struck, we say that something went wrong with the activating conditions. A wider notion, called *propensity*, replaces display with certainty by display with a given probability. A fair coin has a propensity of 1/2 for coming up heads when flipped. This means that when the activating conditions are in effect (here, a fair flipping mechanism), there is a 50 percent probability for the display property of heads. Once more, I leave vast room for filling in, especially as to one's views about probability.

I will understand superimposed properties in terms of propensities. If the property Q is the superposition of properties  $P_1$  and  $P_2$ , then Q is a property in its own right, but it also includes a propensity to yield as a measurement result the superimposed properties  $P_1$  or  $P_2$ , when the right "measurement" activating conditions are in place, with the probabilities given by the probability amplitudes.

My propensity reading for superpositions, under a wide range of further clarifications of 'propensity', competes with certain so-called hiddenvariable interpretations. These take the stand that when a quantum theory describes a superposition, in fact one of the superimposed properties is already manifest, and this is the property that would be found if a measurement were to take place. Because I am particularly skeptical of hidden-variable theories, I will, without further apology, plunge ahead with what I have said about superpositions understood in terms of propensities. I leave to hidden-variable theorists the job of recharacterizing what I say in terms of their views. As far as I can see, the same moves used to deal with superpositions in conventional quantum mechanics will work as poorly or as well for hidden-variable theorists for superpositions in the context of quantum field theories.

I have said that when the right activating conditions are met, a superposition manifests one of the superimposing properties, with the probabilities given by the probability amplitudes for the state in question. Physics tells us that the relevant activating conditions are measurements of the relevant properties. But what counts as a measurement? And how does measurement result in these transitions from the "possible" to the "actual"? These questions constitute the measurement problem. Many answers have been proposed. None, to my mind, are satisfactory. All quantum theories face the measurement problem, and nothing that I will say will help with it in any way. My design is, instead, to make as much as possible of what I say consistent with all approaches to this problem.

### OVERVIEW OF THIS BOOK

The name 'quantum field theory' leads us to expect a theory that must primarily be thought of as a field theory. The single most important point I hope to make in this book is that this impression is wrong. When described correctly, the theory is just as much a "particle" theory. Indeed, the theory has both particle and field aspects, and a correct understanding involves seeing that, and seeing why neither approach takes precedence over the other. In their new environment, particle and field (or wave) concepts have evolved from the form of their classical ancestors. Appreciating the theory involves grasping the new forms of the two elements and seeing clearly how they fit together.

Current views of quantum field theory do acknowledge an essential particle aspect. But I believe that they get the particle aspect wrong, and consequently they do not show clearly how the particle and field aspects fit together. Expositions of the particle aspect go wrong when they fail to make clear how the relevant notion of a particle has evolved from the perhaps vague, prequantum notions. This difficulty is then compounded by the usual strategy of exposition, which starts from the classical idea of a field and then applies heuristics for transforming a classical into a quantum theory. One then notes that the result has a particlelike aspect. In one representation, this is described in terms of what we are led to believe is creation and annihilation of particles at precise space-time points. The description's misleading features are compounded by naive readings of Feynman diagrams. These diagrams provide an invaluable tool in describing quantum processes, working with a picture of superimposed creation and annihilation of particles at precise space-time points with trajectories describing the propagation 10 CHAPTER ONE

of particles between these events. All of this will come in for critical examination as we proceed.

I maintain that we will see things more clearly by first clarifying the notion of particle which will be in question. Armed with a clarified notion of quanta, as I will call them, and aided by my specification of how to understand superpositions, I will show that we are describing exactly the same facts when the description runs in terms of "quanta" and when it runs in terms of "fields."

Here, now, is a slightly more detailed synopsis of the plot. (Some readers coming from physics instead of philosophy have found that chapter 2 contains rather more philosophy than they meant to sign up for. Such readers can get by with the following synopsis of chapter 2.)

Chapter 2 deals with the particle concept by asking what has to be eliminated from our prequantum conception. This strategy presents a problem in that there is no one sharply delineated conception we all share. But the problem is manageable inasmuch as the process is one of elimination: We need to say what must clearly be left out to get a concept that fits the facts of the quantum world.

Many would say that on a prequantum conception, a particle always has an exact space-time trajectory. As I am sure all readers know, conventional quantum mechanics already gives up exact trajectories. The uncertainty relations for position and momentum require these never to receive simultaneous exact values in quantum descriptions of particles, but as these ideas are well known I will not examine them further.

Interpreters of quantum theories almost never address the idea that particles, as material objects, are thought of as substantial. Many of us, in our prequantum thinking, think of particles as composed of bits of substance, vaguely thought of as "stuff" in which properties can inhere. In addition, we often think of the individual substance of a given particle as providing the particle with its ultimate identity, its being "this one" as opposed to "that one." In chapter 2 I will spend some time sorting out these admittedly confused, vague, and distressingly metaphysical ideas. Perhaps these ideas cannot be made satisfactorily clear, and they certainly are not universally shared. But such problems will not deter us from stating a negative thesis about what must be left out of a satisfactory quantum conception.

<sup>&</sup>lt;sup>4</sup>Hidden-variable theorists will demur. However, if we focus on what *quantum mechanics*, as opposed to a hidden-variable extension, says about particles, the conflict is resolved.

Conventional presentations of the quantum mechanics of many particle systems appeal to particle labels. For example, for a two-particle system the description says there is one particle labeled '1' and a second particle labeled '2'. If the particles are the same in all their fixed or state-independent features, such as mass and charge, then the Hilbert space description does not provide any means of permanently distinguishing between the particles on the basis of their properties. Any assignment of properties to particle 1 can be assigned to 2, and any description assigning one set of properties to 1 and another set of properties to 2 has a twin description in which the roles of 1 and 2 are reversed. Still, the use of the number labels tempts many of us to think that there still is a difference between being particle 1 and being particle 2 and to think that descriptions that reverse the roles of the particle are, in principle, descriptions of different ways the world might be.

These are the ideas that must be given up, and though they are vague, we can enforce their rejection by rejecting the particle labels that seem to support them. In outline, the argument will go like this: In the Hilbert space formalism for many particles, with particle labels, there are state descriptions that are symmetric (interchange of particle labels does not change the representing Hilbert space vector), state descriptions that are antisymmetric (interchange of particle labels changes only the sign of the representing vector), and state descriptions that are nonsymmetric, that is, neither symmetric nor antisymmetric. The theory tells us that only the symmetric and the antisymmetric state descriptions represent actually occurring states. The nonsymmetric state descriptions are then superfluous, idle wheels in the formalism. Chapter 2 will introduce a notion of surplus formal structure and a methodological principle that tells us to prefer a description that is not thus burdened with nonfunctioning theoretical machinery.

An alternative descriptive formalism, Fock space, avoids the surplus structure of the usual formulation of conventional quantum mechanics. For the moment all we need to know is that this formalism does not have the descriptive machinery that would enable one to say which particle gets which property. It tells us only what patterns of properties are exhibited. This facilitates a conception of what I will now call quanta, for which there is no "this one" as opposed to "that one" independent of distinguishing properties.

To fix the ideas with some jargon, I will use the expression primitive

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thisness for the property-transcending individuality that particles might be thought to have and that quanta do not have. To put the idea in a different way, things with primitive thisness can be counted; that is, we can think of the particles as being counted out, the first one, the second one, the third, and so on, with there being a difference in principle in the order in which they are counted, a difference that does not depend on which particle has which properties. By way of contrast quanta can only be aggregated; that is, we can only heap them up in different quantities with a total measure of one, or two, or three, and so on, but in these aggregations there is no difference in principle about which one has which properties. The difference between countability and susceptibility to being merely aggregated is like the difference between pennies in a piggy bank and money in a modern bank account.

By eliminating particle labels and nonsymmetric states, Fock space pares down the formal structure of conventional quantum mechanics. But in another respect Fock space adds descriptive power. Given a first state describing one quantum and a second state describing two, we can form a third state, the superposition of the first two. Happily, such states occur copiously in nature. But how are we to understand states with an "indefinite number of quanta?"

It is tempting to see in this last regard a further departure from a prequantum conception of particles. Naively we think of particles as composed of some sort of concrete substance, something that is either definitely there or definitely not there. There seems to be no room in such a conception for an indefinite state that is neither definitely one particle nor definitely two, while all other possibilities are ruled out.

The stand one takes here depends on one's attitude toward superposition. My stand seems not to require further departure from a prior conception of a particle. As I will explain in more detail in chapter 2, there is no reason why we cannot think of a propensity as a propensity to come up with one or another number of particles of any kind we like. Thinking along these lines engenders another puzzle, however. A propensity, as a kind of disposition, is ordinarily thought of as a property. And we generally think of properties as properties of things. If we construe a superposition of a one-quantum and a two-quantum state as a propensity to display either one or two quanta when the activating conditions are set in motion, what is the thing that has this propensity? It can't be the quanta. Before activation of the propensity, neither one nor two quanta have yet been displayed.

I can suggest two approaches to this problem. One is to postulate an entity vaguely referred to as "the world." "The world" is not itself a substance or a thing in any ordinary sense. It is the system that can take on any of the various quantum states described by Fock space. The other approach (perhaps not so sharply delineated from the first) is to note that the problem arose from a presupposition, namely that instantiated properties are always properties of things. Reject this assumption and the problem disappears. Say instead that properties are instantiated or realized without the need for any "substratum" for them to be instantiated in (in retrospect the idea of a substratum, itself propertyless, was perhaps pretty obscure anyway). And now we can say that among the properties that can get instantiated are those of exhibiting one quantum, exhibiting two quanta, and a third, the superposition of the first two.

I can complete the overall picture of the book with a shorter summary of the following chapters. Chapter 3 shows how to build a Fock space quantum description of aggregable quanta. We let  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle$  be a state with  $n_1$  quanta of type 1,  $n_2$  of type 2, through  $n_k$  of type k. Roughly speaking, we let these states be a basis for a Hilbert space, and we generalize the transformations between the old one-quantum basis sets so that the transformations apply also to these multiquantum states. Because these new basis states describe only how many quanta are of which type (have which properties), this is a description appropriate for aggregable quanta instead of countable particles. And as a Hilbert space, we also get superpositions of states with different numbers of quanta.

But what does any of this have to do with field theory? Chapter 4 draws the connection in two ways. First, we can reexpress the Fock space basis states in a basis indexed by a spatial or by a space-time variable. This gives a description with the form of attributing operators to spatial or to space-time points. In addition, by generalizing on the equation of motion for a single quantum, we get an expression that has the form of a field equation. In sum, in this basis, the whole description takes on the familiar field-theoretic form. Alternatively, we can forget about particles and quanta, go back to a classical conception of a field, and apply the heuristics generally used to find quantum-theoretical analogues of classical theories. The result is exactly the same formalism previously derived.

In either of these two ways we see how the same facts are equiv-

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alently described from the point of view of aggregable quanta and of field-theoretic ideas.

A rarely discussed detail concerns how to interpret the state descriptions indexed with a spatial variable. One is tempted to take these to describe quanta exactly localized to a spatial point. This interpretation works smoothly only in a nonrelativistic theory. In a relativistic context we need to examine competing bases, both indexed with a spatial or a space-time variable, and study the surprising relations between these.

Chapter 4 shows how field-theoretic ideas give a natural entrée into the theory. But in what sense is the result really a *field* theory? A popular gloss talks reassuringly of "operator-valued fields," but this way of describing the subject turns out not to correspond at all well to a sensible notion of a field theory. Chapter 5 argues for a different way of construing the field-theoretic aspect of the theory. I include a brief discussion of some prototypical quantum field phenomena, coherent states, vacuum fluctuations, and also Rindler quanta, which some have thought seriously to compromise the use of any particle concept in thinking about quantum field theory.

So far the whole discussion has neglected interactions between quanta or fields. Chapter 6 outlines the most elementary development of the theory when one adds interactions. Interactions immensely complicate the picture because, for realistic descriptions, we have no closed solutions for the resulting theoretical description, forcing us to resort to an approximation scheme. The result—fairly viewed as a new theory or collection of models—involves a tangled skein of superimposed states describing interaction processes. Feynman diagrams, explained with a simple example, make the complications manageable, but they also suggest that one think of the theory in terms of "virtual" quanta that are created at one place, propagate to a new place, and are then annihilated. I urge caution in interpreting such pictures literally.

The method of approximation engenders another problem: Some of the terms in the expansions involve diverging integrals, expressions that become infinite, apparently making nonsense of the whole scheme. Physics has devised an ingenious method of excising the cancerous integrals: renormalization. Many people have described this process as a mathematically unlicensed procedure. Chapter 7 describes in very simple terms how renormalization works and then examines three attitudes toward renormalization described by physicists. On two of these, renormalization involves nothing illegitimate. Indeed, the method stands out

as an example of how one can make up for theoretical ignorance by measuring the value of physical parameters.

Renormalization constitutes only one of a great number of interpretive issues that arise from the details of quantum field theory, and I hope that my efforts in this book will illustrate the sort of further interpretive work from which quantum field theory would benefit. If I had seriously tried to resolve any significant number of these more specific issues, I would still be writing!

I hope that my efforts to deal with renormalization will illustrate the sort of detailed interpretive problems that a larger group of interpreters can pursue. Indeed, I have collected a few of these problems at the end of some of the chapters. Most of the work of this book seeks to build a larger interpretive framework for quantum field theory. The framework's usefulness for thinking about more specific issues will be its most severe test.

#### PROBLEMS

Not a little of the interpretive work in this book rests on interpretive ideas
announced but not defended in this chapter. As you read, reevaluate
claims made throughout when one takes alternative stances toward (a)
the nature of scientific theories and (b) the interpretation of quantum
mechanical probabilities.

## From Particles to Quanta

QUANTUM THEORIES, from their inception, have used particle and wave ideas in combinations quite unlike those occurring in classical mechanics. Early interpreters worried: How can one and the same entity be in some ways a particle and in other ways a wave? Are particle and wave concepts not, after all, mutually exclusive? The ploy of calling quantum entities 'wavicles' just gave an amusing name to the problem instead of solving it.

To address this problem one needs to recognize that wave and particle concepts are neither univocal nor unanalyzable primitives. If we analyze these concepts, perhaps we will find components that will fit together consistently. Conventional one-particle quantum mechanics already gives up exact space-time trajectories, at least in its descriptions. Inexact space-time trajectories will not figure in our deliberations, so I'll move immediately to another feature of particle concepts, namely, the idea that particles are concrete, substantial entities that can have properties and can be named. To have a word for our problem, I'll talk about the idea that each particle is, is composed of, or in some way involves a nameable substance.

#### IDEAS OF SUBSTANCE

In discussing a two-particle system in conventional quantum mechanics one usually begins by declaring that one of the particles will be referred to as 'particle 1' and the other as 'particle 2'. What preconceptions about things thus named might one bring to the theory? Because we are interested in our pretheoretical preconceptions, let's use the more general term 'objects' instead of 'particles', referring to two objects, 1 and 2.

Many people find that the use of such label-names suggests that the thing named has an identity that is independent of its properties. There seems (to some) to be a sense in which we can talk about *this* object

<sup>&</sup>lt;sup>1</sup>Chapter 5 will continue this project, only begun in the present chapter.

(object 1, say), whatever properties it may have, as opposed to *that* object (object 2, say), whatever properties this second object may have. I'll use the philosopher's expression *primitive thisness* for the idea.<sup>2</sup>

Here is another way to bring out the idea. Because an object's primitive thisness is supposed somehow to have to do with its identity, independent of all its properties, if objects have primitive thisness, we should be able to talk about changing all of an object's properties while still talking about the same object. For example, if we have two objects, 1 and 2, it should make sense to talk, counterfactually, about switching all of their properties. If  $P_1$  is a complete roster of object 1's properties and  $P_2$  a complete roster of object 2's properties, then it would have been a different possible, or counterfactual, situation if 1 had  $P_2$  and 2 had  $P_1$ .

Inasmuch as object 1's primitive thisness is supposed to involve its identity, independent of all possessed properties, it ought to make sense to think of stripping away 1's properties until none of them are left. Indeed, we should be able to talk about complete property switching by performing the conceptual thought experiment of gradually stripping away all of 1 and 2's properties and then reclothing them, each in the properties of the other.

Halfway through this thought experiment we are left with a couple of denuded objects, completely without properties, what philosophy has called *bare particulars*. This line of thought naturally reminds us of a second conception of substance: The bare particulars could be the "stuff" in which properties inhere.

<sup>&</sup>lt;sup>2</sup>Philosophers also use the traditional term 'haecceity'. See Adams for a discussion (1979). Redhead and Teller (1991) used the term 'label transcendental individuality', which still seems apt but does not have general coinage in philosophy, and also 'individuatable', which is problematic because it can easily be understood as meaning practical individuatability, requiring individuating properties, which is not what is intended.

<sup>&</sup>lt;sup>3</sup>This conceit works only if we can disregard essential properties, discussed briefly later. The so-called identical particles of quantum theories agree in all properties that might count as essential, so for such cases essential properties pose no problem for the foregoing ploy for expressing the idea of primitive thisness. This account also rests on a prior and unanalyzed conception of what counts as a property. For example, if we allowed 'being identical with object 1' as picking out a genuine property, obviously talk of switching all properties would not make sense. Traditionally philosophers distinguish between qualitative properties (and relations), such as being red, and nonqualitative properties, such as being identical with object 1; and they then restrict the statement in the text to qualitative properties. As far as I know there exists no independent and satisfactory way of drawing the qualitative/nonqualitative distinction, and some will maintain that this difficulty leaves the concept of primitive thisness irredeemably obscure.

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Substance as bearer of properties may be the older and primary substance notion, one that our linguistic practices suggest when we make attributions in subject-predicate form. When we say that object 1 has property P, many suppose a metaphysics to go along with this language, according to which the predicate 'P' picks out a property, "1" picks out a substance, and P inheres in or is instantiated in the substance of object 1. In this role of substance as the "bearer of properties," it should make sense to think of substance "before," or independently of all properties.

Some will worry: Does the idea of propertyless substance or an absolutely bare particular really make sense? Locke famously talked in this regard of substance as an object's "I know not what," suggesting great discomfort with the idea, yet also an inability to let it go.

Intelligible or not, we should note that the conceptions of substance struggling for intelligibility are really associated with two quite intelligible and distinct conceptual jobs. One is the role of bearing properties, the other the role of supplying identity, or of being distinct from other entities. We must be careful not to conflate these two jobs or the notions of substance that are supposed to accomplish these conceptual tasks.

Yet another conceptual job involves supplying that which is supposed to persist over time and to survive temporal change. My properties change from day to day, yet we take it to be the self-same Paul Teller to whom we refer today as we did yesterday. Substance as that which persists through time is a close analogue of primitive thisness as that which counts as the same in face of counterfactually supposed change of properties. In both cases we look for something constant as properties change, but in the new case the change is over time, whereas in the old case it was change from the actual situation to a counterfactually supposed alternative.

Persistence through change over time most clearly brings out further complications in the tangle of substance conceptions. Changes can occur to me and I am still me, but not too many changes or changes of the wrong kind! After death and cremation Paul Teller will be no more. His ashes will not count as Paul Teller. Some philosophers have tried to express this limitation on an object's change through time by saying that some of an object's properties count as essential, meaning by this that if any essential property is lost, the object ceases to exist. For example, perhaps being rational, or at least being alive, is essential to the existence of any human being. Of course, philosophers have a hard time agreeing

on what properties are essential, one of a number of reasons for which some have rejected the idea of essential properties.

Persistence through time also raises another issue. I can persist even though there are changes in the matter of which I am composed. To switch to another example, changing a few bolts and wires on one's car hardly counts as destroying the car and bringing a new one into existence. But matter provided a model for one of the other ideas of substance, that of the bearer of properties. So these ideas, although perhaps related, can't be identified. Indeed, if we have a penny made of a specific hunk of copper, we can destroy the penny by melting down the copper that composes it, and we can retain the penny while, bit by bit, substituting small new parcels of the composing copper.

I hope this sketch provides an intuitive impression of the various substance conceptions that one might include in one's thinking about objects, and also how vexing are the issues of making these conceptions clear. Fortunately, for our purposes, the obscurity of these notions will not get in our way.

As for the problems of essential properties and change of matter, they will simply not come up in our discussion. We will concern ourselves only with cases in which all relevant objects share all those properties that might be thought to be essential, and nothing in the cases will suggest any possibility of change of composing matter. I mentioned these issues only so that we can clearly set them aside.

Although the various conceptions of substance are obscure, we will be able to make do with what I hope is less obscure, the conceptual jobs that the substance conceptions are supposed to accomplish. We now have three of these jobs: Primitive thisness is supposed to accomplish the job of providing or founding an object's identity, at a given time, independently of its properties. (What is obscure is what kind of a conception can do that and how.) Second, substance as that which persists through time is supposed to accomplish the job of providing something that remains the same as an object's properties change over time. (As before, the obscurity resides in what can do this and how.) Third, substance as the bearer of properties is supposed to accomplish the job of providing something in which properties can inhere. (Again, what is obscure is what kind of a conception can play this role and how it will work.)

The jobs of providing identity at a given time and of providing what persists through temporal change appear, in general, to be different.

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Many would say that if we change the composing copper in a penny gradually enough, the penny never stops existing. But it seems doubtful that we can say correctly of this very penny that it would have been the same object if it had been minted from a different hunk of copper. However, I think we can get away with identifying these jobs for the special case of those entities putatively picked out by the number labels in conventional quantum mechanics. Particle 1 is supposed to be the same object that has one set of properties, could have had another set of properties, and whose properties may change over time. As long as we focus on elementary, or noncomposite, entities, there is no question of complications involving change of composing matter. I will use primitive thisness to refer to the substance notion that for quantum entities may be thought to accomplish both jobs of identity through counterfactual change and change through time.

Primitive thisness may well be a distinct notion from that of substance as property bearer. I have no brief on this question for quantum entities, nor, should the two notions be distinct, on what one should say about substance qua bearer of properties. What I want to argue is that the quantal facts give good reasons for rejecting any aspect of quantum entities which might be thought to do the job of primitive thisness.

## THE LABELED TENSOR PRODUCT HILBERT SPACE FORMALISM (LTPHSF)

The mark of primitive thisness enters into the quantum mechanics of many particles through particle labels, and the labels get in through the tensor product formalism. (Because 'particle' carries connotations of primitive thisness, ultimately we want to dispose of that word. But for the moment I will continue to acquiesce in the traditional terminology.) For reference, let's summarize relevant fundamentals.

One starts with the formalism for describing a single particle. Quantum mechanics conceives of a particle as possessing certain fixed or state-independent properties, such as mass and charge. (These could be candidates for essential properties.) A particle also has variable or state-dependent properties, the full range of which is described equally well in terms of states represented as rays in a Hilbert space and in terms

<sup>&</sup>lt;sup>4</sup>After rejecting primitive thisness for noncomposite entities, this claim holds for composite entities as well.

of selfadjoint operators on the Hilbert space. The states, thought of as maximal specifications of compatible properties, change with time, as represented by unitary evolution of the representing rays, governed by a Schrödinger equation.

Now let's describe two distinct particles, distinguished by different fixed properties, such as a proton and an electron. (The case of n particles each with unique fixed properties proceeds similarly.) In this case the distinct fixed properties act as individuating properties, that is, they do the work of primitive thisness, giving a handle with which we can say which particle is which ("the proton," "the electron"). The first particle will be represented by Hilbert space H(1) and the second particle by its distinct Hilbert space H(2).<sup>5</sup> One represents states of the joint system as rays in the tensor product H(1)  $\otimes$  H(2). In particular, if  $|a(1)\rangle$  is a state-representing vector in H(2), then the joint state is represented by the tensor product,  $|a(1)\rangle|b(2)\rangle$  in H(1)  $\otimes$  H(2). If  $\{|a_i(1)\rangle\}$  is a basis in H(1) and  $\{|b_j(2)\rangle\}$  is a basis in H(2), then arbitrary vectors of the form  $|a_i(1)\rangle|b_j(2)\rangle$  span H(1)  $\otimes$  H(2).

Traditionally one applies the same formalism in describing 2 (or n) particles not distinguished by distinct fixed properties. Such particles (for example, two electrons) are called *indistinguishable*, or often *identical*, by which one means not numerically but qualitatively identical in the sense of not having any distinguishing fixed properties. In this situation, each individual particle is represented by the same Hilbert space, H, spanned by a set of one-particle vectors,  $\{|a_i(1)\rangle\}$ . But to represent two indistinguishable particles we need two copies of H, labeled with the indexes '1' and '2'. So again, states are represented by rays in  $H(1) \otimes H(2)$ , spanned by the  $|a_i(1)\rangle|a_j(2)\rangle$ , except that in this situation H(1) and H(2) are copies of the same Hilbert space, H.

#### THE LTPHSF AND PRIMITIVE THISNESS

For the one-particle case, I will follow the usual assumptions that every ray in H represents a distinct state and every Hermitian operator on H represents a property or physical quantity. How should we extend this

<sup>&</sup>lt;sup>5</sup>Strictly speaking, all Hilbert spaces of the same dimension are mathematically isomorphic. So when we here talk of "distinct" Hilbert spaces, we tacitly understand some minimal interpretation that distinguishes them, such as the use of two Hilbert spaces to represent two particles with different masses.

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standard one-particle interpretation to the LTPHSF in which (as we will henceforth suppose) all the factor spaces are copies of the same H?

The assumption that particles carry primitive thisness provides the most natural extension. Suppose that for the one-particle case  $|a\rangle$  describes the state in which one particle has eigenvalue a for operator  $\hat{O}$  on H and  $|b\rangle$  likewise describes a b-valued eigenstate for  $\hat{O}$ . Then primitive thisness permits us to interpret  $|a(1)\rangle|b(2)\rangle$  in  $H(1)\otimes H(2)$  as representing a state in which a particle labeled '1' has eigenvalue a for  $\hat{O}$  and a particle labeled '2' has eigenvalue b for  $\hat{O}$ . We read  $|b(1)\rangle|a(2)\rangle$  analogously, noting carefully that on this interpretation  $|a(1)\rangle|b(2)\rangle$  and  $|b(1)\rangle|a(2)\rangle$  describe distinct physical situations.

In addition, the LTPHSF for indistinguishable particles faithfully represents particles as having primitive thisness. LTPHSF accomplishes this by not providing any property possessed by the labeled particles by virtue of which the particles have their labels. All the labeled particles have the same fixed properties. Each labeled particle may have any of the variable properties represented by distinct rays in H. And if  $\hat{O}$  is a maximal observable (on the factor space, H) with eigenvalues a and b, then  $|a(1)\rangle|b(2)\rangle$  and  $|b(1)\rangle|a(2)\rangle$  represent, on this interpretation, distinct physical situations in which all the potentially distinguishing properties of particles 1 and 2 have been switched.

It may start to seem that the LTPHSF actually requires primitive thisness for particles. This is correct, if we retain the assumption from the one-particle interpretation that to each ray there corresponds a unique state, so that each ray in  $H(1) \otimes H(2)$  represents a state and distinct rays represent distinct states. For, on this assumption,  $|a(1)\rangle|b(2)\rangle$  and  $|b(1)\rangle|a(2)\rangle$  represent distinct physical situations, the first in which particle 1 has value a for  $\hat{O}$  and particle 2 has value b for  $\hat{O}$ , and the second with these values exchanged. This is true for any properties that the theory recognizes as ones in which the particles may differ.<sup>6</sup>

In summary, primitive thisness and the interpretation of the LTPHSF are intimately connected. If we add primitive thisness to the standard interpretive framework for the one-particle case we get the most natu-

<sup>&</sup>lt;sup>6</sup>If one relaxes the one ray—one state assumption when moving to the many-particle case, the resulting descriptive framework admits so-called parastates and parastatistics, symmetry types more complicated than either Bosons or Fermions. I won't take up this complication here. See Redhead and Teller (1991) for a careful treatment of the connections between primitive thisness, the "indistinguishability postulate," and parastatistics.

ral way of reading the LTPHSF, a reading which seems unavoidable if we extend to the rays of  $H(1) \otimes H(2)$  the assumption about the rays of the the one particle case, that each distinct ray represents a distinct physical state.

## DIFFICULTY WITH INTERPRETATION OF THE LTPHSF Using Primitive Thisness

Consider an arbitrary two-particle state,  $|\Psi(1,2;t)\rangle$ , and the fact that the Hamiltonian of the system is always symmetric in the two-particle labels. It follows that if  $|\Psi(1,2;t)\rangle$  is a solution to the Schrödinger equation, then so is  $|\Psi(2,1;t)\rangle$ , and then also all superpositions of these two solutions. But different solutions from this class give different statistical predictions. Consider, for example, two particles, with exact momenta respectively of  $\mathbf{k}_1$  and  $\mathbf{k}_2$  in the x direction with spatial parameters  $\mathbf{x}_1$  and  $\mathbf{x}_2$ . Solutions represented in the position basis include (omitting time dependence)  $e^{i[(\mathbf{k}_1\mathbf{x}_1+\mathbf{k}_2\mathbf{x}_2)]}$  (particle 1 with momentum  $\mathbf{k}_1$  and particle 2 with momentum  $\mathbf{k}_2$ ),  $e^{[i(\mathbf{k}_2\mathbf{x}_1+\mathbf{k}_1\mathbf{x}_2)]}$  (particle 1 with momentum  $\mathbf{k}_2$  and particle 2 with momentum  $\mathbf{k}_1$ ), as well as the symmetric and antisymmetric superpositions of these. The probability for location at  $\mathbf{x}_1$  and  $\mathbf{x}_2$  are in these four cases respectively proportional to 1, 1,  $[1+\cos((\mathbf{k}_1-\mathbf{k}_2)(\mathbf{x}_1-\mathbf{x}_2))]$ , and  $[1-\cos((\mathbf{k}_1-\mathbf{k}_2)(\mathbf{x}_1-\mathbf{x}_2))]$ .

The particle labels play the pivotal role in generating this situation. Taking the labels as indicating primitive thisness, the states  $|\Psi(1,2;t)\rangle$ ,  $|\Psi(2,1;t)\rangle$ , and their superpositions represent different states of affairs, for which the theory gives different predictions. But because we are concerned with indistinguishable particles, ones that cannot be distinguished experimentally, we have no way to determine, in a given case, which of these states has been prepared. From the point of view of preparation, all the states look alike, but they give conflicting predictions, at most one of which can be correct. The excess descriptions with the incorrect predictions must somehow be eliminated from the formalism. We face two questions in this regard: Which states must we eliminate, and how should we interpret this elimination?

The requirements of quantum statistics answer the first question. Let's illustrate with the simplest  $2 \times 2$  system, that is, a system with two particles and a two-dimensional one-particle Hilbert space, with

operator  $\hat{O}$  having just two eigenstates,  $|a\rangle$  and  $|b\rangle$ . First let's suppose that the two-particle system can be in any of these joint states:

$$(2.1) |a(1)\rangle|a(2)\rangle$$

$$(2.2) |b(1)\rangle|b(2)\rangle$$

$$(2.3) |a(1)\rangle|b(2)\rangle$$

$$(2.4) |b(1)\rangle|a(2)\rangle$$

Here think of 'a' as standing for heads and 'b' as standing for tails as the outcomes of tossing each of two coins. Then cases (2.1)–(2.4) can be thought of, respectively, as 2 heads, 2 tails, coin 1 yielding heads while coin 2 yields tails, and coin 1 yielding tails while coin 2 yields heads. Because these are exhaustive independent alternatives, in equilibrium the states (2.1)–(2.4) ought to be equiprobable. But then we would get classical Maxwell-Boltzmann statistics, never observed for quantum particles. To get either Bose or Fermi statistics, cases (2.3) and (2.4) must somehow be counted as one. As long as we assume primitive thisness, it is hard to see how to make sense of this requirement.

The way out of this difficulty is to assume that for reasons to be sought out later on, states (2.3) and (2.4) can never actually occur.<sup>8</sup> Assume instead that only symmetric and antisymmetric states can occur. For the symmetric case the available states are:

$$(2.1) |a(1)\rangle|a(2)\rangle$$

$$(2.2) |b(1)\rangle|b(2)\rangle$$

and

(2.5) 
$$\frac{1}{\sqrt{2}}(|a(1)\rangle|b(2)\rangle + |b(1)\rangle|a(2)\rangle).$$

Because the Hamiltonian is symmetric, a symmetric state can evolve only into a symmetric state. Consequently, if initial conditions always admit only symmetric states, an equiprobable distribution in an equilibrium state will always yield Bose statistics.

<sup>&</sup>lt;sup>7</sup>I assume here and throughout a uniform distribution for basic, equienergetic, equilibrium states, an assumption noted both for its astonishing power in classical statistical mechanics and for its resistance to justification other than its empirical success. But see n. 14.

<sup>&</sup>lt;sup>8</sup>As explained in Redhead (1988), p. 12, and French and Redhead (1988), pp. 235-38.

For the antisymmetric case we have only one available state:

(2.6) 
$$\frac{1}{\sqrt{2}}(|a(1)\rangle|b(2)\rangle - |b(1)\rangle|a(2)\rangle).$$

Again, the symmetry of the Hamiltonian means that temporal evolution will preserve the antisymmetry of the state, yielding Fermi statistics (the exclusion principle) for this case.

This situation requires excluding nonsymmetric states, that is, states that are neither symmetric nor antisymmetric, such as (2.3) and (2.4) in the  $2\times 2$  case. If nonsymmetric states ever occurred as initial conditions, we would expect to see nonquantum statistics. We see only quantum statistics in quantum systems. So nonsymmetric states do not occur. Given what I have said so far, this circumstance might be understood in two alternative ways:

- 1. We might say that the nonsymmetric vectors of the LTPHSF bear a physical interpretation. We know what it would be like for there to be such states, for example, one in which a first particle is a and a second is b, which is a distinct state of affairs from the one in which the first particle was b and the second a. However, in fact such states just never actually occur. One wonders, on this alternative, why they don't occur.
- 2. We might say that nonsymmetric vectors bear no physical interpretation. This requires rejecting primitive thisness for particles. For otherwise the standard interpretation for the one-particle case plus primitive thisness would generate an interpretation for the nonsymmetric vectors, as described earlier. On this alternative it is not clear how the labels of the LTPHSF are to be understood.

### Quanta

On either of these alternatives there is an obvious sense in which the formalism has what we will call (following Redhead) *surplus formal structure*, 9 that is, elements in the formalism with nothing in the real world to which these elements correspond.

Surplus formal structure comes in two forms. On the first alternative of the last section the labels give rise to *strongly surplus structure*, that is, the formalism has interpreted elements that, however, never have

<sup>&</sup>lt;sup>9</sup>Redhead (1975), p. 88.

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actual physical correlates. According to the last section's second alternative, LTPHSF has weakly surplus structure, that is, the formalism includes uninterpreted elements. On this view the labels themselves have no direct significance but function as some kind of auxiliary device to describe (anti)symmetric states in terms of the number of particles present in such a state and in terms of the distribution of properties among these particles, with no significance as to which particle has what properties. On this account the nonsymmetric vectors go completely uninterpreted.

I am going to argue that where possible surplus structure should be shunned. But only when what appears to be surplus really is so, for when a formalism appears to have surplus structure, this may turn out to be really so or it may turn out only to have been apparent. For example, initially the negative-energy solutions of the Dirac equation were good candidates for surplus structure, which then turned out to be only apparently surplus when the negative-energy solutions were reinterpreted as holes, or more accurately as positive-energy solutions of positrons. Black holes might also be thought to provide a similar example of structure that, despite early impressions, turned out not to be surplus.

Nevertheless, there are situations in which we have extremely good reason to think that a part of a theoretical structure really is surplus. Systems of equations with unphysical solutions provide the simplest sort of example (the square of Mary's age less 9 times her age gives 36. How old is Mary?). In quantum mechanics, the absolute phase and the relative phase when there are superselection rules provide further, quite clear-cut examples of genuinely surplus structure. <sup>10</sup>

How is one to tell whether what appears to be surplus structure really is so? Obviously there are going to be no hard-and-fast rules or any absolutely conclusive arguments. However, the following should make a strong case: When we have a second formalism that is free of what are at least apparently surplus elements in a first formalism, and when the second formalism not only has all the correct empirical import of the first but also covers phenomena not covered by the first, we surely have strong grounds for judging the apparently surplus structure of the first

<sup>&</sup>lt;sup>10</sup>Relative phase is generally of the essence and in ways that are not always immediately apparent. The plausible requirement that the absolute phase at each place in space be regarded as arbitrary ("local gauge invariance") seems to force the introduction of so-called gauge fields as a way of enforcing the required comparisons of phases at different places. This introduces the important and fascinating topic of the structure of gauge theories, which I will not attempt to discuss in this book.

formalism to be really surplus. As the exposition in the next chapter will make clear, the Fock space formalism familiar to quantum field theorists provides exactly such an alternative to the LTPHSF.

Why are we better off without surplus structure? Strong surplus structure, as in the first alternative attitude toward the nonoccurring nonsymmetric states, uses descriptions in which we understand, at least conceptually, what would be involved for the states so described to occur. Why, then, do such states just never occur? Other things being equal, a theoretical account is better when it does not let such questions go unanswered.

Physicists have an informal methodological maxim: "What can occur does." I think this aphorism encapsulates the same methodological maxim I am advocating. If a formalism describes a possible occurrence, one expects to see such occurrences, unless one can find some quite specific reason for why the occurrences fail to materialize. For example, statistical mechanics describes fluctuations, which, in extreme cases, would involve unheated cups of coffee starting to boil. We never see such occurrences, and the theory tells us why not in terms of the exceedingly low probability for such occurrences. The nonoccurrence of nonsymmetric states is not like that. The formalism provides no way of accounting for why they are never actualized.

Weak surplus structure, as in the second attitude toward the nonoccurring nonsymmetric states, presents a less clear-cut methodological problem. Still, we prefer a formalism free of uninterpreted danglers. In addition to general considerations of parsimony of our descriptions, uninterpreted segments of a theory can be expected to make it harder to see what the rest of the theory means.

In the case at hand, to let the nonsymmetric states go uninterpreted requires that the labels not be thought of as names of entities with primitive thisness, for otherwise such a reading of the labels would generate interpretations for the nonsymmetric states. But then the labels must be regarded simply as some kind of auxiliary device the function of which is not very clear. In addition, there is the continuing danger that the presence of the labels will mislead us in our interpretive deliberations. Many of us think of objects as having primitive thisness and think of names as attaching directly to objects in virtue of their primitive thisness. The presence of labels in quantum theories makes it harder to put such associations aside.

Some have recommended the following attitude: For n Bosons start

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with the LTPHSF for n Bosons, and then restrict attention to the subspace of symmetric states. (Analogous remarks go for Fermions and the antisymmetric subspace.) The symmetric subspace is a Hilbert space in its own right. It is adequate for treating the quantum mechanics of n Bosons and is itself free of any surplus structure.<sup>11</sup>

As far as I can see, this attitude is a thinly disguised variation on treating the surplus structure as weakly surplus, that is, letting the surplus formal elements go uninterpreted. What's the difference between letting the surplus elements—the nonsymmetric states—go uninterpreted and restricting attention to the symmetric subspace? In both cases we are stuck with the labels as a puzzling and potentially misleading element in the formalism.

Surely we have the best opportunity to see what a formalism indicates about the world when the formalism is free both of elements (the nonsymmetric states) that would have to be ruled out as without significance and of elements (the labels) that are particularly hard to understand without misleadingly leaning on the other parts of the formalism rejected as without significance.

I make the recommendation against surplus structure conditional on having available an alternative free of surplus structure. Leaving details for the next chapter, the Fock space alternative proceeds by letting a state directly specify which property combinations are exemplified and how many times each combination occurs. Here is a schematic example: A state might specify four occurrences of momentum  $\mathbf{k}_1$  and two occurrences of momentum  $\mathbf{k}_2$ . The description uses no particle labels, and so the issue of which particle has which property never comes up. Indeed, to view these descriptions as complete, we must recognize that questions about "which particle" no longer make any sense. <sup>12</sup>

Let's focus for a moment on some new terminology and metaphors

<sup>&</sup>lt;sup>11</sup>The full Fock space, discussed later, can then be realized as the direct sum of the symmetric (or antisymmetric) subspaces. Schweber (1961), ch. 6, follows essentially this approach.

<sup>&</sup>lt;sup>12</sup>It has also been suggested that because certain subspaces of a Fock space, described in the next chapter, are isomorphic to the symmetric subspaces of the LTPHSF, Fock space is indirectly committed to the labels. This suggestion misses the point that the surplus structure is surplus formal structure. A mathematical structure is connected to the world only through the interpretation of the formalism. Isomorphism of underlying mathematical structures or substructures is not sufficient for identifying the interpretive problems of the representing formalisms. If the argument in question were correct, we would have to agree that any application of Hilbert spaces is indirectly committed to truth tables because Hilbert spaces, constructed as lattices, have finite Boolean sublattices.

to help refine the conceptualization appropriate for the alternative descriptive framework. Old-fashioned particles, I will say, can be counted, ordered, and exchanged. In counting out a number of particles, there is always a difference in principle in the order in which they are listed. And it makes sense to think of them with all their variable properties exchanged. By way of contrast, I will use the word quanta for the new conception and say that quanta can merely be aggregated. We can amass quanta without, however, any ordering distinction in the way a stock of quanta is built up. Given an "amount" of quanta, there is no intelligibility to reordering or reassigning them while keeping fixed the property combinations that occur.

Here are two analogies to help clarify the conception: Mr. Magoo can lock up two pennies in his safe-deposit box, take them out years later, and ask intelligible but in practice unanswerable questions such as, which penny went in first, and did some bank employee secretly open the box and switch pennies on him? But if Mr. Magoo makes a deposit to his account with a check for \$2 or with two \$1 checks on subsequent days, similar questions make no sense. If on withdrawing one dollar from his account Mr. Magoo demands to know whether the withdrawn dollar was the first or the second deposited, no one is going to be able to help him. <sup>13</sup>

Here is another example of these ideas. Imagine that you and I stretch a rope some distance between us, and each of us gives our own end a good shake. By so doing we create two traveling bumps, one moving toward the middle from your end, one from my end. These two bumps merge momentarily in the middle, after which we see two bumps continuing down the rope in opposite directions. But does it make any sense to think of a situation in which the two bumps are switched? Could there be such a situation somehow distinct from the original? Hardly! And although initially one may be inclined to think of the two bumps as passing through each other after merging in the middle, it makes just as good sense to think of them as bouncing off each other. Classically, the traveling bumps cannot be labeled. They are not the sorts of things that bear primitive thisness.

One moves from the classical to the quantum edition of this idea by imposing the requirement that the bumps can come only in sizes of one,

<sup>&</sup>lt;sup>13</sup>This money analogy appears in Schrödinger (1950), p. 114, and (1957), pp. 212–16. See also (1950) passim and (1957), pp. 203–23 for a larger expression of an attitude similar to the one I have been developing here but without the same methodological argument. Hesse (1966), p. 50, also used the money analogy.

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two, three, or more equisized units, while being careful to retain the condition that the bumps, restricted to discrete units though they be, still do not bear labels or other symptoms of primitive thisness. It makes no sense to think of switching the "chunks" around. There is no such thing as switching, even conceptually, which would result in a different situation.

With the metaphor of the bumps on the rope—a typical wave phenomenon—one sensibly asks, what in quanta is left of the particle concept? Two particlelike aspects remain in the context of quantum phenomena: As mentioned in the last paragraph, quanta can be aggregated only in discrete units. In addition, on examination, quanta will always be found to manifest themselves, in their entirety, at extremely (though never perfectly) well localized places. Neither of these aspects goes with the classical conception of waves or fields. Although the analogy with classical waves serves well to bring out the way in which quanta are free from primitive thisness, there is nothing essentially wave- or field-theoretic about the concept. We arrive at the new conception by starting with our preconceptions about particles and eliminating aspects often thought to be included. Nowhere on this route to the idea of quanta do we have to appeal to wave- or field-theoretic ideas.

To improve the clarity and accuracy of our interpretive view, we should abandon the LTPHSF, with its implicit conception of particles, in favor of the Fock space formalism, with its correlative conception of quanta. My reason given so far is that Fock space, with states that merely describe the distribution of occurring properties, is free from the at least apparently surplus descriptions of the LTPHSF. But is the apparently surplus really surplus?

The descriptions in question are (descriptions of) the nonsymmetric states, attributing specific properties to specific particles. The facts of quantum statistics strongly suggest that such states never occur.<sup>14</sup> Fock

<sup>14</sup>The argument for nonoccurrence rests on the assumption that in equilibrium there will be a uniform distribution over available states. By choosing other equilibrium distributions one can reconcile quantum statistics with the LTPHSF without nonoccurrence of nonsymmetric states. See van Fraassen (1991), pp. 410-19, for a review. As he points out on pp. 476-80, one can start out with Carnap's m\*, which assigns each distribution of properties an equal probability. This gives Bose statistics. By adding the assumption that every property individuates, one gets Fermi statistics, and the assumption that there is a broader subset of individuating properties gives classical Maxwell-Boltzman statistics for the distribution over nonindividuating properties. But this approach to the labels and primitive thisness brings perplexities of its own. Although symmetry considerations are treacherous in the continuous case as grounds for probability distributions, in the discrete case one wonders: If particles

space adequately describes all the phenomena covered by the LTPHSF, so the methodology of shunning surplus structure (at least for interpretive purposes), other things being equal, dictates in favor of the Fock space description.

In fact, when it comes to describing the phenomena, Fock space is better than equal. Fock space provides for superpositions of states describing different distributions of properties, in particular superpositions of states with different numbers ("amounts") of aggregated quanta. These states with indefinite number of quanta occur in nature—indeed, they play a major role in the expanded theoretical framework. This provides all the more reason to suppose the Fock space description to provide us with a wider, more generally accurate account of the world of material objects.

The experimental facts require states with indefinite number of quanta (called *indefinite-number states*), but they also raise the interpretive question of how such states should be understood. One might see here an additional reason for preferring quanta over particles. Particles, with their primitive thisness, are either there or not there. One might worry that this characteristic conflicts with indefinite-number states thought of as somehow lying in between, for example, having one and having two particles. Some conceptions of superposition may give rise to this

<sup>1</sup> and 2 do not interact and the relevant properties, a and b, are on a par, why should 1-is-a and 2-is-a be treated differently from 1-is-a and 2-is-b, as they are in m\*? More generally, symmetric states have much more structure than the equiprobable cells of m\* because, for example, many different symmetric states can be formed by superposition from two one-particle states. Van Fraassen acknowledges (1991), pp. 501 n. 22, that "there is no question of reducing quantum statistical mechanics as a whole to classical probability theory." Van Fraassen's larger view, I take it, is that the empirical facts don't force us either to adopt or to reject a language that uses labels or an interpretation in which such labels are taken to name objects with primitive thisness. On one hand, he introduces a notion of semantic universalism, according to which "all factual description can be completely given in entirely general propositions" (p. 465). On the face of it, semantic universalism would appear to be in conflict with an interpretation in terms of primitive thisness-bearing individuals. For if there were such, then the facts would include ones specifying which individuals have which properties, facts not characterizable with purely general propositions. On the other hand, he claims that "the models required by semantic universalism are exactly those which can be described equally" either in terms of occupation numbers or in terms of individuals described with labels and having properties (p. 475). The latter proceeds by talking in terms of individuals assigned to cells, but then rejecting all information except that covered by all permutations of one such assignment. I would claim that unless one is prepared to assert that one of the permutation-equivalent descriptions is "true," such use of labels and talk of individuals is ingenuous, being understandable only as an unnecessarily complex way of giving occupation numbers.

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problem, but not so, I think, for the view of superposition that I am presupposing.

Superpositions, on my account, are propensities to reveal one of the superimposed properties under the right "measurement" conditions. If we were to stick with particles in our interpretation, we could still say that an indefinite-number state is one in which no particles are actually present but in which, on measurement, one or another definite number of particles will appear, corresponding to the superposition in question.

The phrase "no particles are actually present" must be interpreted carefully. Here it does not mean that, on measurement, one will definitely find no particles. That would be an exact number state, with zero particles, called the vacuum. An indefinite-number state, on this way of thinking, is the presence of a property that does not involve any already occurring particles but does involve the propensity for one or another definite number of particles, corresponding to the superposition. This interpretive stance mirrors the one familiar from treatment of the double-slit experiment: In the superposition of the particle passing through slit 1 or slit 2, one gets into difficulties if one supposes actual passage through exactly one of the slits. Consequently one denies actual passage through either slit. But this does not mean that an examination at a slit would certainly fail to detect passage there—in the superposition there is a propensity to discover the particle passing through exactly one of the slits should a measurement be made. One can see this pattern of description to be perfectly consistent by interpreting "actual passage through slit i" as "probability 1 for passage through slit i," and "failure of actual passage through slit i" as "probability less than 1 for passage through slit i."

Because indefinite-number states can be understood in terms of particles, these states do not themselves provide an additional reason for preferring quanta to particles. But we should also consider the relative merits of quanta versus particles when it comes to understanding superpositions such as

(2.5) 
$$\frac{1}{\sqrt{2}}(|a(1)\rangle|b(2)\rangle + |b(1)\rangle|a(2)\rangle).$$

If we stick with the labels, we have in (2.5) a superposition in which particle 1 is a and 2 is b, superimposed with the reverse. But one would think that entities with primitive thisness would each definitely have to have a property or definitely fail to have it. How can 1 and 2 be

in a twilight zone between 1-being-a-and-2-being-b and 1-being-b-and-2-being-a?

Let's try the same maneuver used for interpreting indefinite-number states with particles. We try to say that (2.5) does not involve any actual distribution of the properties a and b to 1 and 2, but instead consitutes a propensity for either of the distributions should 1 and 2 be examined for a and b. But this time the maneuver will not work. Either distribution (1-is-a-and-2-is-b) or 1-is-b-and-2-is-a constitutes a nonsymmetric state, which would result in classical Maxwell-Boltzmann statistics. Consequently we have to conclude that these states never occur and that the reading of (2.5) as a propensity for one or another of these nonsymmetric states would be a propensity with never-occurring display properties, which does not make much sense.

None of these difficulties arise if we think in terms of quanta described in a Fock space. On this description, what corresponds to (2.5) is a state with an a-quantum and a b-quantum but with no labels or primitive thisness or sense to the question, "which" quantum has a and "which" has b. For superpositions involving distributions of properties for a fixed number of entities, quanta provide a far more natural interpretation than do particles.

Quanta also provide a substantial leg up over particles when it comes to understanding quantum statistics. To remember how the problem used to look, for a moment put aside my distinction between particles and quanta. For two fair pennies we expect probabilities of 1/4 each for tossing outcomes of HH, TT, HT, and TH. For analogous cases for two Bosons the statistics are 1/3 each for HH, TT, and a third case indifferently described as one H and one T. Innumerable interpreters have found this fact astonishing. Why?

Bose statistics are astonishing if one supposes that the objects in question can be individuated, that is, if there are properties other than the ones over which we are counting the statistics that will function to keep track of which object is which. In classical physics the space-time trajectories were always available in principle to do this job. But quantum theory does not provide objects with exact, individuating, space-time trajectories.

Interpreters, most of them anyway, must understand this clearly enough. Yet they persist in their bewilderment over Bose statistics. This fact is hard to understand, I suggest, unless we suppose that these interpreters rely on an at least tacit assumption of primitive thisness,

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suggested by the particle labels. For on the assumption of primitive thisness, there are still four cases for two particles and two properties, so that an assumption of equiprobability in equilibrium gives the probabilities of 1/4 for all four cases. Conventional quantum mechanics gets the right statistics by imposing the requirement of symmetrization, but against this conceptual background symmetrization must appear as no better than a stopgap measure: It gives us the right answer without providing any insight into why the expected classical statistics fail to occur.

Indeed, I know that some, perhaps many, readers will have found my talk about primitive thisness abstruse, even silly, supposing themselves never to have been caught up in such woolly-headed metaphysics. Yet this seems doubtful for anyone who has found Bose statistics puzzling and who has clearly understood that talk of "indistinguishable particles" means that there are no properties that, like classical trajectories, can serve to individuate. I submit that perplexity in such people is most likely to arise from an assumption, tacit at least, of primitive thisness. Those who find themselves wondering how they could ever have been so puzzled by Bose statistics can try to reconstruct what has changed in their thinking!

Be that as it may, the important point is that if we forswear primitive thisness and think in terms of quanta instead of particles, the puzzles about Bose statistics simply evaporate.

What about Fermions, Fermi statistics, and the exclusion principle? In this case thinking in terms of quanta instead of particles still removes the old expectation of classical statistics, but we now face a new quandary: Given the intuitive picture of quanta that I have suggested, one expects that quanta of one kind can be aggregated without limit. Why, for Fermions, is there a limit of one to a kind?

I have no answer to suggest. At this stage of interpretive work we simply have to take the fact dictated by nature that some kinds of quanta, Bosons, can be aggregated without limit, and another kind, Fermions, are limited to one of a kind. We will see in the next chapter that this fact receives an exceedingly simple expression through a choice of certain commutation relations. The choice of commutators transparently states the distinction between limited and unlimited aggregation but does not explain it. The choice of commutators merely provides a phenomenologically correct description, "put in by hand," as physicists say, to get the facts right.

One might suggest that the theoretically unmotivated choice of commutators undermines the claimed methodological preference for a Fock space description over the old LTPHSF. Both, the claim would run, must make a theoretically unmotivated assumption to get the facts right: The LTPHSF must make an arbitrary restriction to symmetric or antisymmetric states, and the Fock space formalism must make a theoretically unmotivated choice among commutation relations to achieve exactly the same descriptive end.

This comparison is incomplete. True, both make analogously unmotivated assumptions. But the morning after we have stripped the LTPHSF of the unwanted states, we are still left with an interpretive hangover. Do the states cast off from the LTPHSF nonetheless have an interpretation, and if so, why do such states never occur? There is pressure to take the rejected states to be interpreted because of our inclination for interpreting the labels. We can stalwartly refrain from interpreting the rejected states only by refraining from interpreting the labels as functioning as normal names, naming particles with primitive thisness. But now how are the labels to be understood? By looking to the Fock space description for our interpretive insight, we simply avoid all such problems, as well as puzzles about interpreting the superpositions involved in symmetric and antisymmetric states and puzzles about quantum statistics, at least for Bosons.

In addition, there is hope that more can be said. The spin-statistics theorem describes connections among spin, relativity, and the relevant choice among the commutators. It would be gratifying if an application of the formalism of the spin-statistics theorem to the present interpretive framework enabled us to see in a more nearly physical and intuitive way how nature makes room for Fermions as well as Bosons. 15

It's time to make good on all the promises of showing how Fock space expresses the intuitive idea of quanta in a quantum-theoretical framework.

#### PROBLEMS

1. Compare and contrast the ways alternative approaches to the nature of scientific theories address the problem of surplus formal structure.

<sup>15</sup> Chapter 4 will briefly discuss the spin-statistics theorem, but not in a way that throws light on this issue from a field-theoretic perspective.

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- 2. Determine whether or to what extent particle and wave ideas can be reconciled in conventional one-particle quantum mechanics by giving up exact space-time trajectories for particles.
- 3. Transcribe the physical content from the various existing formal presentations of the spin-statistics theorem in a way that improves our informal understanding of why there are Fermions as well as Bosons.

# Fock Space

I HAVE PROMISED to show how Fock space realizes the idea of quanta, understood as entities that can be (merely) aggregated, as opposed to particles, which can be labeled, counted, and thought of as switched. We start with the quantum mechanics familiar from the one-quantum case. Then we add new descriptive machinery, which allows description of aggregations of quanta without, however, any way of talking about the quanta as labeled or of keeping track of "which" quantum has which properties. Instead of using labels, we simply keep track of how many "units" there are exemplifying each specific roster of properties.

Quantum mechanics for one quantum represents states as rays in a Hilbert space,  $H_1$ . We pick a discrete basis for  $H_1$ ,  $|a_1\rangle$ ,  $|a_2\rangle$ ,  $|a_3\rangle$ ,  $\cdots$ , which is also the set of nondegenerate eigenvectors of a maximal observable  $\hat{A}^{(1)}$  on  $H_1$ . Here the eigenvalues  $a_1$ ,  $a_2$ ,  $a_3$ ,  $\ldots$  are used to label the corresponding eigenvectors. To keep expositions simple, I have chosen a maximal observable,  $\hat{A}^{(1)}$ , with a purely discrete basis, and I follow common practice in using the term "observable" ambiguously to refer both to a physical quantity and to the selfadjoint operator on  $H_1$  which represents that quantity. Similarly, when no confusion results, I let slip the distinction between a state and the vector that represents that state.

To facilitate generalization, let's introduce a new notation for the one-quantum basis states  $|a_1\rangle$ ,  $|a_2\rangle$ ,  $|a_3\rangle$ ,... I will write  $|a_1\rangle$  as  $|1,0,0,...\rangle_A$ , understanding by this the state in which there is one quantum with the first eigenvalue,  $a_1$ , of  $\hat{A}^{(1)}$  and no quanta with the second, the third, or any other of  $\hat{A}^{(1)}$ 's eigenvalues. Similarly,  $|0,1,0,...\rangle_A$  represents the same state as does  $|a_2\rangle$ , the state with one quantum with the second eigenvalue,  $a_2$ , of  $\hat{A}^{(1)}$ .

With this notation we can represent a state with two quanta with eigenvalue  $a_1$  of  $\hat{A}^{(1)}$  as  $|2,0,0,\ldots\rangle_A$ . Note that  $|2,0,0,\ldots\rangle_A$  is not  $2|1,0,0,\ldots\rangle_A$ . Indeed,  $|2,0,0,\ldots\rangle_A$  is not at all a vector in  $H_1$ . Instead it will be a new basis vector in the Fock space we are developing. Similarly, we can represent a state with two quanta with eigenvalue  $a_1$  and seven quanta with eigenvalue  $a_3$  as the new basis vector  $|2,0,7,0,\ldots\rangle_A$ .

The general case will be  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$ , a state with  $n_1$  quanta with eigenvalue  $a_1, n_i$  quanta with eigenvalue  $a_i$ , through  $n_k$  quanta with eigenvalue  $a_k$  of  $\hat{A}^{(1)}$ . Note that these basis vectors represent states with only a finite aggregation of quanta. For each ket there is a largest k such that  $n_k \neq 0$ . In other words, in the ket notation  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$ , it is understood that for eigenvalues  $a_m$  with m > k, there are no quanta with eigenvalue  $a_m$  of  $\hat{A}^{(1)}$ . In addition, I include the very important special case of  $n_i = 0$ , all i. This is written  $|0\rangle = |0,0,0,\ldots\rangle$  and is called the vacuum, the state with no quanta.

Please keep clearly in mind that we are simply writing down a form of description that will present to us, as faithfully and perspicuously as possible, the facts we have so far uncovered about quantum entities. We take over wholesale what we know from the quantum mechanics for one quantum and extend that to the case of many quanta, keeping in mind the need for a form of description that does not let in anything suggesting primitive thisness.

There is, however, a fact not yet included in our descriptive framework. The framework can be used to describe either Bosons or Fermions. Because there can be any number of a kind of Boson, in the Bosonic case we specify that in  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$  each  $n_i$  can take on any non-negative integer value. Because there can be only zero or one of a given kind of Fermion, when we use this descriptive framework to describe Fermions we specify that in  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$  each  $n_i$  can be only either zero or one. As I emphasized in the last chapter, at the level of understanding that I am able to present in this book, this fact is simply written in to correspond to what we observe to be the case in nature. No explanation of this fact is provided or suggested.

A Fock space is now, very simply, a Hilbert space spanned by the vectors  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$ , assumed as an orthonormal basis

<sup>&</sup>lt;sup>1</sup>The Fock space we are developing, with basis vectors  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$ , each representing a finite aggregation of quanta, provides an irreducible representation of the commutation relations introduced in the next section. There are also nonseparable Hilbert spaces with states representing infinite aggregations of quanta which provide representations of the commutation relations. But these larger spaces have no vacuum, that is, no zero-quanta state and no number operator, also to be introduced later. Schweber (1961), pp. 163-64, gives a further summary and references. Although most practitioners take quantum field theories to employ Fock space, which is a separable Hilbert space, the complications brought on by Haag's theorem might force the relevance of nonseparable Hilbert spaces, depending on how these complications are resolved. I will very briefly discuss the implications of Haag's theorem in chapter 6.

and understood to be generalized on the one-quantum case as I explained earlier.

It is useful to introduce a collection of number operators,  $\hat{N}_i^{(A)}$ , with eigenvalues  $n_i$ :  $\hat{N}_i^{(A)}|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A = n_i|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$ . The eigenvalues,  $n_i$ , are called occupation numbers. Because the  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$  are a basis, for a fixed A and variable i the  $\hat{N}_i^{(A)}$  constitute a maximal commuting set of observables for a Fock space.

From the  $\hat{N}_i^{(A)}$  we can reconstruct other observables of interest. For example, letting  $\hat{A} = \sum_i a_i \hat{N}_i^{(A)}$ , we get an operator that agrees exactly with the original  $\hat{A}^{(1)}$  on the one-quantum states and that constitutes an additive generalization of the original  $\hat{A}^{(1)}$  on multiquantum states. Note that the  $\hat{A}$  thus constructed is additive. For example, if  $\hat{A}^{(1)}$  is the square of the individual quantum momentum,  $\hat{A}$  is the sum of squares of the individual momenta in a multiple-quantum state, not the square of the total momentum. Nonadditive operators on the Fock space may be built up from the raising and lowering operators, which I will introduce shortly.

We can also represent the Fock space H as  $H_0 \oplus H_1 \oplus H_2 \oplus H_3 \oplus \cdots$ , the direct sum of Hilbert spaces  $H_0, H_1, H_2, H_3, \ldots$ , which are the Hilbert spaces with no quanta, exactly one quantum, exactly two quanta, and so on.  $H_1$  is our original Hilbert space. In terms of the new basis it is spanned by the following states:

$$|1, 0, 0, \ldots\rangle$$
  
 $|0, 1, 0, \ldots\rangle$   
 $|0, 0, 1, \ldots\rangle$   
 $\vdots$ 

H<sub>2</sub> is spanned by the states of the following form:

$$|1, 1, 0, 0, \ldots\rangle$$
  
 $|1, 0, 1, 0, \ldots\rangle$   
 $|0, 1, 1, 0, \ldots\rangle$   
 $\vdots$   
 $|2, 0, 0, 0, \ldots\rangle$   
 $|0, 2, 0, 0, \ldots\rangle$ 

The Hilbert space  $H_3$ , with exactly three quanta, and further  $H_i$  are all treated similarly.

Note that  $H_2$  is not the tensor product  $H_1 \otimes H_1$ .  $H_1 \otimes H_1$ , but not  $H_2$ , includes states such as  $|a_i(1)\rangle|a_j(2)\rangle$  and  $|a_i(2)\rangle|a_j(1)\rangle$ , which are just the labeled states, suggesting primitive thisness, which we want to eschew. When the eigenvalues of the  $\hat{N}_i^{(A)}$  are allowed to take on all non-negative integer values (to obtain a description of Bosons),  $H_2$  is isomorphic to the subspace of  $H_1 \otimes H_1$  composed of all its symmetric states. And when the eigenvalues of the  $\hat{N}_i^{(A)}$  are restricted to the values zero and one (for a description of Fermions),  $H_2$  is isomorphic to the subspace of  $H_1 \otimes H_1$  spanned by its antisymmetric states. We have achieved this description without at any point leaning on the conceptually problematic particle labels. The same sort of comments obviously go for all the  $H_i$ . Thus Fock space provides representations that function as the symmetric and antisymmetric states without the crutch of any superfluous structure, as promised.

In addition, Fock space provides the promised additional structure describing the states of indefinite and variable number of quanta found in nature. The full space includes superpositions of the  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$ . These superpositions represent states with an indefinite number of quanta. Evolution of the state vector within Fock space can represent motion from one definite  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$  to another, representing change in the number of quanta. More generally, evolution represents change in the statistical spread or uncertainty associated with the specific number of quanta.

# THE RAISING AND LOWERING OPERATORS

One can get a lot of help in the study and use of a Fock space by employing raising and lowering operators, which may be specified simply as transformations among the basis states of the Fock space as we introduced it in the last section. More specifically, we "factor" each number operator into two nonselfadjoint operators, a raising operator that carries a vector describing a definite number of quanta into a vector describing one more quantum, and a lowering operator that lowers the described number of quanta by one quantum. That the product of the raising and lowering operators should be the number operator determines the constants of proportionality when the raising and lowering

operators act on basis vectors, and a choice of phase gives us, in the one case, a scheme that works for Bosons, and in the other case one that works for Fermions. These facts already determine the commutation relations for Boson raising and lowering operators, and by also requiring that the description take the same form in all bases, we likewise fix the anticommutation relations for Fermions.

Let's get started by streamlining notation, writing the general  $|n_1, n_2, \ldots, n_i, \ldots, n_k\rangle_A$  simply as  $|\ldots n_i \ldots\rangle$ , showing only the occupation numbers of immediate relevance and dropping the 'A'. And let's first carry out this program for Bosons, for which the occupation numbers in the Fock space can take on any non-negative integer. For each  $\hat{N}_i$  we seek an operator  $\hat{a}_i$  such that  $\hat{N}_i = \hat{a}_i^{\dagger} \hat{a}_i$ , where  $\hat{a}_i^{\dagger}$  is the adjoint of  $\hat{a}_i$ . To accomplish this, we define  $\hat{a}_i$  such that

$$\hat{a}_i | \dots n_i \dots \rangle = c(n_i) | \dots n_i - 1 \dots \rangle$$
 when  $n_i > 0$ ,  $\hat{a}_i | \dots n_i \dots \rangle = 0$  when  $n_i = 0$ .

So far  $c(n_i)$  is an unspecified constant.

As the next step we evaluate the effect of  $\hat{a}_i^{\dagger}$  on a basis vector,  $|...n_i...\rangle$ . The components of  $\hat{a}_i^{\dagger}|...n_i...\rangle$  in the  $|...n_i...\rangle$  basis are

$$\langle \dots n'_i \dots | \hat{a}_i^{\dagger} | \dots n_i \dots \rangle = \langle \dots n_i \dots | \hat{a}_i | \dots n'_i \dots \rangle^*$$

$$= 0, \text{ if } n'_i = 0$$

$$= c^*(n'_i) \langle \dots n_i \dots | \dots n'_i - 1 \dots \rangle^*, \text{ if } n'_i \neq 0.$$

Because the  $|\ldots n_i \ldots\rangle$  are assumed to be orthonormal, we must have  $n_i' - 1 = n_i$  for nonvanishing components of  $\hat{a}_i^{\dagger} | \ldots n_i \ldots\rangle$ , so that

$$\hat{a}_i^{\dagger}|\ldots n_i\ldots\rangle = c_i^*(n_i+1)|\ldots n_i+1\ldots\rangle.$$

The stipulation that  $\hat{N}_i = \hat{a}_i^{\dagger} \hat{a}_i$  is the number operator for the  $a_i$ -quanta now determines the constants  $c(n_i)$  up to a phase:

$$egin{array}{lll} \hat{N_i}|\ldots n_i \ldots
angle &=& \hat{a}_i^{\dagger}\hat{a}_i|\ldots n_i \ldots
angle \ &=& n_i|\ldots n_i \ldots
angle \ &=& c^*(n_i)c(n_i)|\ldots n_i \ldots
angle, \end{array}$$

so that with  $w(n_i)$  an undetermined phase factor,

$$c(n_i) = w(n_i)\sqrt{n_i}.$$

Because the phase is arbitrary, we may take it to be 1. Then

(3.2) 
$$\hat{a}_i | \dots n_i \dots \rangle = \sqrt{n_i} | \dots n_i - 1 \dots \rangle$$
(3.3) 
$$\hat{a}_i^{\dagger} | \dots n_i \dots \rangle = \sqrt{n_i + 1} | \dots n_i + 1 \dots \rangle.$$

The foregoing enables us quickly to show that the commutators  $[\hat{a}_i, \hat{a}_i^{\dagger}] = \delta_{ij}$  and  $[\hat{a}_i^{\dagger}, \hat{a}_i^{\dagger}] = [\hat{a}_i, \hat{a}_j] = 0.^2$  For i = j,

$$\begin{split} \langle \dots n_i' \dots | \hat{a}_i \hat{a}_i^{\dagger} - \hat{a}_i^{\dagger} \hat{a}_i | \dots n_i \dots \rangle \\ &= \langle \dots n_i' \dots | \hat{a}_i \hat{a}_i^{\dagger} | \dots n_i \dots \rangle - n_i \langle \dots n_i' \dots | \dots n_i \dots \rangle \\ &= \sqrt{n_i + 1} \langle \dots n_i' \dots | \hat{a}_i | \dots n_i + 1 \dots \rangle - \\ & \qquad \qquad n_i \langle \dots n_i' \dots | \dots n_i \dots \rangle \\ &= (n_i + 1) \langle \dots n_i' \dots | \dots n_i \dots \rangle - n_i \langle \dots n_i' \dots | \dots n_i \dots \rangle \\ &= 1 \text{ if } n_i' = n_i \text{ and} \\ &= 0 \text{ if } n_i' \neq n_i \text{, by the orthonormality of the } | \dots n_i \dots \rangle. \end{split}$$

Thus  $[\hat{a}_i, \hat{a}_i^{\dagger}] = \hat{I}$ .

When  $i \neq j$ , and  $n_i > 0$ ,

$$\langle \dots n'_i \dots n'_j \dots | \hat{a}_i \hat{a}_j^{\dagger} - \hat{a}_j^{\dagger} \hat{a}_i | \dots n_i \dots n_j \dots \rangle$$

$$= \sqrt{n_j + 1} \sqrt{n_i} \langle \dots n'_i \dots n'_j \dots | \dots n_i - 1 \dots n_j + 1 \dots \rangle$$

$$- \sqrt{n_i} \sqrt{n_j + 1} \langle \dots n'_i \dots n'_j \dots | \dots n_i - 1 \dots n_j + 1 \dots \rangle$$

$$= 0.$$

When  $n_i = 0$  we get  $\hat{a}_j^{\dagger} \hat{a}_i | \dots n_j \dots n_j \dots \rangle = 0$  simply because  $\hat{a}_i | \dots n_i \dots n_j \dots \rangle = 0$ , because  $\hat{a}_i$  applied to any basis vector with  $n_i = 0$  gives zero. Because  $i \neq j$ ,  $\hat{a}_i \hat{a}_j^{\dagger} | \dots n_i \dots n_j \dots \rangle = 0$  likewise. The calculation of  $[\hat{a}_i^{\dagger}, \hat{a}_j^{\dagger}] = [\hat{a}_i, \hat{a}_j] = 0$  proceeds similarly.

Let's turn to the raising and lowering operators for Fermions. We start with a Fock space like that for Bosons, but with the specification that the occupation numbers can take on only the values zero and one. Again, we want operators  $\hat{a}_i$  such that the number operator  $\hat{N}_i = \hat{a}_i^{\dagger} \hat{a}_i$ , and we again begin by choosing  $\hat{a}_i$  such that

$$\hat{a}_i | \dots 1_i \dots \rangle = c(1) | \dots 0_i \dots \rangle, \text{ and }$$
 
$$\hat{a}_i | \dots 0_i \dots \rangle = 0.$$

<sup>&</sup>lt;sup>2</sup>Throughout the book, c-numbers are taken to multiply the identity matrix in operator equations.

The same calculation that we used for Bosons (3.1) shows that

$$\hat{a}_i^{\dagger}|\ldots 0_i \ldots\rangle = c^*(1)|\ldots 1_i \ldots\rangle.$$

Using (3.4), (3.5), and  $\hat{N}_i = \hat{a}_i^{\dagger} \hat{a}_i$ , we can show that |c(1)| = 1:

$$|\ldots 1_i \ldots\rangle = \hat{N}_i |\ldots 1_i \ldots\rangle$$
  
 $= \hat{a}_i^{\dagger} \hat{a}_i |\ldots 1_i \ldots\rangle$   
 $= c^*(1)c(1)|\ldots 1_i \ldots\rangle.$ 

A consistent description of Fermions requires a context-dependent choice of phase of  $\pm 1$ , as will be explained in the next section.

Because  $n_i = 0$  or 1,  $\hat{a}_i^{\dagger} | \dots 1_i \dots \rangle$  must equal zero. To see this more directly, suppose that  $\hat{a}_i^{\dagger} | \dots 1_i \dots \rangle \neq 0$ . Then  $|\hat{a}_i^{\dagger} | \dots 1_i \dots \rangle|^2 > 0$ , that is,  $\langle \dots 1_i \dots |\hat{a}_i \hat{a}_i^{\dagger} | \dots 1_i \dots \rangle > 0$ . But this is impossible. The action of  $\hat{a}_i$  on any vector, and so on  $\hat{a}_i^{\dagger} | \dots 1_i \dots \rangle$ , will leave either zero or a vector all the components of which will have zero in the  $i^{th}$  place, all of which are orthogonal to  $\langle \dots 1_i \dots |$ .

Because  $\hat{a}_i | \dots \hat{0}_i \dots \rangle = \hat{a}_i^{\dagger} | \dots \hat{1}_i \dots \rangle = 0$ , and there are only occupation numbers zero and one, we can immediately conclude that  $\hat{a}_i \hat{a}_i = \hat{a}_i^{\dagger} \hat{a}_i^{\dagger} = 0$ .

If in addition to the foregoing descriptions we use the requirement that the descriptions should take the same form in all bases, consistency requires Fermion raising and lowering operators to satisfy anticommutation relations: For operators  $\hat{a}$  and  $\hat{a}'$ ,  $[\hat{a},\hat{a}']_+ = \hat{a}\hat{a}' + \hat{a}'\hat{a}$ . Expressions of the form [,] will represent the usual commutator, but if there is any possibility of confusion, its notation may include a subscripted minus sign:  $[\hat{a},\hat{a}'] = [\hat{a},\hat{a}']_- = \hat{a}\hat{a}' - \hat{a}'\hat{a}$ . Fermion raising and lowering operator anticommutators have the same values as their Boson commutator counterparts:  $[\hat{a}_i,\hat{a}_j^{\dagger}]_+ = \delta_{ij}, [\hat{a}_i,\hat{a}_j]_+ = [\hat{a}_i^{\dagger},\hat{a}_j^{\dagger}]_+ = 0$ , as I now proceed to show.

Because  $\hat{a}_i\hat{a}_i=\hat{a}_i^{\dagger}\hat{a}_i^{\dagger}=0$ ,  $[\hat{a}_i,\hat{a}_i]_+=[\hat{a}_i^{\dagger},\hat{a}_i^{\dagger}]_+=0$ . For  $i\neq j$  we proceed as follows: The state  $\frac{1}{\sqrt{2}}(\hat{a}_i^{\dagger}|0,0,0,\ldots)+\hat{a}_j^{\dagger}|0,0,0,\ldots\rangle)$  bears the natural interpretation of a state with one quantum, which is the superposition of an i quantum and a j quantum. Consequently,  $\frac{1}{\sqrt{2}}(\hat{a}_i^{\dagger}+\hat{a}_j^{\dagger})$  bears the natural interpretation of a raising operator for this new kind of quantum. We are dealing with Fermions, and we want our description to be basis independent. In particular two raising operators for any quantum, such as  $\frac{1}{2}(\hat{a}_i^{\dagger}+\hat{a}_j^{\dagger})^2$ , should give zero on any state:

$$0 = (\hat{a}_i^{\dagger} + \hat{a}_i^{\dagger})(\hat{a}_i^{\dagger} + \hat{a}_i^{\dagger})$$

$$= \quad \hat{a}_{\pmb{i}}^{\dagger}\hat{a}_{\pmb{i}}^{\dagger} + \hat{a}_{\pmb{i}}^{\dagger}\hat{a}_{\pmb{j}}^{\dagger} + \hat{a}_{\pmb{j}}^{\dagger}\hat{a}_{\pmb{i}}^{\dagger} + \hat{a}_{\pmb{j}}^{\dagger}\hat{a}_{\pmb{j}}^{\dagger}.$$

Because  $\hat{a}_{i}^{\dagger}\hat{a}_{i}^{\dagger} = \hat{a}_{j}^{\dagger}\hat{a}_{j}^{\dagger} = 0$ , we have  $[\hat{a}_{i}^{\dagger}, \hat{a}_{j}^{\dagger}]_{+} = \hat{a}_{i}^{\dagger}\hat{a}_{j}^{\dagger} + \hat{a}_{j}^{\dagger}\hat{a}_{i}^{\dagger} = 0$ . Taking the adjoint immediately gives  $[\hat{a}_{i}, \hat{a}_{j}]_{+} = 0$ .

We evaluate  $[\hat{a}_i, \hat{a}_i^{\dagger}]_+$  by calculating its action on the basis vectors  $|\ldots 0_i \ldots\rangle$  and  $|\ldots 1_i \ldots\rangle$ . Because  $\hat{a}_i|\ldots 0_i \ldots\rangle = \hat{a}_i^{\dagger}|\ldots 1_i \ldots\rangle = 0$ , we then have

$$\begin{array}{rcl} (\hat{a}_i\hat{a}_i^{\dagger} + \hat{a}_i^{\dagger}\hat{a}_i)|\ldots 0_i\ldots\rangle & = & \hat{a}_i\hat{a}_i^{\dagger}|\ldots 0_i\ldots\rangle \\ & = & c(1)c^*(1)|\ldots 0_i\ldots\rangle \\ & = & |\ldots 0_i\ldots\rangle \end{array}$$

and

$$\begin{array}{rcl} (\hat{a}_i \hat{a}_i^{\dagger} + \hat{a}_i^{\dagger} \hat{a}_i) | \dots 1_i \dots \rangle & = & \hat{a}_i^{\dagger} \hat{a}_i | \dots 1_i \dots \rangle \\ & = & c^*(1) c(1) | \dots 1_i \dots \rangle \\ & = & | \dots 1_i \dots \rangle. \end{array}$$

Finally, we need to evaluate  $[\hat{a}_i,\hat{a}_j^{\dagger}]_+$  for  $i\neq j$ . It is easy to see that  $[\hat{a}_i,\hat{a}_j^{\dagger}]_+$  applied to  $|\ldots 0_i\ldots 0_j\ldots\rangle, |\ldots 1_i\ldots 1_j\ldots\rangle$ , and  $|\ldots 0_i\ldots 1_j\ldots\rangle$  gives zero by using  $\hat{a}_i|\ldots 0_i\ldots\rangle=\hat{a}_j^{\dagger}|\ldots 1_j\ldots\rangle=0$  for any  $|\ldots 0_i\ldots\rangle$  and any  $|\ldots 1_j\ldots\rangle$ . For the remaining basis vectors we use the anticommutation relations already shown, one in the form  $\hat{a}_i\hat{a}_i^{\dagger}=\hat{I}-\hat{N}_i$ , and the fact that for some phase factor,  $c, |\ldots 1_i\ldots 0_j\ldots\rangle=c\hat{a}_i^{\dagger}|\ldots 0_i\ldots 0_j\ldots\rangle$ :

$$\begin{split} (\hat{a}_i\hat{a}_j^\dagger + \hat{a}_j^\dagger\hat{a}_i)|\dots 1_i \dots 0_j \dots\rangle &= c(\hat{a}_i\hat{a}_j^\dagger + \hat{a}_j^\dagger\hat{a}_i)\hat{a}_i^\dagger|\dots 0_i \dots 0_j \dots\rangle \\ &= c\{-(\hat{I} - \hat{N}_i)\hat{a}_j^\dagger + \hat{a}_j^\dagger(\hat{I} - \hat{N}_i)\}|\dots 0_i \dots 0_j \dots\rangle \\ &= c\{\hat{N}_i\hat{a}_j^\dagger - \hat{a}_j^\dagger\hat{N}_i\}|\dots 0_i \dots 0_j \dots\rangle \\ &= 0. \end{split}$$

because in both terms  $\hat{N}_i$  operates on a vector representing zero quanta of kind i.

### BUILDING UP FOCK SPACE FROM THE VACUUM

The raising and lowering operators provide a particularly perspicuous description of the whole space as built up from the vector representing the state with no quanta, called the vacuum.

One customarily writes the vacuum as  $|0\rangle = |0, 0, \dots 0 \dots \rangle$ . For the moment considering only Bosons,  $\hat{a}_i^{\dagger}|0\rangle$  then represents a state with one quantum of kind i,  $\hat{a}_i^{\dagger}\hat{a}_j^{\dagger}|0\rangle$  represents a state with one i-quantum and one j-quantum, and so on. Any basis vector,  $|n_1 \dots n_i \dots, n_k\rangle$ , labeled with the integers,  $n_1 \dots n_i \dots, n_k$ , can clearly be represented in this fashion, by applying raising operators to  $|0\rangle$ ,  $\hat{a}_1^{\dagger}$  occurring  $n_1$  times ...  $\hat{a}_i^{\dagger}$  occurring  $n_i$  times ... and  $\hat{a}_k^{\dagger}$  occurring  $n_k$  times. Because each basis vector has a largest k for which  $n_k \neq 0$ , only finitely many raising operators are needed to construct a given basis vector. But the coefficients, (3.3), require that we put a normalization in this construction. Because, in general,  $\hat{a}_i^{\dagger}|\dots n_i \dots\rangle = \sqrt{n_i+1}|\dots n_i+1\dots\rangle$  a recursive construction gives

$$|\ldots n_i \ldots\rangle = \prod_i \frac{1}{\sqrt{n_i!}} (\ldots \hat{a}_i^{\dagger} \ldots) |0\rangle$$
, with  $n_i$  occurrences of  $\hat{a}_i^{\dagger}$ .

Because the raising operators all commute, the operator order is immaterial for Bosons.

For Fermions, the order of the anticommuting raising operators makes a difference in sign, and because there are only occupation numbers of zero and one, there is no further issue about normalization.

Conventionally, one deals with the sign issue by building up the basis vectors from the vacuum with raising operators of increasing index:

$$|1,1,1,\dots n_i \dots\rangle = \hat{a}_1^{\dagger}\hat{a}_2^{\dagger}\hat{a}_3^{\dagger}\cdots \hat{a}_i^{\dagger}\hat{a}_j^{\dagger}\cdots \hat{a}_k^{\dagger}|0\rangle, \quad i < j < k.$$

With this convention  $\hat{a}_i^{\dagger}|\dots 0_i\dots\rangle = \pm |\dots 1_i\dots\rangle$ , with + if there are an even number of occupied states with index less than i and - with an odd number of such states. (This is the point at which a consistent Fermion description constrains a choice of phase for the raising and lowering operators.)

# ALTERNATIVE NUMBER REPRESENTATIONS

I presented Fock space in the  $\hat{A}$ -number representation, with the basis vectors constituting generalizations of a basis for the one-quantum states, the eigenvectors  $|a_1\rangle$ ,  $|a_2\rangle$ ,  $|a_3\rangle$ ,... of a maximal one-quantum observable,  $\hat{A}^{(1)}$  on  $H_1$ . In the one-quantum space,  $H_1$ , we can transform to another orthonormal basis,

$$|b_i
angle \ = \ \sum_j |a_j
angle \langle a_j|b_i
angle$$

$$=\sum_{m j} c_{m j m i} |a_{m j} 
angle.$$

In the Fock space representation, this is equivalently written as

$$\hat{b}_{m{i}}^{\dagger}|0
angle = \sum_{m{j}} c_{m{j}m{i}}\hat{a}_{m{j}}^{\dagger}|0
angle.$$

This relation for the one-quantum states motivates taking the operator  $\hat{b}_i^{\dagger} = \sum_j c_{ji} \hat{a}_j^{\dagger}$  as a raising operator for  $\hat{B}$ -quanta, the quanta characterized as superpositions of the  $\hat{A}$ -quanta according to the unitary transformation  $c_{ji}$ .

It is easy to see that the  $\hat{b}_i^{\dagger}$  and their adjoints,  $\hat{b}_i$ , have all the formal properties of raising and lowering operators operating on a  $\hat{B}$ -number representation basis built up from  $|0\rangle$  in a manner exactly analogous to the construction of the  $\hat{A}$ -number basis that results from the  $\hat{a}_i^{\dagger}$  applied to  $|0\rangle$ . The key lies in the commutation relations, which, established for the  $\hat{a}_i$  and  $\hat{a}_i^{\dagger}$ , carry over immediately to the  $\hat{b}_i$  and  $\hat{b}_i^{\dagger}$ :

$$\begin{split} [\hat{b}_{i},\hat{b}_{i'}^{\dagger}]_{\pm} &= \sum_{j} \sum_{j'} c_{ji}^{*} c_{j'i'} [\hat{a}_{j},\hat{a}_{j'}^{\dagger}]_{\pm} \\ &= \sum_{j} c_{ji}^{*} c_{ji'}, \text{ since } [\hat{a}_{j},\hat{a}_{j'}^{\dagger}]_{\pm} = \delta_{jj'} \\ &= \delta_{ii'}, \end{split}$$

because the  $c_{ii}$  represent a unitary transformation. Also,

$$[\hat{b}_i^\dagger,\hat{b}_{i'}^\dagger]_\pm = \sum_j \sum_{j'} c_{ji} c_{j'i'} [\hat{a}_j^\dagger,\hat{a}_{j'}^\dagger]_\pm = 0$$

because  $[\hat{a}_{j}^{\dagger}, \hat{a}_{j'}^{\dagger}]_{\pm} = 0$ . Finally,  $[\hat{b}_{i}, \hat{b}_{i'}]_{\pm} = 0$  follows as the adjoint relation.

With the commutators at hand, we easily build up a  $\hat{B}$ -number representation analogous to the  $\hat{A}$ -number representation.<sup>3</sup> To sketch

<sup>&</sup>lt;sup>3</sup>What immediately follows is much closer to conventional presentations of Fock space than the preceding sections. The conventional presentations start with the raising and lowering operators and their commutation relations as suggested by analysis of the quantized harmonic oscillator and show how these call for the number-representation basis states. I have presented the material in the reverse order, starting with the number-representation states as a basis for a Hilbert space, and then showing how such a space comes equipped with raising and lowering operators with the familiar properties. The point of the unconventional order of presentation is to show how, by starting with the idea of aggregable quanta, a few natural assumptions

this development for Bosons, we introduce the  $\hat{B}$ -number representation as

$$(3.6)|\ldots n_i\ldots\rangle_B=\Pi_i \frac{1}{\sqrt{n_i!}}(\ldots \hat{b}_i^\dagger\ldots)|0\rangle, \text{ with } n_i \text{ occurrences of } \hat{b}_i^\dagger.$$

Note that because the  $\hat{b}_i^{\dagger}$  are defined as  $\sum_j c_{ji} \hat{a}_j^{\dagger}$ , there is no question here but that the vacuum state  $|0\rangle$  is the same as the one used in developing the  $\hat{A}$ -number representation. Also, because nothing has been said about the order of the  $\hat{b}_i^{\dagger}$ , we are tacitly making use of the fact that the  $\hat{b}_i^{\dagger}$  commute. This construction immediately gives the right coefficients for applying  $\hat{b}_i^{\dagger}$  to a basis vector. By the construction

$$(3.6), |\ldots n_i + 1 \ldots \rangle_B = \frac{\hat{b}_i^{\dagger}}{\sqrt{n_i + 1}} |\ldots n_i \ldots \rangle_B. \text{ Hence } \hat{b}_i^{\dagger} |\ldots n_i \ldots \rangle_B = \sqrt{n_i + 1} |\ldots n_i + 1 \ldots \rangle_B.$$

We establish that the  $\hat{b}_i$  constitute lowering operators on the  $|...n_i...\rangle_B$ , as well as the coefficients for the  $\hat{b}_i$  with an inductive argument appealing to the commutation relations and the coefficients for the  $\hat{b}_i^{\dagger}$ . We will also use the fact that  $\hat{b}_i|0\rangle = \sum_j c_{ji}^* \hat{a}_j|0\rangle = 0$ , because  $\hat{a}_j|0\rangle = 0$  for each  $\hat{a}_j$ . For all  $n_i$ 

$$(3.7) \qquad \hat{b}_{i}|\dots n_{i}+1\dots\rangle_{B} = \frac{1}{\sqrt{n_{i}+1}}\hat{b}_{i}\hat{b}_{i}^{\dagger}|\dots n_{i}\dots\rangle_{B}$$
$$= \frac{1}{\sqrt{n_{i}+1}}(\hat{I}+\hat{b}_{i}^{\dagger}\hat{b}_{i})|\dots n_{i}\dots\rangle_{B}.$$

For  $n_i=0$ ,  $\hat{b}_i|\ldots 0_i\ldots\rangle_B=0$ . Thus  $\hat{b}_i|\ldots 1_i\ldots\rangle_B=|\ldots 0_i\ldots\rangle_B$ . For  $n_i>0$ , we use the  $\hat{b}_i^{\dagger}$  coefficients and argue inductively using  $\hat{b}_i|\ldots n_i\ldots\rangle_B=\sqrt{n_i}|\ldots n_i-1\ldots\rangle_B$  in arguing the case for  $n_i+1$ . Thus

$$b_{i}|\dots n_{i}+1\dots\rangle_{B} = \frac{1}{\sqrt{n_{i}+1}}(\hat{I}+\hat{b}_{i}^{\dagger}\hat{b}_{i})|\dots n_{i}\dots\rangle_{B}$$
$$= \sqrt{n_{i}+1}|\dots n_{i}\dots\rangle_{B}.$$

The coefficients for the  $\hat{b}_i^{\dagger}$  and  $\hat{b}_i$  immediately yield the conclusion that  $\hat{N}_i^{(B)} = \hat{b}_i^{\dagger} \hat{b}_i$  acts as a number operator on the  $|\dots n_i \dots \rangle_B$ . To distinguish the number operators arising from the various one-quantum

lead almost automatically to the Fock space description. In my presentation I have closely followed Merzbacher (1970), pp. 508-17. To some extent Schweber's presentation resembles the one found here, though I feel his presentation still carries along too much of the "particle" way of thinking. A brief similar presentation is in Fetter and Walecka (1971), pp. 12ff.

bases,  $A^{(1)}$ ,  $B^{(1)}$ ,..., I am using superscripts:  $\hat{N}_i^{(A)} = \hat{a}_i^{\dagger} \hat{a}_i$ ,  $\hat{N}_i^{(B)} = \hat{b}_i^{\dagger} \hat{b}_i$ ,

The discussion for Fermions is very similar, with a few minor adjustments to take account of the occurrence of anticommutators instead of commutators. Because the raising operators anticommute, we specify that the basis vectors,

$$|1,1,\ldots n_i\ldots\rangle = \hat{b}_1^\dagger \hat{b}_2^\dagger \hat{b}_3^\dagger \cdots \hat{b}_i^\dagger \hat{b}_j^\dagger \cdots \hat{b}_k^\dagger |0\rangle, \quad i < j < k,$$

are formed with raising operators of increasing index, exactly as in the  $\hat{A}$ -number representation. This results in exactly the same sign convention we used earlier. For the  $\hat{b}_i$ , (3.7) has only one case,  $n_i = 0$ :

$$egin{array}{lll} \hat{b}_i | \dots 1_i \dots 
angle & & \pm \hat{b}_i \hat{b}_i^\dagger | \dots 0_i \dots 
angle \ & & \pm (\hat{I} - \hat{b}_i^\dagger \hat{b}_i) | \dots 0_i \dots 
angle \ & & & \pm | \dots 0_i \dots 
angle, \end{array}$$

with + for an even number of occupied states with index less than i, and – otherwise. Finally, these facts immediately show that  $\hat{N}_i^{(B)} = \hat{b}_i^{\dagger} \hat{b}_i$  acts formally like a number operator.

The orthonormality of the  $|\ldots n_i \ldots \rangle_B$  follows from that of the  $|\ldots n_i \ldots \rangle_A$  and the commutation relations, for both Bosons and Fermions, straightforwardly if somewhat tediously checked.

# CONTINUOUS SPECTRA AND THE ROLE OF THE SPATIAL VARIABLE

Up to this point my exposition has addressed only operators with discrete spectra. Continuous spectra involve further formal complications. In many respects the complications and the resources for handling them carry over straightforwardly to Fock space from a Hilbert space for one quantum.

A one-quantum operator on  $H_1$  with a continuous spectrum does not have eigenvectors that are, strictly speaking, in  $H_1$ . One can deal with this either by imposing periodic boundary conditions, thereby approximating a continuous spectrum with a discrete spectrum, or by calculating with delta functions, which provide a most effective tool in spite of constituting a physical fiction.<sup>4</sup>

<sup>&</sup>lt;sup>4</sup>Though one now knows, using the theory of distributions, how to provide mathematically sound foundations for the universal practice of using delta functions in

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Nothing really changes when we move from  $H_1$  to  $H_2$ ,  $H_3$ , or any specific  $H_n$ . The same mathematics that were used with one variable now can be done with two, three, or n variables. Thus no new mathematics is needed to describe continuous spectra in application to states with a fixed, finite number of quanta. The situation does change if we want to describe continuous spectra with full generality in Fock space, with no bounds on the number of quanta, which, in principle, might be in question, for then we must describe the situation in terms of an unlimited number of variables.<sup>5</sup>

The momentum and especially the position variable constitute continuous quantities of particular importance for interpretive questions. Suppose we started in a basis representing exact momentum states, so that  $\hat{a}^{\dagger}(\mathbf{k})$  is the creation operator for a particle of momentum  $\mathbf{k}$  and  $|\mathbf{k}\rangle = \hat{a}^{\dagger}(\mathbf{k})|0\rangle$  is the state with exactly one quantum of momentum  $\mathbf{k}$ . In a non-relativistic theory we can imitate the Fourier transform method familiar from traditional formulations of quantum mechanics. If  $\langle \mathbf{k}|\mathbf{x}\rangle$  is the transformation function between the momentum and position basis in the traditional one-quantum Hilbert space,  $H_1$ , then, in the point-valued delta function idealization, a state with one quantum exactly located at  $\mathbf{x}$  is represented as  $|\mathbf{x}\rangle = \int d^3\mathbf{k} |\mathbf{k}\rangle \langle \mathbf{k}|\mathbf{x}\rangle = \int d^3\mathbf{k} \langle \mathbf{k}|\mathbf{x}\rangle \hat{a}^{\dagger}(\mathbf{k})|0\rangle$ . This motivates our treating the raising operator  $\hat{\Psi}^{\dagger}(\mathbf{x}) = \int d^3\mathbf{k}\langle \mathbf{k}|\mathbf{x}\rangle \hat{a}^{\dagger}(\mathbf{k})$  as one that takes the vector representing the vacuum to one representing a state with one quantum located at  $\mathbf{x}$ , or any state to a new state with an additional quantum located at  $\mathbf{x}$ .

One must remember, however, that all of this involves delta function idealizations, so that correct physical descriptions really emerge only after integration over a volume. For example, we can think of  $\hat{\Psi}^{\dagger}(\mathbf{x})\hat{\Psi}(\mathbf{x})$  as a "number density" operator, giving the "density" of particles at a point in space. But  $\hat{\Psi}^{\dagger}(\mathbf{x})\hat{\Psi}(\mathbf{x})|\mathbf{x}'\rangle = \delta(\mathbf{x}-\mathbf{x}')|\mathbf{x}'\rangle$ , so that the "eigenvalue" is either zero or infinity. Talk about the "particle density" oper-

manipulating the theory, such devices still constitute a severe physical idealization. For example, even in a nonrelativistic theory, localization within an arbitrarily small interval involves arbitrarily large, and so nonphysical, energies. When relativistic considerations are taken into account, things get much worse.

<sup>&</sup>lt;sup>5</sup>One can get the effect of an unlimited number of variables by describing things in terms of functions that are themselves allowed to vary. Physics and mathematics have filled in at this point with techniques of functional differentiation (differentiation of a functional with respect to a function) and functional integration (integration with respect to a measure defined on a space of functions). I will not discuss these functional methods, which are currently much used in work on quantum field theory.

ator is really shorthand for such things as  $\int d^3\mathbf{x}\hat{\Psi}^{\dagger}(\mathbf{x})\hat{\Psi}(\mathbf{x})$ , which has eigenstates with integer eigenvalues.

Where do we get the  $\hat{a}^{\dagger}(\mathbf{k})$ , the raising operators for states of exact momentum? As for exact position, states of exact momentum constitute a physical idealization, descriptions of which are nonetheless of great practical value in manipulating the theory. The introduction of the  $\hat{a}^{\dagger}(\mathbf{k})$  will seem particularly natural when one approaches the subject from a field-theoretic point of view. As we will see in the next chapter, on this approach, one naturally thinks of the parameter  $\mathbf{k}$  as ranging over the wave numbers of the plane waves into which one can decompose a classical field. After quantization, these wave numbers are reinterpreted as momenta.

In the next chapter I will describe these ideas in greater detail, as well as their connections with the ideas of Fock space and the raising and lowering operators as I have presented them in this chapter. However, at the moment I want to emphasize the point that no field-theoretic heuristic is required. Instead one can start from the idea that the world contains entities that can be aggregated, called quanta. One can then describe these with a Fock space and stipulate that (in a point-valued idealization) a collection of raising operators,  $\hat{a}^{\dagger}(\mathbf{k})$ , parameterized by the continuous variable  $\mathbf{k}$ , will work to build up vectors describing exact momentum states from the vector  $|0\rangle$ .

The role of position in a relativistically correct theory is much more complicated. It is often said that in conventional quantum mechanics position appears as an operator, whereas in quantum field theory it appears as a parameter. This is misleading and if read literally, is strictly speaking not correct. In both theories position appears both as a parameter and as an operator. It is easy to miss the fact that in this regard we have the same situation in both nonrelativistic and relativistic theories, because in nonrelativistic theories there is a natural correspondence between position in its parameter role and its operator role, whereas in relativistic theories the two roles separate more clearly. Moreover, in the familiar wave function representation of nonrelativistic theories, the distinction is almost completely obscured.

One can see the point about nonrelativistic theories quite sharply in the Dirac notation. If we represent one-quantum states in the position basis, each ket gets labeled with the variable  $\mathbf{x} : |\mathbf{x}\rangle$ . Here 'x' clearly is functioning as a label or parameter. The "psi-function,"  $\Psi(\mathbf{x})$ , is really the x component of the ket  $|\Psi\rangle : \Psi(\mathbf{x}) = \langle \mathbf{x} | \Psi \rangle$ . In the Dirac notation,

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the position operator is represented by  $\int d^3\mathbf{x}'|\mathbf{x}'\rangle\mathbf{x}'\langle\mathbf{x}'|$ . Its application to an arbitrary ket,  $|\Psi\rangle$ , gives  $\int d^3\mathbf{x}'|\mathbf{x}'\rangle\mathbf{x}'\langle\mathbf{x}'|\Psi\rangle = \int d^3\mathbf{x}'\mathbf{x}'\Psi(\mathbf{x}')|\mathbf{x}'\rangle$ . The  $|\mathbf{x}\rangle$  component of this vector is  $\langle\mathbf{x}|\int d^3\mathbf{x}'\mathbf{x}'\Psi(\mathbf{x}')|\mathbf{x}'\rangle = \int d^3\mathbf{x}'\delta(\mathbf{x}-\mathbf{x}')\mathbf{x}'\Psi(\mathbf{x}') = \mathbf{x}\Psi(\mathbf{x})$ . Thus, in the familiar psi-function representation the variable,  $\mathbf{x}$ , appears as an "operator." But, strictly speaking, the position operator is  $\int d^3\mathbf{x}'|\mathbf{x}'\rangle\mathbf{x}'\langle\mathbf{x}'|$ . The correspondence lies in the fact that a basis ket parameterized by position,  $|\mathbf{x}\rangle$ , is also (in the point-valued idealization) an eigenstate of strict localization.

In a relativistic theory one likewise can use position to parameterize states and the raising and lowering operators. But the way this proceeds, and the relation to a position operator, is much more complicated. The (locally covariant) Fourier transforms of the momentum representation raising operators do not give a set of raising operators for states interpretable as states of exact localization. There is, however, another set of states and a corresponding operator, which merit interpretation as states of strict localization and as a position operator, though these have some surprising properties. I will discuss these complications in the next chapter.<sup>6</sup>

# CONCLUSION AND SUMMARY

The development so far completes the argument begun in chapter 2: Our use of particle labels and the LTPHSF to describe multiquanta systems is a historical accident (an accident, I conjecture, to which many of us are prone as a result of implicitly thinking of particles as having primitive thisness). A Fock space description, free of the surplus structure of the LTPHSF, provides the most parsimonious description of multiquanta states and so the description that arguably gives us the best picture of the nature of quanta. And this picture presents us with a view of entities quite free of primitive thisness.

The presentation of Fock space also shows that it provides a description of the quantum mechanics of many-quanta systems of any kind, with no necessary tie to a field-theoretic setting or a relativistic descrip-

<sup>&</sup>lt;sup>6</sup>I have found fault with the conventional wisdom that in conventional quantum mechanics position appears as an operator, whereas in quantum field theory it appears as a parameter. However, one can squeeze an important truth from this aphorism, namely, the fact that in quantum field theory position serves to parameterize operators, whereas in conventional quantum mechanics it parameterizes only states. n. 19 in ch. 4 will elaborate on this point.

tion. All one needs is the quantum mechanics for a single quantum, and the procedure we have surveyed applies to provide a description for an indefinite number of quanta. In view of this generality, the Fock space representation gives us an attractive view of the ontology of any quantum theory, at least insofar as issues about the nature of "particles" or "quanta" are in question.

But what does all of this have to do with something that all practitioners see fit to call a field theory?

#### PROBLEMS

1. How does the physical content of quantum field theory appear when one views the theory from the point of view of functional methods? To what extent do these methods lead to similar interpretive conclusions reached in this book, and to what extent do they suggest new interpretive elements?

# Free Quantum Field Theory

#### FIELD THEORIES

FREE QUANTUM FIELD THEORY is the theory of quantum fields in which the effect of interactions is entirely neglected. But we had better start by saying what we mean by a field theory. A field theory presupposes a continuum of space-time points. This involves a number of technical considerations that do not concern us here. (The continuum constitutes a differentiable manifold, which can be described by coordinates and to which an affine connection and a metric can be assigned.) The idea of a field enters as the idea that values of physical quantities can be attributed to the space-time points. Specification of the values of all relevant quantities to each space-time point specifies a configuration of the field. And we have a field theory insofar as we have field equations, that is, laws, usually in the form of differential equations, constraining the values of quantities at different space-time points.

I want to focus on the idea that values of various quantities can be attributed to the space-time points. In any specific case we talk about such attribution in much the way we talk about predication of simple properties: One attributes a humdrum property (for example, the color red) to an object (for example, my shirt) by associating a predicate (the word, 'red') with a referring expression ('my shirt'). Similarly, in describing the value of a field quantity (for example, a gravitational potential) at a point, one associates a mathematical entity (for example, a real number) representing the value of the quantity with numerical coordinates representing the point.

What kinds of quantities can be attributed to the space-time points? We know familiar examples in the form of scalars (a matter density, a gravitational potential), vectors (electric and magnetic fields), and higher-order tensors (the stress-energy tensor). When I ask what one means by "the quantum field" I am often told that it is just the association of an operator with each of the space-time points. Just as one may characterize a field by associating a scalar, a vector, or a higher-order tensor with space-time points, there is no reason why one cannot

further generalize and similarly associate other mathematical entities with the points, for instance, operators (or more generally tensors with operator-valued components).

This specifies a perfectly clear formal sense to the notion of the quantum field. I will further examine what this involves in the next chapter, but for now I propose simply to plunge ahead and lay out the well-known connection between the Fock space description of the last chapter and the quantum field in the formal sense just described. I want to do this in order to lay out for the reader the conception of the quantum field as it is generally understood by practitioners and also in order to make clear the connection between this conception and the less orthodox order of presentation given in the last chapter.

#### THE TIME-INDEPENDENT OPERATOR-VALUED FIELD

Chapter 3 already presented the core idea. If  $\{|\mathbf{x}\rangle\}$  is the momentum basis and  $\{|\mathbf{x}\rangle\}$  is the position basis for the one-quantum case, then a change from the momentum to the position basis gives raising operators  $\hat{\Psi}^{\dagger}(\mathbf{x}) = \int d^3 \tilde{\mathbf{k}} \langle \mathbf{k} | \mathbf{x} \rangle \hat{a}^{\dagger}(\mathbf{k})$ , where  $\hat{a}^{\dagger}(\mathbf{k})$  is the raising operator for a quantum with momentum  $\mathbf{k}$ . The function  $\hat{\Psi}^{\dagger}(\mathbf{x})$  now constitutes an operator-valued field (over space at a fixed time) in at least the formal sense just described, because it associates an operator with each value of  $\mathbf{x}$ .

To follow this and succeeding ideas clearly, newcomers to the sub-

<sup>&</sup>lt;sup>1</sup>The tilde over the k marks normalization constants, which I omit throughout. Thus, in a nonrelativistic context one may take  $d^3\tilde{\mathbf{k}} = d^3\mathbf{k}/(2\pi)^{3/2}$ . In manifestly covariant formulations of relativistic theories one absorbs factors into the momentum space measure to make the measure Lorentz invariant. For Bosons, one most straightforwardly does this by choosing the invariant normalization  $d^3\tilde{\mathbf{k}} =$  $d^3\mathbf{k}/(2\pi)^{3/2}\omega(\mathbf{k})$ , thereby also requiring the invariant normalization for momentum eigenstates,  $\langle \mathbf{k} | \mathbf{k}' \rangle = \omega(\mathbf{k}) \delta^3(\mathbf{k} - \mathbf{k}')$ . Although these are the best conventions for making invariance manifest, they create a pedagogical problem: In presentations of field versions of the nonrelativistic theory, and also in many elementary presentations of relativistic field quantization, authors tacitly use  $\langle \mathbf{k} | \mathbf{k}' \rangle = \delta^3(\mathbf{k} - \mathbf{k}')$  normalization, resulting in the presence and absence of a  $\omega(\mathbf{k})$  factor, respectively for expressions for commutators and energy operators where one intuitively in the two cases would not and would expect them. In the elementary presentation of field quantization below it seems pedagogically best to cater to nonrelativistic intuitions and use  $\langle \mathbf{k} | \mathbf{k}' \rangle = \delta^3(\mathbf{k} - \mathbf{k}')$  normalization of the  $|\mathbf{k}\rangle$  states. This corresponds to  $d^3\tilde{\mathbf{k}} = d^3\mathbf{k}/[(2\pi)^{3/2}\sqrt{\omega(\mathbf{k})}]$ , in effect simply canceling out a factor of  $\sqrt{\omega(\mathbf{k})}$  in the integral expressions for  $\hat{\Psi}(\mathbf{x},t)$ , below. Footnotes will keep track of expressions for  $\langle \mathbf{k} | \mathbf{k}' \rangle = \omega(\mathbf{k}) \delta^3(\mathbf{k} - \mathbf{k}')$  normalization.

ject must bear in mind how we can go back and forth between states and raising operators for the one-quantum case. To review with the simplifying assumption of discrete spectra, we can start in an arbitrary one-quantum basis,  $\{|a_i\rangle\}$ . We redescribe these states with the raising operators  $\hat{a}_i^{\dagger}: \hat{a}_i^{\dagger}|0\rangle = |a_i\rangle$ . If  $\{|b_i\rangle\}$  is a second basis, we have

$$|b_i\rangle = \sum_j \langle a_j | b_i \rangle |a_j \rangle$$
  
=  $\sum_j \langle a_j | b_i \rangle \hat{a}_j^{\dagger} |0 \rangle$ .

Introducing  $\hat{b}_i^{\dagger}:\hat{b}_i^{\dagger}|0\rangle=|b_i\rangle$ , we have

$$\hat{b}_{i}^{\dagger}|0
angle =\sum_{i}\langle a_{j}|b_{i}
angle \hat{a}_{j}^{\dagger}|0
angle ,$$

suggesting

$$\hat{b}_{\pmb{i}}^{\dagger} = \sum_{\pmb{j}} \langle a_{\pmb{j}} | b_{\pmb{i}} 
angle \hat{a}_{\pmb{j}}^{\dagger},$$

where  $\langle a_j|b_i\rangle$  is the transformation function between the  $\{|a_i\rangle\}$  and  $\{|b_i\rangle\}$  bases. For the one-quantum case,  $\hat{b}_i^{\dagger}$  and  $\sum_j \langle a_j|b_i\rangle \hat{a}_j^{\dagger}$  are equivalent, and we postulate that the equivalence holds for multiquantum cases.

We will get the best intuitive grasp on the analogical reasoning that follows if we move freely back and forth between the formulations

$$|b_i
angle = \sum_j \langle a_j |b_i
angle |a_j
angle$$

and

$$\hat{b}_{\pmb{i}}^{\dagger} = \sum_{\pmb{j}} \langle a_{\pmb{j}} | b_{\pmb{i}} 
angle \hat{a}_{\pmb{j}}^{\dagger}.$$

I have presented this intuitive summary in terms of discrete bases  $\{|a_i\rangle\}$  and  $\{|b_i\rangle\}$ . In using the (idealized point-valued) momentum and position bases,  $\{|\mathbf{k}\rangle\}$  and  $\{|\mathbf{x}\rangle\}$ , we use the continuous momentum and position parameters,  $\mathbf{k}$  and  $\mathbf{x}$ , instead of discrete indexes, i and j, and corresponding integrals instead of summations. In this application the equivalence between one-quantum state and raising-operator formulations appears as

$$|\mathbf{x}\rangle = \int d^3 \tilde{\mathbf{k}} \langle \mathbf{k} | \mathbf{x} \rangle | \mathbf{k} \rangle,$$

going over to

$$\hat{\Psi}^{\dagger}(\mathbf{x}) = \int d^{3}\tilde{\mathbf{k}} \langle \mathbf{k} | \mathbf{x} \rangle \hat{a}^{\dagger}(\mathbf{k}).$$

Using the transformation function  $\langle \mathbf{k} | \mathbf{x} \rangle = e^{-i\mathbf{k} \cdot \mathbf{x}}$ , we have

$$|\mathbf{x}
angle = \int d^3 ilde{\mathbf{k}} \mathrm{e}^{-i\mathbf{k}\cdot\mathbf{x}} |\mathbf{k}
angle,$$

which goes over to

$$\hat{\Psi}^{\dagger}(\mathbf{x}) = \int d^{3}\tilde{\mathbf{k}} \mathrm{e}^{-i\mathbf{k}\cdot\mathbf{x}}\hat{a}^{\dagger}(\mathbf{k}).$$

This, usually presented in its adjoint,

$$\hat{\Psi}(\mathbf{x}) = \int d^3 \tilde{\mathbf{k}} \mathrm{e}^{i\mathbf{k}\cdot\mathbf{x}} \hat{a}(\mathbf{k}),$$

is the time-independent operator-valued, or quantum field, as it often appears in texts.

I have called the basis  $\{|\mathbf{x}\rangle\}$  a 'position basis': We would like to think of it as eigenstates for exact position. This interpretation works for a nonrelativistic field-theoretic version of the Schrödinger theory, where we take the momentum space measure  $d^3\tilde{\mathbf{k}} = \frac{d^3\mathbf{k}}{(2\pi)^{3/2}}$  and  $\langle \mathbf{k} | \mathbf{x} \rangle = e^{-i\mathbf{k} \cdot \mathbf{x}}$ . But proceeding in this way for a relativistically correct theory produces descriptions with very bad transformation properties. As we will see later, to obtain a relativistic description with well-behaved transformation properties (and to get a formulation in agreement with common practice) using  $\langle \mathbf{k} | \mathbf{x} \rangle = \mathrm{e}^{-i\mathbf{k}\cdot\mathbf{x}}$  requires using the relativistically invariant momentum space measure  $d^3\tilde{\mathbf{k}} = \frac{d^3\mathbf{k}}{(2\pi)^{3/2}\omega(\mathbf{k})}$ . This combination spoils the interpretation of the  $\{|\mathbf{x}\rangle\}$  as a genuine position basis. The  $|\mathbf{x}\rangle$  are still indexed by the spatial parameter, x, and each  $|\mathbf{x}\rangle$  is still like a wave packet in that it is concentrated within a Compton wavelength of x. But there is no Hermitian operator with the  $|x\rangle$  as eigenstates with eigenvalues x. Readers already familiar with the theory will want to keep this in mind as we proceed. I urge novices to put this problem aside for the moment. I will return to discuss the issue at the end of the chapter.

## THE TIME-DEPENDENT OPERATOR-VALUED FIELD

Let's introduce time dependence into the operator-valued field. In so doing we will be introducing a description in which the time dependence is carried by the operators, so that we will be working in the Heisenberg picture in which vectors representing states of a physical system remain fixed, and the time dependence is all carried by the operators. (I will discuss the Heisenberg picture and how it is to be understood in detail in chapter 5. For the moment, this rough description should suffice.)

In the last section we presented the operator-valued field,  $\hat{\Psi}^{\dagger}(\mathbf{x})$ , in terms of the momentum-raising operators  $\hat{a}^{\dagger}(\mathbf{k})$ . It will be particularly easy to introduce time dependence through the  $\hat{a}^{\dagger}(\mathbf{k})$  and their eigenstates  $\hat{a}^{\dagger}(\mathbf{k})|0\rangle = |\mathbf{k}\rangle$  because, for free quanta, momentum eigenstates are also energy eigenstates, which fact trivializes solution of the equation of motion. But we have to be a little careful about how the momentum eigenstates are understood. Introducing time dependence into the momentum-raising operators, writing  $\hat{a}^{\dagger}(\mathbf{x},t)$ , we get  $\hat{a}^{\dagger}(\mathbf{x},t)|0\rangle = |\mathbf{k},t\rangle$ , a ket parameterized both by the momentum and by the time variable. The ket  $|\mathbf{k}, t\rangle$  is the k-momentum eigenstate of the Heisenberg picture time-dependent momentum operator at time t. Because in the Heisenberg picture the momentum operator is time dependent, its eigenvectors are likewise time dependent. The possibly confusing point to keep straight here is that  $|\mathbf{k}, t\rangle$  is not a time-evolving representation of some physical state—in the Heisenberg picture physical states are represented by time-independent vectors. Instead, at each time, t,  $|\mathbf{k}, t\rangle$  is the time variable k-eigenvector of the momentum operator, a different vector at different times.

I fussed with the correct understanding of  $|\mathbf{k},t\rangle$  because the point, together with the usual sign conventions, dictates an unexpected sign in the equation of motion we will need. Because  $|\mathbf{k},t\rangle$  is a time-evolving momentum eigenvector of a time-evolving operator, we represent its change in time not by  $\hat{U} = \exp(-i\hat{H}t)$  but by  $\hat{U}^{\dagger} = \exp(i\hat{H}t)$ :  $\hat{U}^{\dagger}(t)|\mathbf{k},0\rangle = |\mathbf{k},t\rangle$  ( $\hat{H}$  assumed to be time independent).<sup>2</sup>  $\hat{H}$  is a selfadjoint operator corresponding to the system's energy. That  $\hat{U}(t)$  can be thus expressed in terms of  $\hat{H}$  is connected with the fact that the energy operator is the generator of infinitesimal time displacements, in turn connected with conservation of energy. I will have better occasion to consider that topic later.

With  $\hat{H}$  time independent, we can differentiate  $|{\bf k},t\rangle=\hat{U}^{\dagger}|{\bf k},0\rangle$  to get a "Schrödinger equation":

(4.1) 
$$-i\frac{\partial}{\partial t}|\mathbf{k},t\rangle = \hat{H}|\mathbf{k},t\rangle.$$

<sup>&</sup>lt;sup>2</sup>Suppose that in the Schrödinger picture,  $|o\rangle$  is an o-eigenstate of operator  $\hat{O}$ :  $\hat{O}|o\rangle = o|o\rangle$ . Then the corresponding o-eigenstate of the Heisenberg operator  $\hat{O}(t) = \hat{U}^{\dagger}\hat{O}\hat{U}$  is  $\hat{U}^{\dagger}|o\rangle$ :  $\hat{U}^{\dagger}\hat{O}\hat{U}\hat{U}^{\dagger}|o\rangle = \hat{U}^{\dagger}\hat{O}|o\rangle = o\hat{U}^{\dagger}|o\rangle$ .

Again, we have the unexpected sign because this is the equation of motion, not of a state in the Schrödinger picture, but of an eigenstate of a time-evolving operator in the Heisenberg picture.

Previously we expressed  $|x\rangle$  as

$$|\mathbf{x}\rangle = \int d^3 \tilde{\mathbf{k}} \langle \mathbf{k} | \mathbf{x} \rangle | \mathbf{k} \rangle = \int d^3 \tilde{\mathbf{k}} e^{-i\mathbf{k}\cdot\mathbf{x}} | \mathbf{k} \rangle.$$

We can just as well expand  $|\mathbf{x}, t\rangle$  in the  $\{|\mathbf{k}, t\rangle\}$  basis, and using the fact that  $\langle \mathbf{k}, t | \mathbf{x}, t \rangle = \langle \mathbf{k} | \hat{U}(t) \hat{U}^{\dagger}(t) | \mathbf{x} \rangle = \langle \mathbf{k} | \mathbf{x} \rangle$  ( $\hat{U}(t)$  unitary) we have

(4.2) 
$$|\mathbf{x},t\rangle = \int d^{3}\tilde{\mathbf{k}}\langle\mathbf{k},t|\mathbf{x},t\rangle|\mathbf{k},t\rangle$$
$$= \int d^{3}\tilde{\mathbf{k}}\langle\mathbf{k}|\mathbf{x}\rangle|\mathbf{k},t\rangle$$
$$= \int d^{3}\tilde{\mathbf{k}}e^{-i\mathbf{k}\cdot\mathbf{x}}|\mathbf{k},t\rangle.$$

We now make our basic analogical move from the one-quantum state to the corresponding raising operator and get

$$\hat{\Psi}^{\dagger}(\mathbf{x},t) = \int d^{3}\tilde{\mathbf{k}}\mathrm{e}^{-i\mathbf{k}\cdot\mathbf{x}}\hat{a}^{\dagger}(\mathbf{k},t).$$

Because we are concerned with free quanta (the fictitious case of quanta unaffected by interactions), we can say more about the time dependence by using the facts that for free quanta the  $|\mathbf{k}\rangle$  are eigenstates of energy as well as momentum and that the energy is a function of the momentum (as well as the mass, which, however, is fixed for a fixed kind of quantum). When a system has momentum eigenvalue  $\mathbf{k}$  (or a classical momentum,  $\mathbf{k}$ ), I will write its energy eigenvalue (or its classical energy) as ' $\omega(\mathbf{k})$ '. The nonrelativistic functional relation is  $\omega(\mathbf{k}) = \mathbf{k}^2/2m$ , and the relativistic relation is  $\omega^2(\mathbf{k}) = \mathbf{k}^2 + m^2$ . In a good bit of our work we will be able to use just the expression ' $\omega(\mathbf{k})$ ', thereby covering both the nonrelativistic and the relativistic cases (and, in principle, other functional relations as well). The functional relations also carry over to the operators: If  $\hat{\mathbf{P}}$  is the momentum operator, then the energy operator is  $\hat{H} = \omega(\hat{\mathbf{P}})$ , which I will also sometimes write as  $\hat{H}(\hat{\mathbf{P}})$ . In particular we have  $\hat{H}_{nonrel} = \hat{\mathbf{P}}^2/2m$  and  $\hat{H}_{rel}^2 = \hat{\mathbf{P}}^2 + m^2$ .

We can now use this general formulation to spell out the quantum field's time dependence in a quite general way that applies to both

<sup>&</sup>lt;sup>3</sup>Throughout I take c = 1 as well as  $\hbar = 1$ .

the nonrelativistic and the relativistic cases (as well as, in principle, to others).

In (4.2) the time dependence is carried by the  $|\mathbf{k}, t\rangle$ , the momentum and energy eigenstates for free quanta. Let's spell out this time dependence with the "Schrödinger equation" (4.1):

$$-i\frac{\partial}{\partial t}|\mathbf{k},t\rangle = \hat{H}(\hat{\mathbf{P}})|\mathbf{k},t\rangle$$
$$= \omega(\mathbf{k})|\mathbf{k},t\rangle,$$

because  $|\mathbf{k}, t\rangle$  is an energy eigenstate. Solving, we get

$$|\mathbf{k},t\rangle = e^{i\omega(\mathbf{k})t}|\mathbf{k}\rangle$$
  $(|\mathbf{k}\rangle = |\mathbf{k},0\rangle).$ 

(4.2) becomes

(4.4) 
$$|\mathbf{x},t\rangle = \int d^{3}\tilde{\mathbf{k}}e^{-i(\mathbf{k}\cdot\mathbf{x}-\omega(\mathbf{k})t)}|\mathbf{k}\rangle,$$

and with our basic analogical move, (4.3) becomes

(4.5) 
$$\hat{\Psi}^{\dagger}(\mathbf{x},t) = \int d^{3}\tilde{\mathbf{k}}e^{-i(\mathbf{k}\cdot\mathbf{x} - \omega(\mathbf{k})t)}\hat{a}^{\dagger}(\mathbf{k})$$

with  $\hat{a}^{\dagger}(\mathbf{k}) = \hat{a}^{\dagger}(\mathbf{k}, 0)$ .

The function  $\hat{\Psi}^{\dagger}(\mathbf{x},t)$  constitutes, in at least the formal sense, a quantum field, because it is an assignment of an operator to each  $(\mathbf{x},t)$ . It is also the subject of a field theory because, as we show next, it satisfies a differential equation.

Continuing our general presentation applicable to both the nonrelativistic and relativistic cases, we use " $\omega(-i\nabla_{\mathbf{x}})$ " to represent the differential operator,  $\omega(-i\nabla_{\mathbf{x}}) = -\nabla_{\mathbf{x}}^2/2m$  and  $\omega(-i\nabla_{\mathbf{x}}) = \sqrt{-\nabla_{\mathbf{x}}^2 + m^2}$ , in the nonrelativistic and relativistic cases respectively. To display the same field equations familiar from other introductory presentations we write these equations not for  $\hat{\Psi}^{\dagger}(\mathbf{x},t)$  of (4.5), but for its adjoint,  $\hat{\Psi}(\mathbf{x},t)$ :

$$\left(i\frac{\partial}{\partial t} - \omega(-i\nabla_{\mathbf{x}})\right)\hat{\Psi}(\mathbf{x}, t)$$

$$= \int d^{3}\tilde{\mathbf{k}} \left(i\frac{\partial}{\partial t} - \omega(-i\nabla_{\mathbf{x}})\right) e^{i(\mathbf{k}\cdot\mathbf{x} - \omega(\mathbf{k})t)}\hat{a}(\mathbf{k})$$

$$= \int d^{3}\tilde{\mathbf{k}}(\omega(\mathbf{k}) - \omega(\mathbf{k})) e^{i(\mathbf{k}\cdot\mathbf{x} - \omega(\mathbf{k})t)}\hat{a}(\mathbf{k})$$

$$= 0.$$
(4.6)

One is tempted here to take  $-i\nabla_{\mathbf{x}}$  to be the momentum operator, so that  $\omega(-i\nabla_{\mathbf{x}})$  would be the energy operator. This is not right in the present application. When working in the position basis,  $-i\nabla_{\mathbf{x}}$  and  $\omega(-i\nabla_{\mathbf{x}})$  provide representations of the momentum and (free) energy operators. But  $\omega(-i\nabla_{\mathbf{x}})$  is here applied not to a position basis representation of a vector (in Dirac's terminology, a "representative"), but to an operator. The appearance here of things that have the form of the position representation of the momentum and energy operators is not just an accident. But the order of presentation now under way, although it clarifies the connection of the quantum field to the Fock space description of quanta, does not show how the position representation of operators gets into the picture. How this happens will appear more clearly when, later, I lay out the conventional presentation of the quantum field.

Exactly what field equation is in question depends on the functional form of  $\omega(-i\nabla_{\mathbf{x}})$ . For  $\omega(-i\nabla_{\mathbf{x}}) = -\nabla_{\mathbf{x}}^2/2m$  we have

(4.7) 
$$\left(i\frac{\partial}{\partial t} + \frac{\nabla_{\mathbf{x}}^2}{2m}\right)\hat{\Psi}(\mathbf{x},t) = 0,$$

the nonrelativistic Schrödinger equation for the free quantum field. For  $\omega(-i\nabla_{\mathbf{x}}) = \sqrt{-\nabla_{\mathbf{x}}^2 + m^2}$  we have

(4.8) 
$$\left[i\frac{\partial}{\partial t} - \sqrt{-\nabla_{\mathbf{x}}^2 + m^2}\right] \hat{\Psi}(\mathbf{x}, t) = 0,$$

a relativistic field equation, related to the Klein-Gordon one-quantum equation. Until fairly recently most workers looked askance at the differential operator  $\sqrt{-\nabla_{\mathbf{x}}^2+m^2}$ . It seemed not well defined in general, and its expression as a power series expansion uses arbitrarily high derivatives, so that the operator is nonlocal. Today physicists cheerfully apply any function of  $\frac{\partial}{\partial x}$ , say  $f(\frac{\partial}{\partial x})$ , to  $e^{i\mathbf{k}\cdot\mathbf{x}}$  to get  $f(\frac{\partial}{\partial x})e^{i\mathbf{k}\cdot\mathbf{x}}=f(i\mathbf{k})e^{i\mathbf{k}\cdot\mathbf{x}}$ . However, to avoid the square root of a differential operator the older tradition applied the squares of the operators  $i\frac{\partial}{\partial t}$  and  $\omega(-i\nabla_{\mathbf{x}})$  to get (with a change of sign)

$$\left(\frac{\partial^2}{\partial t^2} - \nabla_{\mathbf{x}}^2 + m^2\right) \hat{\Psi}(\mathbf{x}, t) = 0,$$

<sup>&</sup>lt;sup>4</sup>Such operators are now called *pseudo-differential operators*.

<sup>&</sup>lt;sup>5</sup>What happens at one point is not fully determined by what happens in an infinitesimally small neighborhood.

the Klein-Gordon operator field equation. The Klein-Gordon equation has a larger space of solutions than (4.8), a fact that initially led to difficulties but ultimately proves extremely important, as we will see.

# ALTERNATIVE MOTIVATIONS FOR THE FIELD-THEORETIC DESCRIPTION

I have presented our subject matter by beginning (in the last chapter) with the one-quantum formalism and generalizing it to a formalism that describes indefinitely many quanta, using a Fock space description with raising and lowering operators. At that point in the exposition the formalism had no apparent connection with field theory. We now see that the theory can also be given a field-theoretic representation by expression in the position basis, also borrowed from the one-quantum formalism. What is it about this last fact that merits calling the whole theory "quantum field theory"?

Historically the subject developed in roughly the reverse order from that presented here. It was discovered that, starting from a classical field description, one could impose quantum conditions, yielding a description involving raising and lowering operators, which in turn led to a Fock space description. Depending on the details of the approach, quantization of a classical field proceeded through or resulted in a description that had the form of a collection of independent quantized harmonic oscillators. The quantized oscillator description presented raising and lowering operators, describing evenly spaced quantized "excitation states," in turn reinterpretable in terms of a Fock space description.

Given this historical development, many have tended to see the subject matter as intrinsically "field theoretic." In particular, inasmuch as the subject is seen as that of a "quantized field," many tend to think of the quanta as "quantized field excitations."

The existence of a non-field-theoretic alternative, as presented in the last chapter, shows that a field-theoretic perspective provides only one way of thinking about the subject. However, it is an important perspective, historically and practically. In the next chapter, I will critically examine the ways in which this perspective is and is not legitimately described as "field-theoretic" and the ways in which the "field-theoretic" and particlelike aspects of the theory fit together. In preparation we need to spell out the more traditional formulation.

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## TRADITIONAL APPROACHES TO THE QUANTIZED FIELD

Traditionally, quantum fields have been introduced through two prima facie distinct but in fact closely interrelated approaches—field quantization and second quantization. Before discussing either it is useful to set out traditional first quantization, in the wave-mechanical formulation, because first quantization involves ideas that are reapplied in field and in second quantization. A discussion of first quantization also provides the opportunity for a useful digression to present the basic idea of first-quantized, single-particle relativistic equations, the Klein-Gordon and Dirac equations.

### First Quantization

Classically, values of physical quantities are represented by numbers, while the physical quantities themselves are represented by functions that take states in a state or "phase" space to numbers, the numbers representing the values of the quantities in the states. The intuitive idea behind first quantization is that instead of using numerically valued phase space functions (sometimes called *c-numbers* for 'classical numbers' or 'commuting numbers'), one represents physical quantities with noncommuting operators (sometimes called *q-numbers* for 'quantum numbers' or even 'queer numbers')<sup>6</sup> satisfying some specified commutation relations. Numbers corresponding to values of physical quantities reenter only indirectly in the form of eigenvalues, probabilities, or, more generally, expectation values. In sum, one first quantizes a classical theory by substituting operators satisfying commutation relations for numerically valued functions representing physical quantities (substituting q-numbers for c-numbers).

In the older wave-mechanical formulation for one quantum the new kind of states are complex-valued functions of space and time. The operators transform one such function into another, and the expectation values are given in terms of the inner product: For operator  $\hat{O}$ , the expectation value of  $\hat{O}$  in state  $\Psi(\mathbf{x}t)$  is  $\int d^3\mathbf{x}\Psi^{\dagger}(\mathbf{x}t)\hat{O}\Psi(\mathbf{x}t)$ . In a more general formulation, the "wave functions" can be seen as representations of states in the position basis. In Dirac's notation, the psi function,  $\Psi(\mathbf{x})$  is just  $\Psi(\mathbf{x}) = \langle \mathbf{x} | \Psi \rangle$ .

<sup>&</sup>lt;sup>6</sup>Dirac (1977), p. 129.

These wave-mechanical ideas go back to de Broglie: With a free particle with exact momentum and energy we associate a "wave," the wave number of which corresponds to the particle's momentum,  $\mathbf{p}=\mathbf{k}$ , and the frequency of which corresponds to the particle's energy,  $E=\omega$ . Using the most general expression of a sinusoidal wave with exact wave number and frequency, we write the particle's wave as  $e^{i(\mathbf{k}\cdot\mathbf{x}-\omega(\mathbf{k})t)}$ . Using  $e^{i(\mathbf{k}\cdot\mathbf{x}-\omega(\mathbf{k})t)}$  as the free quantum eigenstate for momentum and energy indicates that classical momentum should be replaced by the momentum operator

$$\mathbf{p_{cl}} \to \hat{\mathbf{P}} = -i\nabla_{\mathbf{x}}$$

and that classical energy can go over to an "energy operator,"

$$(4.10) E_{\rm cl} \to \hat{E} = i \frac{\partial}{\partial t}.$$

Equation (4.10) is not the (free quantum) energy operator,  $\hat{H}(\hat{\mathbf{P}})$ , introduced earlier. In a more careful Hilbert space formulation we should distinguish between, on the one hand, operators that transform one vector representing a state at a time into another such vector representing a state at the same time and, on the other hand, the operator  $i\frac{\partial}{\partial t}$  characterizing the function  $\hat{U}(t)$ , which describes how a given state evolves over time. The connection between  $i\frac{\partial}{\partial t}$  and  $\hat{H}(\hat{\mathbf{P}})$  is not an operator identity but an equality between  $i\frac{\partial}{\partial t}\Psi(\mathbf{x},t)$  and  $\hat{H}(\hat{\mathbf{P}})\Psi(\mathbf{x},t)$ , reflecting an empirical fact that has to do with the energy's connection with infinitesimal time displacements, in turn connected with the conservation of energy.

Nonetheless, authors sometimes refer to  $i\frac{\partial}{\partial t}$  as an (the?) energy operator. More often, and more appropriately, authors often appeal to (4.9) and (4.10) as "heuristic substitutions," used informally to motivate various wave-mechanical ideas, and in particular the introduction of the quantum field. Often the heuristic can be given a rigorous reinterpretation by accurately describing the wave-mechanical formulation as the position representation, taking account of the connection between energy and infinitesimal time displacements, and the like. In the case of standard introductions of the quantum field, it may be harder to carry out such a reinterpretation precisely because one appeals to the heuristic in the process of making a substantive generalization of the formalism.

 $<sup>^{7}\</sup>mathbf{p} = \hbar\mathbf{k}$  and  $E = \hbar\omega$ . Remember that throughout I am supposing units in which  $\hbar = 1$ .

One more idea completes our review of relevant basic wave-mechanical ideas. If one starts with the heuristic (4.9), an analogy with Poisson brackets in classical physics suggests the commutation relation  $[\hat{\mathbf{X}}, \hat{\mathbf{P}}] = i$ . The commutator  $[\hat{\mathbf{X}}, \hat{\mathbf{P}}] = i$  is realized in the wave-mechanical formulation by taking  $\mathbf{x}_{\text{cl}} \to \hat{\mathbf{X}} = \mathbf{x}$ . In other words, in the wave-mechanical formulation (equivalently, in the position representation) the operator  $\hat{\mathbf{X}}$  is represented by  $\mathbf{x}$ , that is, by multiplying the psi function  $\Psi(\mathbf{x},t)$  by the variable  $\mathbf{x}$ , so that  $\hat{\mathbf{X}}: \Psi(\mathbf{x},t) \to \mathbf{x}\Psi(\mathbf{x},t)$ . This fact reinforces the myth, discussed in the last chapter, that in the nonrelativistic theory "position occurs as an operator and not as a parameter." Accurately speaking, in the nonrelativistic theory multiplying by the position variable represents the position operator in the position representation,  $\Psi(\mathbf{x}) = \langle \mathbf{x} | \Psi \rangle$ . In both the nonrelativistic and the relativistic theories, position occurs as a parameter of the position basis vectors  $|\mathbf{x}\rangle$ .

Traditional introductions of the quantum field proceed in analogy to the introductions of the wave-mechanical first-quantized wave equations, so it is useful to review the pattern of argument. I restrict attention to free quanta, where the energy is a function of the momentum,  $E = \omega(\mathbf{p})$ . We make the heuristic substitution (4.9) for  $\mathbf{p}$  and (4.10) for E, giving  $i\frac{\partial}{\partial t} = \omega(-i\nabla_{\mathbf{x}})$ . Again, I emphasize that this is not an identity. Instead the equality means that when  $\Psi(\mathbf{x},t)$  represents the evolving state of a quantum system in the position basis, both sides applied to  $\Psi(\mathbf{x},t)$  give the same result, so that we have the generalized "Schrödinger equation,"

(4.11)  $\left[i\frac{\partial}{\partial t} - \omega(-i\nabla_{\mathbf{x}})\right] \Psi(\mathbf{x}, t) = 0.$ 

The reader may remark that the form of the adjoint of (4.6), remembering that  $\omega(\mathbf{k}) = \omega(-\mathbf{k})$ , recapitulates (4.11). But the content is very different: (4.6) was an equation governing an operator-valued field, whereas (4.11) governs a c-valued function, the wave-mechanical psi function, more accurately seen as the position representation of the quantum state.

<sup>&</sup>lt;sup>8</sup>See n.19 in this chapter for the grain of truth in this myth.

<sup>&</sup>lt;sup>9</sup>Earlier, while presenting basics of Fock space and the quantum field, I followed standard notation and used 'k' as the momentum variable. However, the wave-mechanical tradition now under discussion usually uses 'p' as the momentum variable, keeping explicit the empirical relation between momentum and wave number:  $\mathbf{p} = \hbar \mathbf{k}$ . Although I regret not using uniform notation, all things considered it seems best to use the notation of the tradition under discussion, and so here to use 'p' as the momentum variable. Soon I will revert to using 'k' as the momentum variable. Because I am using units in which  $\hbar = 1$ ,  $\mathbf{p} = \mathbf{k}$  and the notational change is immaterial.

The parallel between operator-valued and c-valued cases can be continued. It is useful to give the solutions of (4.11) a Fourier representation:

(4.12) 
$$\Psi(\mathbf{x},t) = \int d^3 \tilde{\mathbf{k}} a(\mathbf{k},t) e^{i\mathbf{k}\cdot\mathbf{x}}.$$

Note carefully that here  $a(\mathbf{k}, t)$  is a c-valued Fourier coefficient, not a lowering or raising operator; indeed, it is not an operator at all. Putting (4.12) into (4.11) we get

$$0 = \int d^3 ilde{\mathbf{k}} \left( i rac{\partial}{\partial t} - \omega (-i 
abla_{\mathbf{x}}) 
ight) a(\mathbf{k},t) \mathrm{e}^{i \mathbf{k} \cdot \mathbf{x}},$$

and using  $\omega(-i\nabla_{\mathbf{x}})e^{i\mathbf{k}\cdot\mathbf{x}} = \omega(\mathbf{k})e^{i\mathbf{k}\cdot\mathbf{x}}$ ,

$$0 = \int d^3 \tilde{\mathbf{k}} [i \dot{a}(\mathbf{k},t) - \omega(\mathbf{k}) a(\mathbf{k},t)] \mathrm{e}^{i \mathbf{k} \cdot \mathbf{x}}.$$

Because, to get zero, each coefficient of  $e^{i\mathbf{k}\cdot\mathbf{x}}$  must be zero we have

(4.13) 
$$i\dot{a}(\mathbf{k},t) - \omega(\mathbf{k})a(\mathbf{k},t) = 0,$$

so that

(4.14) 
$$a(\mathbf{k},t) = a(\mathbf{k})e^{-i\omega(\mathbf{k})t}$$
, with  $a(\mathbf{k}) = a(\mathbf{k},0)$ .

Finally, substituting into (4.12) gives

(4.15) 
$$\Psi(\mathbf{x},t) = \int d^{3}\tilde{\mathbf{k}}a(\mathbf{k})e^{i(\mathbf{k}\cdot\mathbf{x}-\omega(\mathbf{k})t)}.$$

Parallel to the case of the quantum field, we get a specific first-quantized "wave equation" by putting in a specific functional form for  $\omega(\mathbf{k})$ . In particular, by choosing  $\omega(\mathbf{k}) = +\mathbf{k}^2/2m$ , (4.11), in the light of (4.9), becomes the nonrelativistic Schrödinger equation,

(4.16) 
$$\left(i\frac{\partial}{\partial t} + \frac{\nabla_{\mathbf{x}}^2}{2m}\right)\Psi(\mathbf{x}, t) = 0.$$

Choosing a relativistic expression for  $\omega(\mathbf{k})$ ,  $\omega(\mathbf{k}) = \sqrt{\mathbf{k}^2 + m^2}$  gives a relativistically correct equation, but one that, as I mentioned, seemed problematic because of the square root of the differential operator. The same comments apply to the negative root,  $\omega(\mathbf{k}) = -\sqrt{\mathbf{k}^2 + m^2}$ . Historically, one followed the heuristic substitutions used in developing (4.11) but applied them to the squares of the quantities in order to avoid the

square root. Starting from the relativistic expression  $E^2 = \mathbf{p}^2 + m^2$  and using the heuristic substitutions (4.9) and (4.10), we get

$$-\frac{\partial^2}{\partial t^2} = -\nabla_{\mathbf{x}}^2 + m^2.$$

As before, this is not an identity but is taken to mean that we get the same result when both operators are applied to  $\Psi(\mathbf{x},t)$ . Thus

$$\left(rac{\partial^2}{\partial t^2} \ - 
abla_{f x}^2 + m^2
ight)\Psi({f x},t) = 0,$$

and letting  $\square_{\mathbf{x}} = \frac{\partial^2}{\partial t^2} - \nabla_{\mathbf{x}}^2$  we have

$$(4.17) \qquad (\square_{\mathbf{x}} + m^2)\Psi(\mathbf{x}, t) = 0.$$

(One sees this also with all the signs reversed.) This is the Klein-Gordon first-quantized wave equation.

Using  $\omega^2(-i\nabla_{\mathbf{x}}) = -\nabla_{\mathbf{x}}^2 + m^2$ , (4.17) is also written

(4.18) 
$$\left(\frac{\partial^2}{\partial t^2} + \omega^2(-i\nabla_{\mathbf{x}})\right)\Psi(\mathbf{x},t) = 0,$$

and putting in the Fourier expansion (4.12) we get

$$0 = \int d^{3}\tilde{\mathbf{k}} \left( \frac{\partial^{2}}{\partial t^{2}} + \omega^{2} (-i\nabla_{\mathbf{x}}) \right) a(\mathbf{k}, t) e^{i\mathbf{k}\cdot\mathbf{x}}$$

$$= \int d^{3}\tilde{\mathbf{k}} [\ddot{a}(\mathbf{k}, t) + \omega^{2}(\mathbf{k}) a(\mathbf{k}, t)] e^{i\mathbf{k}\cdot\mathbf{x}}.$$
(4.19)

Again, to get 0, all coefficients of  $e^{i\mathbf{k}\cdot\mathbf{x}}$  must be 0, so that

$$0 = \ddot{a}(\mathbf{k}, t) + \omega^2(\mathbf{k})a(\mathbf{k}, t)$$

with solutions

(4.20) 
$$a(\mathbf{k},t) = a(\mathbf{k})e^{-i\omega(\mathbf{k})t}$$
, with  $a(\mathbf{k}) = a(\mathbf{k},0)$ 

(4.21) 
$$a(\mathbf{k},t) = c(\mathbf{k})e^{i\omega(\mathbf{k})t}, \text{ with } c(\mathbf{k}) = a(\mathbf{k},0).$$

The  $a(\mathbf{k}, t)$  of lines (4.20) and (4.21) are distinct solutions of (4.17). Because in a moment we will consider general superpositions of these distinct solutions, we write the time-independent Fourier coefficients with different letters, ' $a(\mathbf{k})$ ' for the first and ' $c(\mathbf{k})$ ' for the second.

Putting these into (4.12) we get

(4.22) 
$$\Psi^{+}(\mathbf{x},t) = \int d^{3}\tilde{\mathbf{k}}a(\mathbf{k})e^{i(\mathbf{k}\cdot\mathbf{x}-\omega(\mathbf{k})t)}$$

(positive-frequency, positive-energy solutions) and

(4.23) 
$$\Psi^{-}(\mathbf{x},t) = \int d^{3}\tilde{\mathbf{k}}c(\mathbf{k})e^{i(\mathbf{k}\cdot\mathbf{x}+\omega(\mathbf{k})t)}$$

(negative-frequency, negative-energy solutions). To understand the terminology 'positive'/'negative' and 'frequency'/'energy' solutions, keep in mind that  $\omega(\mathbf{k})$  is always taken to be positive:  $\omega(\mathbf{k}) = |\sqrt{\mathbf{k}^2 + m^2}|$ . The exponential  $e^{i(\mathbf{k}\cdot\mathbf{x}-\omega(\mathbf{k})t)}$  is positive frequency inasmuch as it represents a wave process progressing in the direction of the wave vector,  $\mathbf{k}$ . And  $e^{i(\mathbf{k}\cdot\mathbf{x}-\omega(\mathbf{k})t)}$  is a positive-energy solution because it is an eigenfunction of  $i\frac{\partial}{\partial t}$  with positive eigenvalue  $\omega(\mathbf{k})$ . Thus  $\Psi^+(\mathbf{x},t)$  is the general superposition of positive-frequency / positive-energy eigenfunctions. Similarly  $\Psi^-(\mathbf{x},t)$  is the general superposition of eigenfunctions corresponding to a wave process proceeding in the negative sense of the wave vector,  $\mathbf{k}$ , and having negative eigenvalues,  $-\omega(\mathbf{k})$ , of the operator  $i\frac{\partial}{\partial t}$ .

Finally, the most general solutions of the Klein-Gordon equation are the superpositions of the positive- and negative-energy solutions,

(4.24) 
$$\Psi(\mathbf{x},t) = \Psi^{+}(\mathbf{x},t) + \Psi^{-}(\mathbf{x},t)$$
$$= \int d^{3}\tilde{\mathbf{k}}[a(\mathbf{k})e^{i(\mathbf{k}\cdot\mathbf{x}-\omega(\mathbf{k})t)} + c(\mathbf{k})e^{i(\mathbf{k}\cdot\mathbf{x}+\omega(\mathbf{k})t)}].$$

Although the Klein-Gordon equation circumvented the complications of a root of a differential operator, it suffered difficulties involving the negative-energy solutions. If there really were available negativeenergy states associated with negative energies of arbitrarily large magnitudes, then, in the presence of interactions that allow transitions between energy states, one would expect a system to cascade down into lower and lower energy states. Also, there were difficulties with the inner product required for the space of solutions (4.24) to the Klein-Gordon equation. The integrand of the inner product was not susceptible to a probability density interpretation, because it is not positive definite.

One could in part deal with these difficulties by restricting attention to the subspace of positive-energy solutions. This is equivalent to adopting the space of solutions to (4.11) using  $\omega(-i\nabla_{\mathbf{x}}) = \sqrt{-\nabla_{\mathbf{x}}^2 + m^2}$ . The

space of negative-energy solutions can also, by itself, be interpreted as the space of states for a kind of quantum. We will soon see how these options have a very pleasing reinterpretation in the field and secondquantized versions of the theory.

Early in the study of relativistic wave equations it was felt that the problems with the Klein-Gordon equation stemmed, at least in part, from the fact that it was second order in time. Accordingly, Dirac set out to formulate a wave equation that was both first order in time and free of the square root of a differential operator. This could be done if one could somehow express

$$(4.25) E = \omega(\mathbf{p}) = \sqrt{\mathbf{p}^2 + m^2}$$

as a linear function of p, for then, after the heuristic substitution of (4.9) and (4.10) one would have a wave equation linear in both  $\frac{\partial}{\partial t}$  and  $\nabla_{\mathbf{x}}$ . In other words, we would like to be able to find an  $\alpha_1, \alpha_2, \alpha_3$ , and  $\beta$  such that

$$\alpha_1 p_1 + \alpha_2 p_2 + \alpha_3 p_3 + \beta m = \sqrt{\mathbf{p}^2 + m^2}.$$

Dirac noticed that one could have  $(\sum_i \alpha_i p_i + \beta m)^2 = \mathbf{p}^2 + m^2$  by taking the  $\alpha_i$  and  $\beta$  to be  $4 \times 4$  matrices satisfying the relations

$$\alpha_1^2 = \alpha_2^2 = \alpha_3^2 = \beta^2 = \hat{I}$$

$$\alpha_i \alpha_j + \alpha_j \alpha_i = 0,$$
  $i, j = 1, 2, 3; i \neq j$ 

and

$$\beta \alpha_i + \alpha_i \beta = 0, \qquad i = 1, 2, 3.$$

Following the by-now familiar procedure of using the heuristic substitutions, this time into (4.11) and (4.25), to formulate a wave equation, we get the Dirac equation

(4.26) 
$$\left(i\frac{\partial}{\partial t} + i\boldsymbol{\alpha} \cdot \nabla_{\mathbf{x}} - \beta m\right) \Psi(\mathbf{x}, t) = 0,$$

with  $\alpha$  the three-component "vector" with matrix components  $\alpha_1, \alpha_2$ , and  $\alpha_3$ . Because the  $\alpha_i$  and  $\beta$  are  $4 \times 4$  matrices,  $\Psi(\mathbf{x}, t)$  must be taken to be a four-component "vector," called a (Dirac, or four-component) spinor, <sup>10</sup> with components  $\Psi_i(\mathbf{x}, t)$ , i = 1, 2, 3, 4.

<sup>&</sup>lt;sup>10</sup>Distinct from the two-component, or Pauli, spinors familiar from nonrelativistic quantum mechanics.

The inner product required for the space of solutions to the Dirac equation did provide a positive definite probability density. However, the Dirac equation also suffered the problem of negative-energy solutions. Appealing to the fact that the four-component Dirac spinors turned out to describe spin 1/2 quanta, satisfying the exclusion principle, Dirac proposed the following outlandish resolution to the problem of the negativeenergy solutions. Because, in light of the exclusion principle, each energy state can be occupied by at most one quantum, let's suppose that all the states with negative energy are in fact filled. Then a quantum in a positive-energy state cannot cascade down through the negative-energy states. Moreover, one of the negative-energy quanta can be boosted into a positive-energy state, leaving behind a hole in the negative-energy sea. This hole will behave like a positive-energy quantum, with a charge the opposite of that of the quanta comprising the sea. Many must have felt truly astonished when two years after Dirac advanced these prima facie crazy ideas Anderson observed electron-positron pair creation.

It's a romantic story. But we will see that in the context of the field and second-quantized versions of the theory one can more soberly reinterpret the negative-energy solutions of a kind of quanta as positive-energy solutions of the corresponding antiquanta. This alternative works without the implausible metaphysics of the sea of negative-energy quanta, and moreover it works not only for Fermions but also for Bosons, which do not satisfy the exclusion principle. These ideas will be easy to explain after we introduce the basic ideas of both field quantization and second quantization.

## Field Quantization

First quantization proceeds by replacing c-valued quantities with operators constrained by commutation relations. Field quantization comprises a special application of this strategy.

In outline, we start with a classical physical field. We analyze the field into Fourier components and find that the amplitudes for the components fit the formal description of independent classical harmonic oscillators, as a consequence of the classical field satisfying the classical wave equation or a classical Klein-Gordon equation. Finally we first quantize these formal harmonic oscillators in the way familiar from quantum mechanics by letting each oscillator's q and p variables be operators  $\hat{\mathbf{Q}}$  and  $\hat{\mathbf{P}}$ , satisfying  $[\hat{\mathbf{Q}}, \hat{\mathbf{P}}] = i$ .

The electromagnetic field, the example with which these ideas originated, is the only classically recognized physical field to which this procedure applies.  $^{11}$  The free classical electromagnetic field, expressed in terms of a vector potential, satisfies the Klein-Gordon equation with m=0. Therefore, following standard treatments, I can present the ideas in a little more detail by applying them to a Klein-Gordon field with the value of m unspecified. Often what passes as field quantization also assumes that the classical field is real valued. This assumption makes it more difficult to see the role of the negative-energy solutions and does not in any way affect the ideas involved in the quantization procedure. By not assuming that the field is real valued I will be able in most respects to cover the cases of field and second quantization together. I also suppress a number of complications connected with the special case of the electromagnetic field, having to do with gauge invariance and photon spin.  $^{12}$ 

I emphasize that in this presentation of field quantization the field to which we apply the first-quantization procedure is thought of as a classical field. As I mentioned, traditional presentations of field quantization accordingly take the field to be real valued, and because it is the electromagnetic field that is in question, these presentations assume m=0. Thus I am distorting tradition when, to treat the case more generally, I relax both these assumptions, and I am thereby also in danger of obscuring the traditional distinction between field and second quantization. I nonetheless proceed in this way because so-doing saves having to do most of the same steps twice over, and so-doing facilitates treatment of the negative-frequency / negative-energy states, making it clearer than traditional presentations how this works. By emphasizing that field quantization just means first quantization of a classical field I intend to defuse the possibly misleading repercussions of allowing the field to be complex valued and to have  $m \neq 0$ .

We consider a Klein-Gordon field,  $\Psi(\mathbf{x},t)$ , satisfying (4.17). We write  $\Psi(\mathbf{x},t)$  in terms of its Fourier components,  $\Psi(\mathbf{x},t) = \int d^3 \tilde{\mathbf{k}} a(\mathbf{k},t) \mathrm{e}^{i\mathbf{k}\cdot\mathbf{x}}$ . Note that  $\mathbf{k}$  is simply a Fourier expansion coefficient. At this point we

<sup>&</sup>lt;sup>11</sup>More exactly, the only *basic* physical field. Classical sound waves in solids are similarly quantized, yielding quanta called *phonons*. One also quantizes the Navier-Stokes equation for the fluid mass density in the quantum theory of fluids.

<sup>&</sup>lt;sup>12</sup>These complications are especially nettlesome because the electromagnetic field is a gauge field with a gauge freedom, and one must introduce constraints of some kind to eliminate the nonphysical "degree of freedom" to which the gauge freedom gives rise. Similar problems arise in all the currently discussed physically applicable field theories, such as the electro-weak theory and QCD.

have no reason to connect 'k' with momentum. From satisfaction of the Klein-Gordon equation, (4.17), it follows that (see [4.19])  $\int d^3 \tilde{\mathbf{k}} [\ddot{a}(\mathbf{k},t) + \omega^2(\mathbf{k})a(\mathbf{k},t)]e^{i\mathbf{k}\cdot\mathbf{x}} = 0$ . For the whole to be zero the coefficient of each  $e^{i\mathbf{k}\cdot\mathbf{x}}$  must be zero so that we have

(4.27) 
$$\ddot{a}(\mathbf{k},t) + \omega^2(\mathbf{k})a(\mathbf{k},t) = 0.$$

This would be a formal description of a harmonic oscillator except that the  $a(\mathbf{k}, t)$  are complex valued. To get a formal harmonic oscillator description, we take the real part, setting

(4.28) 
$$q(\mathbf{k},t) = \frac{1}{\sqrt{2\omega(\mathbf{k})}} [a(\mathbf{k},t) + a^{\dagger}(\mathbf{k},t)],$$

which also satisfies

$$\ddot{q}(\mathbf{k},t) + \omega^2(\mathbf{k})q(\mathbf{k},t) = 0.$$

The Hamiltonian for such an oscillator is

(4.29) 
$$\frac{1}{2}[p^{2}(\mathbf{k},t) + \omega^{2}(\mathbf{k})q^{2}(\mathbf{k},t)].$$

With this Hamiltonian, the momentum  $p(\mathbf{k},t)$  conjugate to  $q(\mathbf{k},t)$  is  $p(\mathbf{k},t) = \dot{q}(\mathbf{k},t)$ . Because the time dependence of  $a(\mathbf{k},t)$  is, according to (4.27),  $e^{\pm i\omega(\mathbf{k})t}$ ,  $p(\mathbf{k},t) = \dot{q}(\mathbf{k},t)$  gives us two choices for  $p(\mathbf{k},t)$ .

Let's first consider the  $e^{-i\omega(\mathbf{k})t}$  time dependence of (4.27), which corresponds to what in (4.20) and (4.22) was labeled positive-frequency, positive-energy solutions. Then

$$(4.30) p(\mathbf{k},t) = -i\sqrt{\frac{\omega(\mathbf{k})}{2}}[a(\mathbf{k},t) - a^{\dagger}(\mathbf{k},t)].$$

We now have a system with a  $q(\mathbf{k},t)$  and  $p(\mathbf{k},t)$ , formally satisfying the description of the position and conjugate momentum variables of a classical harmonic oscillator. We first quantize by letting these  $q(\mathbf{k},t)$  and  $p(\mathbf{k},t)$  be operators,  $\hat{Q}(\mathbf{k},t)$  and  $\hat{P}(\mathbf{k},t)$ , satisfying  $[\hat{Q}(\mathbf{k},t),\hat{P}(\mathbf{k}',t)]=i\delta^3(\mathbf{k}-\mathbf{k}')$ , the other commutators being zero. Accordingly we must reinterpret  $a(\mathbf{k},t)$  and  $a^{\dagger}(\mathbf{k},t)$  as operators,  $\hat{a}(\mathbf{k},t)$  and  $\hat{a}^{\dagger}(\mathbf{k},t)$ , and substitution of (4.28) and (4.30) into  $[\hat{Q}(\mathbf{k},t),\hat{P}(\mathbf{k}',t)]=i\delta^3(\mathbf{k}-\mathbf{k}')$  immediately gives

(4.31) 
$$[\hat{a}(\mathbf{k},t), \hat{a}^{\dagger}(\mathbf{k}',t)] = \delta^{3}(\mathbf{k} - \mathbf{k}'),$$

and the other commutators are zero. 13

Let us also reexpress the Hamiltonian, (4.29), in terms of  $\hat{a}(\mathbf{k},t)$  and  $\hat{a}^{\dagger}(\mathbf{k},t)$ , taken as operators. This quickly gives the operator Hamiltonian

$$\hat{H}_{\mathbf{k}} = rac{1}{2}\omega(\mathbf{k})[\hat{a}^{\dagger}(\mathbf{k},t)\hat{a}(\mathbf{k},t) + \hat{a}(\mathbf{k},t)\hat{a}^{\dagger}(\mathbf{k},t)]$$

and with the help of (4.31)

(4.32) 
$$\hat{H}_{\mathbf{k}} = \omega(\mathbf{k})[\hat{a}^{\dagger}(\mathbf{k}, t)\hat{a}(\mathbf{k}, t) + \frac{1}{2}\delta^{3}(0)].^{14}$$

The  $\hat{a}^{\dagger}(\mathbf{k},t)$  and  $\hat{a}(\mathbf{k},t)$  are exactly the raising and lowering operators for a quantized harmonic oscillator with the  $\hat{H}_{\mathbf{k}}$  of (4.32) as its Hamiltonian.

We have started from a classical field,  $\Psi(\mathbf{x}, t)$ , analyzed it in terms of the amplitudes for its Fourier components, and first quantized these as harmonic oscillators. In this procedure the Fourier amplitude c-numbers are reinterpreted as operators, so that  $\hat{a}(\mathbf{k}, t)$  becomes the lowering oper-

$$\hat{H}_{\mathbf{k}} = \omega(\mathbf{k}) \left[ \hat{a}^{\dagger}(\mathbf{k},t) \hat{a}(\mathbf{k},t) + \frac{1}{2} \right],$$

which results when one uses box normalization. Box normalization gives  $\delta_{ij}$  where  $\delta^3(\mathbf{k} - \mathbf{k}')$  occurs in (4.31), and so  $\delta_{ii} = 1$  where  $\delta^3(0)$  occurs in (4.32). Even with  $\frac{1}{2}$  instead of  $\frac{1}{2}\delta^3(0)$  in (4.32) this so-called "zero-point energy" leads to an infinite expression: With box normalization we sum instead of integrate to get, for the total Hamiltonian.

$$\hat{H}_{\mathrm{tot}} = \sum_{\mathbf{k}} \omega(\mathbf{k}) \left[ \hat{a}^{\dagger}(\mathbf{k}, t) \hat{a}(\mathbf{k}, t) + \frac{1}{2} \right],$$

which includes the infinite constant  $\frac{1}{2} \sum_{\mathbf{k}} \omega(\mathbf{k})$ . With  $\delta$ -function normalization the expression for the zero-point energy is  $\frac{1}{2} \delta^3(0) \int d^3 \tilde{\mathbf{k}} \omega(\mathbf{k})$ .

All standard presentations treat the zero-point energy with the remark that only energy differences are significant, so that a constant can always be discarded. Because the constant is infinite, this seems to call for further comment. I will be able to address this issue more effectively later.

The only  $\delta$ -function normalization statement of the zero-point energy I know of in the literature is Schweber (1961), p. 184, where in his formula (192) the  $\delta^3(0)$  is redescribed as a quantization volume, V.

<sup>13</sup>With  $\langle \mathbf{k} | \mathbf{k}' \rangle = \omega(\mathbf{k}) \delta^3(\mathbf{k} - \mathbf{k}')$  normalization, we have  $[\hat{a}(\mathbf{k}, t), \hat{a}^{\dagger}(\mathbf{k}', t)] = \omega(\mathbf{k}) \delta^3(\mathbf{k} - \mathbf{k}')$ .

<sup>14</sup>With  $\langle \mathbf{k} | \mathbf{k}' \rangle = \omega(\mathbf{k}) \delta^3(\mathbf{k} - \mathbf{k}')$  normalization, this reads  $\hat{H}_{\mathbf{k}} = \hat{a}^{\dagger}(\mathbf{k}, t) \hat{a}(\mathbf{k}, t) + \frac{1}{2} \delta^3(0)$ . In effect a factor of  $\omega(\mathbf{k})^{1/2}$  gets absorbed into each of  $\hat{a}^{\dagger}(\mathbf{k}, t)$  and  $\hat{a}(\mathbf{k}, t)$  for invariant normalization. The reader may notice that, again with the text's  $\langle \mathbf{k} | \mathbf{k}' \rangle = \delta^3(\mathbf{k} - \mathbf{k}')$  normalization, instead of (4.32), one usually sees

ator for the quantized harmonic oscillator associated with the  $\mathbf{k}^{th}$  mode. Substituting these operators into the expression for the classical field, we get

(4.33) 
$$\hat{\Psi}^{+}(\mathbf{x},t) = \int d^{3}\tilde{\mathbf{k}}\hat{a}(\mathbf{k},t)e^{i\mathbf{k}\cdot\mathbf{x}},$$

which is exactly the adjoint of the operator-valued field (4.3) which we developed from the Fock space description of aggregable quanta. The superscript '+' reminds us that (4.33) is not yet a completely general solution. It corresponds only to the  $e^{-i\omega(\mathbf{k})t}$  time dependence of (4.27), corresponding to the positive-frequency solutions, (4.22), of the one-particle equation.

The  $\hat{a}^{\dagger}(\mathbf{k},t)$  and  $\hat{a}(\mathbf{k},t)$  are interpreted as the raising and lowering operators for quanta of momentum  $\mathbf{k}$ . What justifies this interpretation? One can see a loose motivation in the fact that these operators arise from the quantization of Fourier amplitudes of a field, and we can think of the Fourier components along the lines of the old de Broglie analogy. Also, once the distracting term  $\frac{1}{2}\delta^3(0)$  has been dropped (which we will discuss in the next chapter),  $\hat{H}_k = \omega(\mathbf{k})\hat{a}^{\dagger}(\mathbf{k},t)\hat{a}(\mathbf{k}t)$  has the form  $\omega(\mathbf{k})\hat{N}(\mathbf{k})$ , the energy for an individual quantum of momentum  $\mathbf{k}$  times our old friend the number operator for quanta of momentum  $\mathbf{k}$ . This again encourages interpreting  $\hat{a}^{\dagger}(\mathbf{k}), \hat{a}(\mathbf{k})$ , and  $\hat{N}(\mathbf{k})$  as, respectively, the raising, lowering, and number operators for quanta of momentum  $\mathbf{k}$ . But one can also appeal to a more detailed argument that connects the momentum operator with the generator of infinitesimal spatial displacements. Here is an outline.

Believing space to be homogeneous, it should make no difference if the entire system subject to our description were moved over uniformly in space. We express this fact by saying that we can get a description of a spatially translated system by a unitary transformation,  $\hat{U} = e^{i\hat{\mathbf{P}}\cdot\mathbf{a}}$ , on the original description, where  $\mathbf{a}$  is a spatial translation vector, and  $\hat{\mathbf{P}}$  is so far an unknown selfadjoint operator. In particular, an operator,  $\hat{O}$ , and its spatial displacement transform,  $\hat{O}'$ , are related by

(4.34) 
$$\hat{O}' = \hat{U}^{-1}(\mathbf{a})\hat{O}\hat{U}(\mathbf{a}) = e^{-i\hat{\mathbf{P}}\cdot\mathbf{a}}\hat{O}e^{i\hat{\mathbf{P}}\cdot\mathbf{a}}.$$

We specialize to an infinitesimal translation,  $\mathbf{a} = d\mathbf{x}$ , and expand  $e^{i\hat{\mathbf{P}}\cdot a}$ , keeping only the first-order terms:

$$(4.35) \qquad \hat{O}' = (\hat{I} - i\hat{\mathbf{P}} \cdot d\mathbf{x})\hat{O}(\hat{I} + i\hat{\mathbf{P}} \cdot d\mathbf{x}) = \hat{O} + i[\hat{O}, \hat{\mathbf{P}}]d\mathbf{x},$$

having dropped the  $(d\mathbf{x})^2$  term.

For 
$$\hat{O} = \hat{\Psi}^+(\mathbf{x})$$
 and  $\hat{O}' = \hat{\Psi}^+(\mathbf{x} + d\mathbf{x})$ , (4.35) becomes

$$\nabla_{\mathbf{x}} \hat{\Psi}^+(\mathbf{x}) \cdot d\mathbf{x} = i[\hat{\Psi}^+(\mathbf{x}), \hat{\mathbf{P}}] \cdot d\mathbf{x}$$

or

(4.36) 
$$\nabla_{\mathbf{x}} \hat{\Psi}^{+}(\mathbf{x}) = i[\hat{\Psi}^{+}(\mathbf{x}), \hat{\mathbf{P}}].$$

We also calculate  $\nabla_{\mathbf{x}} \hat{\Psi}(\mathbf{x})$  by differentiating (4.33). Equating the result with (4.36) gives

(4.37) 
$$\int d^{3}\tilde{\mathbf{k}}\mathbf{k}\hat{a}(\mathbf{k},t)e^{i\mathbf{k}\cdot\mathbf{x}} = [\hat{\Psi}^{+}(\mathbf{x}),\hat{\mathbf{P}}],$$

which constrains  $\hat{\mathbf{P}}$ . Using the commutation relations (4.31), a little calculation shows that (4.37) is satisfied by

(4.38) 
$$\hat{\mathbf{P}} = \int d^3 \tilde{\mathbf{k}} \mathbf{k} \hat{a}^{\dagger}(\mathbf{k}, t) \hat{a}(\mathbf{k}, t).$$

In the field-quantization argument  ${\bf k}$  was introduced as a Fourier expansion parameter for a classical field, with no argued connection with momentum. The operator  $\hat{\bf P}$  was introduced in (4.34) as the infinitesimal generator of spatial displacements. What does all of this have to do with momentum? There are arguments showing that symmetries in a theory give rise to conservation laws, and more specifically the symmetry of displacement in space corresponds to conservation of momentum. This is connected with the fact that momentum is the spatial part of a relativistic 4-vector that transforms like  $({\bf x},t)$ . These arguments are taken to show that the generator of infinitesimal spatial displacements is the operator for total momentum. The use of (4.38) and the commutation relations gives  $\hat{\bf P}\hat{a}^{\dagger}({\bf k},t)|0\rangle = {\bf k}\hat{a}^{\dagger}({\bf k},t)|0\rangle$ , so that  $\hat{a}^{\dagger}({\bf k},t)|0\rangle = |{\bf k},t\rangle$  is the  ${\bf k}$  eigenstate of  $\hat{\bf P}$ . Therefore if  $\hat{\bf P}$  can be interpreted as the operator for total momentum,  $\hat{a}^{\dagger}({\bf k},t)$  works as a momentum-raising operator.

The argument I have presented is incomplete in this respect: I have remarked only that (4.38) satisfies (4.37). Does (4.38) satisfy (4.37) uniquely? Using the fact that  $\hat{\Psi}^+(\mathbf{x})$  and  $\frac{\partial}{\partial t}\hat{\Psi}^+(\mathbf{x})$  are a complete set of operators, one can extend the argument given to show that (4.37)

<sup>15</sup> For one particularly thorough statement of an argument for momentum in the first-quantized nonrelativistic theories, see Fonda and Ghirardi (1970), pp. 65–70. Their treatment of relativistic quantized field theories is on pp. 370–77. The argument for relativistic quantized field theories is sketched in many texts, but often authors simply assert that the infinitesimal generator of spatial translations is the momentum operator. I have not been able to find a treatment that I find fully satisfying.

fixes  $\hat{\mathbf{P}}$  as (4.38) up to an additive constant, that is, an additive constant multiple of the identity,  $\mathbf{c}\hat{I}$ . Clearly the argument cannot do better. For constant  $\mathbf{c}$ , and any operator,  $\hat{O}$ ,  $[\hat{O}, \hat{\mathbf{P}}] = [\hat{O}, \hat{\mathbf{P}} + \mathbf{c}]\hat{\mathbf{I}}$  and  $e^{-i\hat{\mathbf{P}}\cdot\mathbf{a}}\hat{O}e^{i\hat{\mathbf{P}}\cdot\mathbf{a}} = e^{-i(\hat{\mathbf{P}}+\mathbf{c}\hat{I})\cdot\mathbf{a}}\hat{O}e^{i(\hat{\mathbf{P}}+\mathbf{c}\hat{I})\cdot\mathbf{a}}$ . Most authors simply comment that because this additive constant is arbitrary, it can be conventionally set to zero. In fact, when one takes into consideration the connections between momentum and other quantities such as energy and angular momentum, statement of the larger theory becomes much more complicated for any choice other than  $\mathbf{c} = 0$ .

What about energies? Equation (4.33) resulted from the quantization of positive-energy solutions of the Klein-Gordon equation, with  $e^{-i\omega(\mathbf{k})t}$  time dependence of the  $\hat{a}(\mathbf{k},t)$ . This certainly motivates interpreting  $\omega(\mathbf{k})$  as the energy of the  $\hat{a}^{\dagger}(\mathbf{k},t)|0\rangle$  quanta. We can also follow exactly the same pattern of argument just outlined for momentum. If we introduce  $\hat{H}$  as an infinitesimal generator of time displacements, steps exactly analogous to (4.35)–(4.37) yield

$$(4.39) \qquad \qquad \hat{H} = \int d^3 \tilde{\mathbf{k}} \omega(\mathbf{k}) \hat{a}^\dagger(\mathbf{k},t) \hat{a}(\mathbf{k},t) + \mathbf{c} \hat{I},$$

where I have included the arbitrary constant c.16

The arguments connecting time translation invariance with conservation of energy then identify the infinitesimal generator of time translations,  $\hat{H}$ , with the energy operator.

In the case of the energy operator,  $\hat{H}$ , the arbitrary additive constant is welcome insofar as one regards the zero of the energy as arbitrary. Note well that the arbitrary additive constant, c, is here finite and so does not involve the embarrassment of the infinite-zero-point energy,  $\frac{1}{2}\delta^3(0)$ , in (4.32). I think the right thing to say about the infinite constant involved in (4.32) is that because the whole field quantization argument for introducing  $\hat{\Psi}(\mathbf{x},t)$  is anyway heuristic, something must have gone wrong along the way to (4.32). Indeed, in chapter 6 I will entertain a specific hypothesis about what constitutes the mistake. At this stage in the discussion it seems sensible to say that the field quantization argument fails to give a sensible expression for the energy operator. Fortunately, the consideration of time translation invariance leads to the sensible (4.39).

<sup>&</sup>lt;sup>16</sup>Readers who carry out the argument will find that to get signs to come out right, they will have to introduce the time translation unitary transformation as  $\hat{T} = e^{-i\hat{H}t}$ , where the space translation operator had used +i, a difference required for a manifestly covariant formulation.

Let's return to the field-quantized solutions of the Klein-Gordon equation.

One detail still needs cleaning up. What happens when we use the  $e^{i\omega(\mathbf{k})t}$  time dependence of  $a(\mathbf{k},t)$  corresponding to the negative-frequency/negative-energy solutions of the Klein-Gordon equation?<sup>17</sup> Because these solutions are independent of the positive-frequency/positive-energy solutions, let us again write them with distinct Fourier coefficients,  $c(\mathbf{k},t)$ :

(4.40) 
$$\Psi^{-}(\mathbf{x}t) = \int d^{3}\tilde{\mathbf{k}}c(\mathbf{k},t)e^{i\mathbf{k}\cdot\mathbf{x}}.$$

The superscript '-' here notes that these are the negative-frequency solutions.

Everything proceeds exactly as before except that the analogue of (4.30) has the opposite sign:

$$(4.41) p(\mathbf{k},t) = i\sqrt{\frac{\omega(\mathbf{k})}{2}}[c(\mathbf{k},t) - c^{\dagger}(\mathbf{k},t)],$$

so that the sign of (4.31) also changes, giving

(4.42) 
$$[\hat{c}^{\dagger}(\mathbf{k},t),\hat{c}(\mathbf{k}',t)] = \delta^{3}(\mathbf{k} - \mathbf{k}').$$

Once again we have the formal description of a quantized harmonic oscillator if we read  $\hat{c}^{\dagger}(\mathbf{k}, t)$  as the lowering operator for the oscillator and

Because Dirac's coupled first-order equations equivalently do the work of half the number of second-order equations, the source of the negative-energy solutions is really the same in the Dirac and Klein-Gordon equations.

<sup>&</sup>lt;sup>17</sup>Some have objected that the following treatment of the negative-energy solutions is limited in scope. As presented here, the negative-energy solutions result from the fact that the Klein-Gordon equation is second-order in time. The Dirac equation is first-order in time, so it might seem that the present treatment does not apply.

This is not really right. To show what has gone wrong, I'll start with an analogy. Because we can factor the quadratic equation  $x^2-a^2=0$  as (x+a)(x-a)=0, satisfaction of  $x^2-a^2=0$  is equivalent to satisfaction of either of the two first-order equations, x+a=0 and x-a=0. Operator equations are less straightforward. For the second order  $(\frac{\partial}{\partial t}+a)(\frac{\partial}{\partial t}-a)\phi=0$ , define  $\phi_1=\phi$  and  $\phi_2=(\frac{\partial}{\partial t}-a)\phi_1$ . Then satisfaction of  $(\frac{\partial}{\partial t}+a)(\frac{\partial}{\partial t}-a)\phi=0$  is equivalent to satisfaction of either of the pair of first-order equations  $(\frac{\partial}{\partial t}-a)\phi_1=0$  and  $(\frac{\partial}{\partial t}+a)\phi_2=0$ . The Dirac equation presents further complications. The first-order equations are coupled because what corresponds to the factorization in the above model is inhomogeneous. But for our purposes, the point is the same: Exactly the same work done by an equation second-order in time is equivalently done by two equations first-order in time. Sakurai (1967), pp. 79-80, presents this approach to the Dirac equation, originally due to van der Waerden.

 $\hat{c}(\mathbf{k},t)$  as the raising operator. To make this clear, let us reparameterize, setting

(4.43) 
$$\hat{d}(\mathbf{k},t) = \hat{c}^{\dagger}(-\mathbf{k},t),$$

and

$$\hat{d}^{\dagger}(\mathbf{k},t) = \hat{c}(-\mathbf{k},t).$$

After quantization equation (4.40) then becomes

(4.44) 
$$\hat{\Psi}^{-}(\mathbf{x},t) = \int d^{3}\tilde{\mathbf{k}}\hat{d}^{\dagger}(-\mathbf{k},t)e^{i\mathbf{k}\cdot\mathbf{x}}.$$

To put this into the standard notation, we make a change of integration variable,  $k \rightarrow -k$ , giving

(4.45) 
$$\hat{\Psi}^{-}(\mathbf{x},t) = \int d^{3}\tilde{\mathbf{k}}\hat{d}^{\dagger}(\mathbf{k},t)e^{-i\mathbf{k}\cdot\mathbf{x}}.$$

Again, the superscript '-' reminds us that (4.44) gives only that part of the quantized field incorporating the  $e^{i\omega(\mathbf{k})t}$  time dependence of (4.27), corresponding to the negative-frequency solutions, (4.23), of the one-particle equation. However, we now refrain from calling these "negative-energy solutions." The reinterpretation, (4.43), of the raising and low-ering operators for the negative-frequency solutions make it clear that what had at first appeared formally as negative-energy solutions are, when properly understood, descriptions of positive-energy antiquanta.

One might wonder, why do we need to change the sign of k in (4.43)? Here is the conventional gloss: We have reversed the roles of raising and lowering operators. But adding a quantum of momentum k is, when it comes to keeping the momentum books, tantamount to removing a quantum with momentum -k. Hence the change in sign of k. One can, and in conventional informal glosses does, say the same thing about the energy: Adding a quantum of positive energy,  $\omega(k)$ , is tantamount to removing a "quantum" of negative energy,  $-\omega(k)$ . (Throughout I am using the expression ' $\omega(k)$ ' to represent the positive energy corresponding to k.) This corresponds exactly to what has happened in the formalism in the reinterpretation of the negative-energy first-quantized solutions as positive-energy field-quantized solutions describing antiquanta.

We can improve on this informal gloss, which in any case seems a holdover from the not very satisfactory thinking in terms of holes in Dirac's negative-energy sea. We simply reapply the argument for the momentum operator. The analogue of (4.37) is now

(4.46) 
$$\int d^{3}\tilde{\mathbf{k}}\mathbf{k}d^{\dagger}(\mathbf{k},t)e^{-i\mathbf{k}\cdot\mathbf{x}} = -[\hat{\Psi}^{-}(\mathbf{x}),\hat{P}],$$

and (4.38) is replaced by

(4.47) 
$$\hat{\mathbf{P}} = \int d^3 \tilde{\mathbf{k}} \mathbf{k} \hat{d}^{\dagger}(\mathbf{k}, t) \hat{d}(\mathbf{k}, t).^{18}$$

The change in sign of k made in the reparameterization (4.43) leads, via the change in integration variable, to the change in sign in the exponential in (4.46). Differentiation of  $\hat{\Psi}^-$  then leads to a  $-\mathbf{k}$  where before we had a k in calculating the left-hand side of (4.46). But this change is compensated by the fact that  $\hat{d}^{\dagger}(\mathbf{k},t)$  appears in (4.46) where  $\hat{a}(\mathbf{k},t)$  had appeared in (4.37). The crucial step comes in the difference in the commutation relations of a lowering versus a raising operator with the number operator:

$$[\hat{a}(\mathbf{k},t),\hat{a}^{\dagger}(\mathbf{k}',t)\hat{a}(\mathbf{k}',t)] = \delta^{3}(\mathbf{k}-\mathbf{k})\hat{a}(\mathbf{k},t)$$
$$[\hat{a}^{\dagger}(\mathbf{k},t),\hat{a}^{\dagger}(\mathbf{k}',t)\hat{a}(\mathbf{k}',t)] = -\delta^{3}(\mathbf{k}-\mathbf{k})\hat{a}^{\dagger}(\mathbf{k},t),$$

easily calculated from the commutation relations (4.31). If we had not switched  $-\mathbf{k}$  for  $\mathbf{k}$  in (4.43), the argument for the momentum operator would have given the infinitesimal generator of positive spatial translations as the negation of the total momentum operator.

Note that the reinterpretation, (4.43), is in step with Dirac's metaphor of "holes" in the negative-energy sea. The lowering (raising) of the number of "negative-energy quanta" by one corresponds to the raising (lowering) of the number of holes by one, where the holes themselves act like positive-energy quanta with momentum reversed from their positive counterparts. In a more complete discussion the holes also act like quanta with opposite spin and charge. Thus the "holes" are the antiquanta of the quanta in the original description. But in the fieldquantized version of the theory none of this metaphor need be taken literally. Those solutions which in the first-quantized theory had the form of negative-energy solutions correspond, after field quantization, to the lowering and raising operators for things again with the formal description of further harmonic oscillators, the excitation states of which we understand as quanta, all with positive-energy states. In addition, the story of the negative-energy sea worked, even as analogy, only for Fermions, because the analogy required the negative-energy states to be filled. The present reinterpretation of the negative-energy solutions applies equally to Bosons and Fermions.

<sup>&</sup>lt;sup>18</sup>Equations (4.38) and (4.47) give the momentum operators for quanta and the corresponding antiquanta respectively. The total momentum operator for the full quantum field, (4.48), is simply the sum of these operators.

What, finally, do traditional presentations count as the full quantum field? The traditional stance takes the position that it must include what before field quantization occurred as the positive-energy solutions—with Fourier coefficients  $a(\mathbf{k}t)$ —and the negative-energy solutions—with Fourier coefficients  $c(\mathbf{k},t)$ —so that we write for the full quantized field the sum of (4.33) and (4.45):

(4.48) 
$$\hat{\Psi}(\mathbf{x},t) = \int d^3 \tilde{\mathbf{k}} [\hat{a}(\mathbf{k},t) e^{i\mathbf{k}\cdot\mathbf{x}} + \hat{d}^{\dagger}(\mathbf{k},t) e^{-i\mathbf{k}\cdot\mathbf{x}}],$$

or with the time dependence spelled out,

$$(4.49) \quad \hat{\Psi}(\mathbf{x},t) = \int d^3 \tilde{\mathbf{k}} [\hat{a}(\mathbf{k}) e^{i(\mathbf{k} \cdot \mathbf{x} - \omega(\mathbf{k})t)} + \hat{d}^{\dagger}(\mathbf{k}) e^{-i(\mathbf{k} \cdot \mathbf{x} - \omega(\mathbf{k})t)}],$$

with  $\hat{a}(\mathbf{k})$  the lowering operators for one kind of quanta and  $\hat{d}^{\dagger}(\mathbf{k})$  the raising operators for the corresponding antiquanta.

For completeness, I must mention an alternative way in which field quantization is often presented. We again start with the general solution to the Klein-Gordon equation,  $\Psi(\mathbf{x},t) = \int d^3 \tilde{\mathbf{k}} a(\mathbf{k},t) e^{i\mathbf{k}\cdot\mathbf{x}}$ . For the moment, we assume that this general solution is real valued. Let's think of the value of  $\Psi(\mathbf{x},t)$  varying over time, for fixed  $\mathbf{x}$ , as a kind of generalized coordinate ("position" in an abstract, nonphysical space). The Lagrangian formalism enables one to choose a conjugate ("momentum") variable, which in the case of the Klein-Gordon field is  $\Pi(\mathbf{x},t)$  $\dot{\Psi}(\mathbf{x},t)$ . We now have, for each spatial point,  $\mathbf{x}$ , a generalized coordinate variable,  $\Psi(\mathbf{x},t)$ , and a conjugate variable,  $\Pi(\mathbf{x},t)$  (a "position" and a "momentum" variable). Let's first quantize these systems by taking  $\Psi(\mathbf{x},t)$  and  $\Pi(\mathbf{x},t)$  to be operators,  $\hat{\Psi}(\mathbf{x},t)$  and  $\hat{\Pi}(\mathbf{x},t)$ , satisfying  $[\hat{\Psi}(\mathbf{x},t),\hat{\Pi}(\mathbf{x}',t)]=i\delta^3(\mathbf{x}-\mathbf{x}')$ , and other commutators zero. One can then formally write  $\hat{\Psi}(\mathbf{x},t)$  in Fourier modes, taking the Fourier coefficients,  $a(\mathbf{k}, t)$ , to be operators,  $\hat{a}(\mathbf{k}, t)$ . The commutation relations  $[\hat{\Psi}(\mathbf{x},t),\hat{\Pi}(\mathbf{x}',t)] = i\delta^3(\mathbf{x}-\mathbf{x}')$  then give  $[\hat{a}(\mathbf{k},t),\hat{a}^{\dagger}(\mathbf{k}'t)] = \delta^3(\mathbf{k}-\mathbf{k}')$ (other commutators zero), and we have the same description as before. Roughly speaking, it makes no formal difference if we start by resolving into Fourier modes and then first quantize the coefficients of the modes of the classical field, or instead first quantize the field variable at each point and then resolve into Fourier modes. 19

<sup>&</sup>lt;sup>19</sup>Messiah (1961), pp. 960–69, gives an elegant demonstration that, for first quantizing a field, one works equivalently by taking  $\Psi(\mathbf{x},t)$  and  $\Pi(\mathbf{x},t)$ , or  $q(\mathbf{k},t)$  and  $p(\mathbf{k},t)$ , or  $a(\mathbf{k},t)$  and  $a^{\dagger}(\mathbf{k},t)$  as the fundamental pair of variables to be first quantized.

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In my presentation, this second approach to field quantization applies to real-valued fields. Traditionally one also applies it to complex-valued fields, thinking of them as pairs of real-valued fields, or by treating the field and its complex conjugate as if each were an independent field.

It's worth mentioning the similarities and contrasts in the ways in which the two approaches to field quantization constitute analogical generalizations of methods used in first quantization. In the approach I presented earlier, which started with analysis into Fourier modes, the method identified aspects of the description that had the formal features of harmonic oscillators and then applied the familiar method of quantizing harmonic oscillators. For each oscillator we identify an "oscillator coordinate," q, and its "conjugate momentum," p, and first quantize these by reinterpreting them as operators,  $\hat{Q}$  and  $\hat{P}$ , satisfying  $[\hat{Q}, \hat{P}] = i$ . The second approach works by identifying degrees of freedom of the system, finding a pair of conjugate variables, q and p, for each degree of freedom, and then for each of these degrees of freedom applying exactly the same strategy of replacing the variables with operators  $\hat{Q}$  and  $\hat{P}$ , satisfying  $[\hat{Q}, \hat{P}] = i$ . In conventional quantum mechanics we identify three degrees of freedom for the motion of each quantum. In the second approach to field quantization we identify the degrees of freedom as the values of the field at each point in space. But we treat the variously identified degrees of freedom similarly. In the second approach we resolve into Fourier modes only after quantizing.

Before leaving field quantization I want to emphasize that the whole line of argument is, at best, analogical only. There is no question of material oscillators in space which are subjected to first quantization. And the value of a field at a point is not really the position value of a material quantum. In both cases the analogical reasoning works, not because it directly reflects physical oscillating systems or positions of quanta, but

tized. This presentation of field quantization also provides a useful way of thinking about the relation between conventional quantum mechanics and quantum field theory. Think of a quantum theory as starting with the independent degrees of freedom of a classical system and quantizing by associating a pair of conjugate operators with each degree of freedom. Conventional quantum mechanics always has finitely many degrees of freedom, three for each quantum described. (This description has to be complicated if we include spin.) A classical field theory, however, has uncountably many degrees of freedom and when quantized results in a theory with uncountably many operators, parameterized either with a momentum variable, in the harmonic oscillator approach, or with a position variable in the approach just described. Thus in quantum field theory position serves as a parameter on quantum-field-theoretic operators, and in conventional quantum mechanics it serves only as a parameter on states. This provides the grain of truth promised n. 6 of ch. 3 and n. 8 of this chapter.

because, apparently in a thoroughgoing way, when nature exhibits classical behavior involving what are formally conjugate variables, nature's more detailed behavior exhibits the characteristics described when these conjugate variables are first quantized.

The analogical character of the reasoning becomes yet more pronounced in its second-quantized formulation.

#### Second Quantization

Second quantization involves one simple idea appended to the method of field quantization. In field quantization we start with something we were thinking of as a classical field and then proceed in either of the two ways described in the last section. In second quantization we proceed in exactly the same way except that instead of starting with the description of a classical field, we start with the state function resulting from ordinary first quantization in the one-quantum theory: That is, we proceed exactly as in field quantization except that we treat the first-quantized state function exactly as if it were itself a description of a physical field! This line of thinking works whether one starts with the first-quantized state function of the nonrelativistic Schrödinger theory, the full complex-valued state function solutions to the Klein-Gordon equation, or the four-component, complex-valued solutions to the Dirac equation.

It is worth sketching the situation for the nonrelativistic Schrödinger theory because this case differs from the others by the absence of negative-energy solutions in the first-quantized version and the corresponding absence of raising and lowering operators for antiquanta in the second-quantized version. Recall (4.11), our general format for a first-quantized theory. Analyzing into Fourier components as in (4.12) and substituting into (4.11) gives (4.13) and then (4.14), the equation of motion for the Fourier coefficients. Because this is first-order in the time derivative there are no negative-energy solutions. Differentiating (4.14) twice with respect to time and then applying (4.14) again for the value of  $a(\mathbf{k},t)$  gives (4.27), so that one can proceed exactly as in the case of field quantization applied to positive-energy solutions. Note, however, that although (4.27) also has a  $e^{i\omega(\mathbf{k})t}$  solution, this does not correspond to a solution to (4.11), which is first-order in time, at least as long as  $\omega(\mathbf{k})$  is always taken as positive, as is required in the nonrelativistic case.

The Dirac equation is also first-order in time. But the four-component

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structure of the equation, reflecting the relativistic relation  $E^2 = \mathbf{p}^2 + m^2$ , results in negative-energy solutions very much like those of the Klein-Gordon equation.<sup>20</sup> On second quantizing—that is, applying the procedure of field quantization to the first-quantized Dirac state function, or "Dirac field"—the first-quantized negative-energy solutions emerge as lowering and raising operators for positive-energy antiquanta, or positrons, much as in (4.45) in the treatment of the Klein-Gordon field.

There is one noteworthy difference. The four-component structure of the Dirac field results in a difference in sign in the negative-energy solution part of the total first-quantized state function, further resulting in a negative sign that applies to part of the corresponding Hamiltonian. As a consequence the whole second-quantization procedure works only if one uses anticommutators instead of commutators in the quantization procedure: Otherwise the resulting Hamiltonian is not positive semidefinite. <sup>21</sup> This is just right, because the Dirac equation describes spin 1/2 quanta that satisfy the exclusion principle and Dirac-Fermi statistics, which, as we have seen in chapters 2 and 3, follow from the anticommutation rules (plus an assumption of equal distribution of probability to basic states).

I want to emphasize the point that, as with field quantization, the argument underlying second quantization is analogical only. Although occasionally some (for example, Schrödinger at the dawn of modern quantum mechanics) have tried to think of the state function as the description of a physical field, one soberly regards the maneuver of second quantization as a formal device that yields the same theory as that produced by writing down a theory describing a variable number of quanta that individually, and for fixed numbers of quanta generally, behave as described in the first-quantized theory. In addition, when one presents the theory in terms of quantization of the infinitely many degrees of freedom of the field, the theory presents itself as first and foremost a theory about the field quantities described in terms of local field operators. Of course, the theory is manifestly about this subject. But the objective of chapters 2 and 3 was to show that this is not the only interpretive entrée to the theory, while chapter 4, among other things, demonstrates that these points of view are not exclusive but instead complement each other. Chapter 5 will examine the ways in which presentations in terms of field operators do and do not merit description as a "field theory."

<sup>&</sup>lt;sup>20</sup>See n. 17 in this chapter.

<sup>&</sup>lt;sup>21</sup>See Sakurai (1967), pp. 150-52, for a simple exposition of the point.

I also want to refer again to the formulation in terms of field operators because of its bearing on the subject of the next section. Insofar as the theory concerns quantities that are indexed by space-time points, one will want to require that the values of these quantities be causally independent when the points in question are space-like related. This requirement, known as the microcausality condition, figures centrally in our next topic.

#### MICROCAUSALITY AND THE SPIN-STATISTICS THEOREM

One might wonder: Instead of (4.48), why not use the more general combination of operators for quanta and their antiquanta,

(4.50) 
$$\hat{\Psi}(\mathbf{x},t) = \int d^3 \tilde{\mathbf{k}} [\xi \hat{a}(\mathbf{k},t) e^{i\mathbf{k}\cdot\mathbf{x}} + \eta \hat{d}^{\dagger}(\mathbf{k},t) e^{-i\mathbf{k}\cdot\mathbf{x}}]?$$

It's worth mentioning this point, not only for its own interest, but because it involves an important part of a proof of the spin-statistics theorem.

Quantum field theory generally imposes what is called the microcausality condition, according to which operators for physical quantities, evaluated at space-like related points, must commute. Failure of commutativity in quantum theories generally corresponds to the impossibility of simultaneously having precise values for the two operators concerned. But why should this be true, from a physical point of view? Recall some of the traditional gedanken-experiments. For example, for simultaneous position and momentum measurement of a single quantum, having in place a precise position-measuring device physically excludes a precise momentum-measuring device. Intuitively, if  $\hat{O}(\mathbf{x},t)$ , an operator for a quantity at  $(\mathbf{x},t)$ , failed to commute with  $\hat{O}'(\mathbf{x}',t')$  an operator for a quantity at space-like related (x', t'), we would expect there to have to be some physical connection between measuring devices at the two space-like related points which would enable the operation of one to interfere with the operation of the other. And relativity seems to tell us that there can be no such connections between space-like related points.

This rough intuition can be filled out along the following lines: Suppose  $[\hat{O}(\mathbf{x},t),\hat{O}'(\mathbf{x}',t')] \neq 0$  for space-like related  $(\mathbf{x},t),(\mathbf{x}',t')$ . Then no state can be a simultaneous eigenstate for both  $\hat{O}'(\mathbf{x},t)$  and  $\hat{O}'(\mathbf{x}',t')$ . These circumstances would then facilitate the possibility of superluminal signaling, which relativity is supposed to prohibit. Observers at  $\mathbf{x}$  and

 $\mathbf{x}'$  arrange to start in (what will be) an eigenstate of  $\hat{O}(\mathbf{x},t)$ . This then is not an eigenstate of  $\hat{O}'(\mathbf{x}',t')$ . At t' the observer at  $\mathbf{x}'$  makes a measurement of  $\hat{O}'(\mathbf{x}',t')$ , thereby throwing the state into a noneigenstate of  $\hat{O}(\mathbf{x},t)$ , so that there is now dispersion for  $\hat{O}(\mathbf{x},t)$ . No signaling yet. But if the two observers set up so that they can perform this experiment many times in a short time interval centered at t, then observer 2 can signal observer 1 by choosing to measure or not measure for  $\hat{O}'(\mathbf{x},t)$ , with observer 1 decoding the message simply on the basis of whether he/she detects dispersion for  $\hat{O}(\mathbf{x},t)$ .

Therefore relativity seems to require  $[\hat{O}(\mathbf{x},t),\hat{O}'(\mathbf{x}',t')]=0$  for space-like related  $(\mathbf{x},t)$  and  $(\mathbf{x}',t')$  and physical observables  $\hat{O}$  and  $\hat{O}'$ . Now,  $\hat{\Psi}(\mathbf{x},t)$  of (4.48) is not Hermitian and so not generally taken to be an observable. But physical observables are quadratic functions of  $\hat{\Psi}(\mathbf{x},t)$ . Therefore a sufficient condition for the required commutativity is that for space-like related  $(\mathbf{x},t)$  and  $(\mathbf{x}',t')$  the commutators (or anticommutators) for the field operators be zero. If  $(\mathbf{x},t)$  and  $(\mathbf{x}',t')$  are space-like related, there is a frame in which t=t', which we can take to be zero. So we can finally give the form in which the microcausality condition is generally assumed as

$$(4.51) \qquad [\hat{\Psi}(\mathbf{x},0),\hat{\Psi}(\mathbf{x}',0)]_{\pm} = [\hat{\Psi}^{\dagger}(\mathbf{x},0),\hat{\Psi}^{\dagger}(\mathbf{x}',0)]_{\pm}$$
$$= [\hat{\Psi}(\mathbf{x},0),\hat{\Psi}^{\dagger}(\mathbf{x}',0)]_{\pm} = 0.$$

We are now ready to return to the proposed factors  $\xi$  and  $\eta$  in (4.50). Let us assume that the raising and lowering operators for both the quanta and their antiquanta satisfy *either* commutation or anticommutation relations:

$$[\hat{a}(\mathbf{k},0),\hat{a}^{\dagger}(\mathbf{k}',0)]_{\pm} = [\hat{d}(\mathbf{k},0),\hat{d}^{\dagger}(\mathbf{k}',0)]_{\pm}$$

$$(4.52) = \delta^{3}(\mathbf{k}-\mathbf{k}'), \text{ other (anti)commutators zero.}$$

Equation (4.52) includes (anti)commutators that combine operators for quanta with those for antiquanta, and it is to be read with + or - consistently throughout. These (anti)commutators apply trivially to show that

$$[\hat{\Psi}(\mathbf{x},0),\hat{\Psi}(\mathbf{x}',0)]_{\pm} = [\hat{\Psi}^{\dagger}(\mathbf{x},0)\hat{\Psi}^{\dagger}(\mathbf{x}',0]_{\pm} = 0,$$

with the sign reading as chosen in (4.52). For the last case:

$$[\hat{\Psi}(\mathbf{x},0),\hat{\Psi}^{\dagger}(\mathbf{x}',0)]_{\pm} = \int \int d^{3}\tilde{\mathbf{k}}d^{3}\tilde{\mathbf{k}'} \left[ |\xi|^{2} \mathrm{e}^{i(\mathbf{k}\cdot\mathbf{x}-\mathbf{k}'\cdot\mathbf{x}')} \pm \right]$$

$$|\eta|^2 e^{-i(\mathbf{k}\cdot\mathbf{x}-\mathbf{k}'\cdot\mathbf{x}')} \int \delta^3(\mathbf{k}-\mathbf{k}')$$

$$= \int \frac{d^3\tilde{\mathbf{k}}}{\sqrt{(2\pi)^3\omega(\mathbf{k})}} \left[ |\xi|^2 e^{i\mathbf{k}\cdot(\mathbf{x}-\mathbf{x}')} \pm |\eta|^2 e^{-i\mathbf{k}\cdot(\mathbf{x}-\mathbf{x}')} \right]^{22}$$

and with a change of integration variable  $\tilde{\bf k} \to -\tilde{\bf k}$  on the second term

$$= \int \frac{d^3\tilde{\mathbf{k}}}{\sqrt{(2\pi)^3\omega(\mathbf{k})}} \left[ |\xi|^2 \pm |\eta|^2 \right] \mathrm{e}^{i\mathbf{k}\cdot(\mathbf{x}-\mathbf{x}')}.$$

The microcausality condition (4.51) now requires that  $|\xi|^2 \pm |\eta|^2 = 0$ .

We get two pieces of information from this calculation. First, we must have  $|\xi|=|\eta|$ . Second, for the quantum field operator (4.48) the choice of '+' in (4.52) is, after all, not an option. Here is where the connection with spin comes in, though I can't give the details because I have ignored the complications of spin throughout. When spin is also taken into account, the factor  $|\xi|^2 \pm |\eta|^2$  comes out as  $|\xi|^2 \pm (-)^{2s} |\eta|^2$  for fields of spin s.<sup>23</sup> Thus, if we have to choose between commutators and anticommutators for the raising and lowering operators, the microcausality condition shows that we must use commutators for integer spin and anticommutators for 1/2 integer spin. This is the spin-statistics theorem. It does not follow that these are the only options. Certain higher-symmetry types corresponding to so-called trilinear parafield commutators<sup>24</sup> are also consistent with the microcausality condition.<sup>25</sup>

### Position Eigenstates and Operators

So far my presentation has been almost completely mum about a confusing and generally ignored tangle of issues concerning spatial localization. In a nonrelativistic theory one can start with momentum eigenstates,  $|\mathbf{k}\rangle$ , and use a Fourier transform,

$$|\mathbf{x}\rangle_{\mathrm{NR}} = \int rac{d^3\mathbf{k}}{(2\pi)^{3/2}} \mathrm{e}^{-i\mathbf{k}\cdot\mathbf{x}} |\mathbf{k}\rangle,$$

The  $1/\sqrt{(2\pi)^3\omega(\mathbf{k})}$  comes from  $d^3\tilde{\mathbf{k}}'=d^3\mathbf{k}'/\sqrt{(2\pi)^3\omega(\mathbf{k}')}$ . See n.1 in this chapter.

<sup>&</sup>lt;sup>23</sup>See Weinberg (1964).

<sup>&</sup>lt;sup>24</sup>See Green (1953).

<sup>&</sup>lt;sup>25</sup>The requirement that  $|\xi| = |\eta|$  figures in a similar proof of the PCT theorem. If we start with the  $\hat{\Psi}(\mathbf{x},t)$  of (4.50), with  $|\xi|^2 = |\eta|^2$ , and make a spatial inversion, a charge conjugation (reversing the roles of quanta and antiquanta), and a temporal inversion, the result is the original  $\hat{\Psi}(\mathbf{x},t)$  shifted by an overall phase of  $\xi^*/\eta$ .

to introduce states,  $|\mathbf{x}\rangle_{NR}$ , parameterized by the spatial variable, 'x'. "NR" stands for "nonrelativistic." These states are, in the  $\delta$ -function idealization, orthonormal:  $\langle \mathbf{x}|\mathbf{x}'\rangle = \delta^3(\mathbf{x}-\mathbf{x}')$ . Orthonormality constitutes a crucial consideration in thinking of the  $|\mathbf{x}\rangle_{NR}$  states as position eigenstates: If we are to think, again in  $\delta$ -function idealization, of  $|\mathbf{x}\rangle_{NR}$  being a state in which one quantum is certainly located at  $\mathbf{x}$ , we require that in state  $|\mathbf{x}\rangle_{NR}$  the quantum is certainly not located at  $\mathbf{x}'\neq\mathbf{x}$ , that is, the overlap  $\langle \mathbf{x}'|\mathbf{x}\rangle = 0$ , that is, the  $|\mathbf{x}\rangle_{NR}$  collectively comprise a set of orthonormal states. Finally, in a quantum-field-theoretic version of the nonrelativistic theory, we can introduce a raising operator,  $\hat{\Psi}_{NR}^{\dagger}(\mathbf{x})$ , such that  $|\mathbf{x}\rangle_{NR} = \Psi_{NR}^{\dagger}(\mathbf{x})|0\rangle$ , so that we think of  $\hat{\Psi}_{NR}^{\dagger}(\mathbf{x})$  as a raising operator for a quantum exactly localized at  $\mathbf{x}$ .

This whole senario gets badly thrown off track by the requirements of writing a Lorentz invariant theory. We again start with momentum eigenstates,  $|\mathbf{k}\rangle$ , and we want to use a transformation function to introduce states parameterized by  $\mathbf{x}$ . We seek to use  $e^{-i\mathbf{k}\cdot\mathbf{x}}$  for the transformation, but to keep the description Lorentz invariant we must integrate not with  $\int \frac{d^3\mathbf{k}}{(2\pi)^{3/2}}$  but with  $\int \frac{d^3\mathbf{k}}{(2\pi)^{3/2}\sqrt{\omega(\mathbf{k})}}$ :

(4.54) 
$$|\mathbf{x}\rangle_{\mathrm{KG}} = \int \frac{d^{3}\mathbf{k}}{(2\pi)^{3/2}\sqrt{\omega(\mathbf{k})}} e^{-i\mathbf{k}\cdot\mathbf{x}} |\mathbf{k}\rangle.^{26}$$

The "KG" subscript stands for 'Klein-Gordon', for these states are exactly the ones that arise through second quantization of the first-order Klein-Gordon equation in a Lorentz invariant theory. That is, if we introduce a raising operator  $\hat{\Psi}_{\rm KG}^{\dagger}({\bf x})$ :  $\hat{\Psi}_{\rm KG}^{\dagger}({\bf x})|0\rangle = |{\bf x}\rangle_{\rm KG}$ ,  $\hat{\Psi}_{\rm KG}^{\dagger}({\bf x})$  is, with time-dependence suppressed, just the adjoint of the  $\hat{\Psi}^{+}({\bf x},t)$  of (4.33).

I have presented this heuristic alternative introduction of the  $|\mathbf{x}\rangle_{KG}$  states and raising operator  $\hat{\Psi}^{\dagger}_{KG}(\mathbf{x})$  to make quite clear how things go wrong if one tries to interpret the  $|\mathbf{x}\rangle_{KG}$  states as states of exact localization. As explained, we can so interpret the  $|\mathbf{x}\rangle_{NR}$  only because the  $|\mathbf{x}\rangle_{NR}$  are orthonormal. Putting that factor  $\sqrt{\omega(\mathbf{k})}$  into the denominator of the transformation function to make the description covariant ruins their orthonormality. The kets  $|\mathbf{x}\rangle_{KG}$  and  $|\mathbf{x}'\rangle_{KG}$  are not orthogonal,

This is the correct expression with  $\langle \mathbf{k} | \mathbf{k}' \rangle = \delta^3(\mathbf{k} - \mathbf{k}')$  normalization of the  $|\mathbf{k}\rangle$ . For manifest covariance multiply numerator and denominator by  $\sqrt{\omega(\mathbf{k})}$  and restore the suppressed time dependence. See n. 1 in this chapter.

for  $\mathbf{x} \neq \mathbf{x}'$  so that  $|\mathbf{x}\rangle_{KG}$  cannot be interpreted as a one-quantum state certainly occurring at  $\mathbf{x}$  and *not* occurring at  $\mathbf{x}'$ , for any  $\mathbf{x}' \neq \mathbf{x}!^{27}$ 

What to do? The solution seems easy: Just knock that troublesome factor of  $\sqrt{\omega(\mathbf{k})}$  back out of the denominator of the integrand in (4.54):

(4.55) 
$$|\mathbf{x}\rangle_{\text{NW}} = \int \frac{d^3\mathbf{k}}{(2\pi)^{3/2}} e^{-i\mathbf{k}\cdot\mathbf{x}} |\mathbf{k}\rangle.^{28}$$

The subscript "NW" stands for 'Newton-Wigner', the authors who argued (1949) on much more general grounds than presented here that the  $|\mathbf{x}\rangle_{\mathrm{NW}}$  and allied field and position operators are the uniquely correct way to get a description of exactly localized position states in a Lorentz invariant theory. Although the  $|\mathbf{x}\rangle_{\mathrm{NW}}$  of (4.55) arise by the same transformation function from momentum eigenstates  $|\mathbf{k}\rangle$  as do the  $|\mathbf{x}\rangle_{\mathrm{NR}}$ , the two descriptions are really different because the common transformation function is applied to momentum eigenstates from, respectively, a nonrelativistic and a relativistic theory.

But this "solution" brings with it a host of ills. The state descriptions,  $|\mathbf{x}\rangle_{\mathrm{NW}}$ , are not Lorentz invariant. Consequently, the physical states picked out by such descriptions are not Lorentz invariant, in the following sense: Suppose that a quantum is Newton-Wigner localized at  $\mathbf{x}$  as described in reference frame 1. That is, observer 1's stationary position detectors would be certain to find the quantum within a definite, arbitrarily small spatial-detection volume centered around  $\mathbf{x}$  at an arbitrarily well specified instant of time, with both space and time described in reference frame 1. In reference frame 2, moving with respect to reference frame 1, the same state receives a description that, although still concentrated around  $\mathbf{x}$ , nonetheless gives a nonzero amplitude for detection spread out all over space in the sense of reference frame 2. Consequently, observer 2's stationary detectors could find the quantum at points arbitrarily far from  $\mathbf{x}$ .

If this last fact were not bad enough, consider again a quantum in

 $<sup>^{27}|\</sup>mathbf{x}\rangle_{\mathrm{KG}}$  does, however, describe a state highly localized around  $\mathbf{x}$ , qualitatively speaking, within a Compton wavelength.

<sup>&</sup>lt;sup>28</sup>With  $\langle \mathbf{k} | \mathbf{k}' \rangle = \omega(\mathbf{k}) \delta^3(\mathbf{k} - \mathbf{k}')$  normalization, this becomes  $|\mathbf{x}\rangle_{\rm NW} = \int \frac{d^3\mathbf{k}}{(2\pi)^3/2} \sqrt{\omega(\mathbf{k})} e^{-i\mathbf{k}\cdot\mathbf{x}} |\mathbf{k}\rangle$ . One way to see why the distinction between  $|\mathbf{x}\rangle_{\rm NW}$  and  $|\mathbf{x}\rangle_{\rm KG}$  does not arise in a nonrelativistic approximation is to remember that the energy,  $\omega(\mathbf{k}) = \sqrt{m^2 + \mathbf{k}^2}$ , includes the rest energy arising from the mass. When we approximate  $\omega(\mathbf{k})$  by m, it becomes a constant, which no longer makes a significant difference between (4.53) and (4.55).

<sup>&</sup>lt;sup>29</sup>See Wightman (1962) and Wightman and Schweber (1955).

state  $|\mathbf{x};t=0\rangle_{\mathrm{NW}}$ . For any  $\epsilon>0$ , the amplitude for  $|\mathbf{x};t=\epsilon\rangle_{\mathrm{NW}}$  is still concentrated around  $\mathbf{x}$  but is nonetheless nonzero at points arbitrarily far from  $\mathbf{x}$ . This seems to suggest that quanta, as described by the  $|\mathbf{x}\rangle_{\mathrm{NW}}$  states, can propagate at arbitrarily high, superluminal speeds.<sup>30</sup>

The  $|\mathbf{x}\rangle_{KG}$  states are not genuine localized states. The  $|\mathbf{x}\rangle_{NW}$ , although genuine localized states, behave scandalously. Have we simply not yet found the right way to describe localized position in relativistically correct theories? Unfortunately Newton and Wigner (1949) have shown that the  $\hat{\mathbf{X}}_{NW}$  and its eigenstates,  $|\mathbf{x}\rangle_{NW}$ , are the only ones that satisfy conditions that are plausibly said to be required of a position operator and its eigenstates.<sup>31</sup>

Some respond to this situation by saying, so much the worse for our classical conception of a strictly localized particle. After all, to localize one quantum (or any definite number) within an arbitrarily small boundary requires arbitrarily high energies, which in turn results in quantum-antiquantum pair creation. The antiquantum can annihilate with the quantum supposedly localized in the original volume, while the "new" quantum may be some distance away. Precisely because of the idea I have emphasized in chapter 2, that quanta do not have any primitive thisness, these facts spoil the attempted experimental localization. There is no saying that the quantum now existing outside of the localization volume is "distinct from" or "the same as" the quantum

<sup>&</sup>lt;sup>30</sup>See Fleming (1965), Hegerfeld (1974), and Hegerfeld and Ruigsemaars (1980).

 $<sup>^{31}</sup>$ There are additional questions about localization for photons because they are massless: As m goes to 0, the Compton wavelength,  $\hbar/mc$ , goes to infinity. This fact might lead one to wonder whether there are also problems for photons with the Newton-Wigner position operator and localized states. Indeed, photons do not have Newton-Wigner localized states; and Newton and Wigner have given a general argument, turning on the fact that photons are characterized by spin one and zero mass, for the conclusion that there cannot be an operator that applies to photon states and that satisfies all the conditions that these authors maintain ought to be satisfied by a position operator.

Do these facts show that photons should not be considered real quanta, perhaps that they correspond to what should be seen as the quantum analogue of field phenomena, whereas fermions count as the quantum analogue of classical particles? I am skeptical: Photons can be approximately localized—roughly to within a wavelength. Other arguments for the conclusion in question are that (virtual) photons mediate the force between electrons and that large aggregates of Bosons in the same state have a classical field limit, whereas Fermions cannot be thus aggregated. But, as Redhead has argued (1983, pp. 79–80; 1988, pp. 15–16), what happens in the limit need not be a reliable guide to what is true of finite cases, and (virtual) electrons mediate the interaction of light with light.

<sup>&</sup>lt;sup>32</sup>For example, Henley and Thirring (1962), pp. 45-46, 51.

annihilated inside the localization volume. There is only the fact that when the show is over exactly one quantum is left, and it lies outside the localization volume.

At least formally, this is a dubious argument, because we get descriptions of pair creation only when we add interaction to the theory. So far, we are exploring the conceptual structure of the description of free quanta, that is, a description in which no interactions have yet been taken into account.<sup>33</sup>

Although the last is not, in the context of a theory of free quanta, a good argument against the conceptual coherence of exactly localized quanta, the unfortunate transformation properties of the Newton-Wigner states also cast prima facie doubt on their interpretability as states of exact localization. Let us return to the facts described five paragraphs earlier. Suppose we have a one-quantum state, the state description of which takes, in reference frame 1, the form  $|\mathbf{x}\rangle_{NW}$ , a Newton-Wigner state of "exact localization at x." In reference frame 2, moving with respect to reference frame 1, the same state is described as spread out over all space (though still concentrated at x). Now is this quantum localized at x or not? Is any positive, coherent interpretation possible for these states, in particular an interpretation that would connect these states with (perhaps very idealized) observational consequences? The description appropriate to frame 1 seems to have the quantum certain to be found at x, while the description appropriate to frame 2 seems to allow a probability, however small, for finding the quantum at points arbitrarily far from x. These facts seem flat out inconsistent.

A way out of this dilemma is to say that contrary to what we have

<sup>&</sup>lt;sup>33</sup>Henley and Thirring (1962) themselves comment, on pp. 41–42, that in the free theory  $[\hat{N}_{\mathbf{k}}, \hat{H}] = 0$ , so the number of quanta in momentum state  $|\mathbf{k}\rangle$  is constant in time. The gloss above is their attempted qualitative physical reconstruction of their formal result (pp. 44–45), that for the number density operator  $\hat{N}(\mathbf{r}), [\hat{N}(\mathbf{r}), \hat{N}(\mathbf{r}')] \neq 0$  for  $\mathbf{r} \neq \mathbf{r}'$ , which, quite independently of the physical argument, appears to contradict the possibility of localized Newton-Wigner states  $|\mathbf{x}\rangle_{\mathrm{NW}}$ . The apparent conflict with the formal facts I have summarized about states  $|\mathbf{x}\rangle_{\mathrm{NW}}$  arises because Henley and Thirring have defined  $\hat{N}(\mathbf{r})$  from the relativistically invariant field operators  $\hat{\phi}(\mathbf{x})$  and  $\hat{\phi}^{\dagger}(\mathbf{x})$ . Thus  $\hat{N}(\mathbf{r})$  is relativistically invariant, and their conclusion does not hold for the noninvariant Newton-Wigner number density operator,  $\hat{N}_{\mathrm{NW}}(\mathbf{x},t) = \hat{\Psi}^{\dagger}_{\mathrm{NW}}(\mathbf{x},t)\hat{\Psi}_{\mathrm{NW}}(\mathbf{x},t)$ . This follows from the commutators satisfied by  $\hat{\Psi}_{\mathrm{NW}}(\mathbf{x},t)$ :  $[\hat{\Psi}_{\mathrm{NW}}(\mathbf{x},t),\hat{\Psi}_{\mathrm{NW}}(\mathbf{x}',t)] = [\hat{\Psi}^{\dagger}_{\mathrm{NW}}(\mathbf{x},t),\hat{\Psi}^{\dagger}_{\mathrm{NW}}(\mathbf{x}',t)] = 0$  and  $[\hat{\Psi}_{\mathrm{NW}}(\mathbf{x},t),\hat{\Psi}^{\dagger}_{\mathrm{NW}}(\mathbf{x}',t)] = \delta^3(\mathbf{x}-\mathbf{x}')$ .

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always assumed, the state of motion of a position detector is one of the properties relevant to its functioning as a position detector, and consequently position itself is in some way relative to a state of motion. According to this account position becomes a relational property, but the overall description nonetheless remains Lorentz invariant inasmuch as the *relation* between position and states of motion can be equally well described from the point of view of any reference frame.

To make this all clear it is best to describe the relation in question not in terms of reference frames, but in the language of hyperplanes. Let  $\mathbf{x}, t$  be the coordinate variables of reference frame 1 and  $\mathbf{x}', t'$  be the coordinate variables of reference frame 2. Now including explicit specification of times, we use the  $\mathbf{x}, t$  coordinates to specify a state of exact localization at  $(\mathbf{x}_1, t_1)$ :  $|\mathbf{x} = \mathbf{x}_1; t = t_1\rangle_{\mathrm{NW}}$ . The points with time coordinate  $t = t_1$  constitute a space-like hyperplane, hyperplane 1, the points simultaneous with  $(\mathbf{x}_1, t_1)$  in frame 1. We consider a detector 1, stationary in frame 1, and so we say, stationary "on hyperplane 1." This detector is designed to respond with certainty at  $(\mathbf{x}_1, t_1)$  in state  $|\mathbf{x} = \mathbf{x}_1, t = t_1\rangle_{\mathrm{NW}}$ .

Next we consider a second space-like hyperplane 2 through  $(\mathbf{x}_1,t_1)$  but tilted with respect to hyperplane 1. Hyperplane 2 is constituted by the points simultaneous in frame 2 with coordinates  $(\mathbf{x}_1',t_1')$ . As represented in the  $\mathbf{x}',t'$  coordinates the state  $|\mathbf{x}=\mathbf{x}_1,t=t_1\rangle_{\mathrm{NW}}$  is concentrated within a Compton wavelength of  $(\mathbf{x}_1',t_1')$  but has nonzero amplitude at points on hyperplane 2 any distance from  $(\mathbf{x}_1',t_1')$ . We suppose we have a second detector, detector 2, in every relevant respect similar to detector 1, except that detector 2 is stationary in frame 2, or stationary on hyperplane 2, and so moving with respect to frame 1.

Consistency now is restored by supposing that detector 2 is not certain to respond at  $(\mathbf{x}_1, t_1)$  but has a response distribution for points on hyperplane 2 corresponding to the state description as given in frame 2. When detector 1 responds, we say that it detects position relative to or on hyperplane 1, and that the quantum has been localized at  $(\mathbf{x}_1, t_1)$  relative to or on hyperplane 1. Detector 2 detects position, and localizes quanta, relative to or on hyperplane 2. Because the two detectors detect and localize quanta relative to different hyperplanes, there is no inconsistency in the difference in response distributions. All this works by taking position, or exact localization of quanta, to be relative to a hyperplane, and when a quantum is exactly localized relative to one hyperplane, it is dispersed relative to any other hyperplane tilted with

respect to the first. Finally this whole scheme can be given a Lorentz invariant description in which, in all reference frames, one gets the same description of the relation of a quantum being localized (or dispersed) on any given hyperplane.<sup>34</sup>

In this section I have discussed localization only in the context of the free theory, in which we assume no interaction. In the real world, particles always interact. As physicists say, "We can't turn the interactions off." And when we admit interactions, we must face the argument outlined earlier, that localization to arbitrarily small volumes requires arbitrarily high energies, which, in a theory with interactions, will result in quantum pair production. Conventional physicists' wisdom seems to take this consideration to spoil any possibility of localization to arbitrarily small volumes.

Do these facts show that all localization properties of states that are in principle realizable can be adequately described with the Klein-Gordon states (or their Dirac analogues)? These states describe quanta as virtually certain to be in a volume of width of a Compton wavelength but as having infinite spread and failing to have the sort of delta function behavior that one would expect of a true position basis. Do the problems of energy of localization and pair production show that the foregoing limitations are just right for location as described by relativistic quantum theories?

At this point I can say only that this last argument is inconclusive. True, a state with extreme certainty for location must involve extreme uncertainty for momentum, and so for energy. But this fact does not tell us where the energy must be manifested. True, if we try to localize a quantum by "banging" it, say with radiation, we will get pair creation in the vicinity of localization. But these facts do not rule out the possibility of other, indirect means of preparing an arbitrarily well localized quantum. And even if, as is quite likely, there are no such indirect means, interpreters of quantum theories will find it interesting to see how much of a conception of exact localization we can salvage within a relativistic quantum-theoretical framework, and how this salvage operation seems to require relativization of the idea of strict localization to hyperplanes of simultaneity.

<sup>&</sup>lt;sup>34</sup>See Fleming (1965, 1989) and further references therein.

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#### PROBLEMS

- Reinterpret the heuristic substitutions (4.9) and (4.10) as accurately as
  possible, both in their applications in first quantization and in field quantization.
- 2. Critically examine existing arguments for the conclusion that in quantum field theory the infinitesimal generator of spatial displacements is the operator for total momentum. Provide a satisfying version of the argument that makes its physical content clear.
- 3. In a famous paper Bohr and Rosenfeld tried to justify the field commutation relations for the electric and magnetic field operators by extending the Heisenberg microscope, disturbance style of argument for the uncertainty relations (Bohr and Rosenfeld, 1933). (One can find a good summary in Heitler [1954], pp. 81ff.) In outline they argued that one would have to use a charged test particle to measure the electric and magnetic fields, and that uncertainty in the position of the test particle would translate into uncertainty in the measured electric field, while uncertainty in the velocity of the test particle would similarly translate into uncertainty in the measured magnetic field. Consequently, they argued, the uncertainty relations applying to the test particle must carry over to the electric and magnetic field, which uncertainty can be expressed in the commutation relations for the electric and magnetic field operators. Brown and Redhead (1981) criticize disturbance interpretations of the uncertainty relations generally and briefly remark that their critical examination should apply to the Bohr and Rosenfeld analysis. Critically evaluate this debate.

# What the Quantum Field Is Not: Field and Quantal Aspects of Quantum Field Theory

CHAPTER 3 SHOWED THAT the subject matter of so-called "quantum field theory" does not need to be presented as a field theory. Why then is the subject so called? One obvious and satisfactory answer is simply historical: The subject was created by applying the technique of first quantization to a classical field theory—field quantization—and then by an analogical maneuver applied to state functions treated as if they were classical fields—second quantization.

A second answer presents more of a problem. The quantum field is often described as an "operator-valued field," which is too easily thought of as something with a special kind of value—represented by an operator—which varies from place to place and from time to time. I want to show that this gloss constitutes an entirely wrong-headed way of thinking about the subject. After this first order of business, we will take another look at the ways in which "quantum field theory" really does incorporate important field-theoretic aspects and how these fit, after all, quite smoothly with its "particlelike" alter persona.

As I explained at the beginning of chapter 4, by a field theory I understand laws, such as differential equations, applied to a description that attributes physical quantities to space-time points. This chapter's first problem is simply this: Quantum field theory can be given the form of a field theory in one way through an association of mathematical entities—operators—with space-time points. But the subject is not thereby shown to be more than formally a field theory unless it thereby associates values of physical quantities with the space-time points. Consequently, if my characterization of the notion of a field is accepted, the standard "operator-valued field" description would work only insofar as operators represent values of some physical quantity. On this basis I will argue that although quantum field theory can be construed as a

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field theory, this must be done by a very different way of slicing the conceptual pie.

As a preliminary I want to put aside as not really relevant the self-adjointness of field operators. What practitioners usually call the quantum field—operator-valued fields such as those described by (4.3) and (4.48)—often are not constituted by Hermitian operators. It is standard practice in the interpretation of quantum mechanics to take only Hermitian operators to correspond, in whatever sense, to physical quantities. The thinking is that only Hermitian operators are "real valued," in the sense of having real-valued eigenvalues, and only real values can represent values of physical quantities.

But complex values can be taken indirectly to represent values of physical quantities through separate appeal to real and imaginary parts. Furthermore, even if space-time parameterized operators such as those described by (4.3) and (4.48) are, strictly speaking, themselves not taken to represent a physical field because the operators are not Hermitian, nonetheless defined Hermitian operators might be taken directly to represent a physical field. For example, in the context of the nonrelativistic Schrödinger theory the selfadjoint  $\hat{\Psi}^{\dagger}(\mathbf{x},t)\hat{\Psi}(\mathbf{x},t)$  might be taken to represent the density of quanta. (The complications outlined at the end of chapter 4 compromise any such interpretation of  $\hat{\Psi}^{\dagger}(\mathbf{x},t)\hat{\Psi}(\mathbf{x},t)$  in the relativistic case.) Definitions of Hermitian operators corresponding to the electric and magnetic fields work similarly.

Most of what I say in this discussion will concern operator-valued fields "generically," applying to any case in which operators are systematically assigned to space-time points. Readers who think that complex values can represent physical quantities are invited to include examples such as operators governed by last chapter's (4.3) and (4.48). Readers who have qualms can restrict their list of examples to Hermitian cases.

## CLASSICAL QUANTITIES AND FIELDS

To establish a framework for discussion I want to introduce some terminology useful in talking clearly about physical quantities in a classical context. For example, take the way we think and talk about mass. Any specific physical object has some definite mass—1 kilogram, or 2.53 kilograms, or.... Furthermore, anything that can have a mass must have one or another specific mass. Let's think of the specific mass of a given

object as a property of that object. Thus each distinct mass value constitutes a distinct property. But all these properties constitute a natural family. We can express this by taking advantage of the fact that anything that can have one of the mass properties must have exactly one of the mass properties. We will say that the determinable, mass, is simply the collection of the individual mass properties. And in general we will say:

A Determinable is a collection of properties such that anything that can have one of the properties in the collection must have exactly one of the properties. The values of a determinable are its individual properties. Values of a determinable are ordinarily represented by mathematical entities such as real numbers and vectors.

Note that this terminology enables us to clarify an ambiguity in the use of a word such as 'mass'. By 'mass' one can mean the specific mass of an object: The mass of this marble is ten grams. Or by 'mass' one may mean the determinable, mass. To differentiate, I will italicize a word when it is being used to refer to a determinable as opposed to a specific value of the determinable.

This terminology now applies to sharpen our talk about fields, classically conceived. We begin with the idea that space-time points can have properties and then consider determinables the values of which are properties of space-time points. A simple example is the determinable mass density. Often we want to consider assignments of values to each of the space-time points:

A Field Configuration for a determinable (or collection of determinables) is a specific assignment in which each space-time point gets assigned a value of the determinable (or a value of each determinable in the collection).

Armed with the idea of specific field configurations, we can also think of each specific field configuration itself as a value whose corresponding determinable is the collection of all possible configurations. For clarity I introduce the following additional expressions:

<sup>&</sup>lt;sup>1</sup> 'Possible' here could mean *logically possible*, in which case 'field' would refer to any possible field configuration, whether or not it was physically possible. Or one might mean *physically possible*, in which case 'field' would refer to physically possible configurations, for example, any configuration satisfying relevant field equations. One can opt for either usage as long as it is clear. My exposition is made a little more straightforward by opting for the "physically possible" usage.

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A Constitutive Determinable is a determinable the values of which are properties of individual space-time points. A Field Determinable is a determinable the values of which are the full field configurations.

All this may seem like terminological overkill until one reflects that it helps untangle a subtle ambiguity in the word 'field', which can equally well be used to refer to a specific field configuration or to what I am calling a field determinable, as when one talks of "the electromagnetic field," the "gravitational field," and so on. I believe that this ambiguity has often obscured the proper understanding of the "quantum field." Thus the disambiguating terminology will prove crucial to giving an accurate description.

# QUANTITIES AND VALUES IN QUANTUM THEORIES

The ideas of determinables and their values are inherently classical notions, appropriately used to characterize classical fields. To generalize the field concept to quantum theories, we must apply some analogue of the classical ideas of determinables and their values. An important part of our problem is that none of the foregoing conceptions of determinables and their values have exact analogues in quantum theories. Perhaps what corresponds most closely to classical values of determinables in quantum theories are eigenvalues. But in general a quantum theory does not assign exact values—eigenvalues—to quantities. Usually the closest quantum theoretical analogues are the theory's probabilities, or, more generally, its expectation values. And because there is interpretive disagreement as to how to understand the (at least apparent) absence of exact values in quantum mechanics, we do not have the foundation on which accurately to chart the similarities to and differences from classical theories in this respect.

What about determinables? Because operators determine an eigenvalue spectrum, operators appear to be quantum theories' closest analogue to classical determinables. 'Operator' here appears with a systematic (and traditional) ambiguous usage. The word is used to refer to something physical, that which is supposed to be the quantum analogue of a classical determinable. 'Operator' is also used to refer to a mathematical entity, specifically to a linear map from vectors to vectors in a Hilbert space, where this mathematical object is taken to represent the physical thing to which 'operator' refers in its first sense. One way in

which quantum-theoretical operators (in the physical sense) differ from determinables of classical theories lies in the dual circumstance of the absence of exact values and of the noncommutative structure of the representing mathematical objects. But I am not here going to dive into the task of thoroughly comparing operators with classical determinables. All I will need is the idea that operators are the closest analogue in quantum theories to determinables in classical theories.

What I have so far said works best when put in terms of the Schrödinger picture, which describes operators as fixed and states as evolving over time. As the states evolve, so do the values—probabilities and expectation values—that a fixed operator associates with an evolving state. But one can equivalently express the formalism of a quantum theory in terms of the Heisenberg picture, in which the states are fixed and operators evolve. Let's briefly review.

The experimental content of quantum theories can always be expressed in terms of expectation values,  $\langle \hat{O} \rangle_{\Psi(t)} = \langle \Psi(t) | \hat{O} | \Psi(t) \rangle$ . The states themselves evolve according to  $|\Psi(t)\rangle = \hat{U}(t) |\Psi(0)\rangle$ , with  $\hat{U}(t) = \mathrm{e}^{-i\hat{H}t}$ . This means that the evolving expectation values can be written as  $\langle \hat{O} \rangle_{\Psi(t)} = \langle \Psi(0) | \mathrm{e}^{i\hat{H}t} \hat{O} \mathrm{e}^{-i\hat{H}t} | \Psi(0) \rangle$ . Consequently, it makes no difference to calculating the  $\langle \hat{O} \rangle_{\Psi(t)}$  whether we take the operators,  $\hat{O}$ , to be constant in time and the states to evolve according to  $|\Psi(t)\rangle = \mathrm{e}^{-i\hat{H}t} |\Psi(0)\rangle$  or whether we regard the states as fixed in time,  $|\Psi\rangle = |\Psi(0)\rangle$ , while the operators evolve according to  $\hat{O}(t) = \mathrm{e}^{i\hat{H}t}\hat{O}(0)\mathrm{e}^{-i\hat{H}t}$ . One formulates a quantum theory in what is called the Schrödinger picture by taking the operators to be fixed in time with the states evolving, and the equivalent formulation in the Heisenberg picture keeps the states constant while the operators evolve in time as just described.

When a quantum theory is formulated in the Heisenberg picture we face potential further confusion over quantum analogues of classical values and determinables. We are used to thinking of determinables as fixed and values as evolving. A formalism in which operators evolve over time supplies a powerful temptation to think of the operators themselves as some kind of value. We must address this temptation because in the operator-valued "quantum field," the operators evolve over time as well as vary with the spatial coordinates.<sup>2</sup>

<sup>&</sup>lt;sup>2</sup>Virtually all practical discussions describe the field operators as evolving not according to the Heisenberg picture, but according to the interaction picture in which, roughly speaking, the states evolve according to just the interaction part of the total Hamiltonian and the operators evolve according to the free part of the Hamiltonian. The next chapter will introduce the relevant ideas. All that matters for our conceptual

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If operators evolve over time, do they have to be thought of as some kind of evolving values? No. The key here is to keep clearly in mind that as a Heisenberg operator evolves, at each time it still corresponds to its full spectrum of values, the same spectrum at all times. In other words, a Heisenberg operator does not correspond to what is, at one time, a single value of a physical quantity which value then changes over time. Instead, at a fixed time a Heisenberg operator still corresponds to all its possible values (eigenvalues, probabilities, and expectation values), which value being relevant in a given actual case depending on which time-independent state is relevant in the given case. Only the Heisenberg operator plus a time-independent state picks out a specific value.

What then does the time evolution of a Heisenberg operator represent the evolution of? By charting the evolution of relative phases, such an operator encodes the evolution of the value of the physical quantity to which it corresponds, not for any one fixed system, but for any system that can have a value of the quantity. In other words, a Heisenberg operator collectively describes the pattern of evolution for the values of the quantity in question for any system that can have a value for that quantity. In yet other words, the time evolution of a Heisenberg operator encodes the information carried by the Schrödinger equation in application to the quantity in question. All this is very different from something thought of as itself one evolving value.

## WHAT THE "QUANTUM FIELD" IS NOT

These preliminaries now facilitate statement, in several forms, of the proper way to line up the important disanalogies between classical fields and the so-called "operator-valued quantum field." I have repeatedly acknowledged the positive formal, or mathematical analogy: The operator-valued quantum field configuration is a field in the formal sense in that it is specified by associating mathematical entities—operators—with the space-time points. But our comparison of operators with determinables rather than with values of a determinable signals that such a description is badly off the mark.

It is doubly tempting to think of the association of operators with space-time points as analogous to a classical field configuration. The

concerns at the moment is that the operators evolve over time. Consequently I will here ignore the distinction between Heisenberg and interaction pictures.

first temptation arises simply because we have mathematical entities, playing a central role in the theory, systematically assigned to the space-time points. But we have seen that the assigned entities—operators—are more like determinables than like assigned values. And we are tempted because in the time dependence of the operators we seem to have a case of something—we are again tempted to think that they are values—evolving over time. But we have just seen, in the case of Heisenberg operators in quantum theories generally, that this impression is misleading. A temporally evolving operator does not represent an evolving value; it represents the pattern of evolution of all values of the quantity in question, as determined by the relevant Schrödinger or other equation. At any given space-time point, the associated operator corresponds not to the value of some physical quantity but to the full spectrum of values of some quantity, which value being applicable being determined by the state that happens to obtain.

Accordingly, the assignment of these "field operators" to the spacetime points does not look at all like a classical field configuration.

To see the point in another way, let us try to think through what an analogue of an "operator-valued quantum field configuration" would look like classically. Consider some constitutive determinable for a field. say the electric vector, with values, E. Specification of a configuration of the electric field involves specification of a value, E, to each space-time point. As we have seen, this is not at all analogous to the "quantum field" that assigns something like a determinable to each space-time point. To get a classical analogue of the "quantum field" we would have, perversely, to think of each electric-vector-at- $(\mathbf{x}, t)$  as a distinct determinable. In other words, we would have to take the electric-vector $at-(\mathbf{x},t)$  as one determinable, the electric-vector-at- $(\mathbf{x}',t')$  as a second determinable, and so on. Each of these distinct determinables has the same spectrum, but each takes on its own value. We can now think of the classical electric field in terms of an assignment of these "different" determinables, counting an assignment of the "different" determinables to the space-time points as a kind of field configuration. I will call this the perverse reading of the classical field, counting such an assignment as a kind of field configuration. The so-called "operator-valued quantum field" is a field configuration only in something like this perverse sense.<sup>3</sup>

<sup>&</sup>lt;sup>3</sup>In some cases of classical fields the perverse sense seems more or less perverse than in others. It seems pretty perverse for the example of the electric field, and perhaps more so for a matter-density field. But what about tangent vectors along a curve in the context of general relativity? In this context we are reminded that

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The analogy between the "operator-valued quantum field configuration" and the classical field in the perverse sense, as I have so far described it, breaks down in an important respect. As I have explained for the case of temporal evolution at a point, the Heisenberg operators track the way any value of a given quantity will evolve over time. The analogous circumstance holds for variation over space: The operators of the "operator-valued quantum field" track the variation of values over space as well as over time. As so far described, the perverse reading's determinables at points don't do that. They don't encode the connections between values at different space-time points. But we can easily enrich the perverse reading to bring it in this respect closely in line with the quantum field case.

Let 'f' vary over classical field configurations of whatever constitutive determinables are in question (for example, mass density). That is, for any specific value of f, f is a full specification of the value of the constitutive determinable at each space-time point, (x,t). I use the notation 'f' instead of ' $f(\mathbf{x},t)$ ' for the field configurations because in common usage ' $f(\mathbf{x},t)$ ' is often ambiguous between, on the one hand, the full function that maps values of (x, t) onto values of f and, on the other hand, specific values of f for some given value of (x, t). Here we must avoid this ambiguity! Now let F stand for the associated field determinable, that is, the determinable the values of which are the specific field configurations, f. But F can be broken down into component determinables,  $F|_{(\mathbf{x},t)}$ .  $F|_{(\mathbf{x},t)}$ , for fixed  $(\mathbf{x},t)$ , is to be thought of as the determinable whose values are  $f(\mathbf{x}, t)$ , with fixed  $(\mathbf{x}, t)$  and variable f. The  $F|_{(\mathbf{x},t)}$  are the determinables-at-space-time-points of the perverse reading, but now with a little more structure, which enables us to chart connections between different  $(\mathbf{x},t)$ .  $F|_{(\mathbf{x},t)}$  and  $F|_{(\mathbf{x}',t')}$  are, formally, distinct determinables. But they are related inasmuch as the same state

comparison of vectors at different points requires specification of a connection, and then the comparison is still relative to transport along a curve. In this case it does not seem so unnatural to say that the tangent space at each point along a curve constitutes or corresponds to a determinable, and a different determinable at each point. However, the example of tangent spaces does no more than dramatically make the point that whenever the value of a quantity is assigned to different space-time points, one tacitly presupposes a standard of comparison. For example, assignment of instances of a specific shade of the color red to two different locations tacitly presupposes a comparison of colors at different places, a standard that, in principle, could be set up in various ways. The need for a standard of comparison does not seem to me to show that we are concerned with different determinables at different places. The case of field operators differs substantially from the classical case, sensibly construed, in that the theory specifically assigns the (quantum-mechanical analogues) of different determinables to different space-time points.

that fixes the value of the determinable at one (x, t) also fixes the values for the determinables at other (x', t'). And if the state satisfies some field equations, then the last remark is just to say that these values will collectively satisfy the field equations.

The "operator-valued quantum field, say  $\hat{\Psi}(\mathbf{x},t)$  (with variable  $\mathbf{x}$  and t), is a field configuration at best in whatever sense  $F|_{(\mathbf{x},t)}$  is a classical field configuration. I have called the analogy perverse, but it does point the way to getting the analogy right. Classically, F makes perfectly good sense as a field determinable, suggesting that we think of the "operator-valued quantum field,"  $\hat{\Psi}(\mathbf{x},t)$ , in the same way. If  $\hat{\Psi}(\mathbf{x},t)$  is a field determinable, what then are the field configurations that constitute the specific values of the field determinable? The natural and smoothly working candidates are the expectation values,  $\langle \phi | \hat{\Psi}(\mathbf{x},t) | \phi \rangle$ . We have said that eigenvalues, probabilities, and more generally expectation values are what correspond most naturally in quantum theories to values of classical determinables. And expectation values,  $\langle \phi | \hat{\Psi}(\mathbf{x},t) | \phi \rangle$ , give exactly the desired association of definite values to the space-time points.<sup>4</sup>

The considerations I have been urging emerge in yet one more way when one pays attention to the distinction between a general and a specific solution to a field equation and to the sense in which "the operator-valued quantum field" is the (a?) "quantized" solution to the same equation. Let  $g(\mathbf{x}, t; \mathbf{k})$  be a complete set of solutions to a linear field equation;<sup>5</sup> that is, an arbitrary solution can be written in the form

(5.1) 
$$\Psi(\mathbf{x},t) = \int d^3\mathbf{k}q(\mathbf{k})g(\mathbf{x},t;\mathbf{k}).$$

The  $q(\mathbf{k})$  can be set arbitrarily, as long as the integral is well defined. The intention is that (5.1) is not really itself a solution to the equation. Instead, it is a schema: Fill in some specific function,  $q(\mathbf{k})$ , and a solution results. One often summarizes this idea by saying that (5.1) itself is "the general solution" to the equation. By representing arbitrary superpositions of a complete set of solutions,  $g(\mathbf{x}, t; \mathbf{k})$ , the "general solution"

<sup>&</sup>lt;sup>4</sup>Andrew Wayne pointed out to me that the analogy of expectation values  $\langle \phi | \hat{\psi}(\mathbf{x},t) | \phi \rangle$  to classical field configurations is still flawed in at least two respects: (1) The values assigned to space-time points are expectation values for physical quantities, not actual values for physical quantities; and (2) There is a field configuration analogy for *every* space-time-indexed operator, so that, in this form of description, there is much less unity than in the classical case.

<sup>&</sup>lt;sup>5</sup>The point I am about to make holds also for a nonlinear field equation, but it is harder to state without a simple closed form in which to express the general solution.

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carries exactly the information carried by the original field equation. A specific instance of the general solution carries additional information, typically thought of as initial and boundary conditions that pick out the specific solution in question.

When we apply field or second quantization to (5.1), the function  $q(\mathbf{k})$ is reinterpreted as an operator-valued function,  $\hat{q}(\mathbf{k})$ . But now we have lost the sense in which the values of  $\hat{q}(\mathbf{k})$  can be set arbitrarily! Classically, the idea of the general solution, (5.1), was that one could settle arbitrarily on values for the function,  $q(\mathbf{k})$  (as long as the resulting function was sufficiently well behaved), and get an arbitrary solution to the field equation. Again, in this sense the "general solution" itself carries no more than the information of the original field equation. But after field or second quantization, there is no such thing as substituting specific numerical values for the parameter ' $q(\mathbf{k})$ '. The quantized solution to a field equation is not a solution, it is in some sense the solution, in something like the way that (5.1) is "the general solution" to the field equation. Insofar as the quantized solution is like the general solution to a classical field equation, the quantized solution carries the information of the field equation, but not what corresponds to initial and boundary conditions.

This description is not yet quite right. First, the operator-valued solution to a field equation is constrained not only by the field equation but also by the imposed commutation relations. In addition, given an operator-valued solution of the field equation, any unitary transformation of that solution is also, at least formally, a solution. This fact proves useful in one approach to the S-matrix, in describing the S-matrix as the unitary transformation connecting what are called *in-fields* and *out-fields*, both solutions to the free field equation which, very roughly speaking, describe the situation, respectively, before and after interaction occurs. But the initial (in) and final (out) situation are not descriptions of some specific array of quanta. They are again descriptions of what will happen given any arbitrarily specified collection of quanta. Actual cases are given by expectation values.

Once more, the analogy between the classical and quantum cases is far from exact. But a "quantum field" corresponds more closely to a "general solution" to a field equation than to a specific solution reflecting complete initial and boundary values. As the analogue of a classical "general solution," we again describe the "operator-valued quantum field" most accurately as a field determinable, something that charts the

spatiotemporal relations of any of a large set of possible field configurations. Only when we add initial conditions, by specifying a specific input of quanta, do we get a field configuration, described in the quantum case by the expectation values for the possible experimental outcomes.

Some readers will find this conclusion quite obvious. But others have been led astray by the systematic ambiguity of the word *field* between field determinable and field configuration, reinforced by the practice of talking about the time-dependent quantum field operators as if they represented some specific value that varies over time and space. So far in this chapter I have argued that the common expression "operator-valued field" is misleading. The correct way to see the field-theoretic content is to see field operators as (analogous to classical) field determinables and to see the field configurations in the expectation values given by applying field operators to specific states.

# FITTING TOGETHER THE IDEAS OF FIELDS, QUANTA, AND SUPERPOSITION

Given any state,  $|\phi\rangle$ , and field operator,  $\hat{\Psi}(\mathbf{x},t)$ ,  $\langle\phi|\hat{\Psi}(\mathbf{x},t)|\phi\rangle$  counts as a field configuration. On this attitude any state can be reconstructed in a field-theoretic way. What room does this leave for any particlelike notions in the theory?

Here's a short answer:  $|\phi\rangle$  can be any state, including a state for an exact number of quanta. Because an exact number state, say  $|n\rangle$ , can be deployed in the description of a field configuration,  $\langle n|\hat{\Psi}(\mathbf{x},t)|n\rangle$ , the theory shows us how smoothly to combine considerations about exact number of quanta and field configurations. But this fact does not yet tell us how we have resolved what at least had seemed to be a conflict between field and particle concepts.

A terminological contrast will help in talking about the issue. I will use the words 'field' and 'quantum' in the relatively precise senses I have introduced at the beginning of chapter 4 and in chapters 2 and 3 respectively. (I will review below.) I follow what I take to be at least one traditional usage and use 'waves' to talk about field configurations that may be superimposed. I will use the word 'particle' with its prequantum meaning, with at least the suggestion of exact trajectories and primitive thisness, although the word is to some extent vague and fails to have clearly settled criteria of application. In particular, I will use the word

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'particle' when attempting to dissect the felt conflict between the ideas of fields and particles. The strategy is to move to concepts whose mutual fit can be more clearly judged because the concepts are more clearly delineated.

Traditionally, field and particle ideas were said to be incompatible. Field configurations (waves) are dispersed in space. Particles are not. And field configurations (waves), but not particles, superimpose. I do not dispute that there is some kind of conflict here. But to see that conflict more clearly we need to disassemble the relevant concepts to see which parts of one are in conflict with which parts of the other. More important, after disassembly, it will be easy to see how to reassemble the viable parts into a consistent picture.

Chapter 2 already did the necessary work for 'particle'. There we identified and excluded as inapplicable in the quantum domain the ideas of exact trajectories and primitive thisness. We retained the ideas of discreteness and of (at least highly exact) localizability. We used the word 'quanta' for the resulting concept.

The idea of superposition will play an important role. Recall from chapter 1 that strictly speaking a first property is the superposition of a second and third property if a description of the first results by taking the mathematical sum of standard descriptions of the two others.

Sometimes people interpret the word 'field' so that the field configurations of a field are assumed closed under superposition. But we can separate superposition from a weaker field concept. Let's understand field in the weak sense as the idea of fields introduced in chapter 4: Field configurations are assignments of values of quantities to space-time points, where the values may be governed by field equations. On this conception, superposition of field configurations is not assumed. Indeed, if the field equations are nonlinear, the field configurations of the field will in general not be closed under superposition. Field in the strong sense will in addition assume superimposability of the field configurations: The superposition of two field configurations is again a configuration of the field. As I mentioned, I take talk of waves to be talk of field configurations in this stronger sense.

We are now ready to examine the compatibility of reassembled concepts with parts taken from the field and particle families. I will outline two cases, only the second of which really concerns quantum theories. For the first, the idea of fields in the weak sense, separated from superposition as it is, is compatible with a particle concept from which only

primitive thisness has been eliminated. For we can represent particles field theoretically by assigning the value 1 to locations occupied by a particle and the value 0 elsewhere. We need to add an assumption of impenetrability, and then each particle is individuated by its space-time trajectory. We can elaborate the description of particles by associating various particle properties (mass, charge, and so on) with the space-time locations occupied by particles. Having done so, two field configurations will not superimpose when they assign particles to the same space-time location. So the account works only with the idea of a field in the weak sense. Although nothing in this account corresponds exactly to the idea of primitive thisness, the space-time trajectories function as a kind of ersatz primitive thisness, doing all the work that one could want from that concept.<sup>6</sup>

The foregoing is not suitable for quantum theories: It requires well-defined, nonoverlapping trajectories. But a very different way of combining concepts will put together quanta with fields in the strong, superimposing sense. Classical descriptions or conventional quantum mechanics made it hard to see this possibility because in these contexts it was not clear what one might mean by superimposing entities described with any of the particle family of concepts. In these contexts particles are taken to be things in which properties inhere, or to which properties attach. Superposition applies to properties. One can say that two states superimpose when the states concern a fixed roster of particles, inasmuch as states were taken to be maximal descriptions of the properties of particles. But what does one mean by saying that two particles—two things—superimpose?

I don't claim that this problem is insurmountable. Instead I want to call attention to the fact that this problem is simply sidestepped by formulating a theoretical description with the Fock space formalism and interpreting superpositions as I have done in terms of propensities. Such a description does not attribute states to objects in the world. Instead states simply characterize propensities for what will be manifested with what probability under various activating conditions. Among the items for which there can be propensities for manifestation is the occurrence of various numbers of quanta exhibiting various properties. And two descriptions each characterizing (certain or merely probable) manifestation of quanta can superimpose to characterize a state in which either

 $<sup>^6</sup>$ In this account of a field-theoretic description of particles I have closely followed Redhead (1983), pp. 58–62.

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of the two manifestations will occur with probabilities calculated from the superposition formalism.

These remarks make clear how the idea of quanta can fit with superposition but does not yet bring in any field concept. However, we have already seen how this part of the story goes at the beginning of chapter 4. The propensities for manifestation can be (though they do not have to be) expressed as propensities for manifestation at specific space-time points, or at least in very well defined space-time volumes.

I have outlined how a description using the Fock space formalism can consistently combine the concepts of superposition, fields, and quanta understood as entities with discreteness and at least a high degree of localizability, but without exact trajectories or primitive thisness. Can one consistently add primitive thisness or exact trajectories to this conceptual blend? The issue will be judged academic by anyone who views the facts of the quantum world as giving us considerable independent reason to reject primitive thisness and exact trajectories. But for the record: Yes, it seems that both could be reintegrated into the scheme if it were not otherwise constrained by the facts of the quantum world. As mentioned in chapter 2, I see no reason why a state cannot superimpose propensities for, say, exactly one and exactly two entities, each with primitive thisness. If the one-entity state is realized, there may well be no intelligible answer to the question, which one is it? But that in itself is not a difficulty if we recognize that in the superimposed state there are no actual primitive-thisness-bearing entities, whereas after actualization there is, either one or two of them, newly coming into existence. As for exact trajectories, these could still occur as special cases, though not in states involving superpositions of exact trajectory states.

#### Some Examples of Field Phenomena

So far we have spent a lot of effort examining the field aspect of quantum field theory, but most of it concerned field descriptions of (propensities for) manifestation of quanta.

The theory also functions to describe phenomena that are more naturally described as field phenomena, although in at least some of these cases application of the epithets 'field phenomena' and 'particle phenomena' may be a bit arbitrary. I will begin with a discussion of the contrast

between the two epithets and then take a brief look at three interesting cases.

In all quantum theories we say that two quantities are complementary if their operators do not commute,<sup>7</sup> and it is true generally that when a state has an exact value for one quantity, it will not have an exact value for complementary quantities. Let's consider the special case of the real quantum field operator (4.49) with  $\hat{a}^{\dagger} = \hat{a}^{\dagger}$ :

$$\hat{\Psi}_R(\mathbf{x},t) = \int d^3 \tilde{\mathbf{k}} [\hat{a}(\mathbf{k}) \mathrm{e}^{i(\mathbf{k}\cdot\mathbf{x} - \omega(\mathbf{k})t)} + \hat{a}^{\dagger}(\mathbf{k}) \mathrm{e}^{-i(\mathbf{k}\cdot\mathbf{x} - \omega(\mathbf{k})t)}].$$

 $\hat{\Psi}_R(\mathbf{x},t)$ , a linear superposition of raising and lowering operators, is selfadjoint and provides a simplified model of examples of operators for field quantities such as the electric field. The point from which to start in this discussion is the fact that the number operator,  $\hat{N}$ , and  $\hat{\Psi}_R(\mathbf{x},t)$  do not commute. Thus a state with an exact number of quanta will be inexact for quantities such as the electric field, and vice versa. In important cases, field quantities and  $\hat{N}$ , the quantity for the number of quanta, are complementary.

One must not hastily generalize on the last example to conclude that all and only field quantities are complementary to  $\hat{N}$ , or that somehow the field content of quantum field theory is contained in the states that are inexact for the number of quanta. For instance, consider a simple (and not physically interesting) example of a state not exact for the number of quanta, a superposition of a one-quantum and a two-quantum state,  $|s\rangle = |1\rangle + |2\rangle$ , which has no particular connection with phenomena sensibly described as field phenomena. Conversely, in the nonrelativistic theory  $\hat{\Psi}^{\dagger}(\mathbf{x},t)\hat{\Psi}(\mathbf{x},t)$  can be construed as a quanta density operator, and it commutes with the number operator. But a quantity that assigns a matter density to each space-time point is as good a candidate as any for a field quantity.

Thus there are no hard-and-fast rules for saying what in quantum field theory gets called a field phenomenon. Practitioners apply the label as a result of perceived analogies or connections with classical cases and to some extent as a matter of convention.

For a first example of something most appropriately called a field phenomenon, let's briefly look at *coherent states*. We begin by restricting attention to quanta with a fixed value of k. We know that the time development goes according to a frequency  $\omega(\mathbf{k})$ :  $e^{-i\omega(\mathbf{k})t}$ . Consequently, any

<sup>&</sup>lt;sup>7</sup>Let's restrict attention to noncommuting pairs of operators with no eigenvectors in common.

indefiniteness in t entails an indefiniteness in phase of the time development. This gives rise to an uncertainty relation between the number of quanta and the phase, which we can motivate in the following qualitative way.

Start with the time-energy uncertainty relation,  $\Delta \hat{H} \Delta t \geq 1$ . Because we are considering only states involving quanta with momentum  $\mathbf{k}$ , we can take  $\hat{H} = \omega(\mathbf{k})\hat{N}_{\mathbf{k}}$ , so that  $\Delta \hat{H} = \omega(\mathbf{k})\Delta \hat{N}_{\mathbf{k}}$ . As the phase,  $\phi$ , is  $\omega(\mathbf{k})t$ , we have  $\Delta \phi = \omega(\mathbf{k})\Delta t$ . Together these give  $\Delta \hat{N}_{\mathbf{k}}\Delta \phi \geq 1$ .

This qualitative conclusion indicates that to have a radiative state with coherence (that is, correlations) in its phase relations, there has to be uncertainty in the number of quanta. It turns out that the maximally coherent states are given as

$$\exp\left(-\frac{1}{2}|\alpha|^2\right)\sum_n\frac{\alpha^n}{\sqrt{n!}}|n\rangle.$$

The parameter  $\alpha$  can be any complex number, and the states are eigenstates of the nonselfadjoint  $\hat{a}(\mathbf{k})$ , with eigenvalues  $\alpha$ . These states are particularly useful in describing laser light.

For a second example we look at so-called vacuum fluctuations, a surprising consequence of the inexactness of a field quantity in an exact number state. Consider the vacuum expectation values for  $\hat{\Psi}_R(\mathbf{x},t)$  and  $\hat{\Psi}_R^2(\mathbf{x},t)$ . We have  $\langle 0|\hat{\Psi}_R(\mathbf{x},t)|0\rangle=0$ . For  $\langle 0|\hat{\Psi}_R^2(\mathbf{x},t)|0\rangle$  we have to consider terms proportional to  $\langle 0|\hat{a}(\mathbf{k})\hat{a}(\mathbf{k})|0\rangle$ ,  $\langle 0|\hat{a}^{\dagger}(\mathbf{k})\hat{a}(\mathbf{k})|0\rangle$ , and  $\langle 0|\hat{a}^{\dagger}(\mathbf{k})\hat{a}^{\dagger}(\mathbf{k})|0\rangle$ , all of which are zero because  $\hat{a}(\mathbf{k})|0\rangle=\langle 0|\hat{a}^{\dagger}(\mathbf{k})=0$ ; and  $\langle 0|\hat{a}(\mathbf{k})\hat{a}^{\dagger}(\mathbf{k})|0\rangle$ , which is not zero. In fact, on integration over  $\mathbf{k}$  the last leads to a divergent integral for  $\langle 0|\hat{\Psi}_R^2(\mathbf{x},t)|0\rangle$ . This expectation value is for a quantity at a point, whereas real measurements must take place over some finite volume. This thought motivates defining an average of  $\hat{\Psi}_R^2(\mathbf{x},t)$  over a small volume,  $\hat{\Psi}_{R,\mathrm{Av}}(\mathbf{x},t)=\frac{1}{V}\int_V\hat{\Psi}_R^2(\mathbf{x},t)$ . The expectation value  $\langle 0|\hat{\Psi}_{R,\mathrm{Av}}^2(\mathbf{x},t)|0\rangle$  is finite but still different from zero.

<sup>&</sup>lt;sup>8</sup>In quantum theories generally time is not an operator, so that the time-energy uncertainty relation mixes uncertainty for the value of an operator with indefiniteness in a parameter. In this intuitive sketch I have followed through consistently and arrived at an uncertainty relation for a number operator with the phase not considered an operator. However, one can usefully define a phase operator and arrive at a corresponding number-phase uncertainty relation.

<sup>&</sup>lt;sup>9</sup>The divergence of  $\langle 0|\hat{\Psi}_R^2(\mathbf{x},t)|0\rangle$  is better analogized to the infinities involved in contact forces for perfectly rigid objects in classical mechanics than to the divergences requiring renormalization.

On the face of it, nonzero vacuum expectation values may seem very strange. The vacuum,  $|0\rangle$ , is supposed to be the state devoid of anything, the state with absolutely nothing in it. As such, how can a quantity exhibit a non-null value in  $|0\rangle$ ? For a non-null value to occur, would there not have to be something to have that value?

To see how to get around this difficulty, we need to be as clear as possible about how the difficulty arises. As set out in the last paragraph, the felt conflict gets started because we presuppose that for a property to be exemplified there must be something to exemplify the property. What could it be that exemplifies values for  $\hat{\Psi}_R^2(\mathbf{x},t)$  in the state  $|0\rangle$ ? In the vacuum there are no quanta, so it can't be quanta. Could it be the space-time points themselves? But if the space-time points exemplified some property, we would expect the quantity in question to be quantized, and it seems that after all there would be some quanta around.

Describing the source of the conflict so specifically makes it easier to state an alternative. We should understand the quantum state as specifying propensities for the manifestation of properties, but we can consistently deny that there has to be anything that exemplifies the state.

A more detailed development of this alternative viewpoint turns on the general fact about complementary quantities: When a state is exact for one quantity it is inexact for complementary quantities. That is, one and the same state that specifies a definite value for one quantity will specify no definite value, but only propensities for complementary quantities.

Now apply this general fact to  $|0\rangle$ .  $|0\rangle$  has a definite value for  $\hat{N}$ : It is the state with no quanta. Because  $\hat{N}$  does not commute with either  $\hat{\Psi}_R(\mathbf{x},t)$  or  $\hat{\Psi}_R^2(\mathbf{x},t)$ , in  $|0\rangle$  these quantities have no definite value. This does not mean that for each value of these quantities that value will definitely not occur, but only that in  $|0\rangle$  there is no value that is definitely and so in that sense actually present. Thus in  $|0\rangle$  there are no occurring values of  $\hat{\Psi}_R(\mathbf{x},t)$  or  $\hat{\Psi}_R^2(\mathbf{x},t)$ , values for which one might think that there would have to be quanta to have those values. Finally, the state  $|0\rangle$ , although involving the propensity for values of quantities not commuting with  $\hat{N}$ , is not itself a property of anything. It simply occurs.

This construal resolves the problem.  $|0\rangle$  is the vacuum, a state in which there are no actually occurring entities. But it is also a state in which there are propensities for values of quantities, such as  $\hat{\Psi}_R(\mathbf{x},t)$ 

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and  $\hat{\Psi}_R^2(\mathbf{x},t)$ , which do not commute with  $\hat{N}$ . Therefore in the vacuum there are nonzero probabilities for values of  $\hat{\Psi}_R(\mathbf{x},t)$ —which in retrospect should have been as puzzling as the fact that  $\langle 0|\hat{\Psi}_R^2(\mathbf{x},t)|0\rangle \neq 0$ —and of  $\hat{\Psi}_R^2(\mathbf{x},t)$ . Because the eigenvalues of  $\hat{\Psi}_R^2(\mathbf{x},t)$  are non-negative,  $\langle 0|\hat{\Psi}_R^2(\mathbf{x},t)|0\rangle > 0$ .

My last example is Rindler quanta. Interpreters disagree sharply on how to consider these, and I will argue that they qualify as full-fledged quanta. But I group them here with field phenomena because, as will emerge shortly, the considerations that resolve the dispute about them are exactly the same as those which resolve the felt conflict about vacuum fluctuations.

Up to this point I have tacitly assumed a flat space-time and assumed that all observers move inertially. On these assumptions there is one state,  $|0;M\rangle$ , the Minkowski vacuum, which all observers agree is the vacuum, the state with no quanta. Now consider a particle detector that registers no particles when moving inertially in the state  $|0;M\rangle$ . Suppose that  $|0;M\rangle$  is in fact the state that obtains and that this same detector is in uniformly accelerated motion. Then it will respond as if in a thermal bath of quanta, called  $Rindler\ quanta$ .

This fact has caused consternation in the interpretive literature, which talks about "particles" instead of "quanta" and has not stopped, as we have done, to analyze the particle concept to see which aspect of the concept may be causing trouble. Generally, the argument seems to be that particles are the sort of things that are either there or not there.  $|0;M\rangle$  is supposed to be the vacuum, the state in which no particles occur. In such a state, the literature asks, how could a particle detector detect particles, no matter how it is moving? Some (for example, Davies [1984]) conclude that the whole particle concept has to be junked and retreat to instrumentalism in the style of Bohr.

In preparation for our resolution of this problem we need a qualitative outline of the formal description of the relevant facts. The raising and lowering operators of Minkowski quanta (quanta detected by an inertially moving detector) came from quantizing the positive-frequency solutions of the Klein-Gordon equation. (The negative-frequency solutions gave rise to operators for the antiquanta.) But what counts as a positive-frequency solution can depend on the time parameter used; the space of positive-frequency solutions is Lorentz invariant but not generally covariant. If we characterize solutions with the form of positive-frequency solutions but use the proper time parameter of an accelerating observer,

we get a very different space of positive- and negative-frequency solutions, which in turn, on quantization, gives rise to a different set of raising and lowering operators for quanta.

These Rindler raising and lowering operators are expressible as superpositions of the Minkowski raising and lowering operators, and states with a definite number of Minkowski quanta are superpositions of states with different numbers of Rindler quanta. In particular,  $|0;M\rangle$  is a superposition of Rindler quanta states, including states for arbitrarily large numbers of Rindler quanta. In other words,  $|0;M\rangle$  has an exact value of zero for the Minkowski number operator and is simultaneously highly indefinite for the Rindler number operator.

The formal description exactly parallels that of vacuum fluctuations, and exactly the same resolution of the problem applies. In  $|0;M\rangle$  there is no definite number of Rindler quanta. There is only a propensity for detection of one or another number of Rindler quanta by an accelerating detector. A state in which a quantity has no exact value is one in which no values for that quantity are definitely, and so actually, exemplified. Thus in  $|0;M\rangle$  no Rindler quanta actually occur, so the status of  $|0;M\rangle$  as a state completely devoid of quanta is not impugned.

To be sure, this interpretive state of affairs is surprising. To spell it out one step further, in  $|1,0,0,\ldots\rangle_M$  there is one actual Minkowski quantum, no actual Rindler quanta, and all sorts of propensities for manifestation of Rindler quanta, among other things. In  $|1,0,0,\ldots\rangle_R$  the same comment applies with the role of Minkowski and Rindler quanta reversed. It turns out that there are various kinds of quanta, and a state in which one kind of quantum actually occurs is a state in which there are only propensities for complementary kinds of quanta. Surprising, but perfectly consistent and coherent.

Rindler quanta really provide a special and simple case of a much wider range of cases: The same kinds of considerations that arise for Rindler quanta arise in general curved space-times. In curved spacetime, each world line, including each geodesic, carries with it its own

<sup>&</sup>lt;sup>10</sup>One might worry here and in the parallel case of vacuum fluctuations: In a true vacuum, how can activating (measurement) conditions be in place? This is indeed a serious interpretive problem, but really a problem with the original problems to be resolved, not with the proposed solutions to those problems. In a true vacuum, with no measurement apparatus or other activating conditions, no Rindler quanta or vacuum fluctuations will ever occur. To get the problem going one must make an idealization in which the meaurement apparatus or other activating conditions are somehow excluded as not really being part of the "world" that is in the vacuum state.

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sense of proper time. Corresponding to each of these is an idiosyncratic separation of positive- and negative-frequency solutions of the Klein-Gordon equation. In turn, to each sense of the division between positive- and negative-frequency solutions there corresponds an idiosyncratic kind of raising and lowering operators and associated kind of quanta. A state that constitutes the vacuum for one kind of quanta is a superposition of various numbers of other kinds of quanta. As far as I can see, the kind of comments that ease the conflict between the concept of quanta and the facts about the Rindler quanta apply equally to this more general case.

One may sensibly charge that by this time we have wandered off so far from the original concept of particles that the concept has been given up entirely. We reject exact trajectories and (for those who included it) primitive thisness. We retain discreteness and a high degree of localizability, but strict localization has to be relativized to a hyperplane. Now we find in addition that there are various "kinds" of quanta, each relative to a state of motion of an observer. All inertial observers in flat space-time agree, but not accelerating observers or observers following accelerated trajectories or geodesics in curved space-time. And these various "kinds" of quanta relate to each other as do Minkowski quanta and typical field quantities.

If this counts as completely giving up the particle concept, so be it. The terminology is not important. The important point is that we have traced the lineage from the particle concept, noting similarities and differences along the way, and we see more clearly how the conceptual parts fit together in the new theoretical framework. After that a dispute about the appropriateness of terminology carries little interest.

One should also remember that although Minkowski quanta constitute only a special case, they provide a very important family of theoretical models. Remember the stance announced in chapter 1: Science produces models, similar only in certain respects and to certain degrees to the modeled aspects of the world. For a vast portion of the phenomena in which we are interested, inertial motion in flat space-time constitutes a fabulously close degree of similarity to what is modeled. Insofar, Minkowski quantum field theory provides a wealth of information about what the world is like in respects that are important to us.

At the inception of quantum field theory, Dirac judged that his quantization of the electromagnetic field gave "a complete harmony between the wave and light quantum descriptions" (1927, p. 245). Does our dis-

cussion vindicate Dirac? On my reading, Dirac was referring, roughly speaking, to the dual approaches to quantum field theory presented, respectively, in chapters 3 and 4. The present chapter makes clear that such considerations do not exhaust the "wave" or "field-theoretic" aspects of the theory. If one is willing to identify quantal descriptions with descriptions having exact values in eigenstates of the number operator and wave descriptions with descriptions having exact values in eigenstates of at least some operators that do not commute with the number operator, then we have a precise statement of the so-called complementarity or duality of "wave" and "quantal" descriptions: These descriptions are complementary or dual exactly insofar as the operators in question do not commute, in precisely the way that position and momentum are complementary in conventional quantum mechanics.

It may be better to say, however, that we have transcended more than reconciled "wave" and "particle" conceptions, transcended them in a way that still allows us to look back to classical applications and see why the older and more coarsely drawn wave and particle conceptions there apply so well. My account might also be seen as spelling out the thought behind the word 'wavicle', which seemed for many decades to be just a fanciful description, and what I take to be application of the same idea in Redhead's term "ephemeral" for the quantum-field-theoretic entities that seem in some ways like waves and in other ways like particles. <sup>11</sup> I hope the foregoing will give these older ideas some further substance.

#### PROBLEMS

- 1. Provide a detailed comparison of the ways in which the things represented by operators generalize on the classical idea of a determinable, paying close attention both to the analogies and to the disanalogies.
- 2. According to some interpreters, the special theory of relativity involves a generalization or evolution of prior conceptions of mass, of simultaneity, and perhaps of other concepts. Compare the changes in conceptions of waves and particles induced by quantum theories with changes in concepts induced by the special theory of relativity.

<sup>&</sup>lt;sup>11</sup>I disparaged Redhead's "ephemerals" in Teller (1983, pp. 106-8). How like an author to come around to an idea only after he has put in his two bits on it! Indeed, I spoke disparagingly of "wavicles" in this very book, on page 16.

## Interactions

SO FAR THIS BOOK has described only free quanta, that is, fictitious quanta that do not interact with each other. But there are no such quanta. As physicists like to say, "We can't turn the interaction off."

The problems of free quanta—both technical and conceptual—are child's play in comparison with those of a realistic theory in which interaction has been added to the description. In the remainder of this book I will make only an extremely modest beginning on the conceptual side of this bill of fare. The present chapter will give an intuitive description of how the simplest kind of quantum field theory looks when interaction is added to the theory, a description that will focus on the very fragmentary extent of our understanding and that will bring out the manner in which one of the most pressing conceptual problems—infinite renormalization—arises along the way. The next chapter will discuss renormalization. I want to stress that renormalization is only one of a host of conceptual issues that need to be examined, so that this and the subsequent chapter will at best provide an introduction to the topic and an example of the kind of conceptual clarification that is possible.

The first problem with quantum field theory with interactions ("interacting quantum field theory") is that one can't begin to solve the quantized-field equations with realistic interaction terms added. So physicists apply their stock-in-trade—they approximate. Much of what goes by the name of 'quantum field theory' is the study of the first few terms of a perturbation expansion which impress only because they yield numerical results in stunningly good agreement with experiment.

Self-interaction poses another problem. Consider two similarly charged particles, say two electrons: They repel. Classically we describe this repulsion in terms of a mediating electromagnetic field, which takes on a life of its own, most prominently in the phenomenon of light. In quantum field theory the electromagnetic field is also quantized, and its quanta, photons, play a role in the interaction between electrons.<sup>1</sup>

<sup>&</sup>lt;sup>1</sup>Electrons analogously mediate interaction between photons, a process in quantum field theory with no classical analogue.

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Now let's consider an isolated electron. If there were a second electron somewhere close by, the second electron would be repelled, and we describe this potential for repulsion of a second electron in terms of an electromagnetic field arising in connection with the first electron. But electric fields don't come labeled by the electrons that give rise to them: Given a field, our isolated electron can't "tell" whether that field arose from some other electron or from itself. Therefore, in both classical and quantum theories, a complete description needs to include the interaction between an electron and the field to which that electron itself gave rise. In quantum field theory this problem appears in the form of self-interaction terms in the perturbation expansion, terms that for fifteen years seemed to make nonsense of the theory because they involved divergent integrals. This chapter will conclude with a qualitative sketch of how these terms arise. The next chapter will explain how the difficulty was circumvented through the renormalization program and will examine the various ways in which this program might be understood.

I'll mention one more formidable problem. According to something called *Haag's theorem* there appears to be no known consistent mathematical formalism within which interacting quantum field theory can be expressed. Experts wildly disagree in their attitudes toward Haag's theorem. Barton (1963, p. 157) expresses one extreme when he writes that subject to certain assumptions that are clearly needed for the theory, "the statement popularly known as Haag's theorem" shows "that no field theory exists which differs from that of a free field." At the other extreme, most practitioners simply dismiss the issue. (A great many physicists work with a brazenly cavalier attitude toward mathematical niceties!)

Everyone must agree that as a piece of mathematics Haag's theorem is a valid result that at least appears to call into question the mathematical foundations of interacting quantum field theory, and agree that at the same time the theory has proved astonishingly successful in application to experimental results. What seems less clear is how the assumptions of the theorem should be brought to bear on both the product and the interpretation of the theory. It is possible that in these respects physics is striding ahead of mathematical understanding, as it did in the cases of Dirac's use of "delta functions" and Newton's use of the calculus. It may also be possible that there is something deeply wrong with the theory, in spite of its formidable successes. Or there may be only a less exciting

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difficulty in seeing clearly the use of the assumptions of Haag's theorem in a detailed, consistent development of a very complex theory.

I have no light to throw on these important questions. In this chapter my exposition will proceed along lines almost universally accepted by practitioners of the theory, disregarding Haag's theorem; and at the end I'll have some interpretive comments about the resulting formalism. At one point only will I have a few words more to say about one way of describing the difficulty Haag's theorem presents and a possible response to the difficulty.

#### THE INTERACTION PICTURE

Starting with a description of free quanta, how does one add a description of interactions? One may introduce additional interaction terms in a number of ways, for example, by including them in a Lagrangian, or equivalently by directly including them in field equations for free quanta, such as the Klein-Gordon or Dirac equation. Usually one works on calculational details by using an alternative to the Schrödinger and Heisenberg pictures called the interaction (or Dirac) picture.

By way of preparation, let's use ' $\hat{H}_0$ ' to represent the Hamiltonian of the free theory, the fictitious theory in which all interactions are disregarded. We use ' $\hat{H}_I$ ' to represent the interaction Hamiltonian, that is, the addition representing interactions. Then the full Hamiltonian of the interacting theory is written as:  $\hat{H} = \hat{H}_0 + \hat{H}_I$ .

To summarize the review in chapter 5, in the Schrödinger picture the states evolve and the operators stay put. In the Heisenberg picture the states stay put and the operators evolve according to  $\hat{O}^H(t) = e^{i\hat{H}(t-t_0)}\hat{O}^S \ e^{-i\hat{H}(t-t_0)}$ . Here a superscript 'S' means that the operator or state is represented in the Schrödinger picture, and a superscript 'H' similarly signifies the Heisenberg picture. The interaction picture is a kind of middle ground between these two in which the operators evolve according to the Hamiltonian of the free theory,  $\hat{H}_0^S$ , and, roughly speaking, the states evolve according to the interaction Hamiltonian that is added to the free Hamiltonian. A superscript 'I' signifies the interaction picture. Note carefully that a subscript 'I' signifies the interaction Hamiltonian in one or another picture. Thus ' $\hat{H}_I^S$ ', ' $\hat{H}_I^H$ ', and ' $\hat{H}_I^I$ ' signify the interaction Hamiltonian in the Schrödinger, Heisenberg, and interaction pictures respectively.

In the Schrödinger picture the states evolve according to the unitary operator  $\hat{U}^S(t,t_0): |\Psi^S(t)\rangle = \hat{U}^S(t,t_0)|\Psi^S(t_0)\rangle, \ \hat{U}^S(t,t_0) = e^{-i\hat{H}^S(t-t_0)}$ . To effect the transformation to the interaction picture, we use the fact that for any unitary transformation,  $\hat{V}$ ,  $\hat{I} = \hat{V}^{-1}\hat{V} = \hat{V}^{\dagger}\hat{V}$  so that  $\langle \hat{O} \rangle_{\Psi^S(t)} = \langle \Psi^S(t) | \hat{V} \hat{V}^{\dagger} \hat{O}^S \hat{V} \hat{V}^{\dagger} | \Psi^S(t) \rangle$ . Letting  $\hat{V} = e^{-i\hat{H}_0^S(t-t_0)}$ , we see that the full interacting theory is equivalently formulated by letting

$$\hat{O}^I(t) = e^{i\hat{H}_0^S(t-t_0)} \hat{O}^S e^{-i\hat{H}_0^S(t-t_0)}$$

and

$$|\Psi^I(t)
angle = e^{i\hat{H}_0^S(t-t_0)}|\Psi^S(t)
angle.$$

In the interaction picture  $\hat{O}^{I}(t)$  evolves as if the full Hamiltonian were  $\hat{H}_0^S$ .  $|\Psi^I(t)\rangle$  evolves as follows: First the state evolves from  $|\Psi^I(t_0)\rangle =$  $|\Psi^S(t_0)\rangle$  under the influence of the full Hamiltonian,  $\hat{H}^S = \hat{H}_0^S + \hat{H}_L^S$ to  $|\Psi^S(t)\rangle = e^{-i\hat{H}^S(t-t_0)}|\Psi^S(t_0)\rangle$ . Then the state is "rolled back" with  $\hat{H}_0^S$  operating in reverse to  $|\Psi^I(t)\rangle = e^{i\hat{H}_0^S(t-t_0)}|\Psi^S(t)\rangle$ . Although one casually describes  $|\Psi^I(t)\rangle$  as the state evolving from  $t_0$  under the influence of  $\hat{H}_I^S$  alone,  $|\Psi^I(t)\rangle \neq e^{-i\hat{H}_I^S(t-t_0)}|\Psi^I(t_0)\rangle$ : Because in general  $\hat{H}_I^S$  and  $\hat{H}_0^S$  do not commute,  $e^{i\hat{H}_0^S(t-t_0)}e^{-i(\hat{H}_0^S+\hat{H}_I^S)(t-t_0)}\neq e^{-i\hat{H}_I^S(t-t_0)}$ . The fact that  $\hat{H}_I^S$  and  $\hat{H}_0^S$  do not, in general, commute also results in

a time dependence of  $\hat{H}_{I}^{I}$ , which we indicate by writing  $\hat{H}_{I}^{I}(t)$ .

## THE S-MATRIX OPERATOR EXPANSION

Why use the interaction picture? The problem of the motion of the operators in the free theory is solved: We have the operator-valued solutions of the free Klein-Gordon and Dirac equations. We do not know how to solve for the motion under interaction. But if the interaction is small, we hope to be able to use known perturbation techniques to give approximate solutions as "refinements" on the known free motion. The interaction picture facilitates this maneuver by characterizing the known free motion entirely as motion of the operators, while the perturbative refinement of the overall motion is described as a small refinement on the otherwise stationary state.

This plan leads to the perturbative expansion of the S-matrix. outline we start with the Schrödinger equation for Schrödinger picture states, use this to develop a similar Schrödinger equation for the interaction picture states, and then show how the unitary transformation corresponding to this equation can be expanded in a sequence of approximations.

The Schrödinger equation for Schrödinger picture states is

(6.1) 
$$i\frac{\partial}{\partial t}|\Psi^{S}(t)\rangle = \hat{H}^{S}|\Psi^{S}(t)\rangle.$$

We calculate the time derivative of the interaction picture states thus:

$$(6.2) i \frac{\partial}{\partial t} |\Psi^{I}(t)\rangle = i \frac{\partial}{\partial t} e^{i\hat{H}_{0}^{S}(t-t_{0})} |\Psi^{S}(t)\rangle$$
$$= -\hat{H}_{0}^{S} e^{i\hat{H}_{0}^{S}(t-t_{0})} |\Psi^{S}(t)\rangle + e^{i\hat{H}_{0}^{S}(t-t_{0})} i \frac{\partial}{\partial t} |\Psi^{S}(t)\rangle,$$

and applying (6.1),

$$= -\hat{H}_{0}^{S} e^{i\hat{H}_{0}^{S}(t-t_{0})} |\Psi^{S}(t)\rangle + e^{i\hat{H}_{0}^{S}(t-t_{0})} (\hat{H}_{0}^{S} + \hat{H}_{I}^{S}) |\Psi^{S}(t)\rangle$$

$$= e^{i\hat{H}_{0}^{S}(t-t_{0})} \hat{H}_{I}^{S} e^{-i\hat{H}_{0}^{S}(t-t_{0})} e^{i\hat{H}_{0}^{S}(t-t_{0})} |\Psi^{S}(t)\rangle$$

$$= \hat{H}_{I}^{I}(t) |\Psi^{I}(t)\rangle.$$

The evolution of the interaction picture state,  $|\Psi^I(t)\rangle$ , can also be described in terms of a unitary operator,  $\hat{U}^I(t,t_0)$ :  $|\Psi^I(t)\rangle = \hat{U}^I(t,t_0)|\Psi^I(t_0)\rangle$ . So (6.2) is equivalent to

(6.3) 
$$i\frac{\partial}{\partial t}\hat{U}^I(t,t_0) = \hat{H}_I^I(t)\hat{U}_I(t,t_0)$$

Because from now on we will work exclusively with operators and states in the interaction picture, we may streamline exposition by omitting the superscript 'I'.

The fact that  $\hat{H}_I(t)$  depends on t presents a problem in trying to solve (6.3). (The fact that we require  $\hat{U}(t,t_0)$  to be unitary rules out the trivial  $\hat{U}(t,t_0)=0$  solution to [6.3].) But (6.3) may be formally integrated and the boundary condition  $\hat{U}(t_0,t_0)=\hat{I}$  applied to give

(6.4) 
$$\hat{U}(t,t_0) = \hat{I} - i \int_{t_0}^t dt_1 \hat{H}_I(t_1) \hat{U}(t_1,t_0).$$

The difficulty in finding a closed solution now shows up in the fact that  $\hat{U}(t, t_0)$  appears on both sides of (6.4). But if  $\hat{H}_I(t)$  is in some sense "small," we can hope to use a series of approximations, as follows:

Let's use  $\hat{U}^{(0)}(t,t_0) = \hat{I}$  as a zeroth approximation, or guess, at  $\hat{U}(t,t_0)$ . Substituting  $\hat{U}^{(0)}(t,t_0)$  for  $\hat{U}(t,t_0)$  into (6.4) gives a first approximation,

$$\hat{U}^{(1)}(t,t_0) = \hat{I} - i \int_{t_0}^t dt_1 \hat{H}_I(t_1).$$

Now we can substitute  $\hat{U}^{(1)}(t,t_0)$  for  $\hat{U}(t,t_0)$  into (6.4), giving

$$\hat{U}^{(2)}(t,t_0) = \hat{I} - i \int_{t_0}^t dt_1 \hat{H}_I(t_1) \left[ \hat{I} - i \int_{t_0}^{t_1} dt_2 \hat{H}_I(t_2) \right] 
= \hat{I} - i \int_{t_0}^t dt_1 \hat{H}_I(t_1) + (-i)^2 \int_{t_0}^t dt_1 \int_{t_0}^{t_1} dt_2 \hat{H}_I(t_1) \hat{H}_I(t_2).$$

Continuing in this way we get the expansion

(6.5) 
$$\hat{U}(t,t_0) = \sum_{n=0}^{\infty} (-i)^n \int_{t_0}^t dt_1 \cdots \int_{t_0}^{t_{n-1}} dt_n \hat{H}_I(t_1) \cdots \hat{H}_I(t_n).$$

If it did not lead to some useful results, the expansion (6.5) might look like wishful thinking.<sup>2</sup> When  $\hat{H}_I(t)$  is not "small," as in older theories of strong interactions, there is no hope of making constructive use of (6.5). When  $\hat{H}_I(t)$  is "small," as in quantum electrodynamics (QED) and at short distances in quantum chromodynamics (QCD), there is still the question of whether the expansion converges, and if so whether it converges to the right limit. As far as I know no one knows the answers to these questions.<sup>3</sup> All we really have to go on is that clever use of (6.5) leads to numbers in remarkable agreement with experiment.

Equation (6.5) is used to describe the scattering of quanta, in which quanta are thought of as coming in from the distant past, interacting, and thus resulting in a superimposed state with amplitudes for observable quanta in the distant future. Idealizing the distant past and distant future respectively as  $t_0 \to -\infty$  and  $t \to +\infty$ , we use (6.5) to define the S-matrix operator ('S' for 'scattering'):<sup>4</sup>

$$(6.6) \hat{S} = \lim_{\substack{t \to +\infty \\ t_0 \to -\infty}} \hat{U}(t, t_0)$$

<sup>2</sup>This perhaps overstates the case, because there is a formal connection between the S-matrix operator expansion to which (6.5) immediately leads and standard time-dependent perturbation theory of conventional quantum mechanics. See Sakurai (1967), pp. 184–85 for an outline.

<sup>3</sup>Dyson (1952) gave a plausibility argument that the S-matrix operator expansion is asymptotic in the coupling constant, which describes the strength of the interaction. This means that for a fixed value of the coupling constant the series may not converge. But as the coupling constant goes to zero, increasingly long initial segments of the expansion converge to the right limit. This would justify use of initial terms in the expansion. However, Dyson's argument does not seem to be regarded as conclusive.

 $^4\hat{S}$  is an operator, but owing to interest in its matrix elements in a basis of  $|in\rangle$  and  $|out\rangle$  states (see below), usually it is called the 'S-matrix'. I defer to this terminological practice, while recognizing that (6.6) defines an operator, by using the expression 'S-matrix operator'.

$$= \sum_{n=0}^{\infty} (-i)^n \int_{-\infty}^{+\infty} dt_1 \int_{-\infty}^{t_1} dt_2 \cdots \int_{-\infty}^{t_{n-1}} dt_n \hat{H}_I(t_1) \cdots \hat{H}_I(t_n).$$

With a purely formal manipulation, this can be rewritten in the form

$$(6.7) \quad \hat{S} = \sum_{n=0}^{\infty} \frac{(-i)^n}{n!} \int_{-\infty}^{+\infty} dt_1 \cdots \int_{-\infty}^{+\infty} dt_n T[\hat{H}_I(t_1) \cdots \hat{H}_I(t_n)],$$

where  $T[\hat{H}_I(t_1)\cdots\hat{H}_I(t_n)]$  means that the operators  $\hat{H}_I(t_i)$  are ordered so that their time indexes are decreasing, left to right.<sup>5</sup> Finally, to facilitate putting the theory in a manifestly covariant form, we write the interaction Hamiltonian,  $\hat{H}_I(t)$ , in terms of an interaction Hamiltonian density:  $\hat{H}_I(t) = \int d^3\mathbf{x} \hat{\mathcal{H}}_I(\mathbf{x},t)$ . Using variables, x, for vectors in four-dimensional space-time, we can finally write  $\hat{S}$  as

(6.8) 
$$\hat{S} = \sum_{n=0}^{\infty} \frac{(-i)^n}{n!} \int_{-\infty}^{+\infty} d^4x_1 \cdots \int_{-\infty}^{+\infty} d^4x_n T[\hat{\mathcal{H}}_I(x_1) \cdots \hat{\mathcal{H}}_I(x_n)].$$

## |in | AND |out | STATES

To use  $\hat{S}$  to describe scattering we start with an in-state,  $|in\rangle$ , ordinarily an (idealized) pure momentum state of a quantum beginning in the "infinite" past.  $\hat{S}|in\rangle$  is then the state that results in the infinite future. In general,  $\hat{S}|in\rangle$  will be a superposition of all kinds of quanta, while we are interested in the probability amplitudes for specific experimental results. For example, if we start with an  $|in\rangle$  describing an electron and a photon of given momenta, we may be interested in the probability amplitude for a resulting out-state,  $|out\rangle$ , describing, say, an emerging electron and photon with given new momenta in the "infinite" future.

Discussion of the in- and out-states provides an occasion for a very brief description of S-matrix theory and its relation (or lack of it) to quantum field theory. Quantum field theory postulates states in a Hilbert space with operators indexed for all space-time points intervening between in- and out-states. That theory then tries to draw out

<sup>&</sup>lt;sup>5</sup>This manipulation requires that  $\hat{H}_I(t_i)$  have an even number of Fermion operators so that rearranging the terms will not make a difference in sign.

an S-matrix operator from this more detailed space-time description, as explained in the last section. S-matrix theory skips all this intervening paraphernalia. Instead it simply postulates a unitary operator,  $\hat{S}$ , which is supposed to describe which in-states evolve into which out-states. If  $|i;in\rangle$  is an initial in-state,  $\hat{S}|i;in\rangle$  is the out-state into which  $|i;in\rangle$  will develop. If  $|f;out\rangle$  is some final out-state of experimental interest,  $S_{if} = \langle f;out|\hat{S}|i;in\rangle$  is the overlap, that is, the probability amplitude for  $|f;out\rangle$  resulting from  $|i;in\rangle$ . S-matrix theory postulates certain general properties of the matrix elements  $S_{if}$  and never dirties its hands with the quantum field operators  $\hat{\Psi}(\mathbf{x},t)$  or the intervening states on which they act. Historically, advocates of what was called the "autonomous S-matrix program" held that descriptions of such intervening quantities are without "physical meaning."

It is also worth mentioning that starting from the S-matrix operator, one can define an interpolating quantum field operator (Barton [1963], pp. 38-43), that is, a collection of space-time indexed operators of the same form given in the quantum-field-theoretic approach and yielding the same S-matrix operator. Does this fact indicate that the S-matrix and field-theoretic points of view are somehow equivalent? Such a claim is highly controversial and would most likely be hotly contended by partisans on both sides. On the one hand S-matrix theorists would appeal to the fact that an S-matrix operator highly underdetermines the corresponding interpolating field. They would then claim that all experimental results can be expressed in terms of the scattering amplitudes, that is, the elements of the S-matrix, and conclude that there really are no space-time facts intervening between the in- and out-states, over and above the information contained in the S-matrix operator. Field theorists, with a more robust attitude toward an intervening space-time description, on the other hand, would dispute the claim that all the experimental facts can be expressed in terms of S-matrix elements. They might also claim that considerations such as simplicity, symmetry, and other plausibility considerations can be reasonably used to narrow the

<sup>&</sup>lt;sup>6</sup>Keep in mind that this description is in the interaction picture with time-dependent states. In the literature one sees the same statement expressed in the Heisenberg picture with time-independent states. If  $|i,in\rangle_H$  is the time-independent Heisenberg picture preparation state and  $|f,out\rangle_H$  is the Heisenberg picture final state of experimental interest,  $S_{ij} = {}_H\langle f;out|i;in\rangle_H$ .

<sup>&</sup>lt;sup>7</sup>See Cushing (1990), p. 172, and chapter 7 passim. It also bears mentioning that the S-matrix theory of the 1960s appealed to properties that seemed promising for strong interaction phenomena but not at all promising for electromagnetic and weak interactions.

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ambiguity in the interpolating field by narrowing the choice among experimentally adequate interaction Hamiltonians.<sup>8</sup>

To return to field theory and its in- and out-states, these in fact involve a complication. In conventional developments of the theory, one begins with the assumption that the momentum eigenstates for the free theory provide a complete basis. But even insofar as a quantum is completely unlocalized, it cannot be described as a momentum eigenstate of the free theory. As briefly described earlier, even when we may neglect interaction with other real quanta, the full interacting theory describes the quantum as self-interacting, so that a momentum eigenstate of a real, or so-called *dressed*, quantum is actually a superposition of momentum eigenstates of the free theory. The complication is addressed as part of the renormalization program in what is called wave-function renormalization.<sup>9</sup> Because the next chapter will examine the general conceptual issues of renormalization, I will not specifically discuss any of the technical aspects of wave-function renormalization.

However, this discussion of dressed in- and out-states does provide an opportunity to say a little more about the difficulties raised by Haag's theorem. One can consistently assume a unitary S-matrix operator describing how a given dressed in-state in the infinite past will evolve into a superposition of dressed out-states in the infinite future. But Haag's theorem tells us that there is no unitary transformation from a dressed in-state to a superposition of dressed states of quanta at finite times. This completely spoils the view of quantum field theory as giving us a picture, however abstract, of how states of quanta evolve between preparation and measurement, which was supposed to be an attraction of quantum field theory over S-matrix theory. Indeed, in view of this formal fact, it would seem that we have no consistent theory of the dynamical evolution of quanta between preparation and measurement.

The difficulty can be put more formally in terms of the last section's operator,  $\hat{U}$ . There is no difficulty with the  $\hat{U}(t,t_0)$  of (6.5), for finite times, t and  $t_0$ . But Haag's theorem tells us that if we take one of the limits, say  $t_0$   $t_0 - \infty$  of (6.6), the resulting operator,  $\hat{U}(t, -\infty)$ , is no

<sup>&</sup>lt;sup>8</sup>Only some S-matrix practitioners of the 1960s maintained the interpretive stance described in the last two paragraphs. Many practitioners considered the approach a phenomenological heuristic, to be used pending a more satisfactory theoretical approach.

<sup>&</sup>lt;sup>9</sup>Wave-function renormalization applies straightforwardly only to free, asymptotic states. The situation for bound states is more complicated.

longer unitary, indeed is mathematically very badly behaved. Common practice assumes unproblematic limiting behavior of unitary time evolution operators, but it turns out that this assumption is inconsistent with other assumptions of the theory.

One is tempted here simply to abandon the infinite time limits. After all, actual preparations and measurements take place at finite times—passage to  $t=\pm\infty$  is an idealization. But without passing to the limits there is no known way of treating wave-function renormalization, the "dressing" of the noninteracting states, to take account of the self-interaction of quanta not otherwise interacting with other quanta. Much of the literature cites an argument due to Feynman (1949) and Dyson (1949, 1951), which offers a description according to which the interaction is gradually "turned off" as one passes to the infinite past and future limits. <sup>10</sup>

Another method, known as the LSZ approach, 11 takes the in- and outstates to be dressed and connects them to field operators at finite times by a condition called weak convergence, which allows separate convergence of matrix elements, instead of strong convergence, which would require convergence of the operators taken as a whole. This account does not run afoul of Haag's theorem but comes at a significant interpretive cost. The raising and lowering operators in terms of which one defines the field operators at finite times do not satisfy the canonical commutation relations,  $[\hat{a}(\mathbf{k},t),\hat{a}^{\dagger}(\mathbf{k}',t)] \neq \delta^{3}(\mathbf{k}-\mathbf{k}')$ . As a result, the familiar operator  $\hat{N} = \int d^3 \mathbf{k} \hat{a}^{\dagger}(\mathbf{k}, t) \hat{a}(\mathbf{k}, t)$ , which used to have the form of a number operator, no longer bears a number operator interpretation for finite t. Consequently, the LSZ option severely restricts the way in which the theory of dressed states can be viewed as a theory of "particles," even in the already extenuated sense of "quanta," for such an interpretation will now apply only to the dressed in- and out-states. On the LSZ approach the "bare" states at finite times still bear an interpretation as quanta, but these have a zero overlap with the dressed states.

The LSZ approach restricts the dressed states, the ones interpretable as real quanta, to the infinite past and future, thereby significantly limiting the interpretation of the theory in terms of quanta. But the restriction does not ruin the interpretation on the larger stance I have taken

<sup>&</sup>lt;sup>10</sup>I have seen evaluations of this argument which range from "careful" (Sakurai, 1967, p. 285) to "hand waving" (sources speaking on condition of anonymity).

<sup>&</sup>lt;sup>11</sup>See Schweber (1961), pp. 742 ff. for a summary and references.

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toward the nature of interpretation in general and the interpretation of quantum states in particular. I take quantum states to be features of the world involving propensities for a variety of manifestations. The quantal aspects of such states reside in their propensities for manifesting quantal phenomena, specifically discreteness and localizability understood as manifestation at highly localized places. These manifestations occur (at least) on the occasion of the right kind of events that we count as preparations and measurements. The present theory can recognize such events described only in the idealization of occurring in the "infinite past" and "infinite future." In turn, we regard this idealization as one of the ways in which the interpretation's similarity relation between the model and the world is only approximate.

These few words on the ramifications of Haag's theorem do not do the slightest justice to this important topic. But the subject commands no consensus, and I return to my exposition of common practice, which simply puts the whole issue aside.

## THE INTERACTION HAMILTONIAN

To use the S-matrix operator expansion, (6.8), one needs to fill in an explicit form for the interaction Hamiltonian density,  $\hat{\mathcal{H}}_I(x)$ , which in turn will depend on the specific theory in question. I will illustrate with a very brief and simplified outline of the situation for quantum electrodynamics, sweeping under the carpet a wealth of technical details that would only obscure the very general conceptual picture that I am presenting here.

QED describes the interaction of electrons and positrons—the quanta of the quantized Dirac equation—with photons—the quanta of the quantized electromagnetic field. So we expect  $\hat{\mathcal{H}}_I(x)$  to be constructed out of the quantized electromagnetic field operators,  $\hat{A}(x)$ , and the quantized solution to the Dirac equation,  $\hat{\Psi}(x)$ . As before, 'x' here occurs as a four-component space-time variable. (For the remainder of the chapter I will use this four-vector notation conventionally used in the context of manifest covariance instead of the '(x, t)' notation used in chapter 4.)  $\hat{\Psi}(x)$  is like (4.48) in including lowering operators of the quanta—electrons—and raising operators for the antiquanta—positrons. As mentioned in chapter 4, the adjoint of the Dirac equation is distinct from the Dirac

equation, so a complete description can be expected also to use solutions to the adjoint equation,  $\hat{\Psi}^{12}$ 

The job of picking an  $\hat{\mathcal{H}}_I$  is simplified by imposing various invariance conditions, such as Lorentz and gauge invariance. In the face of these conditions the simplest candidate for  $\hat{\mathcal{H}}_I$  results simply by multiplying the three relevant field operators, together with a "coupling constant," -e, which measures the strength of the interaction:

(6.9) 
$$\hat{\mathcal{H}}_I(x) = -e\hat{\overline{\Psi}}(x)\hat{A}(x)\hat{\Psi}(x)^{.13}$$

The choice (6.9) is ultimately justified by the fact that it produces the right numbers. It can also be motivated by an analogical argument from the description of the electromagnetic interaction in a Lagrangian formulation of classical theory.<sup>14</sup>

A much cruder plausibility argument for (6.9) goes like this: For classical fields the simplest plausible assumption is that an interaction between fields should be proportional to the strength of each of the fields involved. As I have been at pains to emphasize in the last chapter, after quantization the field operators are not themselves properly thought of as values of some field quantity. Instead, they are more like determinables. But then multiplying the field operators might be hoped to yield a "determinable," the values of which would correspond to the strength of field interaction in any specific state of the interacting fields. Therefore simply multiplying the field operators is a plausible guess at a Hamiltonian for the field interaction energy.

This detail will not concern us.

<sup>13</sup>I must briefly record the enormous simplifications in (6.9).  $\hat{\Psi}(x)$  and  $\bar{\Psi}(x)$  actually have four separate components in a spin space and  $\hat{\mathcal{H}}_I = -e\bar{\Psi}(x)\gamma^\mu\hat{\Psi}(x)\hat{A}_\mu(x)$ . The  $\gamma^\mu$  are each  $4\times 4$  matrices in the spin space, and each  $\hat{A}_\mu$  is an operator on the photon Fock space. Because  $\hat{\Psi}(x) = \hat{\Psi}^\dagger \gamma^0$  and  $(\gamma^0)^2 = \hat{I}$ , by ignoring the difference between  $\hat{\Psi}(x)$  and  $\hat{\Psi}^\dagger(x)$  and omitting the  $\mu$  indices, I am in effect looking at only the first of four terms in  $\hat{\Psi}(x)\gamma^\mu\hat{\Psi}(x)\hat{A}_\mu(x)$  and treating the raising and lowering operators as satisfying commutator instead of anticommutator relations. The omitted details are essential for doing calculations, but they do not matter for getting the most basic ideas across. Finally, (6.9) is not yet normal-ordered. I will discuss this important correction later.

<sup>&</sup>lt;sup>14</sup>This is the stance usually taken in texts toward so-called *minimal coupling* in QED, though often the motivation is not made very clear. A particularly clear statement of the form of the argument, emphasizing its analogical character, is given by Ziman (1969), pp. 200–1.

#### NORMAL ORDERING

To work in detail with  $\hat{\mathcal{H}}_I(x)$  we will need to break up each of its field operators into its positive- and negative-frequency parts similar to (4.24), where we split a first-quantized wave function into two such parts. After second quantization, the negative-frequency / negative-energy term is reinterpreted in terms of lowering and raising operators for positiveenergy antiquanta, as explained in chapter 4. After this reinterpretation, for both quanta and antiquanta a positive-frequency term involves a lowering operator, and a negative-frequency terms involves a raising operator ([4.48] and [4.49]). This notation is counterintuitive and hard for newcomers to the subject, so I will introduce an obvious mnemonic notation: 'PhL(x)' for the photon lowering operator, 'ER(x)' for the electron raising operator,  $\widehat{PL}$  for the positron lowering operator, and so on. Each of these operators is a Fourier transform of lowering or raising operators for pure momentum states and as such is parameterized by the space-time variable, 'x'. But for reasons explained at the end of chapter 4, as well as further reasons reviewed later, it is very misleading to think of these operators as representing the actual creation and annihilation of quanta at specific space-time points.

Classically, configurations of the electromagnetic field are real valued. Consequently after field quantization, the photon field operators are self-adjoint, with the field raising and lowering operators being each other's adjoint. The result is that, unlike the solution to the Dirac equation, we do not get two "fields," one for quanta and one for their antiquanta. One summarizes this situation by saying that "photons are their own antiquanta." The electromagnetic field operator,  $\hat{A}(x)$ , involves just one space-time parameterized lowering operator and just one such raising operator:

(6.10) 
$$\hat{A}(x) = \hat{A}^{+}(x) + \hat{A}^{-}(x)$$

$$=_{\text{notational variant }} \widehat{PhL}(x) + \widehat{PhR}(x).$$

Turning to the field operators for electrons and positrons, for the kinds of reasons discussed in connection with (4.48),  $\hat{\Psi}(x)$  contains an electron lowering operator and a positron raising operator.  $\hat{\overline{\Psi}}(x)$  reverses these:

$$(6.11) \qquad \hat{\Psi}(x) = \hat{\Psi}^{+}(x) \text{ (electron lowering operator)} \\ + \hat{\Psi}^{-}(x) \text{ (positron raising operator)} \\ = \underset{\text{notational variant}}{\widehat{EL}(x) + \widehat{PR}(x)}$$

(6.12) 
$$\widehat{\overline{\Psi}}(x) = \widehat{\overline{\Psi}}^+(x) \text{ (positron lowering operator)}$$

$$+ \widehat{\overline{\Psi}}^-(x) \text{ (electron raising operator)}$$

$$= \underset{\text{notational variant } \widehat{PL}(x) + \widehat{ER}(x).$$

The expression (6.9) for  $\hat{\mathcal{H}}_I(x)$  is not yet quite correct. Let's focus on part of (6.9),  $e\widehat{\Psi}(x)\hat{\Psi}(x)$ . This is interpretable as the charge-density operator.<sup>15</sup> When there are no quanta present, we expect the charge density to be zero, that is, we expect a zero-vacuum expectation value,  $\langle 0|\widehat{\Psi}(x)\hat{\Psi}(x)|0\rangle=0$ .

It is important to appreciate that we do not expect  $\langle 0|\hat{\Psi}(x)\hat{\Psi}(x)|0\rangle$  = 0 simply on the mistaken ground that all quantities should have a zero vacuum expectation value. Indeed, in chapter 5, we already saw an example of a nonzero vacuum expectation. Recall that  $\langle 0|\hat{\Psi}_R^2(\mathbf{x},t)|0\rangle\neq 0$ . This is not a conclusion we want to give up. Because  $\hat{\Psi}_R(\mathbf{x},t)$  does not commute with  $\hat{N}$ , we expect that  $\hat{\Psi}_R(\mathbf{x},t)$  will not have an exact value in the exact number state  $|0\rangle$ , so that its absolute value, or square, will have a positive expectation in the vacuum.

The product  $\widehat{\overline{\Psi}}(\mathbf{x},t)\widehat{\Psi}(\mathbf{x},t)$  is different. As the charge-density operator, it commutes with the operator for total charge.<sup>16</sup> We expect it to have an exact value in an exact number state, and in particular we expect  $\langle 0|\widehat{\overline{\Psi}}(\mathbf{x},t)\widehat{\Psi}(\mathbf{x},t)|0\rangle=0$ . But when we write out the expression in terms of positive- and negative-frequency parts, we see that the vacuum expectation is not zero:

$$(6.13) \qquad \langle 0|\widehat{\overline{\Psi}}(x)\widehat{\Psi}(x)|0\rangle$$

$$= \langle 0|\widehat{PL}(x)+\widehat{ER}(x)]\widehat{[EL}(x)+\widehat{PR}(x)]|0\rangle$$

$$= \langle 0|\widehat{PL}(x)\widehat{EL}(x)|0\rangle + \langle 0|\widehat{PL}(x)\widehat{PR}(x)|0\rangle$$

$$+\langle 0|\widehat{ER}(x)\widehat{EL}(x)|0\rangle + \langle 0|\widehat{ER}(x)\widehat{PR}(x)|0\rangle.$$

In evaluating this expression we use the simple fact that for any raising and lowering operators,  $\hat{a}^{\dagger}$  and  $\hat{a}$ ,  $\hat{a}|0\rangle = \langle 0|\hat{a}^{\dagger} = 0$ . This imme-

<sup>&</sup>lt;sup>15</sup>When we undo the simplifications described in n. 13, it will be the charge-current density operator.

 $<sup>^{16}\</sup>hat{Q} = -e \int d^3\tilde{\mathbf{k}}[\hat{a}^{\dagger}(\mathbf{k})\hat{a}(\mathbf{k}) - \hat{d}^{\dagger}(\mathbf{k})\hat{d}(\mathbf{k})], \ \hat{a}^{\dagger}(\mathbf{k})$  the electron raising operator,  $\hat{d}^{\dagger}(\mathbf{k})$  the positron raising operator. Readers who try to check this claim will find that their check will work only by using commutators instead of anticommutator relations for the raising and lowering operators. This is because the expository simplifications, described in n. 13, drop out all of the 4-component structure in the description of electrons and positrons, and it is their 4-component structure that imposes the anticommutator relations.

diately shows that all but the second term in (6.13) are zero. But  $\langle 0|\widehat{PL}(x)\widehat{PR}(x)|0\rangle$  is not zero. What is to be done?

We somehow have to adjust  $\widehat{\Psi}(x)\widehat{\Psi}(x)$  so that the physical quantity it represents has a zero vacuum expectation value. The brute force way of accomplishing this is just to redefine the quantity with the vacuum expectation value subtracted:

(6.14) 
$$\hat{\overline{\Psi}}(x)\hat{\Psi}(x) \rightarrow N[\hat{\overline{\Psi}}(x)\hat{\Psi}(x)]$$

$$=_{\text{def}} \hat{\overline{\Psi}}(x)\hat{\Psi}(x) - \langle 0|\widehat{PL}(x)\widehat{PR}(x)|0\rangle.$$

The normal ordered product,  $N[\hat{\overline{\Psi}}(x)\hat{\Psi}(x)]$ , can be elegantly simplified by using the fact that

$$\begin{split} \langle 0 | \widehat{PL}(x) \widehat{PR}(x) | 0 \rangle &= \\ &= \langle 0 | \widehat{PL}(x) \widehat{PR}(x) | 0 \rangle + \langle 0 | \widehat{PR}(x) \widehat{PL}(x) | 0 \rangle \\ &\quad \text{(because } \widehat{PL}(x) | 0 \rangle = 0) \\ &= \langle 0 | [\widehat{PL}(x), \widehat{PR}(x)]_{+} | 0 \rangle \\ &= [\widehat{PL}(x), \widehat{PR}(x)]_{+} \text{ (because an anticommutator} \\ &\quad \text{of Fermion fields is just a c-number)} \\ &= \widehat{PL}(x) \widehat{PR}(x) + \widehat{PR}(x) \widehat{PL}(x). \end{split}$$

In other words, subtracting the vacuum expectation value comes down to subtracting

 $\widehat{PL}(x)\widehat{PR}(x) + \widehat{PR}(x)\widehat{PL}(x)$ .

Substituting this into the definition (6.14), and multiplying out the sums of positive- and negative-frequency terms (see [6.11] and [6.12]), gives

$$N[\widehat{\overline{\Psi}}(x)\widehat{\Psi}(x)] = \widehat{PL}(x)\widehat{EL}(x) - \widehat{PR}(x)\widehat{PL}(x) + \widehat{ER}(x)\widehat{EL}(x) + \widehat{ER}(x)\widehat{PR}(x).$$

The normal ordered product,  $N[\widehat{\Psi}(x)\widehat{\Psi}(x)]$ , differs from  $\widehat{\Psi}(x)\widehat{\Psi}(x)$  in the following way: In the term of  $\widehat{\Psi}(x)\widehat{\Psi}(x)$  in which a raising operator stands to the right of a lowering operator we have reversed the order, so that the lowering operator stands to the right, and we have changed the sign. The 'N' stands for 'normal order': The normal ordering of an operator,  $\hat{O}$ , results from  $\hat{O}$  by rewriting with lowering operators to the right of raising operators. More exactly, if  $\hat{O}_1\hat{O}_2$  occurs in  $\hat{O}$ ,  $\hat{O}_1$  a

Fermion lowering and  $\hat{O}_2$  a Fermion raising operator and  $[\hat{O}_1, \hat{O}_2]_+ \neq 0$ , replace  $\hat{O}_1\hat{O}_2$  by  $-\hat{O}_2\hat{O}_1$ . If  $\hat{O}_1$  and  $\hat{O}_2$  are, respectively, Boson lowering and raising operators and  $[\hat{O}_1, \hat{O}_2]_- \neq 0$ , replace  $\hat{O}_1\hat{O}_2$  by  $+\hat{O}_2\hat{O}_1$ .

The simple illustrative argument I have given applies quite generally to show that the effect of normal ordering is exactly to subtract any nonzero vacuum expectation value. In outline, the general argument observes that whenever a lowering operator,  $\hat{a}$ , stands right most in  $\langle 0|\cdots \hat{a}|0\rangle$ , the whole is zero because  $\hat{a}|0\rangle=0$ , and if there are no lowering operators, such a vacuum expectation value must have the form  $\langle 0|\hat{a}^\dagger\cdots|0\rangle$ , and  $\langle 0|\hat{a}^\dagger=0$ . Thus in those cases in which it is appropriate simply to impose a zero vacuum expectation value, this is done straightforwardly by normal ordering. In a moment I will discuss which cases these are. In particular, normal ordering needs to be applied to  $\hat{\mathcal{H}}_I$  as defined in (6.9) to get a satisfactory interaction Hamiltonian density.

This whole discussion of normal ordering seems quite sensible, except for one detail I have deliberately postponed mentioning. The quantity subtracted by normal ordering, the vacuum expectation value  $\langle 0|\hat{\overline{\Psi}}(x)\hat{\Psi}(x)|0\rangle$ , is infinite! The effect of normal ordering is to "discard" an infinite quantity, and mathematically that should make no sense.

The infinite vacuum expectation values discarded by normal ordering are bound to strike one as problematic in much the same way as the infinite values discarded in renormalization, to be discussed in the next chapter. In both cases it looks as if one is subtracting an infinite quantity, leaving behind a finite remainder that is somehow supposed to make physical sense. It must seem odd that renormalization has occasioned much conceptual dismay among physicists and interpreters, while the prima facie similar problem connected with normal ordering does not seem to bother people. Why the difference in attitude?

In this exposition the need for normal ordering—a nonzero, and in fact infinite, vacuum expectation value—arose in the course of a discussion introducing the interaction Hamiltonian density,  $\hat{\mathcal{H}}_I$ . But the argument guiding us toward a choice for  $\hat{\mathcal{H}}_I$  was in any case loose and analogical, constituting no more than an educated guess. The infinite vacuum expectation problem just shows that our initial guess was wrong. Subtracting an infinite vacuum expectation value constitutes nothing worse than guessing again, resulting in a guess shown to be sound by agreement with experiment. In the discussion no violence is done to logic, because the argument was merely analogical, not logical, to begin with.

One sometimes hears physicists make what I take to be the same

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point in the following way: In setting up a quantum field theory we are guided by generalizing on classical theories. But in a classical theory the order of factors makes no difference—at most the order we are used to is a matter of tradition. The order of the operators, which generalize on classical c-numbers, does make a difference. We must try out the various orders until we get the right one.

All of this is very different from the situation with the infinities discarded in renormalization. In the present discussion of normal ordering we are concerned only with analogical guessing. As I said, no violence to logic is done, because logic was not in question to begin with. But historically the need for infinite renormalization arose after practitioners thought that the theory and approximation scheme had been set up, at a point at which they thought they could start calculating, using the exact laws of mathematics. If, as appears to be the case, at this point one must use mathematically illegitimate tricks, concern is an appropriate response.

These considerations appear equally in another application about which one usually hears in elementary expositions, the "discarded zero-point energy." Look at equation (4.32):

$$\hat{H}_{\mathbf{k}} = \omega(\mathbf{k})[\hat{a}^{\dagger}(\mathbf{k},t)\hat{a}(\mathbf{k},t) + \frac{1}{2}\delta(0)].$$

We were quantizing a classical field by analyzing it in terms of Fourier components and then applying the strategy of quantizing the amplitude of each component as if it were a harmonic oscillator. As (4.32) shows, following this strategy through leaves each  $\mathbf{k}^{\text{th}}$  mode with a zero-point energy of  $\frac{1}{2}\delta(0)$  in the no-quantum, or vacuum, state. Customarily one excuses the  $\delta(0)$  by reverting to box normalization in the presence of which the factor of  $\delta(0)$  does not occur. One then argues that only energy differences ultimately matter, so that we may neglect the constant term of 1/2, leaving us with  $\hat{H}_{\mathbf{k}} = \omega(\mathbf{k})\hat{a}^{\dagger}\hat{a}$ . But the full quantized field involves modes for all values of  $\mathbf{k}$ , so that we must integrate over  $\mathbf{k}$ , and after integration we see that we are again discarding an infinite quantity.

All of this is a special case of the discussion of normal ordering. A glance at the line just before (4.32),

$$\hat{H}_{\mathbf{k}} = rac{1}{2}\omega(\mathbf{k})[\hat{a}^{\dagger}(\mathbf{k},t)\hat{a}(\mathbf{k},t) + \hat{a}(\mathbf{k},t)\hat{a}^{\dagger}(\mathbf{k},t)],$$

shows that by normal ordering  $\hat{H}_{\mathbf{k}}$  one gets exactly the same result as by dropping the zero-point energy. And again, because at the relevant

point of the discussion we are setting up the theory, not calculating with a theory already fully described, no violence has been done to logic or familiar mathematics.

When does one apply the prescription to normal order? In setting up a theory one is guided by information, often heuristic and qualitative, about the physics to be described. Normal ordering amounts to a choice of operator ordering that has the effect of ensuring zero vacuum expectation values. Thus it applies in the process of initially choosing the form of a specific theory or model. Once the choice is made, one calculates with the theory, living with whatever operator ordering arises. Only in this way does the theory display the consequences of complementarity of quantities such as the vacuum "fluctuation" phenomenon illustrated with  $\hat{\Psi}_R(\mathbf{x},t)$  in chapter 5.

### EVALUATING SCATTERING AMPLITUDES: FEYNMAN DIAGRAMS

One gets mileage out of the S-matrix operator expansion by using several very simple calculational tricks. We have already made use of the fact that for any raising and lowering operators,  $\hat{a}^{\dagger}$  and  $\hat{a}$ ,  $\hat{a}|0\rangle = \langle 0|\hat{a}^{\dagger} = 0$ . Next, consider vacuum expectation values, that is, the expectation value for some raising and lowering operators,  $\langle 0|\cdots \hat{a}_i\cdots \hat{a}_j^{\dagger}|0\rangle$ . Such expressions must be zero unless for each kind of quanta there are exactly the same number of raising and lowering operators. For unless such a vacuum expectation value is zero for other reasons, it will reduce to  $c\langle n_1|n_2\rangle$  for some coefficient, c. If  $n_1$  and  $n_2$  give different numbers of quanta of the same kind,  $\langle n_1|n_2\rangle = 0$  because states with different exact numbers of the same kind of quanta are orthogonal.

Finally, consider any vacuum expectation of a collection of raising and lowering operators,  $\langle 0|\cdots \hat{a}_i^{\dagger}\hat{a}_i\cdots |0\rangle$ . Applying commutation relations

$$[\hat{a}_i, \hat{a}_j^{\dagger}]_{\pm} = c,$$

we have

$$\hat{a}_i\hat{a}_j^{\dagger}=c\mp\hat{a}_j^{\dagger}\hat{a}_i,$$

which in turn applies to simplify vacuum expectations:

(6.15) 
$$\langle 0|\cdots \hat{a}_i \hat{a}_j^{\dagger} \cdots |0\rangle = c\langle 0|\cdots |0\rangle \mp \langle 0|\cdots \hat{a}_j^{\dagger} \hat{a}_i \cdots |0\rangle.$$

The point of this exercise is that we can reapply it to  $\langle 0|\cdots \hat{a}_{j}^{\dagger}\hat{a}_{i}\cdots|0\rangle$ , and repeating as often as necessary, finally walk the lowering operator

 $\hat{a}_i$  all the way to the right where  $\hat{a}_i|0\rangle$  gives zero. The same procedure can be applied to the other terms, progressively reducing the number of raising and lowering operators, exchanging them for the values, c, of commutators.

Let us begin to see how these facts apply in determining scattering amplitudes. For some specific  $|in\rangle$  and  $|out\rangle$  states we want to know the scattering amplitude  $\langle out|\hat{S}|in\rangle = \langle out|\hat{I}|in\rangle + \langle out|\hat{S}^1|in\rangle + \langle out|\hat{S}^2|in\rangle + \ldots$ , where  $\hat{S}^1$ ,  $\hat{S}^2$ , ... are the first, second, and higher orders in the S-matrix operator expansion (6.8). Written with an interaction Hamiltonian density, the first- and second-order terms take the form:

$$(6.16) \quad \langle out | \hat{S}^{1} | in \rangle = -i \int_{-\infty}^{+\infty} d^{4}x_{1} \langle out | \hat{\mathcal{H}}(x_{1}) | in \rangle$$

$$(6.17) \quad \langle out | \hat{S}^{2} | in \rangle =$$

$$-\frac{1}{2} \int_{-\infty}^{+\infty} d^{4}x_{1} \int_{-\infty}^{+\infty} d^{4}x_{2} \langle out | T[\hat{\mathcal{H}}(x_{1})\hat{\mathcal{H}}(x_{2})] | in \rangle.$$

Thus, to evaluate the scattering amplitude to second order we need to evaluate the first- and second-order expectation values

(6.18) 
$$\langle out|\hat{\mathcal{H}}(x_1)|in\rangle$$

and

(6.19) 
$$\langle out|T[\hat{\mathcal{H}}(x_1)\hat{\mathcal{H}}(x_2)]|in\rangle.$$

Substituting (6.9), (6.10), (6.11), and (6.12) into (6.18) gives

(6.20) 
$$\langle out|N\{\widehat{PL}(x_1)+\widehat{ER}(x_1)]\widehat{PhL}(x_1)+\widehat{PhR}(x_1)] \times \widehat{EL}(x_1)+\widehat{PR}(x_1)]\}|in\rangle.$$

The coupling constant has been omitted. Recall that T means temporal ordering on the time component of 4-vectors. Because (6.16) and (6.18) are parameterized with only one 4-vector, the T has no effect and has been omitted. Also recall that 'N' means normal ordering, which means that for a given kind of quantum in a given term, lowering operators occur to the right of raising operators. When we multiply (6.20) out we get eight terms, each a triple of lowering and/or raising operators, each sandwiched between an  $\langle out|$  and an  $|in\rangle$  state. But nothing can be done until specific  $\langle out|$  and  $|in\rangle$  states are specified.

Let us consider as an example a specific process, Compton scattering, in which we have an interaction between an electron and a photon, each with (very nearly) definite momenta, and where in the resulting state we measure for (very nearly) definite resulting momentum of one electron and one photon. To describe such a case we use an  $|in\rangle$  state of the form  $\hat{a}^{\dagger}(\mathbf{k}_i)\hat{b}^{\dagger}(\mathbf{p}_i)|0\rangle$  and an  $\langle out|$  state of the form  $\langle 0|\hat{b}(\mathbf{p}_o)\hat{a}(\mathbf{k}_o)$ , the adjoint of  $\hat{a}^{\dagger}(\mathbf{k}_o)\hat{b}^{\dagger}(\mathbf{p}_o)|0\rangle$ . Here photon raising and lowering operators for photons with specific momenta  $\mathbf{k}$  are written as  $\hat{a}^{\dagger}(\mathbf{k})$  and  $\hat{a}(\mathbf{k})$ , and electron raising and lowering operators with specific momenta  $\mathbf{p}$  as  $\hat{b}^{\dagger}(\mathbf{p})$  and  $\hat{b}(\mathbf{p})$ . The  $\mathbf{k}_i$  and  $\mathbf{k}_o$  are the initial and final, or "out," photon momenta, and  $\mathbf{p}_i$  and  $\mathbf{p}_o$  are the initial and final, or "out," electron momenta. With these specifications, the first-order term (6.18) becomes

$$\langle 0|\hat{b}(\mathbf{p}_{o})\hat{a}(\mathbf{k}_{o})N\{\widehat{PL}(x)+\widehat{ER}(x)]\widehat{[PhL}(x)+\widehat{PhR}(x)] \times \widehat{[EL}(x)+\widehat{PR}(x)]\}\hat{a}^{\dagger}(\mathbf{k}_{i})\hat{b}^{\dagger}(\mathbf{p}_{i})|0\rangle.$$

We can now immediately see that the first-order terms must be zero. On multiplying out, each of the eight component terms will have  $\hat{a}(\mathbf{k}_o)$ ,  $\hat{a}^{\dagger}(\mathbf{k}_i)$  and either  $\widehat{PhL}(x)$  or  $\widehat{PhR}(x)$ , as well as various electron and/or positron raising and/or lowering operators. Therefore each component term will have a different number of photon raising and lowering operators (one of one, two of the other), and so, between  $\langle 0|$  and  $|0\rangle$ , must give zero. First-order terms can make no contribution to Compton scattering.

Let's examine second-order terms for Compton scattering. Using our choice of  $\langle out |$  and  $|in\rangle$  states and substituting (6.9), (6.10), (6.11), and (6.12) into (6.19), we get

(6.21) 
$$\langle 0|\hat{b}(\mathbf{p}_{o})\hat{a}(\mathbf{k}_{o})T\{N[\widehat{PL}+\widehat{ER})(\widehat{PhL}+\widehat{PhR})(\widehat{EL}+\widehat{PR})]_{x_{1}} \times N[\widehat{(PL}+\widehat{ER})(\widehat{PhL}+\widehat{PhR})(\widehat{EL}+\widehat{PR})]_{x_{2}}\} \times \hat{a}^{\dagger}(\mathbf{k}_{i})\hat{b}^{\dagger}(\mathbf{p}_{i})|0\rangle.$$

I have abbreviated by writing the variables  $x_1$  and  $x_2$  as subscripts of expressions, meaning that the subscript variable occurs throughout the expression.

Equation (6.21) is not nearly as forbidding as it initially looks. After multiplying out, it is a sum of sixty-four component terms, but readers may entertain themselves by verifying that all but four terms are zero on elementary grounds, many simply because of an unequal number of raising and lowering operators. Our conceptual objectives will be served by looking at just one of the nonzero terms by way of illustration.

Consider

(6.22) 
$$\langle 0|\hat{b}(\mathbf{p}_o)\hat{a}(\mathbf{k}_o)T\{\widehat{ER}(x_1)\widehat{PhR}(x_1)\widehat{EL}(x_1)\times A(x_0)\widehat{EL}(x_1)\widehat{EL}(x_1)$$

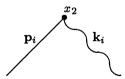
$$\widehat{ER}(x_2)\widehat{PhL}(x_2)\widehat{EL}(x_2)\}\hat{a}^{\dagger}(\mathbf{k}_i)\hat{b}^{\dagger}(\mathbf{p}_i)|0\rangle.$$

(Equation [6.22] is normal ordered, so the 'N' has been omitted.) Evaluation of this specific term can be accomplished with the third trick mentioned at the beginning of this section, of walking lowering operators to the right, eventually exchanging all raising and lowering operators for c-valued commutators. In the general case, certain similar expressions can be grouped together and the "walking to the right" trick applied in a systematic and general way so that all such terms can be evaluated with the aid of an ingenious diagrammatic technique, due to Feynman, Dyson, and Wick. A few words will give the idea of how this technique works.

We begin by considering the  $|in\rangle$  state,  $|in\rangle = \hat{a}^{\dagger}(\mathbf{k}_{i})\hat{b}^{\dagger}(\mathbf{p}_{i})|0\rangle$ , describing a photon of momentum  $\mathbf{k}_{i}$  and an electron of momentum  $\mathbf{p}_{i}$ . On this state the expression (6.22) applies the operator product  $\widehat{PhL}(x_{2})$   $\widehat{EL}(x_{2})$ . Using  $[\widehat{EL}(x_{2}), \hat{a}^{\dagger}(\mathbf{k}_{i})]_{-} = 0$ ,  $[\widehat{EL}(x_{2}), \hat{b}^{\dagger}(\mathbf{p}_{i})]_{+} = c(x_{2}, \mathbf{p}_{i})$ , and  $[\widehat{PhL}(x_{2}), \hat{a}^{\dagger}(\mathbf{k}_{i})]_{-} = c'(x_{2}, \mathbf{k}_{i})$  for c-number commutator values  $c(x_{2}, \mathbf{p}_{i})$  and  $c'(x_{2}, \mathbf{k}_{i})$ , we have

$$\begin{split} \widehat{PhL}(x_2)\widehat{EL}(x_2)\hat{a}^{\dagger}(\mathbf{k}_i)\hat{b}^{\dagger}(\mathbf{p}_i)|0\rangle \\ &= \widehat{PhL}(x_2)\hat{a}^{\dagger}(\mathbf{k}_i)\widehat{EL}(x_2)\hat{b}^{\dagger}(\mathbf{p}_i)|0\rangle \\ &= \widehat{PhL}(x_2)\hat{a}^{\dagger}(\mathbf{k}_i)[c(x_2,\mathbf{p}_i) - \hat{b}^{\dagger}(\mathbf{p}_i)\widehat{EL}(x_2)]|0\rangle \\ &= c(x_2,\mathbf{p}_i)\widehat{PhL}(x_2)\hat{a}^{\dagger}(\mathbf{k}_i)|0\rangle, \quad \text{(since } \widehat{EL}(x_2)|0\rangle = 0) \\ &= c(x_2,\mathbf{p}_i)[c'(x_2,\mathbf{k}_i) + \hat{a}^{\dagger}(\mathbf{k}_i)\widehat{PhL}(x_2)]|0\rangle \\ &= c(x_2,\mathbf{p}_i)c'(x_2,\mathbf{k}_i)|0\rangle, \quad \text{(since } \widehat{PhL}(x_2)|0\rangle = 0). \end{split}$$

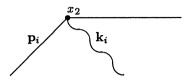
In the Feynman diagrammatic technique this six-line reduction of four operators to two c-valued commutator values is represented by the diagram:



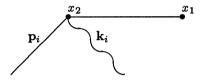
A straight line represents a Fermion (electron or positron), and a wavy line represents a photon. The two lines representing incoming quanta are labeled with their respective momenta and the effect of the electron and photon lowering operators, parameterized by  $x_2$ , is represented by the

termination of the lines at the point labeled  $x_2$ . Uniform rules dictate the inclusion of the commutator values  $c(x_2, \mathbf{p}_i)$  and  $c'(x_2, \mathbf{k}_i)$  with this diagram.

Next in the expression (6.22) the electron raising operator  $\widehat{ER}(x_2)$  applies to  $c(x_2, \mathbf{p}_i)c'(x_2, \mathbf{k}_i)|0\rangle$ . We can represent this in the diagram by starting a new line beginning at  $x_2$ :



Continuing to read (6.22) from right to left, we encounter the operator  $\widehat{EL}(x_1)$ , which can be represented by terminating the new line at a point labeled  $x_1$ :



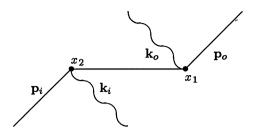
In the systematic rules for the diagrams, the last operator product,

$$\widehat{EL}(x_1)\widehat{ER}(x_2),$$

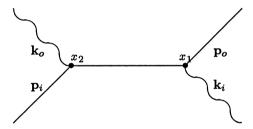
which we have just seen to be represented in the diagram by the internal line beginning at  $x_2$  and ending at  $x_1$ , gives rise to an expression called an *electron propagator*, shown to be a commutator c-value by yet another application of walking the lowering operator to the right.

Finally, the remainder of (6.22),  $\langle 0|\hat{b}(\mathbf{p}_o)\hat{a}(\mathbf{k}_o)\widehat{ER}(x_1)\widehat{PhR}(x_1)$ , gives terms associated with outgoing quanta of momenta  $\mathbf{p}_o$  and  $\mathbf{k}_o$ , in much the same way that  $\widehat{PhL}(x_2)\widehat{EL}(x_2)\hat{a}^{\dagger}(\mathbf{k}_i)\hat{b}^{\dagger}(\mathbf{p}_i)|0\rangle$  gave terms associated with incoming quanta. Completing the diagrammatic representation, we have

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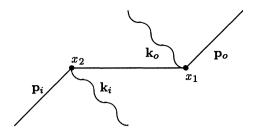
The power in this diagrammatic technique lies, first, in the uniform rules that associate values of terms in expressions such as (6.21) with the various lines and vertices of a diagram. The power lies, further, in the fact that one merely has to write down all topologically distinct diagrams representing the process in question in order to get all the terms to a given order for a process, with the number of vertices corresponding to the order in the S-matrix operator expansion. For example, for Compton scattering—an  $|in\rangle$  electron and photon and an  $|out\rangle$  electron and photon—to second order (2 vertices) one also needs the diagram



As before, the uniform rules dictate immediately what values to associate with the lines and vertices of this diagram.

#### INTERPRETATION OF DIAGRAMS

In this presentation I have deliberately avoided the customary and seemingly obvious interpretation to put on these diagrams. For example, it seems natural to describe the diagram



as a photon and electron coming in, being annihilated at  $x_2$ , where a new electron (called a *virtual electron*) is created, which in turn travels to  $x_1$ , where it is annihilated and the outgoing photon and electron are created. Practitioners irresistibly talk and often think in this language. These habits have fostered a popular description of the subject according to which raising and lowering operators—more usually called "creation" and "annihilation" operators—describe the creation and annihilation of particles in just the way that the diagrams seem to picture. And this way of thinking further suggests an issue about whether the "virtual particles" apparently described by internal lines "really exist."

I counsel resistance to this way of thinking, which I take to be misleading in the extreme. There are a variety of reasons. The whole attitude turns on the supposition that a raising operator ("creation operator") characterizes an event in which a quantum is created and a lowering operator ("annihilation operator") characterizes an event in which a quantum is annihilated. The attitude further turns on the supposition that negative-frequency operators, for example,  $\widehat{ER}(x)$  (=  $\widehat{\Psi}^-(x)$ ), characterize events of creation of a quantum at the space-time point x, and similarly for positive-frequency operators and annihilation events. Finally, and most clearly suspect, the attitude takes the "virtual particles" and the events of their creation and annihilation seemingly characterized by Feynman diagrams as somehow being real even though the terms described by diagrams occur only as components of a giant superposition. All of these suppositions are to be questioned.

Why should raising and lowering operators somehow be taken to describe the creation and annihilation of quanta? I submit that there is no strong reason for so interpreting them. As we saw in chapters 1 and 2, quanta should be thought of as entities without primitive thisness, naturally described with the Fock space formalism. Raising and lowering

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operators constitute no more than a convenient formal tool for working with this formalism, characterizing a relation between states that differ in occupation number by one. It is a huge, and as far as I can see unwarranted, step from these facts to the claim that the operators somehow describe a process of the actual creation and annihilation of quanta.

It is of course tempting to think that raising and lowering operators somehow describe or otherwise correspond to the phenomena of pair creation and annihilation, which we see so graphically depicted in cloud chamber tracks. More generally, practitioners have, since the inception of quantum mechanics, talked about the theory in terms of "state transitions" and "quantum jumps." Do raising and lowering operators describe such processes?

Talk about state transitions and quantum jumps is misleading. Let's carefully review how the theory proceeds. We start with a prepared state, which for simplicity's sake we can suppose to be an eigenstate of some observable. With interaction this pure state evolves, smoothly and deterministically, into a superposition of other eigenstates of the observable in question. Then, on measurement, and only on measurement, we find that one member of the evolved superposition corresponds to our measurement result.

Before the measurement there are no discontinuous state transitions or quantum jumping. There is only continuous evolution, in particular in our example from one eigenstate to a superposition of eigenstates of some observable. Only on measurement, if anywhere in the description, <sup>17</sup> is there a question of a discontinuous transition.

Do raising and lowering operators at least describe what happens on measurement? Yes, in the uninteresting sense that they can be used to describe the eigenstates that result when a measurement takes place. But they do not in any more enlightening way describe what goes on when we make a measurement. At this point these operators are not in the larger account functioning differently from the way they do in description of continuous evolution between preparation and measurement, that is, as a tool for describing the relations between various parts of the formalism.

What of the cloud-chamber pictures of "particle" creation and annihilation? These are simply special cases of measurement, each conden-

<sup>&</sup>lt;sup>17</sup>A number of interpretations of the measurement process do not involve discontinuous physical transitions even on measurement. Instead they attempt to treat measurement in terms of selection of a subensemble. See Teller (1984) for a survey.

sation droplet of a track counting as a separate measurement. Between the last droplet on a track leading to a "pair creation" branch and the first droplets on each of the two new branches, the state evolves from a one-quantum system into a superposition involving at least a small amplitude for the new two-quantum state. The measurement involved in the first droplets on the two new branches constitutes the resolution by measurement ("reduction of the wave packet") that picks out one of the members of the superposition that includes the observed two-quantum state. But, aside from providing a description of the new two-quantum state, there is no reason to think that the raising operators describe a state transition process, any more than they do when they function in the theory between measurements where there are no state transitions but only continuous evolution of a superposition.

There is also a special problem with thinking of negative- and positive-frequency operators as creating and annihilating quanta at specific space-time points. Recall the facts briefly described at the end of chapter 4. In a nonrelativistic theory a state  $\hat{\Psi}^{\dagger}(\mathbf{x},t)|0\rangle$  is plausibly thought of as a (idealized point valued) position eigenstate. But this description breaks down in relativistic theories. And the  $\hat{\Psi}^{\dagger}(\mathbf{x},t)$  of chapter 4 is precisely the negative-frequency-operator solution to relativistic field equations. So even if in general we were right to think of raising and lowering operators as describing the creation and annihilation of quanta, the operators that Feynman diagrams represent would still not stand for creation and annihilation at precise space-time points.

Finally, one too easily overlooks the fact that the expression graphically represented by a Feynman diagram is only a component in a much larger superposition. <sup>18</sup> Typical diagrams appear to describe events of creation of "virtual" quanta, followed by propagation to new space-time locations where the virtual quanta are subsequently annihilated. Each diagram seems so vividly to depict events of creation, propagation, and annihilation that one is tempted to see these events as actually occurring "parts" of a larger process. But there is danger here in equivocating on the word 'part'.

To explain, I start with a simple and homely example. An ordinary chair has what I will call *mereological parts*, such as its back and each of its four legs. A mereological part of a whole has its own trajectory in space and time. And a mereological part, in general, itself has parts

<sup>18</sup> Weingard makes this point about virtual quanta in "Do Virtual Particles Exist?" (1982).

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that are also parts of the original whole—a leg of a chair has parts, say its top and bottom halves, each of which is also a part of the chair.

Now let's contrast mereological parts with what I will call analytic parts. The analytic parts of a vector relative to a basis are its components in the basis. Fourier analysis of a wave form into sine and cosine wave functions constitutes a special case. Clearly there is some sense in which a wave form with one bump



is "composed" of the sine and cosine wave functions into which the one-bump wave can be Fourier analyzed. But it seems dubious to say that the analyzing pure wave forms are "parts" of the analyzed wave in anything like the sense in which the leg of a chair is a part of the chair.

To argue this in detail would take a full-blown theory of mereology, which I do not propose here to undertake. However, I can point out that the notions of mereological part and analytic part have markedly different formal properties. As I mentioned, mereological parts of mereological parts are, in general, mereological parts of the original whole. But analytic parts do not seem to satisfy the "parts of parts are parts" condition. As I defined "analytic part relative to a basis," the relativity to a basis seriously interferes with seeing how the condition is to be satisfied. To get a notion of analytic part satisfying the "parts of parts are parts" condition we could drop the relativization to a basis and simply say that starting with a given whole constituted by a vector quantity, all the vectors in any sum that add to the original whole count as analytic parts of that whole. But then we would have to say that my standing stock still has as analytic parts my simultaneously moving at 100 miles an hour to the north and at 100 miles an hour to the south. Or we would take the ancestral of the analytic part relative to a basis relation, that is, analytic parts relative to a basis, or analytic parts relative to one basis of analytic parts relative to some other basis, and so on. But then traveling at 50 miles an hour due east would count as an analytic part, in this extended sense, of traveling 100 miles an hour due north. There are other ways to try to characterize analytic parts, but generally

characterizations stopping short of relativization to a basis seem to run into this kind of difficulty.

Mereological and analytic parts differ also with respect to space-time trajectories. Nonoverlapping mereological parts of a whole each have their own nonoverlapping space-time tubes, and the space-time tube of the whole is exactly made up of the space-time tubes of all the nonoverlapping parts. The notion of a space-time trajectory may simply not apply in many cases of analytic parts, such as the analytic parts of a force. For a plausible case in which the notion of a trajectory may apply, let's consider the special case of a classically described wave on a stretched string. At a fixed time each analyzing pure wave form occupies the locations at which the string would be located if the pure wave form were the only component present. The independent wave forms then overlap in space, and each overlaps but does not coincide with the original, analyzed wave form. Indeed, except for sinusoidal waves, there will be at most countably many points of overlap out of the continuum of points occupied by the wave forms!

I take these formal differences between mereological and analytic parts to show that analytic parts are not parts in the same sense as mereological parts. Perhaps there is some sense in which analytic parts "exist in" the wholes of which they are components, but one must be careful not to assimilate this sense to the one in which mereological parts are independently existing objects that collectively compose the wholes of which they are parts.

In the case of Fourier components as they appear in quantum-theoretical descriptions, talk of analytic parts is particularly disanalogous to talk of mereological parts. In quantum theories the components represent potentially but not actually existing states. Unless one is willing to adopt one of the difficult routes around the various "no hidden variable" arguments, we have to say that components do not exist in superimposed states, and even hidden-variable theories say only that one of the components is present. (In the special case of the double-slit experiment, we must refrain from saying that the particle actually passes through slit A or through slit B, and even hidden-variable theorists will attribute passage through only one slit!) The proper interpretation of the Feynman diagrams constitutes a special case of this general point about the interpretation of quantum theories.

It is worth reviewing the extent to which terms corresponding to individual Feynman diagrams are elements of a gigantic superposition. Look

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back at (6.16) and (6.17), the schematic formulas for the first and second order  $|in\rangle$ ,  $|out\rangle$ , scattering amplitudes described by the S-matrix operator. These require integration of the x-variables before we get the final scattering amplitudes. In application to our example of Compton scattering, in the second-order term the integrated  $x_1$  and  $x_2$  are precisely the  $x_1$  and  $x_2$  represented in the Feynman diagram. In other words, the expression that the rules have us read off the Feynman diagram have  $x_1$  and  $x_2$  as free variables, which must be integrated before we get the diagram's final contribution to the scattering amplitude. The processes allegedly described by the diagram must be superimposed for all values of  $x_1$  and  $x_2$  before we get a description of what is still only a contribution to a quantum-mechanical amplitude for a real scattering process.

And if this were not enough, we must also remember that the expression we have been discussing is only the second-order contribution in an approximation scheme. The full scattering amplitude is, in principle, given only when the results from second order are further superimposed with contributions from all even higher orders. (Odd orders never make a contribution to Compton scattering for the same reason that the first order made no contribution: unequal numbers of photon raising and lowering operators.)<sup>19</sup>

After protesting so vociferously that Feynman diagrams must not be thought of in a literal-minded way, I want also to acknowledge their astonishing usefulness. The diagrams, together with the rules for writing down corresponding contributions to the S-matrix operator expansion, make practical what would otherwise constitute a formalism so unwieldy that no one could apply it to any but the simplest cases. It is likewise true that for many students the heuristic of thinking in terms of sequences of particle creation and annihilation may provide a useful learning tool.

## SELF-INTERACTION TERMS

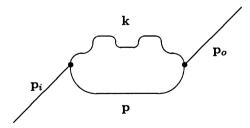
The contributions to a given order of the S-matrix operator expansion are depicted by the topologically distinct Feynman diagrams. At order n

<sup>&</sup>lt;sup>19</sup>I should also mention that often practitioners apply the Feynman diagrammatic technique to a representation in momentum space. Momentum space diagrams do not in any way depict trajectories in physical space. See Redhead (1983), p. 87, for further discussion.

there are n occurrences of the interaction Hamiltonian, each parameterized by a space-time variable,  $x_i$ , with each  $x_i$  represented graphically as a vertex. Thus an  $n^{th}$  order term is represented by a graph with n vertices. The example of the interaction Hamiltonian density, (6.9), has two Fermion and one photon field operators, so that in this case each vertex is attached to two lines corresponding to Fermion operators (represented by straight lines) and one line corresponding to the photon operator (represented by a wavy line). Each positive- and negative-frequency term has an exponential function with a product of a space-time with a momentum variable in the exponent. When the space-time variables are integrated, the effect of these exponentials is that of a delta function enforcing energy-momentum conservation at each vertex.

In this section we are particularly interested in the momentum variable associated with each internal line. An internal line starts with a raising operator (negative-frequency term) and ends with a lowering operator (positive-frequency term). Each of these operators involves an integration over all momenta, but the product of the associated exact momentum-raising and -lowering operators also has the effect of a delta function, so that there is after all just one momentum variable associated with each internal line. In our example of Compton scattering there was only one internal line, of which the value of the associated momentum variable was fixed by the energy-momentum conservation at the vertices.

Let's now look at what happens in situations in which internal lines form a closed loop, for example, a second-order contribution to electron propagation in which the  $|in\rangle$  and  $|out\rangle$  states each have just one electron. Because each vertex has two Fermion and one photon line attached, the diagram is:

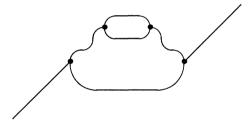


This is an example of what corresponds in a quantum field theory to self-interaction. Electrons interact through the mediation of the 144 CHAPTER SIX

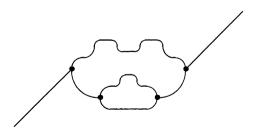
electromagnetic—or photon—field, and here an electron, through the mediation of a photon, is "interacting with itself."

Such self-interaction appears to make nonsense of the theory. The momentum conservation at the vertices does not fix both the internal momentum variables **k** and **p**: One of these is unconstrained, so that the momentum integration is over all values for the unconstrained momentum variable. The problem is that this momentum integral diverges. Its value is infinite.

The same catastrophe arises generally for other internal loops in the theory. And the internal loops compound. As mentioned at the beginning of this section, all topologically distinct diagrams contribute to the S-matrix operator expansion, with the number of vertices corresponding to the order of the term. Where the last diagram depicts the second-order contribution to electron propagation, we can further modify the diagram to depict fourth-order terms by inserting a new loop on one of the internal lines:



and



Further new loops can be introduced on internal lines, corresponding to higher- and higher-order terms in the S-matrix operator expansion, and each new loop in the higher-order terms introduces a new divergent integral.

For about fifteen years these divergent integrals seemed to doom quantum field theory, although already in 1937 Kramers suggested dealing with the impasse with ideas that later developed into the renormalization program. In 1946 Lamb and Rutherford made a very precise measurement of a hitherto unexplained anomalous shift in the energy spectrum of hydrogen (the Lamb shift), and Bethe succeeded in showing, using what some nonphysicists might think a shockingly loose argument, 20 that much of the anomalous energy shift seemed derivable even in a nonrelativistic calculation using the renormalization ideas. Stimulated by Bethe's success, in the next few years Feynman, Tomonaga, Schwinger, Dyson, and others systematically developed these ideas. Although the calculational results produced breathtaking agreement with experiment, many physicists, including Feynman and Dirac, viewed the apparent "discarding of infinities" as mathematically illegitimate. 21 In fact, few physicists now see renormalization as suspect. But it will be worthwhile, in the last chapter, to lay out the ideas of infinite renormalization in a form accessible to nontechnicians and to begin the work of examining various interpretive approaches to this apparent formal sleight of hand.

#### Conclusion

A great many nonphysicists (including many biologists, psychologists, economists, and so on) think of physics as proceeding almost mechanically from exact principles using exact methods. Interacting quantum field theory provides a wonderful example of how far this simplistic picture departs from the realities.

Physics arrives at a quantum field theory as a matter of guesswork. Then, with a theory in place, straightforward mathematical calculation, even from first principles, is still out of the question. Most of the effort of quantum field theorists goes into devising approximation techniques that will squeeze a little information out of very shaky first principles. The resulting scheme, perturbative methods resulting in the S-matrix operator expansion, organized and analyzed with the help of the Feyn-

 $^{21}$ For histories of this subject see Schweber (1981) and (1994) and Rüger (1992).

<sup>&</sup>lt;sup>20</sup>Pais (1983, p. 276) describes the calculation as "total chutzpah," continuing with: "Everybody was furious because Bethe had done it in an absolutely lowbrow dirty kind of way, and everybody knew, of course, that he had to be right."

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man diagrammatic technique, is of unknown formal legitimacy—I have in mind here not renormalization but the issue of whether the S-matrix operator expansion converges. All one really knows is that the method produces numbers with which experimental results can be brought into striking agreement, and the overall scheme aids informally in suggesting both new bits of theoretical description and new experiments.

The techniques of the S-matrix operator expansion outlined here constituted much of practical quantum field theory until about 1960. I want to emphasize that new, very substantial alternatives are now available for many problems. For example, renormalization group techniques yield a great deal of information, and the use of finite space-time lattices provides an entirely different method of approximation. But even today the techniques of the S-matrix operator expansion provide the basis for a great deal of work in quantum field theory. My impression is, also, that the fragmentary nature of the understanding to be found in the approach of the S-matrix operator expansion is representative of much of the further detail of quantum field theory, and indeed of much of physics. <sup>22</sup>

#### PROBLEMS

- 1. Further investigate the implications of Haag's theorem for working quantum field theories. Can the problem really be resolved by using the LSZ approach? Are there other promising resolutions of the problem? The problem clearly involves quantum field theories' uncountably many degrees of freedom. Does this fact provide any useful approaches? Good luck!
- 2. In the text I attempted to explain the prevalent difference in attitude toward renormalization and toward dropping the zero-point energy by saying that in the latter case we are guessing at the formulation of a model or theory, while in the former we are calculating with what we thought was a finished theory. Although this makes good sense as a hypothesized explanation of the historical difference in attitude toward the discarded infinite zero-point energy and the divergences discarded

<sup>&</sup>lt;sup>22</sup>These comments do not apply to the formal and rigorous work in axiomatic field theory, which I have not discussed in this book. Although it is a useful enterprise in the study of formal properties of quantum field theories, axiomatic quantum field theory as it exists today does not appear usefully to describe real physical phenomena.

in renormalization, the reconstruction may be seen as not doing justice to the logic of the two cases. Perhaps one could equally well regard renormalization not as "guessing again," but as a procedure for fashioning consistent theories out of first guesses marred by a serious formal flaw. Explore this suggestion and use it critically to evaluate the claims in the text concerning the different attitudes toward renormalization on the one hand and the discarded zero-point energy and other infinite vacuum expectation values on the other.

- 3. Do "observable results" described by scattering amplitudes exhaust the description of "experimental results" to which one may appropriately appeal in this subject? Is there other indirect evidence or plausibility considerations to which we reasonably appeal when choosing an interaction Hamiltonian or when taking a more sanguine attitude toward the description of what happens at space-time points between the in- and out-events?
- 4. Discuss the similarities and differences in the problem of self-interaction as it comes up in classical theories and in quantum field theory.
- 5. Given any amplitude between two states,  $\langle \psi_2(t_2)|\psi_1(t_1)\rangle$ ,  $t_2>t_1$ , one can introduce resolutions of the identity in terms of possible intermediate states, at small intermediate time intervals,  $t_1 < t_1 + \Delta t < t_1 + 2\Delta t < \cdots < t_1 + n\Delta t = t_2$ . Then, by letting  $\Delta t \to 0$  in a wide range of physically applicable circumstances, the resulting expansion takes the form of a functional integral or more specifically a path integral. The resulting functional integral methods have proved exceedingly useful in working with quantum field theories. Claim: Because the components of a functional or path integral are elements in a superposition describing a probability amplitude, the same interpretive caveats developed in this chapter about Feynman diagrams apply to the (elements of) functional and path integral expansions of probability amplitudes. Problem: Expand on this claim and then critically evaluate it.
- 6. One might cite a further reason for denying the existence of virtual quanta, that is, the quanta represented by internal lines in Feynman diagrams: Virtual quanta are not "on the mass shell." Briefly, energy and momentum are conserved at vertices, but the number and kind of particles change, so that the virtual quanta violate the relativistic relation  $E^2 = m^2 + p^2$  (E and p satisfying  $E^2 = m^2 + p^2$  are said to be on the mass shell). For example, in the first vertex in Compton scattering a virtual electron must "carry away" the energy and momentum of

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the incoming electron and photon. But with the mass of the "outgoing" virtual electron fixed as the same as that of the incoming electron, it is easy to apply  $E^2 = m^2 + p^2$  together with energy and momentum conservation at the vertex to show that if the incoming quanta satisfy the relation, then the outgoing virtual electron must violate it. And, it might be claimed, no actual entity can violate  $E^2 = m^2 + p^2$ .

The issue is not straightforward. In an older formulation of perturbation theory, the virtual quanta were described as being on the mass shell but violating energy conservation, which physicists then customarily excused by appealing to the time-energy uncertainty relations. Problem: Does departure from the mass shell constitute a further argument against the actuality of virtual quanta, or can such a departure be explained away as one sought to do in the older theory by appealing to the time-energy uncertainty relations? In this problem be careful with the notoriously obscure status of the time-energy uncertainty relations.

Further problem: In the folklore of particle physics it is often said that the existence of virtual quanta can be explained by an appeal to the time-energy uncertainty relations. Furthermore, it is claimed that the argument gives a qualitative explanation of the range of the force "carried" by the virtual quanta: Starting with  $\Delta E \Delta t \simeq \hbar$ , we can have a mass of  $m = \Delta E/c^2$  for up to time  $\Delta t$ , and such a quantum, traveling at most with velocity c, can have a range of up to  $d \simeq c \Delta t$ , giving an estimated range for such virtual quanta of  $\hbar/mc$ . Evaluate this line of argument.

### Infinite Renormalization

ONCE UPON A TIME a methodologist of science had a nightmare. He dreamed that some physicists, called "quantum field theorists," used an approximation scheme some of whose terms were infinite. But the quantum field theorists just threw out the infinite terms, kept the remaining finite expressions, and, to add methodological insult to mathematical injury, thereby obtained the best agreement between theory and experiment that existed anywhere in science. As the reader knows, the methodologist woke up to find that his dream was true.

At least, this is the popular description we get of renormalization. Of course, things are not quite so simple. In this chapter I will undertake to explain how renormalization really works and to explore some different ways in which physicists have suggested that one understand the procedure. Heretofore only specialists have been able to fight their way through the tangle of complicated physics needed to understand renormalization as it is presented to physicists. This is not necessary. I will extract the fundamental ideas and problems from the tangle of technical details, so that to follow the exposition one will need no more than an idea of what an integral is. Indeed, the present chapter will not presuppose a knowledge of the foregoing material in this book.

I will proceed with the renormalization of quantum electrodynamics principally in mind, but what I have to say goes for the problems of renormalization of quantum field theories generally. Important recent developments in quantum field theory do involve vitally new concepts. But as far as I can tell they do not involve new methodological puzzles radically different from those of the infinities of the older renormalization argument. In any case, I will almost exclusively discuss the renormalization procedure as it emerged around 1950, with only a brief reference to newer developments.

#### Infinities in the Theory

In this chapter I focus exclusively on the renormalization strategy and its conceptual issues, without being distracted by the details of the theoretical setting. We can do this by considering quantities,  $S_1(m)$ ,  $S_2(m)$ ,  $S_3(m)$ , and so on, which are first-, second-, third-, and higher-order contributions to the electron propagator, all terms entering into the S-matrix expansion as described briefly in the section on self-interaction terms in the last chapter. All we need remember here is that these expressions are contributions to an approximation scheme, and each is a function of the mass of the quantum under description. In my simplified exposition I will talk only about mass renormalization, whereas in real life mass and charge renormalization need to be treated together. But none of these simplifications and omissions will affect the issues we want to put under a conceptual magnifying glass.

The first-order approximation gives nice results that agree quite well with experiment. But when we calculate second-order corrections, infinities occur. No one has proved that the approximation scheme converges. Occurrence of infinities, one would think, should make it clear that some fatal flaw infects the fundamental theory, the approximation scheme, or the system of the two taken together. Surely in the face of these infinities we should revise the calculational method, and if that does not succeed, we should revise or dump the theory. But that is not what has happened.

A somewhat similar problem had already occurred in the classical theory of electromagnetism. Classically, an electron produces an electric field. But given any electric field and an electron, the electron will interact with the field. Thus we must take into account the interaction of an electron with its own field. In classical electromagnetism, one interpreted the energy of this self-interaction as part of (or possibly the whole of) the electron's mass. This works as long as the electron has a finite (that is, nonzero) radius. But with a finite radius it would seem that the electron has structure, and in particular a distribution of charge. Why does the medium carrying the charge distribution not push itself apart under the influence of the repulsive charge? Consequently one would like to describe an electron as a primitive, structure-less point charge. But for a point charge the self-interaction becomes infinite. Classical electromagnetism never satisfactorily resolved this problem.

The attempts to deal with the energy of self-interaction as observed mass carry over to quantum field theory. As in the classical case, in quantum field theory an electron interacts with its own field. This self-interaction gives rise to a quantity that can be seen as part of the electron's mass, which would be fine, except that, as in the classical case, this self-interaction quantity turns out to be infinite.

Let's see how quantum field theorists deal with these infinite terms. In quantum field theory the infinities arise as divergent integrals of the form  $\int_0^\infty g(k)dk$ . Now, an integral from zero to infinity is really a limit:  $\int_0^\infty g(k)dk = \lim_{t\to\infty} \int_0^t g(k)dk$ ; and typically in quantum field theories,  $\int_0^L g(k)dk \cong \ln L$  (the natural logarithm of L). This gives the sense of the infinities in question, which we more properly describe as divergences.

We can study divergences in one way by holding off in taking the limit. More specifically, we consider the theory before we take the L going to infinity limit of  $\int_0^L g(k)dk$ . Instead we say that L has some unspecified but large finite value, called a cutoff for the divergent integral.

The key to dealing with the problematic integral is that we can always put  $S_i(m)$  in the form  $S_i(m - \int_0^L g(k)dk)$ . This embodies the idea from the classical electron theory: the integral describing the self-interaction appears as something subtracted from the mass that originally appeared in the theory.

We need some detail to see how we are going to take advantage of this fact. We start with the simplified description of a theory with no interaction, and so no self-energy—a so-called *free theory*. In the case of quantum electrodynamics this would be a theory of photons (that is, light, or more generally the electromagnetic field) and electrons, where these are not yet described as interacting. In particular, in such a theory electrons are not described as producing an electromagnetic field with which they could self-interact. In the free theory an electron is described as having a mass, which we call  $m_0$ , or the electron's bare mass. (Photons are described as being massless throughout.)

The sequence of approximations now starts taking into account the interaction between electrons and photons (or between electrons and the electromagnetic field). In the first approximation the self-interaction has no effect. The effects of self-interaction show up first in the second-order correction, producing the problematic integral  $\int_0^L g(k)dk$ . As the second-order approximation we get an expression of the form  $S_2(m_0 - \int_0^L g(k)dk)$ . Thus the integral occurs as a value subtracted from the bare mass. In other words, the self-interaction acts like a negative con-

tribution to the electron's mass. If there were no interaction, the mass would be  $m=m_0$ . But with self-interaction, the electron acts (to second order) like a particle with mass  $m=m_0-\int_0^L g(k)dk$ . We call this value the renormalized mass,  $m_r$ , the value of mass that the electron appears to us to have:

$$m_r = m_0 - \int_0^L g(k)dk.$$

Keep in mind that at the present stage of the discussion we express the problematic integral with the finite cutoff, L, so that everything is finite and well defined.

The crucial point is that we can observe only  $m_r$ . In order to observe  $m_0$ , we would have to "turn off" the interaction. But we can't turn off the interaction. We can observe only the total inertial properties of the electron, corresponding to  $m_r$ . Thus we must take  $m_r$  to be the electron's real, observable mass. Note that the problematic  $\int_0^L g(k)dk$ no longer occurs as an independent quantity that must be put into our calculation of  $S_2$ . We observe the mass. We identify this mass with  $m_r =$  $m_0 - \int_0^L g(k)dk$ . In other words, we say that  $m_0$ , the mass of the fictitious free theory, must have some value that differs by just  $\int_0^L g(k)dk$  from  $m_r$ . All this makes perfectly good sense, because we are still taking L, and so  $\int_0^L g(k)dk$ , to be finite. The bare mass,  $m_0$ , by itself has no direct or indirect observable significance. It is in principle unobservable. Its value does not independently affect the value of any observable quantities. Therefore we are free to assume  $m_0$  to have any value required to get the observable mass,  $m_r$ , to have the right, observed value.

All this makes sense while L is finite. However, we must still take the limit of  $\int_0^L g(k)dk$  as L goes to infinity. After taking the limit, we still have a consistent scheme for calculating observable quantities, like  $S_2$ . For each finite value of L, we deal only with finite quantities. And for each finite value of L, the quantity that will diverge,  $\int_0^L g(k)dk$ , is absorbed into  $m_r$ , which always has its constant, observed value. Therefore for each finite value of L,  $m_r$  and the observable quantities that are a function of  $m_r$  not only have finite values but do not blow up as L goes to infinity. In other words, all these quantities have well-defined limits for L going to infinity. Finally, these well-behaved, renormalized quantities include all relevant directly and indirectly observable quantities.

This procedure avoids any formal mathematical problems. We have a perfectly consistent scheme for calculating all physical quantities of interest. Strictly speaking, no "infinities" have been "thrown away." At the same time we must agree that, formally consistent though it may be, conceiving of things in this way involves a very odd conception of the physics. The exact theory seems to tell us that in some sense there is an infinite bare mass that has been correctly balanced by an infinite interaction.

I will elaborate further on this way of thinking about renormalization. And at the end I will compare it with alternatives. But first we need to understand some of renormalization's further startling aspects.

#### FINITE CORRECTIONS

I have described how the renormalization procedure eliminates the divergent integrals. But along the way, the procedure involves the following further, prima facie astonishing maneuver. Before removing the divergent terms, we separate out from them finite radiative corrections. These are not removed but left behind, and they in fact affect the value of  $S_2(m_r)$ . This seems absurd. We can separate a finite from an infinite quantity perfectly arbitrarily. From an infinite term we can leave behind any finite quantity we want. But the radiative corrections that the renormalization procedure leaves behind have observational consequences, such as the Lamb shift and the anomalous magnetic moment. Although one looked for a theoretical description only after initial experimental observation of the effects, clearly the corrections were not chosen simply to reflect observed values. Moreover, the agreement between the calculated and observed radiative corrections provide the numerically most accurate agreement between theory and experiment anywhere in science. Most of the experimental accuracy was achieved only after the corresponding theoretical calculation of values.

The renormalization procedure includes a prescription for separating out these finite corrections. For this procedure to make sense, it must be unambiguous; that is, it must give a unique value independently of the details of how one does the calculations. Furthermore, we want a physical motivation for the procedure.

This procedure turns on a general characterization of how we measure the mass of a particle. We measure inertial mass by momentum exchange, typically using small momenta (on the scale set by today's accelerators). And we can characterize mass measurement situations with a parameter, q, which is small when the momentum exchanged in

the mass measurement is small. This q then enters in a natural way into the analysis of the divergent integral: The integrand includes two terms, one independent of q and the second with q appearing as a coefficient:

$$\int_0^\infty g(k)dk = \int_0^\infty [g_d(k) + qg_f(k)]dk$$
$$= \int_0^\infty g_d(k)dk + q \int_0^\infty g_f(k)dk.$$

Furthermore,  $\int_0^\infty g_d(k)dk$  diverges while  $\int_0^\infty g_f(k)dk$  converges to a finite limit—the subscripts 'd' and 'f' on the differing integrands act as mnemonics for 'divergent' and 'finite'. In other words, by dividing up the integrand into the term that multiplies q and a separate term, we have an unambiguous prescription for dividing up the original divergent integral into divergent and finite parts.

What is the rationale for this separation and for retaining  $q \int_0^\infty g_f(k)dk$  as a radiative correction? I give first a quick statement of the idea. When q=0,  $\int_0^\infty g(k)dk=\int_0^\infty g_d(k)dk$ , so that when we renormalize, the entire integral gets absorbed into  $m_r$ . Since when we measure mass, for all intents and purposes q is zero, the renormalized mass reflects only the term  $\int_0^\infty g_d(k)dk$  from the original divergent integral. But what happens when we measure some quantity in a situation in which  $q\neq 0$ ? The numerical value that we put in for the mass is still the value for  $m_r$  measured when q=0. But now  $q\int_0^\infty g_f(k)dk$  is not zero. It got left out when we renormalized the mass. So we must include  $q\int_0^\infty g_f(k)dk$  as a radiative correction.

That was pretty quick. Let's look at the argument more carefully. First we put in a cutoff and examine the argument when everything is finite:

$$\int_0^L g(k)dk = \int_0^L g_d(k)dk + q \int_0^L g_f(k)dk.$$

Now the observed mass is

$$m_r = m_0 - \int_0^L g(k)dk = m_0 - \int_0^L g_d(k)dk - q \int_0^L g_f(k)dk.$$

We measure the mass when q is very small, and following standard practice in physics, we treat an extremely small quantity as zero. This needs to be justified, and I will come back to this question shortly. When q=0 the observed mass is

$$m_r = m_0 - \int_0^L g(k)dk = m_0 - \int_0^L g_d(k)dk.$$

This is the value for the mass that we write down in our reference manuals. This value for the observed mass does not include any of the potential finite correction  $q \int_0^L g_f(k) dk$ , potential in the sense that we would have to include it in any situation in which q was significantly different from zero. Indeed, let us consider what we must say when we calculate a quantity in some situation in which q clearly differs from zero. We still use the value of  $m_r$  obtained when q=0. This is just the measured value for the mass that we find written down in our reference manual. But now, with  $q \neq 0$ , the term  $q \int_0^L g_f(k) dk$  has been neglected. Thus in this  $q \neq 0$  situation the quantity  $q \int_0^L g_f(k) dk$  must be added back. This is exactly the small radiative correction.

Up to this point we have taken L, and so all the integrals, to be finite. We have not separated the finite  $q \int_0^\infty g_f(k)dk$  from the infinite  $\int_0^\infty g(k)dk$ . We have separated the finite  $q \int_0^L g_f(k)dk$  from the finite  $\int_0^L g(k)dk$ . In this situation there can be no question that we must include the finite correction  $q \int_0^L g_f(k)dk$  when  $q \neq 0$ .

Now we let L go to infinity. The foregoing argument applies correctly for each value of L along the way. Reconsidering the argument for each value of L, we see that we must add back the neglected quantity  $q \int_0^L g_f(k)dk$ . But the limit  $\lim_{L\to\infty} q \int_0^L g_f(k)dk$  is well defined. Consequently in the limit we should add back this well-defined, finite limit.

Does this talk about what happens "in the limit" make any sense? Perhaps such talk is not clear. But I think we can make it clear by reinterpreting talk about what happens "in the limit" as talk about what happens for each finite case that occurs "along the way to the limit." I have in mind a strategy sometimes used in making sense of talk about things like instantaneous velocity. The idea of an object's average velocity in an interval is clear enough. It is, by definition, just the distance the object traverses divided by the time that elapses as the object passes from one end of the interval to the other. But what does one mean by an object's velocity at a point? One way of approaching this question proceeds by reconstructing talk about an object's instantaneous velocity at a point as talk about the sequence of average velocities over a sequence of intervals all of which contain the point and which approach the point as a limit. We can then reconstrue statements about the instantaneous velocity as statements about all these average velocities, generally by exchanging statements about the instantaneous velocity for statements about limiting values of corresponding statements about the

average velocities. For example, we can reconstrue the statement that the difference between two instantaneous velocities has a given value, v, as the statement that the difference between corresponding average values has that value, v, as its mathematical limit.

We can use this kind of strategy to clarify the argument for adding back  $\lim_{L\to\infty} q \int_0^L g_f(k)dk$  to  $m_r$  when  $q\neq 0$  and to clarify our description of the overall situation in which the limit has been taken. Think of a sequence of finite "theories." We obtain each finite theory by putting in a cutoff, L, on all the original theory's integrals. We then have one member of the sequence of theories for each value of L, and the sequence is composed of all the L theories, for L increasing indefinitely. When we take the  $L\to\infty$  limit, we get the full theory, which we can now think about as a kind of ideal limiting "object," each part of which is an ideal "limit point," to be understood in terms of the sequence of corresponding parts in the finite theories. So, if  $L' < L'' < L''' < \ldots$  is an unbounded, increasing sequence of values for L, we consider the theories in which we have

$$\begin{array}{rcl} m_{r} & = & m_{0}(L') - \int_{0}^{L'} g_{d}(k)dk - q \int_{0}^{L'} g_{f}(k)dk \\ \\ m_{r} & = & m_{0}(L'') - \int_{0}^{L''} g_{d}(k)dk - q \int_{0}^{L''} g_{f}(k)dk \\ \\ m_{r} & = & m_{0}(L''') - \int_{0}^{L'''} g_{d}(k)dk - q \int_{0}^{L'''} g_{f}(k)dk \\ \\ \vdots \\ \\ m_{r} & = & m_{0} - \int_{0}^{\infty} g_{d}(k) - q \int_{0}^{\infty} g_{f}(k)dk. \end{array}$$

I want to think of talk about  $m_0$  as tacit talk about the sequence of finite numbers  $m_0(L')$ ,  $m_0(L'')$ ,  $m_0(L''')$ , ...; and talk about  $\int_0^\infty g_d(k)dk$  as tacit talk about the sequence of finite numbers  $\int_0^{L'} g_d(k)dk$ ,  $\int_0^{L'''} g_d(k)dk$ , and so on. This construal enables us to make perfectly good sense of the idea that the difference between two infinite numbers is a well-defined finite number. When q=0 (so that all terms in the right most column drop out), we want to say that in the limit  $m_0 - \int_0^\infty g_d(k)dk = m_r$ . We take this to mean that in each of the finite theories, given by the values  $L = L', L'', L''', \ldots, m_0(L) - \int_0^L g_d(k)dk$  has the constant value  $m_r$ , the observed value of the mass. When  $q \neq 0$  we say almost the same thing. We say that the difference between the

infinite quantities,  $m_0 - \int_0^\infty g_d(k)dk$ , has the value  $m_r + q \int_0^\infty g_f(k)dk$ , the observed mass plus the radiative correction,  $q \int_0^\infty g_f(k)dk$ , meaning thereby that in each of the finite theories  $m_0(L) - \int_0^L g_d(k)dk$  has the value  $m_r + q \int_0^L g_f(k)dk$  where in this case  $q \int_0^L g_f(k)dk$  converges to the well-defined limit,  $q \int_0^\infty g_f(k)dk$ , as  $L \to \infty$ .

This completes our sketch of one way of thinking about renormalization. A few details remain. What, one may wonder, if we don't measure mass at q=0? Suppose we measure mass at some value of q, say  $q'\neq 0$ . Then we absorb  $\int_0^\infty g_d(k)dk+q'\int_0^\infty g_f(k)dk$  into the renormalized mass and leave  $(q-q')\int_0^\infty g_f(k)dk$  behind as the radiative correction. But in this situation we also have a different value of measured  $m_r$ . The measured mass is not really a constant but varies with the measurement context as described by q. Therefore, if we measure mass at a value of  $q\neq 0$ , we have both a radiative correction and a value for  $m_r$  which differ from the values obtained for q=0. One can prove that these differences exactly cancel.

This last point resolves any possible problem involved with the idealization of taking q=0. When we measure mass, q is never exactly zero. But, no matter, since the departure from zero is exactly compensated by a shift in the radiative corrections. In practice, there is no detectable difference between the value of  $m_r$  obtained for the values of q describing actual mass measurement situations and the q=0 limit.

The relation between the observed mass and the parameter, q, point toward some considerations that have become extremely important in recent work in quantum field theory. Let's review for a moment. Conventionally, we measure mass when q=0. Then, for each cutoff, L, we have

 $m_r = m_0 - \int_0^L g_d(k)dk.$ 

But we don't have to measure mass when q=0. If we wanted to, we could decide to measure mass for some value of  $q\neq 0$ . We would then have

 $m_r = \left[m_0 - \int_0^L g_d(k)dk\right] - q \int_0^L g_f(k)dk.$ 

In short, the mass we measure is not really a constant but varies with the parameter q, which describes the amount of momentum exchanged in the measurement interaction or, alternatively, the proximity to which we push the interacting particles up against each other.

This kind of dependence of a constant on an *impact parameter* holds not only for mass but also for charge and other coupling constants in quantum field theory. Dependence of these constants on q is what quantum field theorists mean by the expression running coupling constant. In quantum chromodynamics (QCD) the q dependence of the coupling constant (the "charge") turns out to be very important. In QCD the coupling constant actually gets weaker as quarks get closer together, which results in the quarks' behaving like free particles when they are very close to each other (physicists' slang for this phenomenon is "asymptotic freedom"). It is also believed that as quarks get farther and farther from each other, the coupling constant gets bigger and bigger, without limit. This results in making it impossible to get free quarks (physicists' slang: "confinement").

The fact that the QCD coupling constant increases without any limit as  $q \to 0$ , means that unlike the case for the mass and charge of electrons, we have no natural value of q on which conventionally to settle to fix the QCD coupling constant's "observed" value. Instead one needs to take into account the whole functional dependence of q and the QCD coupling constant in treating renormalization in QCD. This functional dependence has been named the  $renormalization\ group$ , and its study plays a very important role in QCD and other modern quantum field theories.

#### RENORMALIZABILITY

Recall our starting point. We were looking for increasingly accurate approximations to the correct theory by successively adding first-, second-, third-, and higher-order corrections to a fictitious free theory. These corrections include the radiative corrections we have just been talking about as a small, finite part. So far, we have looked only at the first and second terms in the sequence of approximations to the correct theory. In our next step we calculate the third-order corrections. These bring in new divergent integrals, very like those we encountered in the second-order corrections. But we can absorb these into  $m_r$ , just as we did for the second-order corrections, now leaving behind further radiative corrections. We proceed similarly for higher-order corrections.

Proving the renormalizability of quantum electrodynamics involves proving that, order by order, all divergent integrals can be absorbed in

this way. In many respects the details are much more complicated than what I have presented in my streamlined outline of the underlying conception of renormalization. In particular, from the beginning we absorb divergent integrals not just into the electron's observed mass but also, in a similar way, into its observed charge. This illustrates the general point that in renormalizing a theory we may avail ourselves of more than one constant, the value of which we may fix by observation, as the locus for absorbing divergent integrals. But obviously this kind of strategy won't do us any good if we require infinitely many constants to absorb all the problem integrals. Thus we say that a theory is renormalizable if we require only a finite number of observable constants to use in the procedure.

This requirement makes proof of renormalizability highly nontrivial because each order in the sequence of approximations introduces new divergent integrals. Thus, armed with only a fixed, finite number of constants to use, we must prove that for each n all the divergent integrals that occur in orders 2 through n can be collected together in a form that allows absorption into the finite number of observable constants. In terms of my simplified model in which we use only the electron's mass, we would have to show that for each order n, all divergent integrals that occur in orders 2 through n can be collected together into an expression,  $J_n$ , such that  $S_n = S_n(m_0 - J_n) = S_n(m_r + \text{ finite corrections})$ .

Although the nontriviality of the renormalizability requirement makes renormalizability hard to prove, it also means that renormalizability provides a valuable constraint on new theories, greatly narrowing the field left by other constraints. In fact, requiring renormalizability proved to be important in formulating quantum chromodynamics, the quantum field theory for quarks, and electro-weak theory, which gives a unified treatment of weak and electromagnetic forces.<sup>1</sup>

# How Should We Regard Renormalization? Three Alternatives

I now want to turn to the general issue of how to interpret the renormalization strategy of avoiding divergent quantities. I will discuss three distinct stances that people have taken.

<sup>&</sup>lt;sup>1</sup>Weinberg (1980) describes the role of renormalizability in developing the electroweak theory. See especially pages 517–20.

First, one might take the attitude that I (following the informal gloss in many texts) used in my initial presentation. In what I will call the cutoffs approach to understanding renormalization, we make everything finite by putting in cutoffs. We absorb the quantities that, although now finite, will diverge in the limit, and only then do we take the limit. In thus thinking of the absorption of potentially divergent quantities as taking place before taking the limit, we describe an unambiguous and mathematically consistent scheme for arriving at all observable quantities.

Although there is nothing mathematically suspect about the procedure thus described, I have acknowledged that one may sensibly raise questions about the route to this scheme from fundamental theory. Because divergent terms occur along the way, one suspects some problem with the fundamental theory, the method of approximation, or the two taken together. In addition, one would like to say more about the conceptual underpinnings of the scheme. I have tried to gloss this matter in terms of "ideal limiting objects," to be unpacked in terms of the (mathematical) limiting processes to which one, at least tacitly, appeals. Although we have nothing strictly incoherent here, one would like much more detail.

Some quantum field theorists seem to reject the cutoffs approach. As I understand their dissatisfaction, these people regard the intermediate theories, formed with the finite cutoff integrals, as fatally flawed. Such theories do not satisfy several important theoretical constraints. Such theories are not gauge invariant. They are also not unitary, so that they do not conserve probability. The technical details of these constraints need not concern us. The point that matters for us is that those skeptical of the cutoffs approach apparently reject reliance on the cutoff-formulated theories as a conceptual or logical intermediary. Consequently, on a second view, we must understand the conceptual situa-

<sup>&</sup>lt;sup>2</sup>Feynman (1963), p. 145, seems to express this attitude. See also Feynman (1985), pp. 128, 129 n. 1. In attributing the view I have just expressed to Feynman, I must acknowledge some interpolation on his actual words. He clearly feels there is something mathematically problematic about renormalization. The cutoffs approach seems to make a mathematically consistent calculational scheme out of renormalization. The attribution I make to Feynman is a reconstruction that attempts to square his charge of "mathematical illegitimacy" with the consistency of the cutoffs approach.

Dirac also seemed to think that the divergences entail serious questions about the logical status of quantum field theories. See Dirac (1973), pp. 752 and passim, and (1983), pp. 52–55.

tion entirely in terms of the theory with no cutoffs. According to what I will call the *real-infinities* approach, we seem stuck with understanding renormalization in terms of the mathematically inconsistent procedure of "throwing away" real infinities.

To put this argument into its full context, I must correct a simplification I have used up to this point. So far, I have talked about making the divergent integrals finite by putting in a cutoff on the upper bounds of integration. But there are other ways of making these integrals finite or, as physicists say, of regularizing the divergent integrals. Cutoffs provide the most unsatisfactory method of regularization; I have used them only because they are the simplest to explain. Other methods alter the integrand of a divergent integral so as to make the integral well-defined when integrated all the way from zero to infinity. The alteration depends on a parameter (which we can again call 'L'), so that when this parameter goes to infinity, in the limit we get back the original integrand.

Yet another method, called dimensional regularization, begins by finding an expression for a problematic integral over one, two, three, or some other number of dimensions, expressed as a function of the number of dimensions, d. We then generalize this expression so that d can take on any value. For most values of d, the expression is finite; in particular it is finite for  $4-\epsilon$ . For d=4 we get back our original divergent integral. Thus the expression with  $d=4-\epsilon$  provides a regularization of the original integral, so that  $\epsilon$ , going to zero, does the work that L, going to infinity, did in the other forms of regularization.

In general, a method of regularization is a procedure for replacing ("regularizing") the divergent integrals with nondivergent expressions. These expressions are in turn a function of a parameter (to which we will always refer as 'L') so that in the limit as L goes to infinity (or zero) we get back the expressions for the original divergent integrals. We also say that if the theory has had its divergent integrals replaced by regularized integrals, the theory has been regularized.

I can now more completely state the argument for the alternative of "discarding real infinities": For any known method of regularization, the regularized theory fails to have some property that physicists argue an acceptable theory must have. The regularized theories always seem to fail of Lorentz invariance, gauge invariance, or unitarity (conservation of

probability). Consequently, an understanding of renormalization cannot lean on regularized theories as a conceptual or logical intermediary.<sup>3</sup>

Some physicists have told me that dimensional regularization is "completely satisfactory." Dimensionally regularized theories can be made to satisfy gauge properties particularly thoroughly—indeed, this is why the method is so useful. But even here one can raise questions. Is a dimensionally regularized theory unitary? I don't know the answer. Does a dimensionally regularized theory satisfy Lorentz invariance? It's hard to say what the question even means. What does one mean by a coordinate transformation among a fractional number of dimensions? Even when one uses dimensional regularization, the regularized theory cannot be correct. (See Nash [1978], pp. 97–98, for further technical worries.)

Given the current state of mathematics, the real-infinities alternative makes no sense as it stands. One cannot just throw away infinities, much less separate out a finite part and then throw away the infinite remainder. But one could easily take this as a comment about the current state of mathematics. We have precedents for physicists' introducing fruitful techniques that seemed crazy in the light of the mathematics current at the time. In the past such problematic techniques stimulated new developments in mathematics in terms of which the techniques became intelligible, developments that proved to be mathematically very important in their own right. The development of the calculus provides the best example. Similarly, Dirac's use of his delta functions helped to stimulate the development of distribution theory. Possibly one can develop this approach to renormalization by appeal to Conway numbers (Conway [1976]). (I have not investigated this possibility inasmuch as I find the real-infinities alternative less plausible than other approaches.)

The argument for the real-infinities alternative certainly does not force the issue. The regularized theory is supposed to be only an approximation to a strictly correct theory. An approximation need not meet all the

<sup>&</sup>lt;sup>3</sup>Again, see Feynman, (1963), p. 145. See also Sakurai (1967), pp. 294–95. These comments were written before the development of dimensional regularization.

<sup>&</sup>lt;sup>4</sup>The conceptual point made with delta functions does not cut as cleanly as when made with the calculus. Von Neumann's formulation of quantum mechanics showed that one can avoid reliance on delta functions. But on von Neumann's formulation, continuous quantities never take on exact values. One can also argue that this facet of von Neumann's formulation is just right (see Teller, [1979]). I need not sort through these complications, however, to make the point that the computational usefulness of delta functions provided a stimulation for the development of distribution theory.

In light of these facts, it is ironic—and puzzling—that Dirac was apparently untroubled by mathematical questions about his delta functions yet deeply disturbed by the logical status of quantum electrodynamics!

conditions, or theoretical constraints, that we expect a correct theory to meet; it is, after all, supposed to be only an approximation. As such, it need only satisfy the theoretical constraints that one actually uses in manipulating the approximation. For example, if we want to simplify an expression appearing in an approximation by appeal to a constraint, the approximation must satisfy the constraint. It is precisely because regularization by a cutoff did not satisfy constraints (gauge invariance) to which one needs to appeal in proofs of renormalization that physicists searched for a more sophisticated means of regularization. The claim that dimensional regularization is "completely satisfactory" means that it satisfies all the constraints one needs to use in formal manipulations of the theory. But if describing the world as having four space-time dimensions counts as a constraint on a true theory, a dimensionally regularized theory hardly meets all constraints on a true theory! But then again, why should it meet unused constraints if it is an adequate approximation for the tasks at hand?

I want to mention another question about the argument for the real-infinities alternative. The argument was that alternative attitudes to-ward renormalization appeal to some method of regularization, but no regularized theory satisfies all known theoretical constraints. However, this is true only for all presently known methods of regularization. Some facts, I will argue shortly, indicate that we can expect there to be a regularization scheme that meets all theoretical constraints.

Yet one more reason for unease about the real-infinities approach arises when we press to understand it more clearly. According to the real-infinities view, we are asked to think of an infinite self-interaction as balanced by an infinite bare mass or bare charge. But how, for example, should we understand the idea of an infinite bare charge?

On pressing the question I got the following response from one real-infinities advocate. His answer turns on further consideration of how it happens that coupling constants are not really constants but vary as a function of the parameter q. I will need to appeal to the fact that in the case of the electric charge the effects of self-interaction are spread out in space around the charge, so that if one probes very close to the charge, part of the self-interaction effects are avoided. The parameter q measures the amount of momentum exchanged in an interaction. Consequently, if q and the momentum exchanged are large, one probes very close to the charge. Very close to the charge one passes "inside" part of the self-interaction effects, and one sees a larger charge.

The real-infinities advocate used these facts to explain to me one thing one might mean by an "infinite" bare charge. He pointed out that experiment confirms the electric charge to be larger when detected from close up. Now suppose, he suggested, that this phenomenon were true without any limit. Suppose that the closer we go, and so the more we get inside the shielding effects of the self-interaction, the larger the detected charge, without any upper bound or limit. If we now add the view that it makes no sense to talk of literally sitting right on top of a point charge, we have an interpretation of talk about an infinite bare charge in terms of the way we would see, without bound, a larger and larger charge as we probe closer and closer. But this analysis starts to sound just like my earlier proposal for clarifying the cutoffs approach. We need to question more clearly whether one can in fact sustain the real-infinities approach as a distinct way of regarding renormalization.

Let me review and summarize. The argument for the real-infinities approach turned on the assertion that we cannot lean on regularized theories as a conceptual or logical intermediary. But when one presses to understand the real-infinities approach, understanding it in terms of the regularized theories seems, at the moment, to be the only promising candidate. In addition, I have argued that as approximations there is nothing wrong with at least some regularized versions of the theory. At best, the real-infinities advocate can insist that we not appeal to regularized versions to understand the full theory just because the regularized versions are approximations, no matter how satisfactory they are as approximations. This could be a telling reason only if the full theory were itself not an approximation. Yet we know such a claim is wrong. Indeed, the approximative nature of the full theory forms the basis of a third approach to renormalization.

This third attitude begins by noting that the divergent terms are momentum integrals over all momenta, hence over indefinitely large energies. Thus we should take these integrals seriously only if we believe in the theory at all energies. But we know, for example, that quantum electrodynamics must break down at very high energies. Quantum electrodynamics describes only the electromagnetic force. But at high enough energies, an accurate theory will have to take into account other processes, involving other forces. If nothing else, at sufficiently large energies, gravity becomes important, and quantum electrodynamics does not recognize gravity.

The theory must be, at best, an approximation to some divergence-free

"correct theory." Therefore we should keep only the reliable part of the theory and discard the parts in which the theory's implicit assumptions break down. The momentum integrals give a good approximation to a correct description only for energies less than some (large) bound, L. So we should keep only the integrals integrated from zero to L and discard as unreliable the L to infinity parts of the integrations. Or we should use some other means of regularization, which essentially has the same effect. (For the moment I streamline the presentation by talking exclusively in terms of a cutoff regularization with cutoff parameter, L. But everything I say applies in the same way to other means of regularization.)

But we have a problem. We don't know what value to assign to L! Consequently we cannot use any cutoff-dependent expressions. On this view, renormalization functions as a prescription for extracting cutoffindependent quantities from an approximate theory. According to what I will call the mask-of-ignorance approach to understanding renormalization, there is a correct theory that is completely free of infinities. Quantum electrodynamics and other quantum field theories only approximate this correct theory. The approximation breaks down at high energies, and so we must discard the high-energy ends of the momentum integrals. But we don't know exactly where to put the cutoff. Consequently we restrict attention to quantities that will be accurate no matter what the cutoff, as long as it is large enough so that the cutoff terms for the radiative corrections differ negligibly from their limiting values. In this way, throughout the whole discussion we deal only with finite quantities, and talk of letting the cutoff go to infinity at the end should be understood as not knowing exactly what cutoff to use.

Earlier, I suggested that there might be a regularization scheme that met all theoretical constraints. To see why this is plausible, we need to suppose, in the spirit of the mask-of-ignorance approach, that the "correct theory" is completely free of divergence problems. Let us also suppose that our theory is a good approximation to this correct theory. These assumptions make very plausible the supposition that the correct theory has finite expressions that agree closely with the problematic momentum integrals in the domain of integration for which our theory

<sup>&</sup>lt;sup>5</sup>The shudder quotes around "correct theory" indicate that the following discussion does not turn on whether or not one takes seriously the idea of an ultimately correct theory. One needs here no more than the assumption that one can construct a better theory that better accommodates experimental facts and that, furthermore, is free of divergence problems. In what follows the reader should note this aspect of the argument by understanding "correct or more nearly correct theory" where I have, to streamline exposition, written "correct theory."

provides a good approximation to the correct theory. These finite expressions in the correct theory then constitute the desired regularization, meeting all theoretical constraints.

True, we don't know the correct theory. So we don't know the fully satisfactory method of regularization. But this does not matter as long as we restrict attention to expressions that are independent, not just of the cutoff, but also of the regularization scheme we use. The renormalization procedure accomplishes just that. It provides expressions that are independent not only of the exact value of the regularization parameter but also of the details of the regularization scheme itself.

In summary, the mask-of-ignorance view of infinite renormalization starts from the assumption that the correct theory is finite through and through. Something goes wrong with our approximation to the correct theory. Divergences occur. We would like to circumvent these divergence problems by using a regularized version of our theory as the approximation to the correct theory. But we can't do that because we do not know the exact value of the regularization parameter or, indeed, even which regularization scheme we ought to use. Renormalization circumvents this problem by extracting for us experimentally observable quantities that are independent of the details of the regularization scheme, and in particular of the regularization parameter. In our approximation renormalization enforces independence from details of regularization and the value of the regularization parameter.

I note here in an aside that even if we had a fully satisfactory method of regularization in a theory like quantum electrodynamics, we would still need a finite renormalization. For in such a situation a (now-finite) self-interaction would still contribute to the total mass and charge. But this total mass and charge are all we can observe. Therefore we would still have to follow the whole procedure of adjusting the unobservable bare mass and charge with self-interaction terms to get the observable, renormalized mass and charge.

<sup>&</sup>lt;sup>6</sup>Physicists repeat, almost as a slogan, the view that the bare mass and charge are in principle unobservable. This seems plausible in a theory like quantum electrodynamics, but in quantum chromodynamics, in which the idea of a running coupling constant becomes more prominent, the issue of observability becomes more complex. In this context the distinction between bare and renormalized constants wavers, and the issue of in principle unobservability might be found to evaporate. The whole issue, incidentally, is quite separate from the alleged unobservability of quarks. Claims of quark unobservability mean that one cannot observe *free* quarks. But that leaves the possibility of observing bound quarks quite open. Indeed, some want to interpret deep inelastic scattering experiments in just this way.

It is worth examining one prima facie puzzling feature of the mask-of-ignorance approach. How, one might wonder, can renormalizability, understood in this way, function as a constraint on theory construction? Recall that renormalizability was the requirement that all divergent terms can be absorbed into a finite number of observable constants. The requirement that a theory have this property severely narrows the field of options left by other constraints, such as gauge and Lorentz invariance. But how are we now to justify the requirement of renormalizability as a constraint that approximate theories must satisfy? If, as the mask-of-ignorance view claims, the correct theory has no divergent terms, the constraint (that all divergent terms be absorbable) cannot be justified on the ground that a true theory (or all more accurate theories) must satisfy the constraint. So why should we believe that this constraint will guide us toward better theories?

The answer turns on the fact that observationally determined parameters can fill gaps left by theoretical ignorance. We are ignorant inasmuch as we do not now have access to the correct theory. Some members of the physics community have expressed enthusiasm for a new candidate for an exact theory—a "superstring theory"—but this speculation is far from well confirmed; and even if it should turn out to be correct, we still do not have access to the theory, in the sense that at this point physicists can get almost nothing out of it by calculation. When we do not have access to an accurate theory, we have to make do with a less complete surrogate. One way we can do this is to let observationally determined values of parameters fill in where we would have liked to calculate them from deeper principles.

An analogy will help. In principle one might aim to calculate the spring constant of a spring by applying basic theory to the spring. But in practice this is too hard. Not so long ago, the needed theory was not available even in principle. So we just measure the spring constant and use it in Hooke's law to describe the spring's pattern of oscillation.

We can see a theory like quantum electrodynamics as similarly related to a more correct theory. We measure the electron's mass and charge. This gives us information that we cannot get theoretically. In particular, by measuring the mass and charge we make it unnecessary to be able accurately to evaluate those integrals that in our current approximation we can't even get to come out convergent. Or better, by measuring the electron's mass and charge we obtain the information that we otherwise

would have had to obtain by determining the current theory's domain of validity, that is, by determining a correct regularization scheme and value of the cutoff or regularization parameter.

In building new theories, we want to employ the same strategy. We want an approximate theory simple enough so that we can calculate experimental outcomes. The best we can do (presently) involves making observed values of some parameters do the work that ideally we would like to be able to calculate from first principles. But this strategy will not work if we have to determine infinitely many parameters by observation. So we require our approximate theory to be renormalizable; that is, what sopping up of theoretical ignorance we do is done with finitely many observationally determined parameters.

In quantum field theories, this comes to making the theories insensitive to the details of what happens at very high energies. When the energies get too high, our theories seem to sour. But we don't know exactly why or exactly at what energies. Consequently we make our theories independent of these considerations by covering the problematic terms in the theory with observationally determined quantities. Again, this strategy will work only if we can do it with finitely many observationally determined quantities.

I need to emphasize here that for working physicists truth is not good enough. They must have calculability. Indeed, if a choice must be made, often physicists must abandon truth and seek calculable theories that provide adequate approximations. Renormalizability, as a constraint on theories, guides us toward calculable approximations.

#### Conclusion

Many physicists who teach renormalization insist that there is nothing problematic about it. My survey tends to bear out that opinion. But quantum field theories may have other difficulties. In particular, the whole package of theory plus method of application is satisfactory only if the renormalized sequence of approximations converges. No one really knows one way or the other. All we know for sure is that the first few terms give breathtakingly good agreement with experiment.

But that is another issue. I have indicated three ways of thinking about renormalization as it is used by physicists:

- 1. The cutoffs approach: We regularize, absorb finite, regularized quantities, and then take the limit.
  - 2. The real-infinities approach: We literally discard real infinities.
- 3. The mask-of-ignorance approach: We understand renormalization as a way of covering our ignorance of how present false theories approximate a correct, completely finite theory.

I have suggested some reasons for choosing among 1, 2, and 3. But nothing I have said comes close to being conclusive. When I talk with physicists I sense that the majority favor the mask-of-ignorance approach. As I mentioned, some members of the physics community think they may have discovered a "correct theory"—what they refer to as a "superstring theory." One might wonder, if this or some other finite theory proves successful, whether infinite renormalization will be quietly put aside as an embarrassing chapter in the history of physics. This fate seems most unlikely, for reasons that should be clear from what I have said about renormalization as a constraint on theories. In most of their work physicists labor to extract information from unsolvable and imperfect theories. As a particularly ingenious method for extracting such information, renormalization will always remain an exemplar of good physics. Moreover, future theories may be very hard, perhaps even in practice impossible, to calculate. (Superstring theories, some say, may suffer this fate.) In such a case we will always need infinite renormalization to extract usable information. Most likely, infinite renormalization will always have a valued place in the tool kit of physics.<sup>7</sup>

#### PROBLEMS

1. Examine whether the real-infinities approach can be clearly distinguished from the cutoffs approach, and if so in what way.

 $<sup>^{7}</sup>$ The reader may be interested in some further aspects of renormalization, which I discuss in Teller (1988).

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