Seventeen Simple Lectures on General Relativity Theory

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PREFACE

Over the years, while seeking instruction from standard textbooks on basic notions underlying the general theory of relativity, I have been confronted with uncertainties and perplexities at every turn. These detailed lecture notes are a by-product of a determined attempt to clarify, if not to resolve, at least some of them. Although they can serve as a self-contained introduction to the subject, their alternative purpose is to function as a salutary supplement to many of the conventional introductory expositions which are available. In other words, though the raw novice may profit much from them, they will perhaps be of greater benefit to those who have already taken a first, or even a second or subsequent course in the subject. They are naturally somewhat unorthodox in style, not only in their discursiveness, but also on account of the intermingling of problematic aspects of the particular theory in hand with those of physical theory in general. Questions are raised in large numbers. Many of these remain unresolved, whereas the answers to others are surely naive at times, for I am not a philosopher of science. If doubts, uncertainties, and preconceptions are constantly displayed; if at first sight the usual introductory mathematical material seems to be missing; if the words gravitation and curvature effectively make their appearance for the first time in the last lecture; if attention is focused at length on the role played by metalaws or regulative principles in the establishment of the theory: all these and other unfashionable features are matters of pedagogic intent. Still, much of the material usually to be found in introductory accounts is also included, even if here, too, pedagogic motivations have brought a number of modifications with them. For example, in the context of spatial spherical symmetry the vacuum equations are solved so as to give directly what is in effect the Kruskal-Szekeres metric, without an excursion via the Schwarzschild metric; a singular energy tensor associated with this vacuum metric is explicitly exhibited; interior solutions are exemplified by a metric representing a static gaseous sphere whose equation of state makes physical sense, unlike that of the Schwarzschild interior solution, which does not; the Kerr solution, instead of being merely quoted, is derived; and so on.

Inevitably the balance of the material included and the extent to which emphasis is placed on this topic rather than that may seem either harmonious or bizarre, depending on one's point of view. If they be regarded as bizarre I can only plead that these are lecture notes which bear a strong personal imprint of my style of teaching. They certainly are not intended to fulfill the purpose of a comprehensive textbook in which one would look for a detailed reference to every aspect of the theory and its empirical implications, to every formalism, to every mathematical device which might be relevant to it: their purpose is different, as a glance at the list of contents will confirm. By the same token, to describe them as "simple" seems to me to be appropriate, granted that this is not taken to imply a claim that they are therefore necessarily "easy." Of course, a specialist working on some branch of general relativity theory will find what I may say about it "easy," even superficial, and find any warnings not to take this or that for granted superfluous, for he or she will be well aware of the difficulties involved. Yet not everyone is a "specialist," and many casual conversations over the years have convinced me that I am not alone in being plagued by all manner of perplexities.

The beginning of the course may well be felt to be a greater hurdle to be overcome than the rest and therefore the first two lectures are shorter than the other fifteen which are of roughly equal lengths. They contain, I think, no technical material which has not previously appeared in the literature, but, since this is no textbook, I considered it inappropriate to give explicit references. None of the many quotations are ascribed to specific authors, for I may have misunderstood them. However, all the quotations in question are taken from one or another of the books listed at the end of the last lecture. Throughout I am concerned solely with the orthodox relativity theory: no alternative "theories of gravitation" are contemplated, nor, for good reasons, are implications of quantum mechanics taken into account.

As regards prerequisites, these are fairly modest. A general background of physics is of course taken for granted, but that is not to say that familiarity with all of the theories which are briefly mentioned is essential. If one has merely a cursory idea of what, say, the Born-Infeld electrodynamics or process thermodynamics is about, so much the better; if not, one can manage without this knowledge. On the other hand, on a more specific level, I assume, first, a fairly sound acquaintance with the special theory of relativity and its language and, second, a sound knowledge of Euclidean tensor calculus, characterized by the constancy of the components of the metric tensor which need be neither diagonal nor positive definite. As regards notation, it seemed best to set out relevant explanations in a separate appendix. This serves the dual purpose of providing a reminder of the terminology of Euclidean tensor calculus. No knowledge of non-Euclidean tensor calculus is assumed, the relevant mathematics being developed when it is required.

The idea that the preparation of an appropriate course of lectures might help to ameliorate some of my difficulties first occurred to me during a contemplative stay of a few months at the Weizmann Institute of Science, Rehovot, late in 1975. I take this belated opportunity of thanking the Institute and Professor Yigal Talmi in particular for the warm hospitality I received there. The opportunity to put the intention into effect finally came in 1979 while I was resident as a Fellow at Churchill College, Cambridge. The facilities of the Institute of Astronomy were made available to me by Dr. Martin Rees and for this I am most grateful to him. To Professor Hubert Goenner I owe a great debt, for despite other pressing commitments at the time he read the original version of the manuscript. His many comments, whether critical or supportive, were of very great value to me. I took most of them into account as best I could, but I know that much remains of which he disapproves and for this I must take the entire blame. I also derived much benefit from various remarks made by several anonymous referees.

H. A. BUCHDAHL

Cambridge, November 1979
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on
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CONTENTS

LECTURE 1

To begin these lectures I should surely first say something about their intended content and character. Straight away I am faced with a difficulty. Supposing I were to say something like this: "The objective of this course is to give a simple, introductory account of the general theory of relativity, a theory intended to form a framework for the description of physical systems including their gravitational interactions, in contrast with the special theory of relativity in which the latter is disregarded. The examination of underlying conceptions will take precedence over the presentation of self-contained mathematical structures or of extensive formal developments." Then although this account would reflect true intentions, no matter how inadequately, it is not really acceptable, for it uses a language which should not yet be used. There is explicit reference to "gravitation," yet this term is not to be introduced until much later—in the seventeenth lecture—when we shall be in a better position to understand its proper connotation, at any rate on the view taken here. Morover, granted familiarity with the special theory of relativity, so-called, the phrase "general theory of relativity" conveys no obvious meaning to the uninitiated. True, there is a manifest verbal implication that the "general" theory is some generalization of the special. Yet, in what sense is it a generalization, if it be one at all?

I may seem to be belaboring this question to some extent, but without further ado I choose it as an appropriate starting point. As soon as one tries to answer it one is confronted with new questions which demand an answer. Not only that, but one begins to realize just how much various contemporary attitudes which surround the general theory are influenced by all manner of preconceptions, be they semantic, epistemological, ontological or even ideological. By way of just one illustration, how might one react to the views expressed by one author who refuses with polemical vigor to call Einstein's later theory "the 'general theory of relativity' because the latter name is nonsensical" and who holds that Einstein did not properly understand his own theory because he failed to see "that in the new theory he had created the notion of relativity was not among the concepts subject to generalization"?

To begin with, a theory may be held to be a generalization of another theory under various circumstances but to characterize these is no simple matter. Consider some examples, each time contrasting a certain "special" theory with what would normally be agreed to be a generalization of it: Bohr's theory of the hydrogen atom and the theory of atomic spectra based, in part, on the Bohr-Sommerfeld quantum conditions; the geometrical optics of isotropic media and that of anisotropic media; equilibrium thermodyamics and process thermodynamics; Maxwell's theory and the electrodynamics of Born and Infeld. It suffices to take all these theories for granted here without, for the time being, subjecting the notion of a theory as such to closer scrutiny. At any rate, each of these pairs of theories, whatever the generic differences between them, satisfies two criteria: (1) there is a strong family resemblance between the conceptual frameworks of the two theories making up a given pair; and (2) under specifiable circumstances the "general" that is, generalized, theory "collapses" into the special theory, though possibly only in the sense of a limiting case. We might now be tempted to say of *any* pair of theories that one is a generalization of the other if and only if they jointly satisfy both criteria. The trouble with this proposal is that there is rather too much vagueness in the notions of "family resemblance" on the one hand and of "conceptual framework" on the other. Let us, however, not be sidetracked into trying to remedy these deficiencies now. We have at least two tentative criteria in hand for our use; and we fix firmly in our minds that *both* are to be satisfied. In particular, it is not enough for (2) alone to be satisfied. It would seem to me to be quite wrong to regard nonrelativistic quantum mechanics as a generalization of Newtonian particle mechanics, notwithstanding the existence of the correspondence limit. It would likewise be wrong to speak of statistical mechanics as a generalization of phenomenological thermodynamics, notwithstanding the existence of a thermodynamic limit. On the other hand the waters already begin to be slightly murky when one asks whether perhaps special relativistic particle mechanics might be regarded as a generalization of Newtonian particle mechanics.

With these rough and ready ideas in hand it is time to return to the question which gave rise to them in the first place. To answer it we first need to have an understanding of each of the two theories to which it refers. Why then are they known as the special and general theories of relativity? It is hardly good enough to say "because Einstein called them that." Why did he? What are they about? Why "special," why "general"? What is the connotation of "relativity" in each case, if "relativeness" is its dictionary meaning? Furthermore, if "relative" is taken to mean "not absolute" we are faced with various possible meanings which "absolute" might take. Finally, why do we here speak of theories? Perhaps, in some preferred sense of the term "theory," either the special or the general theory or both are not simply theories at all?

These, then, are some of the more specific question which confront us. To do full justice to them is, of course, not feasible within the compass of a few short lectures, if only because their ramifications are so extensive as inevitably to include problems which have been the subject of disputation for centuries. My best course of action would therefore seem to be this: after certain generic preliminaries to develop the basic outlines of the general theory while constantly bearing our unanswered questions in mind. To this end I shall neither pursue a historical approach nor feel constrained to follow contemporary fashions. With the formal framework of the theory established we can go on to consider a few simple special topics if only to round out the picture a little. Once this has been done the time will be at hand to review problems previously encountered: if not to solve them, then at least to take renewed note of them.

To start the ball rolling, let us decide whether we accept the special theory of relativity as a theory. To make any rational decision one has to have criteria on which to base it. In the case in hand we must evidently first agree as to the meaning we wish to attach to the term "theory." Of course, in doing so we should as far as possible conform to common usage. It would not do to understand by "theory" what most others would understand by "hypothesis," for example. On the other hand where there is no universal agreement how are we to know what is common usage? We have to be hardheaded and simply select one of the more commonly held positions relevant to physics. It goes something like this: a theory is an explanatory framework which consists of two parts. The first is an abstract logical calculus, a formal, for example mathematical, system. It consists, on the one hand, of sentences which involve primitive symbols and derived symbols defined in terms of these; and, on the other, the vocabulary and syntax of logic. Some of the sentences are taken as axioms, the rest are theorems deduced from them. This formal scheme defines the logical structure of the theory; but it has as yet no empirical content. This is supplied by the second part of the theory which is a set of rules, variously called "rules of interpretation," "coordinating definitions," and the like. They are essentially semantical rules which provide observational interpretations for at least some of the primitive and derived terms. The theory as a whole now has empirical content: axioms function as physical hypotheses, derived theorems as physical laws, and their validity is then testable by experiment.

Now all this is of course shamefully abbreviated—no mention is made of all sorts of provisos, deficiencies, ambiguities, and so on to which this schematic picture of a theory is subject. For example, to speak of "observational interpretation" is all very well, but just what is one to understand by this? In some particular case a symbol *P* which occurs in the abstract calculus might happen to be given the observational interpretation "pressure of a gas" and this might be held to be acceptable in as far as such a pressure is accessible to direct observation, say by reading a manometer. Should such a measurement, however, qualify as direct observation? Is there not some other theory involved, namely a theory of some measurement process with *its* abstract calculus and rules of interpretation? If so, should this "secondary theory" not have been absorbed in the "primary theory" in the first place? If not, what would the situation be if *P* happened to be interpreted as "proton"? Should we perhaps contemplate hierarchies of theories? There clearly are many perplexities here: I shall simply set them aside, at least for the time being. Still, one particular sin of omission needs to be rectified at once. Earlier I used the phrase "physical law" without comment. Now I must surely say something of what is to be understood by this.

Very briefly, I here take a law to be a universal statement which asserts the existence of a certain uniform connexion, that is, a statement that a physical phenomenon of a certain kind always occurs whenever certain conditions are met. It is worth putting this in a different way. A law, as here understood, alleges that, subject to relevant stipulations and conventions, whenever certain conditions are satisfied certain consequences will be found to obtain. It covers factual as well as possible cases, the distinction between these being exemplified by the notional law "planetary orbits are elliptical": not only is every observed planetary orbit elliptical but it must be the case that anything having certain specified properties—other than that of moving about the sun in an elliptical orbit—will in fact be found to move in this way. Analytic propositions, like those of pure mathematics, are not to be considered to be expressions of laws. In general the meaning of the term "law" is context-dependent. Perhaps with tongue in cheek, let me remind you that to the uninitiated a law is more likely to be a rule of behavior laid down by legislation. In that case he may well go on to inferences concerning the existence and nature of putative legislators. One must not scoff at this, as the history of science shows. At any rate, a physical law of the kind now contemplated is usually established by inductive generalization based on the scrutiny of sets of isolated facts relating to a given class of phenomena and taking note of patterns of regularity or uniformity which they may exhibit. Again all manner of difficulties appear, and these, too, I set aside. Nevertheless, I hope that we are now in a better position to reach a decision as to whether we should think of the special theory of relativity as a "theory."

Evidently we need to ask whether its interpreted sentences function as physical laws or not. Here we must not jump to conclusions. Consider the example of the theory of particle motion when no restriction on speeds is envisaged. Though this may be called a "relativistic theory" its laws are not part of *the* theory of relativity. The latter derives from the dual recognition that (1) no empirical significance can be attached to the phrase "uniform motion with respect to space" and (2) any measurement of the speed of light in vacuo will always yield the same result. All this we shall review in due course. At any rate, one may wish to replace (1) by a statement of the kind "any frame of reference moving with uniform velocity relative to an inertial frame is inertial and the form of the general laws governing physical phenomena is such as not to imply a generic distinction between different inertial frames." Likewise the statement (2) about the velocity of light might be replaced by a prescription concerning the measurement, or instrumental meaning, of spatial distance; and so on. This need not detain us now, for, as you already know, (1) and (2) or their equivalents may be jointly subsumed under the following proposition to which I shall return later: the differential equations governing the evolution of physical systems—of things and fields—must be invariant under Lorentz transformations. With the previous standard scheme of a theory in mind we have to look upon this as an interpreted sentence. It is, however, not a law, for the very idea of subjecting its validity *directly* to an experimental test does not make sense. The same is, I think, true of any other of its interpreted sentences to the extent that these will be concerned with the spacetime vocabulary required for the interpretation of specific theories. In short, one concludes that the special theory of relativity is *not* a theory. We might perhaps call it a "regulative principle" being concerned with the form of theories. In as far as it transcends the idea of a theory as we generally understand it, we may alternatively call it a metatheory. Of course, whenever convenient we shall nevertheless continue to speak of the "special theory of relativity," keeping the conclusions just reached at the backs of our minds.

It is worth pointing out that regulative principles of one kind or another are not particularly uncommon in physics. For instance, given the differential equations of some set of theories, one may adopt the regulative principle that the equations of each theory shall express the stationarity of some action integral, or else that they be of at most the second differential order. Again, one might for some reason impose the regulative principle that every field theory should be such as to admit the introduction of a well-defined local energy density of the field. Of course, the more such principles one introduces simultaneously—sometimes for no clearly discernible reason—the more one runs the risk of eventually finding some of them to be in mutual conflict. As an aside, one may even regard phenomenological