Special Relativity

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For Aditya.

Acknowledgements

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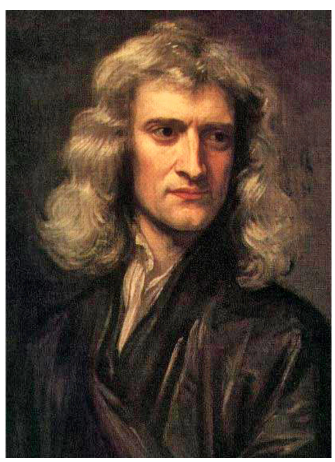
Chapter 1

Newton's laws

> It is quite difficult to present the introduction to mechanics to an intelligent audience without some embarrassment, without the feeling that one should apologize here and there, without the wish to pass quickly over the beginnings. (Hertz, 1894)



Sir Isaac Newton, 1642–1726/27

Sir Isaac Newton—the great man himself—was born in 1642, and died in 1726 or 1727. ...What? How can there be any ambiguity over something so straightforward as the year of Newton's death? In the 1600s, two calendars were in use in Europe: the Julian 'old style' calendar (originally introduced by Julius Caesar in 46 BC), and the Gregorian 'new style' calendar (originally introduced by Pope Gregory XIII in October 1582). While the Julian calendar counts the length of a year as exactly 365¼ days long, meaning that a leap year should occur ever four years, the Gregorian calendar has a more sophisticated prescription for the occurrence of leap years. Here's how the United States Naval Observatory puts it:

> Every year that is exactly divisible by four is a leap year, except for years that are exactly divisible by 100, but these centurial years are leap years if they are exactly divisible by 400. For example, the years 1700, 1800, and 1900 are not leap years, but the year 2000 is. [160]

The Gregorian calendar is now the calendar most widely used across the globe. Unlike the Julian calendar, it makes the average calendar year 365.2425 days long, thereby more closely approximating the 365.2422-day 'solar' year that is determined by the Earth's revolution around the Sun. The merit of the Gregorian over the Julian calendar is that the latter 'drifts' with respect to the solar year (because it does not as accurately line up with the latter): given enough time, Christmas in the Northern hemisphere would occur in summer according to the latter! One does not face these same issues with the Gregorian calendar: in a sense, it's better 'adapted' to salient physical events (in this case, the Earth's going around the sun); in turn, this often renders its descriptions of physical goings-on simpler (for example, the Earth will be at the same point in its orbit around the sun every year according to the Gregorian calendar, but not according to the Julian calendar). To anticipate some terminology which I will use later in this chapter: there is a sense in which the Gregorian calendar better approximates an 'inertial frame'—a coordinatisation of the world such that our description of physical dynamics is simplest—than does the Julian calendar.[1]

In fact, a central question in the philosophy of spacetime physics has to do precisely with the above issues: what does it *mean* for our physical descriptions to be 'well-adapted' to nature? Is it indeed appropriate (as assumed in the above) to regard 'inertial frames' as those in which physical dynamics simplifies maximally, or is there some other, superior way of understanding such structures—perhaps in terms of the structures of space and time themselves? These are pressing questions, to which I will return throughout this book. But they are also tangible questions: the entire set of ideas underlying them is encapsulated in the ambiguity over Newton's death year.

My purpose in this chapter is to expand upon these central themes in the foundations of spacetime theories, as they constitute essential bedrock upon which I will build my philosophical analysis of special relativity in later chapters. In order to proceed, I'll turn again to Newton—this time not to his death date, but rather to his *laws*. As the quote from Heinrich Hertz (1857–1894, famous for his demonstration of the existence of electromagnetic waves) indicates, these

[1]Of course, the Gregorian calendar isn't perfect either: this is why we must introduce 'leap seconds' and other gadgetry in order to forestall 'drift' against the solar year.

turn out to be a conceptual minefield. Grappling with how to understand the content of these laws will, however, afford exactly the right toolkit with which to address the philosophy of special relativity in later chapters.

1.1 Newton's laws

Let me begin by stating Newton's laws in what I hope is an entirely uncontroversial form. These should be familiar to anyone who has studied high school physics:

N1L: Force-free bodies travel with uniform velocity.

N2L: The total force on a body is equal to the product of that body's mass and its acceleration. ($\mathbf{F} = m\mathbf{a}$.)

N3L: Action and reaction are equal in magnitude and opposite in direction—i.e., if one body exerts a force $\mathbf{F}$ on a second body, then the second exerts a force $-\mathbf{F}$ on the first.

I'll bet that many readers are so familiar with these laws that they won't even have read the above. But I'll invite all readers to stare at these three laws for just a minute—after doing so, inevitably, a range of conceptual questions will arise. For example:

1. What does 'force-free' mean?
2. Isn't **N1L** a special case of **N2L**? So why state it as a separate law?
3. (Relatedly:) Is **N1L** supposed to be a definition, or something else?
4. In which frames of reference are these laws supposed to hold?
5. Does **N1L** presuppose **N3L**?

It's only by answering such questions that we can secure a full and clear understanding of the content of Newton's laws. But doing so has long been recognised as no easy business. To complement the quote from Hertz which I've already mentioned, here's the physicist Rigden, writing in 1987:

> The first law ... is a logician's nightmare. ... To teach Newton's laws so that we prompt no questions of substance is to be unfaithful to the discipline itself. [143]

As foreboding as the challenge of making sense of Newton's laws might seem, an honest philosopher of physics must try to make progress here—and, indeed, philosophers have engaged with these questions in a surprisingly diverse range of manners. In my view, in order to appreciate the range of options which are available in answering the above questions, it's helpful to present two approaches to Newton's laws which, in many respects, are polar opposites: these are the 'dynamics first' approach of Harvey Brown [14], and the 'geometry first' approach of Michael Friedman [60]. Indeed, I'll use these two authors (and their respective allies) as poles for navigation not just through this chapter, but over the course of the entirety of this book.[2]

1.2 Inertial frames

I'll begin with the fourth of the five questions listed above: in which frames of reference are Newton's laws supposed to hold?[3] Focusing on **N1L**, it's transparent that this law can't hold in *all* frames of reference, for envisage a force-free body moving with uniform velocity according to some temporal and spatial coordinates, then move to a coordinate system accelerating with respect to the first. In this new coordinate system, the force-free body no longer moves with uniform velocity! Thus, Newton's laws obtain only in particular frames of reference.

We can make these points more quantitative in the following way. In a given coordinate system x^μ $(\mu = 0, \ldots, 3)$,[4] suppose that the path of any free particle can be expressed as

$$\frac{d^2 x^\mu}{d\tau^2} = 0, \tag{1.1}$$

where τ is a monotonic parameter on the path in question. Integration yields

$$x^\mu(\tau) = x^\mu(0) + \tau v^\mu(0), \tag{1.2}$$

where $v^\mu(0) = \frac{dx^\mu}{d\tau}$ at $\tau = 0$, so we obtain straight-line motion in the four-dimensional manifold. *This* is the property which **N1L** tells us holds of force-free particles—so in the frames in which **N1L** holds, we have $\frac{d^2 x^\mu}{d\tau^2} = 0$.

Now perform an arbitrary coordinate transformation $x^\mu \to x'^\mu(x^\nu)$, along with an arbitrary parameter transformation $\tau \to \lambda(\tau)$. Our simple force law $\frac{d^2 x^\mu}{d\tau^2} = 0$ becomes, in the new frame, (cf. [14, p. 17])

$$\frac{d^2 x'^\mu}{d\lambda^2} - \frac{\partial^2 x'^\mu}{\partial x^\rho \partial x^\gamma} \frac{\partial x^\rho}{\partial x'^\nu} \frac{\partial x^\gamma}{\partial x^\sigma} \frac{dx'^\nu}{d\lambda} \frac{dx'^\sigma}{d\lambda} = \frac{d^2\tau}{d\lambda^2} \frac{d\lambda}{d\tau} \frac{dx'^\mu}{d\lambda}. \tag{1.3}$$

[2] One small aside before I proceed further. In presenting the above views, I make no claim that either accurately maps onto what Newton himself would have thought on his laws—which is to say, I make no attempt to engage in Newton exegesis. For some penetrating discussions on how Newton's own views align with these contemporary positions, see [128, ch. 2].

[3] For the time being, I make no distinction between a frame of reference and a coordinate system. Some authors regard the former as consisting in 'extra structure'—I'll return to this idea of 'extra structure' in chapters 5-7, but for the time being I set it aside. (For more on the difference between frames and coordinate systems, see [37].)

[4] It's standard practice in physics to use Greek indices $(\mu, \nu, \ldots)$ to range over the four coordinates of space *and* time (where the 0 coordinate is the time coordinate), and to use Latin indices $i, j, \ldots$ to range over the three spatial coordinates. I'll follow suit in this book. (Note that, up to this point, I've introduced neither the Einstein summation convention nor the notion of an 'abstract' index; I'll come to these later.)

So force-free particles *accelerate* in arbitrary frames (the acceleration is quantified by the two extra terms which have been introduced in this frame: sometimes, these are called ‘fictitious force’ terms)—they only move on straight lines in the inertial frames.

What’s crucial to note in the above is that the frames in which **N1L** holds are the frames in which the very same dynamics takes a particularly simple form. Recalling our discussion of the calendar systems from earlier, let us call the frames of reference in which Newton’s laws hold the *inertial frames* of reference. Knox, indeed, gives the following very sensible definition of inertial frames:

> In Newtonian theories, and in special relativity, inertial frames have at least the following three features:
>
> 1. Inertial frames are frames with respect to which force free bodies move with constant velocities.
> 2. The laws of physics take the same form (a particularly simple one) in all inertial frames.
> 3. All bodies and physical laws pick out the same equivalence class of inertial frames (universality). [85, p. 348]

So, Newton’s laws hold in the inertial frames of reference, which are those coordinate systems in which the dynamical simplify maximally, and in which force-free bodies move with uniform velocities. It’s important to note, though, that the above definition of an inertial frame is what’s known as a *functional* definition: it tells us the properties which we expect (or, indeed, demand) that the objects in question (here, inertial frames) possess, but it does not (as yet) afford us any independent means of identifying those objects (again, here frames), or knowing whether they exist. Indeed, it is exactly at this juncture that authors such as Brown and Friedman begin to follow different courses. Beginning with the existence question, Brown maintains that inertial frames *do* exist in nature:

> A kind of highly non-trivial pre-established harmony is being postulated, and it takes the form of the claim that there exists a coordinate system x^μ and parameters τ such that [$\frac{d^2x^\mu}{d\tau^2} = 0$] holds for each and every free particle in the universe. [14, p. 17]

On the other hand, Friedman denies the existence of inertial frames:

> Newtonian physics is (would be) true even if there are (were) no inertial frames. The First Law deals with the existence of inertial frames only counterfactually: if there were inertial frames (for example, if there were no gravitational forces), free particles would satisfy [$\frac{d^2x^\mu}{d\tau^2} = 0$] in them. [60, p. 118]

The difference between our two authors amounts to this. Friedman’s point is that no particle is *actually* force-free, so inertial frames in the strict sense do not *actually* exist. Brown, on the other hand, would reply that inertial frames at least *approximately* exist. In fact, though, Friedman anticipates this response on behalf of Brown, when he writes:

> This reply is inadequate. Newtonian physics is only approximately true, but not because of the existence of *gravity* [i.e., some universal physical force]. [60, p. 118]

The reader would be forgiven for finding this passage from Friedman puzzling at this stage. It will make more sense once we understand in more detail the differing theoretical commitments of the parties involved—for this reason, I'll defer a detailed discussion of this response until the end of the following section. For the time being, we need only note this: for Brown, **N1L** is a claim about the existence of (approximate) inertial frames in the real world; for Friedman, by contrast, **N1L** is a counterfactual statement, since in fact there are no inertial frames in the actual world. So much for the existence question. But the question of what the inertial frames *are* also remains to be addressed. To make progress here, we must turn now to the first of the question in our above list: what is the meaning of 'force-free'?

1.3 Force-free bodies

To get a better handle on what it means for a particle to be force-free, we must turn to **N2L**, which (recall) says that the total force on a body is equal to the product of that body's (inertial) mass and its acceleration. With **N2L** in mind, a natural further conceptual puzzle arises: isn't **N1L** just a special case of **N2L**, given that the former (it seems) reduces to the latter in the case $\mathbf{F} = 0$? Friedman straightforwardly gives an affirmative answer to this question. On the other hand, Brown gives a negative answer:

> It will be recalled that the acceleration $\ddot{\mathbf{x}}$ of the body is defined relative to the inertial frame arising out of the first law of motion. It is for this reason that the first law is not a special case of the second for $\mathbf{F} = 0$. [14, p. 37, fn. 9]

In other words, for Brown, **N1L** plays the crucial role of telling us *what the inertial frames are*; for this reason, and in this sense, **N1L** is not merely a special case of **N2L**. I'll come back to this, but before doing so let me explain why Friedman *does* think that **N2L** is a special case of **N1L**.

For Friedman, notions of acceleration, and of force, are to be defined in terms of a background spatiotemporal structure. (For the time being, I'll not address the question of the metaphysical status of this spatiotemporal structure, and its relation to material bodies—that is, I'll not address the substantivalism/relationalism debate (on which see [127]); I'll have more to say on this in later chapters, in particular Chapter 10.) In Newtonian mechanics, for Friedman, a particle is genuinely accelerating just in case it follows a curved path with respect to the standard of straightness of paths across time given by (neo-)Newtonian spacetime.[5] A particle is force-free just in case it does not follow a

[5] I will explain the 'neo-' prefix here, as well as the general notion of spacetime in Newtonian mechanics, in Chapters 5 and 6. I hope that nevertheless the points which I'm making here are tolerably clear.

curved path with respect to that standard of straightness.[6] This gives us a *definition* of force-freeness, *and* makes clear that **N1L** is just a special case of **N2L**. Thus, helping oneself to a background spatiotemporal structure as does Friedman affords elegant and simple answers to the questions of what it means for a body to genuinely accelerate, and what it means for a particle to be force-free. Indeed, this approach also affords a very straightforward independent definition of an inertial frame: the inertial frames are those at rest or moving uniformly *with respect to Newtonian absolute space*.[7]

Brown rejects Friedman's spacetime-based answers to the above questions, for in his view such explanations are either opaque (what exactly is the relation between spacetime structure and the motions of material bodies?) or in fact not explanations at all (if spacetime—as is the case for Brown, as we'll see—is ultimately to be reduced to the motions of material bodies and the dynamical laws governing those bodies, then ultimately I need a way of understanding notions of e.g. force-freeness with reference to material bodies only). In a sense, Brown's philosophical attitude is more *empiricist* than that of Friedman: he seeks an understanding of the notion of an inertial frame (say) directly in terms of material entities, rather than in terms of the (for him) more ethereal notion of spacetime. In fact, there's a long tradition, going back to Lange, Lord Kelvin, Tait, and others, of attempting to *empirically ground* the notions of inertial motion, force-freeness, etc. (see [4, ch. 12] for an excellent overview); Brown certainly can be situated as an ally of this tradition.

There are, indeed, a few different ways in which one might seek to define notions of force-freeness etc. in an empiricist manner, *à la* Brown. The approach which Brown favours is to take force-free bodies to be those which are sufficiently isolated with respect to all other bodies in the universe; one *defines* such bodies to be force-free, and defines inertial frames as those in which such bodies move with uniform velocities (recalling the above quote from Brown, we can now see why the fact that there exists a single frame in which all such bodies move with uniform velocities is "[a] kind of highly non-trivial pre-established harmony" [14, p. 17]) [14, p. 16]. Brown takes **N1L** to offer this prescription implicitly; any particle accelerating in such frame is then to be regarded as being subject to a genuine force, as per **N2L**. Note that, if such an approach is successful, no appeal to spacetime structure was needed to afford meaning to the relevant terms under consideration.

Brown's own preferred approach is, however, not the only means by which one might seek an empiricist grounding of the notions of inertial frame, force-freeness, etc. Another option is to be found in what's known as the 'regularity relationalism' of Huggett [76]. I don't need to get into the details of this view here; rather, a sanitised presentation of the prescription will suffice:[8]

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[6] Again, more on what this standard of straightness amounts to in Chapters 5 and 6.

[7] I don't mean to suggest that this definition is devoid of problems: there remain open questions regarding why such frames are those in which the motions of *material* bodies should simplify maximally: recall again our discussion of Newton's death date. I'll return to this issue later.

[8] I should be clear that the following is only *inspired* by Huggett's work; I don't mean to

1. Find the frame in which the dynamical equations governing the greatest number of bodies simplify.

2. By definition, these are the inertial frames.

3. Any body which follows a straight trajectory in these frames is force-free, by definition.

4. (It is a *conspiracy*—the *conspiracy of inertia*—that these force-free bodies all follow straight-line trajectories in these frames.)

5. Any body which does *not* follow a straight-line trajectory in these frames is subject to a genuine force.

6. **N1L** is not a special case of **N2L**, because the accelerations in the latter are with respect to the internal structure picked out in the former.

7. Extra forces in non-inertial frames are to be classified as 'fictitious'.

What are the relative merits of the 'Brown-style' prescription over the 'Huggett-style' prescription, or vice versa? One advantage of the latter is that it makes no initial assumption about the nature of forces in the universe—by contrast, Brown assumes that forces fall off with distances. On the other hand, Huggett's approach assumes that one must have a 'God's eye view' of the entire material content of the universe—Brown, by contrast, does not do this.

For my purposes, it doesn't matter which of these approaches one prefers. (To anticipate, there are also other empiricist approaches to the meaning of 'force-free': for example, Torretti [159] seeks to identify the inertial frames with those frames of reference in which **N3L** holds: I'll get back to this shortly.) The central point is that none of these approaches (seem to) require recourse to spatiotemporal structure in order to afford meaning to the terms under consideration.

> **Question:** Which empiricist approach to the content of Newton's laws do you think is superior, and why?

Having now better understood the differences between Brown and Friedman with respect to the notions of inertial frames and force-free bodies, let me now return to the quote from Friedman which I presented at the end of the previous section. This quote, I claim, is best understood in the following way. Friedman supposes initially that Newton's laws are true, where the relevant terms are to be cashed out in terms of the structure of (neo-)Newtonian spacetime, as we've already seen. He also supposes that material bodies interact with one another via the gravitational force. In a universe of sufficient complexity (such as the actual world, at least when appropriately idealised) the nature of the gravitational interaction will mean that *no* body is truly force-free, in the sense of moving on

claim that he would actually endorse it.

a uniform trajectory with respect to the standard of straightness given by the background spacetime. For Friedman, the nature of the gravitational force does not mean that Newtonian mechanics is in fact false (which would render the theory, in a certain sense, self-undermining), but rather that there simply are no inertial frames embodied as the rest frames of observers in the actual world.

This all makes sense. But the perspective of Brown is very different: he does *not* begin by countenancing entire universes in which such-and-such laws (in this case, Newtonian gravity) obtain; rather, his concern is to afford meaning to notions to certain terms (in this case e.g. 'inertial frame') such that one may then proceed to *build up* one's theoretical commitments. For Brown, a definition of inertial frames (say) which obtains only approximately is still sufficient to build up, in a useful way, the machinery of Newton's laws. In this sense, while Friedman's critique makes sense in the context of his own theoretical commitments, it misfires against the very different methodology of Brown, who has not even constructed the notion of the gravitational interaction at the point when he seeks to define an operationalised notion of inertial frames.

There are various different ways of putting the differences between the two parties here. For 'geometrical' authors such as Friedman, it is quite common to take a 'transcendent' conception of physics (in the Kantian sense of 'stepping outside of the world'), and to account for physical phenomena from that perspective, with all of the metaphysics which it entails (in particular, the metaphysics of particular physical theories, e.g. Newtonian gravity) as inputs. For 'dynamical' authors such as Brown, by contrast, it is more common to take an 'immanent' conception of physics (in the Kantian sense of being 'embedded in the world'), and to construct the relevant metaphysical and physical notions on the basis of empirical studies in the world. This is vague, but I think useful to keep in mind when one reads debates between the relevant authors: failure to keep track of these different attitudes can often lead to individuals talking past one another, as the above passage from Friedman already indicates.[9]

Question: Do you think that Brown's 'dynamics first' approach to the content of Newton's laws to be preferred over Friedman's 'geometry-first' approach, or vice versa? Why?

The question of whether one should have a 'dynamics first' or 'geometry first' approach to the foundations of spacetime will loom large in this book; at this stage, I don't mean to favour one over the other. That said, it might be helpful to present here some further quotes from Brown and his allies, exhibiting both their suspicion towards the latter, as well as the ways in which they view their own positive proposals. First, here's Brown:

[9] When put in this way, it's not completely obvious that the two views are incompatible: one begins with empirical data, 'ascends' (via the 'dynamical' approach) to a set of metaphysical commitments, which one then uses to 'descend' (via the 'geometrical' approach) to explain further data. This tale of ascent and descent is a familiar one in philosophy, going back to Plato's analogy of the cave [124]. (My thanks to Niels Linnemann for discussions here.)

> What is geometry doing here—codifying the behaviour of free bodies in elegant mathematical language or actually explaining it?
>
> ... In what sense then is the postulation of the absolute spacetime structure doing more explanatory work than Molière's famous dormative virtue in opium? [14, pp. 23-24]

And here's DiSalle:[10]

> When we say that a free particle follows, while a particle experiencing a force deviates from, a geodesic of spacetime, we are not explaining the cause of the difference between two states or explaining 'relative to what' such a difference holds. Instead, we are giving the physical definition of a spacetime geodesic. To say that spacetime has the affine structure thus defined is not to postulate some hidden entity to explain the appearances, but rather to say that empirical facts support a system of physical laws that incorporates such a definition. [33, p. 327]

Finally, here are Vassallo and Esfeld:

> Note that we do not presuppose the existence of a spacetime structure ... that defines what it is for a motion to be geodesic, but, rather, the other way round: we define geodesic motion as a particularly simple pattern in the entire history of relational change. [162, p. 106]

1.4 Summary of the views

Let's return to our list of conceptual questions regarding Newton's laws, and consider how both Brown and Friedman would answer these questions. (For the time being I omit the fifth question; I'll discuss that in the following section.) First Brown:

1. Bodies are to be designated 'force-free' on the basis of some to-be-articulated operational procedure.

2. **N1L** isn't a case of **N2L**, because **N1L** allows to identify the inertial frames (those in which force-free bodies move with uniform velocities); having fixed such frames, **N2L** then allows us to identify the particles subject to genuine forces (and what the magnitudes of those forces are).

3. **N1L** isn't a definition—force-free particles aren't *defined* to be those moving with uniform velocity.

4. Newton's laws are supposed to hold in the inertial frames of reference.

[10] For a comparison of the outlooks of Brown and DiSalle, see [77]. In the following quote a 'geodesic' means a straight line in spacetime, according to the standard of straightness of that spacetime: I'll discuss the notion more in Chapters 5 and 6.

As we know by now, the answers which Friedman would give to these four questions are very different:

1. 'Force-free' means moving uniformly with respect to the standard of straightness given by (neo-)Newtonian spacetime.

2. **N1L** is a special case of **N2L**.

3. **N1L** isn't a definition—in fact, it is redundant.

4. As stated in a coordinate-based description, Newton's laws are supposed to hold in the inertial frames, which are the frames 'adapted' to (neo-)Newtonian spacetime (i.e., are the frames at rest or moving uniformly with respect to Newtonian absolute spacetime). Insofar as a world (e.g. an idealised version of the acutal world) may in fact contain no bodies which are truly force-free, **N1L** cannot be operationalised in that world (in this sense, **N1L** obtains only counterfactually).

The reader will notice that, up to this point, I haven't mentioned **N3L**, and haven't addressed the associated question (5), of whether **N1L** is a special case of **N3L**. This is the final piece of the puzzle regarding Newton's laws which it was my allotted task in this chapter to address; I turn now to this issue.

1.5 Newton's third law

What is the conceptual relation between **N3L** and **N1L** and **N2L**? One of the few authors to address this question in any detail is Torretti, who writes:

> [T]he Third Law of Motion furnishes a Newtonian physicist with all he needs for distinguishing, in principle, between a particle acted on by a true force of nature and a free particle accelerating in a particular—necessarily non-inertial—frame. If a material particle α of mass m experiences acceleration $\mathbf{a}$ in an inertial frame F, it will instantaneously react with force $-m\mathbf{a}$ on the material source of its acceleration. There must exist therefore a material system β, of mass m/k, whose centre of mass experiences in F the acceleration $-k\mathbf{a}$. On the other hand, if a particle α accelerates in a non-inertial frame, its acceleration must include a component that is not matched by the acceleration of another material system, in direction opposite to the said component, caused by the action of α on that system. [159, pp. 19-20]

Torretti continues in an endnote:

> The criterion furnished by the Third Law does not, of course, amount to an "operational definition" of a *freely moving particle* and an *inertial frame*. In the above example, the acceleration of β by α's reaction will generally be only a component of β's total acceleration

> and it might not be easy to discern it. But the criterion surely bestows a definite, intelligible meaning on the italicised expressions. [159, p. 287, n. 16]

Torretti's claim here is that a frame in which **N1L** is satisfied is one in which **N3L** is satisfied, and vice versa. Moreover, one can thereby in principle—if not in practice—check whether **N3L** is satisfied in a given frame, and (if so) use this fact to identify operationally/empirically what the force-free particles are (thus, this constitutes a third possible approach to the operational identification of force-free particles, alongside the Brown-style and Huggett-style approaches already discussed above).

Let's focus first on the claim that **N1L** implies **N3L**—equivalently, that **N3L** is presupposed by **N1L**. At least in the context of special relativity, this claim is not correct, for, as Griffiths writes,

> Unlike the first two, Newton's *third* law does not, in general, extend to the relativistic domain. Indeed, if the two objects in question are separated in space, the third law is incompatible with the relativity of simultaneity. For suppose the force of A on B at some instant t is $\boldsymbol{F}(t)$, and the force of B on A at the same instant is $-\boldsymbol{F}(t)$; then the third law applies *in this reference frame*. But a moving observer will report that these equal and opposite forces occurred at *different times*; in his system, therefore, the third law is violated. Only in the case of contact interactions, where the two forces are applied at the same physical point (and in the trivial case where the forces are constant) can the third law be retained. [68, p. 544]

Although Griffths puts the point in terms of an incompatibility between **N3L** and the relativity of simultaneity (see Chapter 7), the fundamental tension is between **N3L** and the relativity *principle* (see Chapter 2): in cases such as the above example, in which the forces between the bodies in question (in that example, α and β) are not mediated by contact interactions, if **N3L** holds in one frame of reference F, then it will not hold in a frame F' in uniform motion with respect to F—that is, **N3L** will not hold in another inertial frame of reference, in violation of the relativity principle.

In response to this, one might reasonably complain that, at least within the context of Newtonian forces, there's no reason to doubt this claim. Moreover, recall from the foregoing discussion that the point of the dynamics-first, more 'operational' outlook of authors such as Brown was to build up one's theoretical commitments on the basis of empirical data, without making theoretical assumptions *ab initio*. Therefore, to make appeals to relativity theory may be to make a *petitio principii* against such authors, who could simply *define* the force-free bodies to be those moving on uniform trajectories in the **N3L**-satisfying frames.

In any case, let's turn now to the other professed direction of implication—that **N3L** implies **N1L**—equivalently, that **N1L** is presupposed by **N3L**. Here, there seem to be counterexamples coming from within the context of Newtonian mechanics. For example, consider a Newtonian universe consisting of one single binary astronomical system, in which two bodies α and β of equal mass rotate about a common centre of mass. Consider a frame rotating about said centre of mass: the force on α will be equal and opposite to the force on β—in spite of the fact that these two bodies will be subject to (equal and opposite) inertial effects. This frame is non-inertial, but **N3L** is satisfied. Thus, any claim that the satisfaction of **N3L** implies that the system in question is being described in an inertial frame of reference is incorrect; rather, the inertial systems are (at best) a *subclass* of the **N3L**-satisfying systems.

The existence of examples like this seem to imply that one cannot *invariably* use **N3L** as a means of operationally identifying the inertial frames—indeed, one can make this point without having to worry about the reverse direction of implication. As before, however, it's not obvious that these concerns need animate those who situate themselves in the 'dynamical' camp.

> **Question:** How general and how serious are problem cases of the kind introduced above? In light of this, to what extent can something of Torretti's claim be salvaged?

1.6 Summary

I don't deny that this has been a difficult first chapter. But, by proceeding from Newton's laws, I hope to have illustrated that one encounters deep, profound, and unresolved questions in the foundations of spacetime theories from the very outset. Proceeding in this way also has the merit of introducing at the beginning a number of crucial concepts which will animate us over the course of the remainder of this book: concepts such as inertial frames, force-free motion, and dynamical versus geometrical understandings of physics. I will, indeed, return to all of these issues in the context of special relativity quite shortly. Before doing so, however, it's necessary to introduce some further concepts—in particular, the concept of a *symmetry* of a physical theory. I turn to this task in the next chapter.

Chapter 2

Symmetries and invariance

> The formulation of these laws requires the use of the mathematics of transformations. The important things in the world appear as the invariants (or more generally the nearly invariants, or quantities with simple transformation properties) of these transformations. The things we are immediately aware of are the relations of these nearly invariants to a certain frame of reference, usually one chosen so as to introduce special simplifying features which are unimportant from the point of view of the general theory. (Dirac, 1930)

My goals in this chapter are threefold: (i) to introduce the notion of a *symmetry* of a physical theory, (ii) to explore how such symmetries might be identified, and (iii) to convince the reader of the significance of symmetry-based reasoning in physics.

2.1 The relativity principle

Suppose you decide to take the sleeper train from Euston to Fort William; outside, it's pitch black—you can't see a thing. Ignoring the mild jostling from side to side which inevitably one experiences on a train, can you tell the speed at which your train is moving? If the train is moving uniformly at 100mph, is there any empirical difference to the situation in which it's at rest in Waverley?

The answer, of course, is *no*—and this is one illustration of what's known as the *relativity principle*: for a subsystem appropriately isolated from the environment, the laws of physics inside the system are exactly the same, whatever the uniform velocity of that system might be. Galileo was one of the first to present the relativity principle in this form; his writings and examples are so elegant that it's worth quoting him at length:

> Shut yourself up with some friend in the main cabin below decks on some large ship, and have with you there some flies, butterflies, and other small flying animals. Have a large bowl of water with some fish in it; hang up a bottle that empties drop by drop into a wide vessel beneath it. With the ship standing still, observe carefully how the little animals fly with equal speed to all sides of the cabin. The fish swim indifferently in all directions; the drops fall into the vessel beneath; and in throwing something to your friend, you need to throw it no more strongly in one direction than another, the distances being equal; jumping with your feet together, you pass equal space in every direction.
>
> When you have observed all these things carefully, have the ship proceed with any speed you like, so long as the motion is uniform and not fluctuating this way and that. You will discover not the least change in all the effects named, nor could you tell from any of them whether the ship was moving or standing still. In jumping you will pass on the floor the same spaces as before, nor will you make larger jumps toward the stern than toward the prow even though the ship is moving quite rapidly. The droplets will fall as before into the vessel beneath without dropping toward the stern, although while the drops are in the air the ship runs many spans. The fish in the water will swim toward the front of the bowl with no more effort than toward the back, and will go with equal ease to bait placed anywhere around the edges of the bowl. Finally the butterflies will continue their flights indifferently toward every side, nor will it ever happen that they are concentrated toward the stern, as if tired out from keeping up with the course of the ship, from which they will have been separated during long intervals in keeping themselves in the air. [61, pp. 186-187]

We saw in the previous chapter that the inertial frames are those frames in which the dynamical equations governing matter take their simplest form, and in which force-free bodies move with uniform velocity. When combined with the relativity principle, we arrive at the conclusion that the laws of physics take their simplest in all of a *class* of inertial frames, which are related by uniform velocity transformations.

But what exactly are these 'uniform velocity transformations' in Newtonian mechanics, and are they the same transformations which take us between inertial frames in special relativity? (Spoiler: no!) To answer the first question, we need to investigate the invariance properties of Newtonian theories. Before doing so, however, there is one other important conceptual distinction to clear up: that between active and passive coordinate transformations.

2.2 Active versus passive transformations

There's an important distinction between *active* and *passive* transformations, the prescriptions for which can be put as follows:

Active transformations: Transform physical system; leave coordinate system unchanged.

Passive transformations: Transform coordinate system; leave physical system unchanged.

The transformations considered in this chapter can be understood either actively or passively; more generally, over the course of this book, I will be explicit about whether a transformation is intended in the active or a passive sense. Although mathematically active and passive transformations have the same net result, conceptually they are clearly very different; these differences have turned out to have quite serious ramifications for various debates in the foundations of spacetime theories.[1]

With the distinction between active and passive transformations in hand, we're in a better position to understand what's going on in case of Galileo's ship (or, equivalently, our original train example). Suppose that we apply an active transformation to a *subsystem* of the universe (e.g. Galileo's ship). Then, assuming:

1. the relativity principle holds, and
2. the physics within the subsystem is *isolated* from that of the environment...

...the physics within the subsystem will be unchanged between the pre- and post-transformed cases. This—an active boost applied to a subsystem, assuming the relativity principle and dynamical isolation—is what's going on in Galileo's ship.

2.3 Galilean transformations

Let's now return to the main project in this chapter: to introduce, and provide some means of ascertaining, the symmetries of Newtonian physical theories. For Newtonian theories, I'll begin by giving the game away: their symmetries at least *include* (but do not necessarily *exhaust*: this will be of significance later) the Galilean transformations. A *Galilean transformation* is any coordinate transformation that can be expressed as the composition of a rigid spacetime translation, a rigid rotation, and a Galilean boost:

[1] The most famous example is the infamous 'hole problem' of general relativity—for surveys on this, see e.g. [116, 129].

Spatial translation	$g_{\mathbf{a}}\left(\mathbf{a} \in \mathbb{R}^3\right):$	$g_{\mathbf{a}}(t, \mathbf{x}) = (t, \mathbf{x} + \mathbf{a})$.
Time translation	$g_b\,(b \in \mathbb{R}):$	$g_b(t, \mathbf{x}) = (t + b, \mathbf{x})$.
Spatial rotation	$g_{\mathbf{R}}\,(\mathbf{R} \in SO(3)):$	$g_{\mathbf{R}}(t, \mathbf{x}) = (t, \mathbf{R}\mathbf{x})$.
Galilean boost	$g_{\mathbf{v}}\left(\mathbf{v} \in \mathbb{R}^3\right):$	$g_{\mathbf{v}}(t, \mathbf{x}) = (t, \mathbf{x} - \mathbf{v}t)$.

How do I show that a given set of physical laws has (say) the Galilean transformations as its symmetries? There are two ways of defining what it means for a given set of laws to be invariant under a given set of transformations: what I'll call the 'space-of-solutions approach', and what I'll call the 'form-of-equations approach'. I'll illustrate both, beginning with the space-of-solutions approach.

Consider the equation of motion

$$\frac{dr}{dt} = -kr. \tag{2.1}$$

This has general solution

$$r(t) = Ae^{-kt}, \qquad A \in \mathbb{R}. \tag{2.2}$$

For any such r and any time translation g_b, we can form the transformed structure $g_b r$:

$$\begin{aligned}(g_b r)(t) &= r(t-b) \\ &= Ae^{-k(t-b)} \\ &= \left(Ae^{+kb}\right)e^{-kt}.\end{aligned} \tag{2.3}$$

This is another solution of the same equation, so we say that our equation is *time-translation invariant.*

More generally, the space-of-solutions approach takes the following form:

Space-of-solutions approach:

1. Identify the set Θ of equations to be investigated.
2. Identify a set S of *structures for* Θ; i.e., identify the type of object that is mathematically appropriate to be a candidate *solution* to Θ.
3. Identify the group G of transformations whose effects on Θ we will be interested in investigating.
4. For general $g \in G$, identify the action of g on S.
5. Ask whether this action of G preserves the subset $D \subset S$ of solutions to Θ.

It's also worth considering the case of a demonstration of *non*-invariance, on the space-of-solutions approach. Let the equation of motion (and hence Θ and S) be as before. Let G be the group B_1 of one-dimensional *boosts* $g_v : x \mapsto x - vt$. The action of any such g_v on S is:

$$(g_v r)(t) = r(t) - vt. \tag{2.4}$$

For the general solution $r(t) = Ae^{-kt}$, the transformed structure is given by

$$(g_v r)(t) = Ae^{-kt} - vt, \tag{2.5}$$

which is *not* identical to Be^{-kt} for any $B \in \mathbb{R}$, i.e., is not a solution of our original equation. So our equation is not *Galilean boost invariant*.

So much for the space-of-solutions approach; let's turn now to the form-of-equations approach. Consider again the equation of motion (2.1). This equation is built from various objects: $\frac{d}{dt}$, r, and k. Under a time-translation g_b,

- $\frac{d}{dt}$ and k transform trivially;
- the function r transforms, as before, as $(g_b r)(t) = r(t - b)$.

The transformed equation is therefore

$$\frac{d}{dt} r(t - b) = -kr(t - b). \tag{2.6}$$

But asserting that our second equation holds for all t is equivalent to asserting that our first equation holds for all t. Thus, the original equation is time translation invariant.

The general format of the form-of-equations approach is this:

Form-of-equations approach:

1. Identify the set of equations Θ to be investigated.
2. Identify the group G of transformations whose effects on Θ we will be interested in investigating.
3. Identify an action of G on each of the ingredients in each equation in Θ.
4. Write down the equations with the transformed ('primed') quantities in place of the untransformed ones.
5. If the result is a set of equations equivalent to the original Θ, then Θ is G-invariant.

And here's a demonstration of non-invariance of an equation on the form-of-equations approach. Consider once again the equation of motion (2.1). Let

G be the group B_1 of one-dimensional boosts $g_v : x \mapsto x - vt$. The ingredients of our equation transform as

$$g_v : \frac{d}{dt} \mapsto \frac{d}{dt}; \tag{2.7}$$
$$g_v : k \mapsto k; \tag{2.8}$$
$$g_v : r(t) \mapsto r(t) - vt. \tag{2.9}$$

So the transformed equation is

$$\frac{d}{dt}\left(r(t) - vt\right) = -k\left(r(t) - vt\right). \tag{2.10}$$

But this is equivalent to the original equation only if $-v = vkt$, which clearly cannot hold for all t. The *non*-equivalence of the untransformed and transformed equations means that the original equation is *not* boost-invariant.

Although I used a very simple toy model above, it's straightforward to apply both of these approaches to more physically relevant theories. A standard first-year presentation of Newtonian gravity for two particles is given by (combining **N2L** and the law of gravitation):

$$\ddot{\mathbf{r}}_i = \frac{G_N m_1 m_2}{|\mathbf{r}_1 - \mathbf{r}_2|^3}\left(\mathbf{r}_i - \mathbf{r}_{i+1}\right), \qquad i = 1, 2. \tag{2.11}$$

Let G be the group B_3 of *three*-dimensional boosts, $\{(g_{\mathbf{v}} : \mathbf{r} \mapsto \mathbf{r} - \mathbf{v}t) : \mathbf{v} \in \mathbb{R}^3\}$. The quantities in our equation transform as

$$\mathbf{r}'_i(t) := (g_{\mathbf{v}}\mathbf{r}_i)(t) = \mathbf{r}_i(t) - \mathbf{v}t, \tag{2.12}$$
$$\ddot{\mathbf{r}}'_i(t) := (g_{\mathbf{v}}\ddot{\mathbf{r}}_i)(t) = \ddot{\mathbf{r}}_i(t), \tag{2.13}$$
$$m'_i := g_{\mathbf{v}} m_i = m_i. \tag{2.14}$$

The transformed equation is:

$$\ddot{\mathbf{r}}'_i = \frac{G_N m_1 m_2}{|\mathbf{r}'_1 - \mathbf{r}'_2|^3}\left(\mathbf{r}'_i - \mathbf{r}'_{i+1}\right), \qquad i = 1, 2. \tag{2.15}$$

Eliminating the primes, we have

$$\ddot{\mathbf{r}}_i = \frac{G_N m_1 m_2}{|\mathbf{r}_1 - \mathbf{r}_2|^3}\left(\mathbf{r}_i - \mathbf{r}_{i+1}\right), \qquad i = 1, 2. \tag{2.16}$$

So the equation (2.11) is form-invariant under Galilean boosts!

Exercise: Generalise this to the N-body problem.

Exercise: Show that Newtonian gravitation is invariant under Galilean boosts using the space-of-solutions approach.

Having now presented both methods for ascertaining whether a given set of equations has a given set of symmetries, there are a couple of conceptual points to make. First: pragmatically, there's some reason to prefer the form-of-equations approach over the space-of-solutions approach, because the former doesn't involve having to figure out what the solutions of the equation under consideration *actually are*. Second: in each case above, we began with an *ansatz* about what the symmetry group of our equation. Figuring out the full symmetry group of a set of equations is highly non-trivial. While there is no general method for doing this, the task can be aided by formulating our theories in certain ways, using certain objects which have familiar symmetry properties. (I'll demonstrate explicitly what I mean by this in Chapter 6.)

Question: Are the space-of-solutions approach and the form-of-equations approach equivalent? Justify your answer.

2.4 Newton on Galilean invariance

I'm now going to indulge in an historical digression. Newton claims to infer Galilean invariance from his laws of motion: after setting out the latter, he infers several corollaries; his 'Corollary V' is:

> The motions of bodies included in a given space are the same among themselves, whether that space is at rest, or moves uniformly forward in a right line without any circular motion. [19, p. 20]

This essentially *states* that the laws of physics are Galilean invariant. Newton's argument for Corollary V is this:

> For the differences of the motions tending towards the same parts, and the sums of those that tend toward contrary parts, are, at first (by supposition), in both cases the same; and it is from those sums and differences that the collisions and impulses do arise with which the bodies mutually impinge one upon another. Wherefore (by Law II) the effects of those collisions will be equal in both cases; and therefore the mutual motions of the bodies among themselves in the one case will remain equal to the mutual motions of the bodies among themselves in the other. A clear proof of which we have from experiment of a ship; where all motions happen after the same manner, whether the ship is at rest, or is carried uniformly forwards in a right line. [19, p. 20]

Newton's reasoning here is morally correct, but it's worth pointing to a couple of non-sequiturs in his argument. First: it does not follow from the laws of motion alone that "it is from those sums and differences that the collisions and impulses do arise with which the bodies mutually impinge upon one another." This requires the *additional assumption* that forces depend only on (vectorial) differences of positions and/or velocities, not on absolute positions or absolute velocities. (Consider a particle affected by the force $\mathbf{F} = -k\mathbf{v}$.) And second: it does not follow that "the effects of those collisions will be equal" unless we further assume that the *mass* of a given body is independent of the body's absolute position and absolute velocity. (Consider particles whose masses are proportional to their absolute speeds.) That said, with these two auxiliary assumptions in place, Galilean invariance of the laws does follows from **N2L** (by essentially Newton's argument). (For more on this, see [14, §3.2].)

2.5 Poincaré transformations

So far in this chapter, I've introduced the Galilean transformations, as well as two different methods for checking whether a given set of equations has a given set of transformations as symmetries. Of course, though, the Galilean transformations are not the only set of transformations of physical interest—and, indeed (to anticipate), the transformations which are most relevant to special relativity are the *Poincaré transformations*. We saw that a Galilean transformation can be expressed as the composition of a rigid spacetime translation, a rigid rotation, and a Galilean boost. A *Poincaré transformation* is any coordinate transformation that can be expressed as the composition of a rigid spacetime translation, a rigid rotation, and a *Lorentz* boost:[2]

Spacetime translation	$g_{a^\mu}\left(a^\mu \in \mathbb{R}^4\right):$	$g_{a^\mu}\left(x^\nu\right) = x^\nu + a^\nu.$
Spatial rotation and Lorentz boost	$g_{\Lambda^\mu{}_\nu}\left(\Lambda^\mu{}_\nu \in SO\left(1,3\right)\right):$	$g_{\Lambda^\mu{}_\nu}\left(x^\nu\right) = \Lambda^\nu{}_\sigma x^\sigma.$

In both cases, we have a rigid translation, a rigid rotation, and a boost. But the boosts are *different* in the two cases. To render this explicit: here's a Galilean boost in the x direction:

$$t' = t, \tag{2.17}$$
$$x' = x - vt. \tag{2.18}$$

By contrast, here's a Lorentz boost in the x direction ($\gamma := 1/\sqrt{1 - v^2/c^2}$):

$$t' = \gamma\left(t - \frac{vx}{c^2}\right), \tag{2.19}$$
$$x' = \gamma\left(x - vt\right). \tag{2.20}$$

Question: Is there any reason to prefer $c \to \infty$ or $v/c \to 0$ as a way of taking the non-relativistic limit of the Lorentz transformations? What addition assumptions does one need to make in order to recover the Galilean boosts from the Lorentz boosts when $v/c \to 0$?

[2]Here, for the first time in this book, I use the Einstein summation convention, according to which repeated indices in a term are summed. I also use four-dimensional index notation, which will be discussed in detail in Chapter 6.

The first set of physical laws which were discovered to be Poincaré invariant were Maxwell's equations. In their typical 3-vector formulation, these read:[3]

$$\nabla \cdot \mathbf{E} = \rho \tag{2.21}$$

$$\nabla \cdot \mathbf{B} = 0 \tag{2.22}$$

$$\nabla \times \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t} \tag{2.23}$$

$$\nabla \times \mathbf{B} = \mathbf{J} + \frac{\partial \mathbf{E}}{\partial t} \tag{2.24}$$

Maxwell's equations are invariant under *Poincaré transformations*; they are not invariant under Galilean transformations. One might, indeed, say that it was the discovery of a set of dynamical laws which were Poincaré invariant rather than Galilean invariant which precipitated the crisis nineteenth-century physics which eventually led to the development of special relativity. It's this crisis which I'm going to discuss in the next chapter.

[3]Note that changing just one sign in Maxwell's equations will change their symmetry properties: see [71].

Chapter 3

The Michelson-Morley experiment and Lorentz's programme

> It matters little whether the ether really exists; that is the affair of the metaphysicians. The esssential thing for us is that everything happens as if it existed, and that this hypothesis is convenient for the explanation of phenomena. After all, have we any other reason to believe in the existence of material objects? That, too, is only a convenient hypothesis; only this will never cease to be so, whereas, no doubt, some day the ether will be thrown aside as useless. (Poincaré, 1888)

I intimated at the end of the previous chapter that the discovery of Maxwell's equations—which are invariant under Poincaré transformations—precipitated a crisis in nineteenth-century physics. Here's a quick and dirty way to see the issue, in terms of symmetries.[1] We have seen that Newtonian mechanics is invariant under Galilean transformations—i.e., translations, spatial rotations, and Galilean boosts. We have also seen that electromagnetism is invariant un-

[1] This presentation is anachronistic, because in fact around the time that these events were unfolding in physics (i.e., the 1880s), the symmetry group of Maxwell's equations was yet to be discovered: see [14, p. 2]. Still, the presentation here has the merit of pedagogical clarity.

der Poincaré transformations—i.e., translations, spatial rotations, and Lorentz boosts. Suppose that we lived in a world in which both of these theories were true. Then the overall invariance group of the physical laws of the world would be the intersection of these two groups, i.e., the group of translations and spatial rotations (no boosts). The lack of boost invariance would then imply (up to translations and spatial rotations) the existence of a preferred frame! So: in the wake of Maxwell's electromagnetism in the nineteenth century, physicists anticipated violations of the relativity principle.[2]

What did people take the significance of this predicted preferred frame to be? From Maxwell's equations, one can derive that the speed of light is c (see e.g. [80]). It is natural to identify this statement as holding true in the above-mentioned preferred frame. In this frame, with respect to what is light moving? 19th Century answer: some background structure: *the ether*, which was supposed to be the medium in which light waves propagate. So (the thought goes) light moves at c in the rest frame of the ether—and this is the preferred frame in which Maxwell's equations hold. This was an extremely sensible thing for 19th Century physicists to think, since it rests only on the assumption that light is like all other waves, insofar as (i) it has a medium, and (ii) its speed is independent of the speed of the source (and, insofar as one takes the wave in question to have a medium, a function only of the speed of that medium).[3]

That light moves at c only in the rest frame of the ether, and moves at $c \pm v$ in a frame moving at velocity v with respect to the ether (because, from Maxwell's equations, the speed of light is independent of the speed of the source—to repeat, the situation here is *exactly the same as for all other waves*[4]), is an *empirical hypothesis*, which should be testable. In the 19th century, physicists indeed did attempt to test this hypothesis—all tests ended in *null results*. The most famous of these experiments is the *Michelson-Morley experiment*, which I'll consider in detail in this chapter. Before doing so, however, I want to say a little more on the conceptual background to these issues.

[2]To repeat the point of the previous footnote: physicists arrived at this conclusion on the basis of the wave-like nature of light (more on which below), rather than on the basis of consideration of symmetry groups. Nevertheless, the central conclusion—the existence of a preferred frame—is the same on either account.

[3]Sometimes, when one first encounters special relativity, (ii) is marketed as a novel feature of light. But this is badly confused, since it's a feature of all waves. As we'll see below, the distinctive feature of the light has to do with the nature of its medium (specifically, it has to do with the fact that we no longer believe that the medium exists!).

[4]Indeed, the wave equation for sound is also invariant under Poincaré transformations—albeit with an invariant speed which is not c, but rather the speed of sound. These parallels raise interesting questions regarding whether (and under what circumstances) one might be led to a relativistic theory on the basis of (say) sound waves, rather than light waves. I won't go into this further here—suffice it to say that exploring the parallels is an illuminating pedagogical exercise. For recent discussions on these matters, see [23, 157, 158].

3.1 Conceptual background

In my experience, one can easily become confused over how physicists were reasoning through these issues in the 19th century, unless one states the issues very precisely, and approaches them with a great deal of care. To that end, I want to say a little more here by way of background. Feynman put things very nicely in his *Lectures on Physics*:

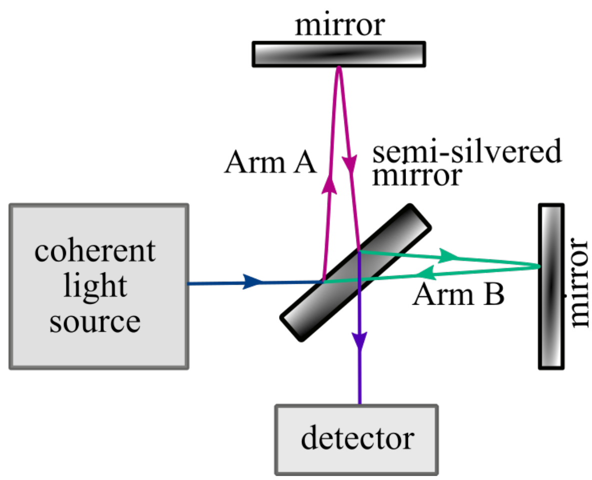
> One of the consequences of Maxwell's equations is that if there is a disturbance in the field such that light is generated, these electromagnetic waves go out in all directions equally and at the same speed c, or 186,000 mi/sec. Another consequence of the equations is that if the source of the disturbance is moving, the light emitted goes through space at the same speed c. This is analogous to the case of sound, the speed of sound waves being likewise independent of the motion of the source.
>
> This independence of the motion of the source, in the case of light, brings up an interesting problem. Suppose we are riding in a car that is going at a speed u, and light from the rear is going past the car with speed c. Differentiating [the Galilean transformation for position] gives
>
> $$\frac{dx'}{dt} = \frac{dx}{dt} - u,$$
>
> which means that according to the Galilean transformation the apparent speed of the passing light, as we measure it in the car, should not be c but should be $c - u$. For instance, if the car is going 100,000 mi/sec, and the light is going 186,000 mi/sec, then apparently the light going past the car should go 86,000 mi/sec. In any case, by measuring the speed of the light going past the car (if the Galilean transformation is correct for light), one could determine the speed of the car. A number of experiments based on this general idea were performed to determine the velocity of the earth, but they all failed—they gave no velocity at all. [56, vol. 1, lecture 15]

The point is that light, like sound (the medium of which is of course the air), was in the nineteenth century taken to have a medium: the ether. Like sound (since light is also a wave, and all waves have this property) the speed of light is independent of the speed of the source; on the assumption of a medium, its velocity is a function of the speed of the medium only. So: although light (by assumption) moves at c in the rest frame of the ether, it will move at $c \pm v$ in a frame moving at velocity v with respect to the ether.[5] But (to anticipate), such potentially detectable effects were, in fact, never observed.

[5] One can in fact show that if one Galilean boosts a plane wave solution to Maxwell's equations (remember: such Galilean boosted—rather than Lorentz boosted—frames are *not* inertial frames of Maxwell's equations), then the wave will move at $c \pm v$ in boosted frame (where the boost is understood passively): see [153].

3.2 The Michelson-Morley experiment

How did physicists attempt to test the above-described predictions? By far the most famous such attempted experimental test was the Michelson-Morley experiment, which I'll now discuss in some detail.[6]



As we've already discussed, assuming that the Earth is moving with some velocity with respect to the ether, there should (the thought went) be deviations from the observed velocity of light, which should be detectable. It was exactly these deviations which the Michelson-Morley experiment was designed to detect. Above, I've drawn a schematic representation of the setup of this experiment; this experiment was designed to work as follows:

1. The interferometer sends a beam of coherent light from a source towards a half-silvered mirror.

2. Here the beam is split into two components that continue at right angles to one another: one down 'arm A' and the other down 'arm B'.

3. A short distance later, each half-beam encounters a second (fully silvered) mirror, and is reflected back. The beams are recombined, and the resulting interference pattern is observed on a detector screen.

4. The observed pattern will depend on:

   (a) the lengths of the arms A and B, and

   (b) *the speed of travel of the light along each arm in each direction.*

In a lab that is moving relative to the ether with speed v, the speed of light relative to the lab frame is expected to be *anisotropic*: it should be $c - v$ in

[6] For further details, see [14, ch. 4].

the direction of the lab's motion, $c+v$ in the opposite direction, and $\sqrt{c^2-v^2}$ in directions perpendicular to that of the lab's motion (the third velocity can be computed via a straightforward application of Pythagoras' theorem). *If* we could ensure that the arms were exactly equal in length, then anything other than constructive interference would indicate the presence of an ether wind. Unfortunately, ensuring this was not technologically feasible when Michelson and Morley performed their experiment. However, regardless of the arm lengths, *rotating* the apparatus should *change* the interference pattern in a predictable manner in a moving frame, and would not if the apparatus were at rest with respect to the ether. Thus we look for this post-rotation change as a signature of the ether wind.

More quantitatively, the reasoning proceeds as follows. Suppose (for simplicity) that the two arms are of equal length, L. Then, the out-and-back time for light to travel along the arm that is parallel to the ether drift should be

$$\Delta t_{\parallel} = \frac{L}{c-v} + \frac{L}{c+v} = \frac{2Lc}{c^2-v^2}. \tag{3.1}$$

The out-and-back time for light to travel along the arm that is *perpendicular* to the ether drift should be

$$\Delta t_{\perp} = \frac{2L}{\sqrt{c^2-v^2}}. \tag{3.2}$$

The time difference before rotation is then given by

$$\Delta t_{\parallel} - \Delta t_{\perp} = \frac{2}{c}\left(\frac{L}{1-\frac{v^2}{c^2}} - \frac{L}{\sqrt{1-\frac{v^2}{c^2}}}\right). \tag{3.3}$$

By multiplying by c, the corresponding length difference before rotation is

$$\Delta_1 = 2\left(\frac{L}{1-\frac{v^2}{c^2}} - \frac{L}{\sqrt{1-\frac{v^2}{c^2}}}\right). \tag{3.4}$$

After rotation, the length difference is given by

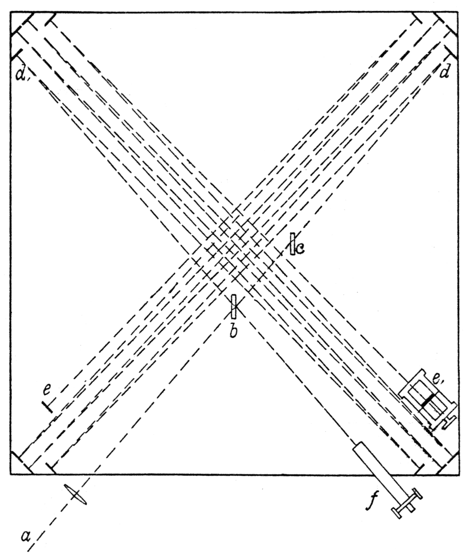
$$\Delta_2 = 2\left(\frac{L}{\sqrt{1-\frac{v^2}{c^2}}} - \frac{L}{1-\frac{v^2}{c^2}}\right). \tag{3.5}$$

Dividing $\Delta_1 - \Delta_2$ by the wavelength λ of the light used in the interferometer, the fringe shift n is found:

$$n = \frac{\Delta_1 - \Delta_2}{\lambda} \approx \frac{2Lv^2}{\lambda c^2}. \tag{3.6}$$

If $L = 11m$, $\lambda = 550nm$ and $v = 30kms^{-1}$, this gives an expected fringe shift of $\Delta n \approx 0.4$—certainly large enough to be observable (*despite* the fact that the effect is 'second order in v^2/c^2').

A couple of comments at this stage. First: one might wonder: isn't a length of $11m$ pretty big for the experiment? Yes—in fact, Michelson and Moreley implemented this *effective* length by using mirrors to bounce light back and forth in their detectors (this method is by now standard, and also used in modern interferometers such as LIGO, which was used to detect gravitational waves, as predicted by general relativity). (Below, I've included a figure from the original paper by Michelson and Morley [107], in which they illustrate this use of mirrors.) Second: one might wonder: where did the velocity of $30kms^{-1}$ come from? The simple answer is that this was a guess: that velocity is the approximate orbital velocity of the Earth around the Sun, so seems as good as any other from the point of view of rendering quantitative the theoretical predictions regarding this experiment.



In any case, we already know the punch-line to this story: The result of Michelson-Morley experiment was *null*—rotating the apparatus did not lead to a detectable fringe shift. Michelson and Morley concluded that "if there be any relative motion between the earth and the luminifeous ether, it must be small" [107, p. 341]; here, 'small' means "probably less than one-sixth of the earth's orbital velocity, and certainly less than one-fourth." This null result was a mystery: this "small relative motion" might obtain by luck at any given instant, but it is difficult to see how it could obtain *throughout* the Earth's orbit. (Assuming that the ether is an inert background, then *of course* the Earth cannot be at rest with respect to it at every point in its orbit; on the other hand, if the ether is not inert, then it would have to be the case that the Earth (say) drags the ether around its orbit, so that there is no detectable relative motion between the two—but that hypothesis was *ad hoc*;[7] moreover, the drag hypothesis was already losing favour around the time that the Michelson-Morley experiment was performed.[8].)

It's worth reiterating the puzzle presented by these null results; we can do so by treating the Earth as in essence an analogue of Galileo's ship. Suppose that the Earth is at rest with respect to the ether at some point in its orbit. Then the Earth will be moving with respect to the ether at some other point in its orbit. It look like the Earth is, therefore, a Galileo ship-type subsystem, which has been actively boosted. If all physics were Galilean invariant, we would expect the same physical laws on the Earth in the two scenarios. But electromagnetism is *not* Galilean invariant (it's Poincaré invariant)—so (the thought goes) we should expect violations of the (Galilean) relativity principle, manifesting themselves in different detected velocities of light in the two cases. How to explain that this was never observed?

3.3 Lorentz's programme

It would be easy, through the lens of post-Einsteinian physics, to denigrate the ether theorists as foolish for having chased after a will-o'-the-wisp in the form of the ether. But it bears stressing that there was no reason at the time to doubt the analogies between light and other waves such as sound and water. Moreover, as we'll see, the work undertaken by physicists such as Fitzgerald, Larmor, and Lorentz in the wake of the Michelson-Moreley null result provided the fuel to ignite Einstein's relativistic revolution. So, these physicists have every right to be dubbed, in Brown's words, "the trailblazers" [14, ch. 4].

Let's begin with Fitzgerald, who in 1889 the suggested that "almost the only hypothesis" capable of reconciling the Michelson-Morley experiment with the apparent fact that the Earth dragged a negligible amount of ether was that

> the length of material bodies changes, according as they are moving through the ether or across it, by an amount depending on the square of the ratio of their velocities to that light. [58]

He continued:

> We know that electric forces are affected by the motion of electrified bodies relative to the ether and it seems a not improbable supposition that the molecular forces are affected by the motion and that the size of the body alters consequently. [58]

The idea here is that we do not observe violations of the relativity principle in the sense of the frame-dependence of the velocity of light, for material bodies contract under velocity boosts in just such a way as to compensate for such effects, and yield the recorded null result. Lorentz, indeed, would arrive at the

[7] One might be reminded here of Descartes' celestial fluid: see [128].

[8] For further discussion of this drag hypothesis, see [144, pp. 172-173] and [115, ch. 8].

same idea in 1892 [96]; Larmor would also adopt the idea in his 1900 book, *Aether and Matter* [88].

To be a little more concrete (and here I follow the presentation in [14, §4.4]), Lorentz introduced a longditudinal factor $\mathcal{C}_{\parallel} = 1 + \delta$ and a transvere factor $\mathcal{C}_{\perp} = 1 + \epsilon$. He claimed that the null result required

$$\epsilon - \delta \sim \frac{v^2}{2c^2}. \tag{3.7}$$

Contraction in this manner would cancel out the different velocities of light, and lead to no phase shift effects at the detector. Lorentz would later push this idea further, with his *theorem of corresponding states* [97]. This was designed to show that *no* first- or second-order ether-wind effects would be discernible in experiments involving optics and electrodynamics. In the second version of this theorem, the Lorentz transformations finally appear; however, until Einstein's work in 1905 (see Chapter 4) [42], Lorentz continued to believe that the true coordinate transformations were the Galilean ones, and that these new transformations were merely a useful formal device.

In sum, the reasoning of the ether theorists can be laid out as follows:

1. When I consider the Earth at rest versus moving with some velocity v, I am to construe those states as being related by Galilean transformations.

2. Since Maxwell's equations are *not* invariant under such transformations, I should expect different electromagnetic physics in the two states—in particular, I should expect a different velocity of light in the pre- and post-transformed states.

3. In light of the null results of experiments such as that of Michelson and Morley, I postulate that material bodies contract under Galilean boosts: that is, I postulate more relativity principle-violating physics to cancel out the first relativity principle-violating physics, and explain why I don't *observe* violations of the relativity principle.

Einstein would reject the first premise here—I'll tackle in detail how he achieved this in the following chapter, but in brief for now: he would argue that when I consider the Earth at rest versus moving with some velocity v, I am (in light of the dynamical constitution of the bodies under consideration) to construe those states as being related by Lorentz transformations, so that (a) the speed of light does not vary from inertial frame to inertial frame, and (b) accordingly, no *ad hoc* compensating dynamical effects are required in order to save the relativity principle.[9]

Continuing to focus for now on the ether theorists' dynamical contraction hypotheses, there is only one further point which I wish to make here. As time

[9] Of course, the Lorentz transformations famously entail the phenomenon of length contraction, which will be discussed in detail in Chapter 9, but for the time being we should take this to have a different conceptual status to the kind of contraction postulated by authors such as Fitzgerald, Larmor, and Lorentz—in effect, Einstein elevated contraction to a *kinematical* effect.

went on, the exact nature of the dynamical contraction hypotheses required in order to underwrite the null results of Michelson-Morely-type experiments became increasingly delicate and *ad hoc*. As Brown writes,

> Lorentz noted that the theorem of corresponding states actually implies that the frequency of oscillating electrons in the light source is affected by motion of the source, and it is this fact that gives rise to the change in frequency of the emitted light. But Lorentz realized that the oscillating electrons only satisfy Newton's laws of motion if it is assumed that both their masses and the forces impressed on them depend on the electrons' velocity relative to the ether. The hypotheses in Lorentz's system were starting to pile up, and the spectre of *ad hoc*ness was increasingly hard to ignore (as Poincaré would complain). [14, p. 56]

Something had to give—enter Einstein.

Chapter 4

Einstein's 1905 derivation of the Lorentz transformations

> The introduction of a "luminiferous ether" will prove to be superfluous inasmuch as the view here to be developed will not require an 'absolutely stationary space' provided with special properties, nor assign a velocity-vector to a point of the empty space in which electromagnetic processes take place. (Einstein, 1905)

In 1905, Einstein published four papers in the journal *Annalen der Physik*, each of which precipitated a revolution in physics. The papers were on:

1. The photoelectric effect. [40]
2. Brownian motion. [41]
3. Special relativity. [49]
4. Mass-energy equivalence. [43]

Quite rightly, the year would come to be known as Einstein's *annus mirabilis.* Einstein's 1905 derivation of the Lorentz transformations in his third *annus mirabilis* paper, *On the Electrodynamics of Moving Bodies* [42], purports to account for all ether wind experiment null results, without recourse to dynamical considerations *à la* Lorentz *et al.* In effect, it elevates contraction from a dynamical effect to a kinematical effect: *all physics must be conditioned such that it is invariant under Lorentz boosts.* In this way, the relativity principle could be reconciled with the Poincaré invariance of Maxwell's equations. Distinctive features of Einstein's approach include the following:

1. It eliminates "asymmetries which do not appear to be inherent in the phenomena" [42]. (Here, Einstein is referring to Lorentz's responses to the ether wind null results.)

2. It accounts for *all* null ether wind results.

3. It does not postulate an ether, or a standard of absolute rest, at all.

4. It is a 'principle theory', rather than a 'constructive theory'.

The idea—already outlined at the end of the previous chapter—is that when one boosts a material body with velocity v, one should (in light of the dynamics of that body—more on this below) take it that the boosted state is related to the original state by a Lorentz transformation, rather than a Galilean transformation. In this way, one need not invoke dynamical contraction hypotheses in order to compensate for the fact that the velocity of light would differ in frames related by Galilean boosts. In other words, for Einstein, the theorists which preceded him had *misunderstood the nature of boosts*. Here's how Einstein himself put the matter towards the start of his article:

> Examples of this sort, together with the unsuccessful attempts to discover any motion of the earth relatively to the "light medium," suggest that the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest. They suggest rather that, as has already been shown to the first order of small quantities, the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good. [42]

My purpose in this chapter is to dissect Einstein's 1905 derivation of the Lorentz transformations, as it appears in [42]. Before doing so, however, I should say something on the above-mentioned distinction between 'principle' and 'constructive' theories of physics.

4.1 Principle and constructive theories

The theory presented in Einstein's 1905 article [42] is something which he would later recognise to be a 'principle theory', rather than a 'constructive theory'. Einstein introduced this distinction in a 1919 article in the London *Times*, where he wrote that:

> Most [theories in physics] are constructive. They attempt to build up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme from which they start out. Thus,

> the kinetic theory of gases seeks to reduce mechanical, thermal, and diffusional processes to movements of molecules ...
>
> [Principle theories, by contrast,] employ the analytic, not the synthetic method. The elements which form their basis and starting point are not hypothetically constructed but empirically discovered ones, general characteristics of natural processes, principles that give rise to mathematically formulated criteria which the separate processes ... have to satisfy ... The theory of relativity belongs to the latter class. [48]

The distinction between principle and constructive theories which Einstein presents in the above passage can be cashed out thus:

Constructive theories: Theories which attempt to provide a detailed dynamical picture of what is microscopically going on, from which predictions for observable phenomena can be derived.

Principle theories: Theories which take certain phenomenologically well-grounded principles, raises them to the status of *postulates*, and derive from them constraints on what the underlying detailed dynamical equations could be like, without attempting to give a fully detailed account of what those equations *are*.

A paradigm example of a principle theory is thermodynamics; the 'phenomenologically well-grounded postulates' in this case are the laws of thermodynamics, from which one derives (say) relations between certain functions of state. The corresponding constructive theory in this case, as Einstein points out in the above passage, would be the (statistical) kinetic theory of gases.

One might think that constructive theories are superior to principle theories, in the sense that the former are able to provide deeper, mechanistic explanations for physical phenomena in a way that the latter are not. But in that case, why was Einstein's 1905 formulation of special relativity—which (in 1919) he declared to be a *principle* theory—so celebrated? One might be motivated to construct a principle theory by wanting to make *some* progress, before the fully detailed microphysical picture (constructive account) is known. Einstein in 1905 saw himself as being in this situation: Lorentz had been pursuing a constructive approach, but Einstein was bothered by deep suspicions that the true equations governing intermolecular forces were very far from being known.[1] It is, however, worth registering Einstein's reservations about principle theories:

---

[1]It doesn't have to be only historical circumstances which justify the use of principle theories—Einstein himself in his 1919 article points out that such theories have the merits of being connected directly with empirical experience, and so of indubitability (here, there are interesting and under-explored connections with the programme of 'constructive axiomatics' promulgated by Reichenbach in 1924 [142]: see [31, 92] for discussion). Moreover, there may be certain explanatory factors which militate in favour of the use of principle theories—as Van Camp writes:

> Constructive theories are grounded in their ability to offer causal-mechanical explanations of phenomena, a type of scientific explanation most prominently advocated by Salmon [146].

> It seems to me ... that a physical theory can be satisfactory only when it builds up its structures from elementary foundations. [55]

> [W]hen we say we have succeeded in understanding a group of natural processes, we invariably mean that a constructive theory has been found which covers the processes in question. [48]

4.2 Einstein's 1905 article

Having recognised that Einstein was following the principle theory approach in his 1905 article—simply *assuming* (on the basis of phenomenological observations, e.g. no observed violations of the relativity principle) that the symmetry group for the laws of mechanics should be the same as the symmetry group for the laws of electromagnetism, without a clear understanding of the dynamics of matter which would underwrite this fact—I'll now present Einstein's derivation of the Lorentz transformations as presented in his 1905 article.

Before I begin, there's one additional point to make. There are questions which one might reasonably have about Einstein's methodology in his article. Since the Lorentz transformations were already known by 1905, what was Einstein adding to extant knowledge? The point is that Lorentz *et al.* derived these transformations on the basis of detailed *dynamical* considerations. By contrast, Einstein would (a) proceed via phenomenological considerations regarding light and the relativity principle (and so would avoid having to make unjustified conjectures regarding underlying dynamics), and (b) would, as we have already seen, elevate the resulting transformations to a *kinematical constraint*. I'll come back to these differences between Einstein and Lorentz later in this chapter.

Good. Without further ado, then, I'll now proceed by extracting the central threads in Einstein's 1905 derivation, and discussing them in turn.

4.2.1 Einstein's operational understanding of coordinates

At the beginning of his article, Einstein is explicit that he has an *operational* understanding of coordinates. This understanding means that he requires spatial coordinates to 'match' the length of rigid measuring rods that are at rest in the system in question, and time coordinates to 'match' the tickings of clocks at rest in that system. The appeal to rigid rods and regular clocks in his adumbration of his understanding of coordinates is something which Einstein would later come to regret, as I'll discuss below (cf. [64]); moreover and more generally, Einstein would struggling throughout his career with how to understand coordinate systems (on this, see [65]).

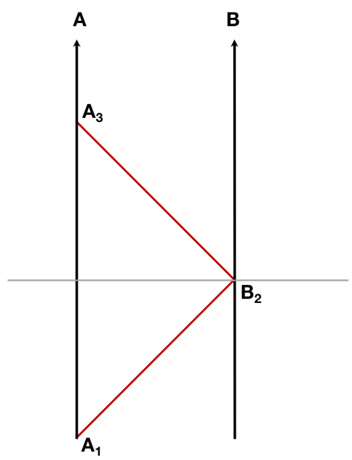
> Principle theories are also explanatory. The primary function of a principle theory is tied to the explanatory role it plays through unification. The theory of explanation as unification was first advanced by Friedman [59] and has been developed since by Kitcher [84]. [161, pp. 23-24]

For further discussion of these issues, see [136] and references therein.

For my purposes in this section, I'll follow Einstein in simply assuming this understanding of coordinates. Indeed and in any case, even to set up *one* coordinate system, we need more than this: we need to *decide* how to synchronise clocks that are spatially separated from one another. Having presented his understanding of coordinates, Einstein next turns his attention to this very matter.

4.2.2 The definition of simultaneity

Consider the following setup: two mirrors A and B are some fixed distance L apart. A photon is fired from A at event (i.e., spacetime point) A_1, bounces off B at B_2, and returns to A at A_3, as per the following diagram (here, space runs along the horizontal axis and time along the vertical axis, as standard):



Now ask: which point on the worldline of mirror A is simultaneous with point B_2 on the worldline of mirror B? The natural answer stipulated by Einstein (following Poincaré) is the following (here 't_A' indicates the time read off by a clock at A; *mutatis mutandis* for B):

$$t_B\left(B_2\right) = t_A\left(A_1\right) + \frac{1}{2}\left(t_A\left(A_3\right) - t_A\left(A_1\right)\right). \tag{4.1}$$

That is, B_2 is simultaneous with the point *half-way* between A_1 and A_3 on A's worldline. This makes the one-way speed of light isotropic. (One would be perfectly within one's rights to ask whether this is the *only* way of 'spreading time through space' in special relativity—I'll return to this issue in Chapter 7.) For the time being, we can treat this as a *conventional choice* made by Einstein for how to synchronise distant clocks: typically, it is referred to as the *Einstein-Poincaré clock synchrony convention*.

4.2.3 Einstein's two postulates

We turn now to what many would regard as the main event: Einstein's two postulates of special relativity. These are the relativity principle (**RP**) (which we've already discussed at some length) and the light postulate (**LP**) (something on which we also remarked in the previous chapter). As stated by Einstein, these read as follows:

RP: The laws by which the states of physical systems undergo change are not affected, whether these changes be referred to the one or the other of two systems of coordinates in uniform translatory motion.

LP: Any ray of light moves in the 'stationary' system of coordinates with the determined velocity c, whether the ray be emitted by a stationary or by a moving body. Hence [*sic*?]

$$\text{velocity} = \frac{\text{light path}}{\text{time interval}},$$

where time interval is to be taken in the sense of the definition in section 1.

Note that both **RP** and **LP** accord with the methodology of a principle theory: as we've already seen, (i) there were no empirically observed violations of **RP**, and (ii) light is a wave, so (like all waves) is such that the speed of the wave is independent of the speed of the source (which is **LP**). (Sometimes, **LP** is identified with the 'constancy of the speed of light', but this is not how the principle is stated. The constancy of the speed of light *in inertial frames* follows from a combination of **RP** and **LP**; in non-inertial frames, the speed of light need not be c.[2]) Einstein's point is going to be that (a) these conditions (plus the extra assumptions involved in the 1905 paper, namely those discussed above and also below) together imply that transformations between frames are the Lorentz transformations, and (b) if all material bodies are governed by equations which are invariant under these transformations, then one no longer predicts violations of **RP**, so there is no longer any need for a preferred frame, *a fortiori* no longer any need for an ether.

4.2.4 Homogeneity, isotropy, and reciprocity

The game is now to derive coordinate transformations from the above principles, along with a couple of others. In particular, Einstein will also need to assume:

1. The homogeneity of space and time. ('Every point in space and time is the same as every other.')
2. The isotropy of space. ('There is no privileged direction in space.')

[2] Cf. footnote 5 in Chapter 3.

Note that homogeneity and isotropy are not equivalent: an example of a homogeneous but anisotropic space would be (say) a set of vectors all pointing in the same direction in a space; an example of an inhomogeneous but isotropic space would be one line γ, with vectors emanating radially from this line (in this case, the space is inhomogeneous, but isotropic about γ).

Also worthy of mention is the principle of 'Reciprocity', which states the following: If two inertial coordinate systems S and S' are such that S' is moving with speed v in the positive x direction relative to S, then S is moving with speed v in the negative x direction relative to S'. As Brown mentions, this principle holds if and only if the Einstein-Poincaré synchrony convention is adopted in both S and S' [14, p. 118]. Von Ignatowski claimed that Reciprocity follows from **RP** alone; however, in the absence of any stipulation regarding a clock synchrony convention, this claim is incorrect (see [159, p. 79]). Berzi and Gorini showed in [10], however, that Reciprocity *can* be derived from a combination of **RP** and spatial isotropy. Although these observations are independently interesting, the main point regarding Reciprocity which I want to make is this: although the principle can be invoked at certain points in Einstein's derivation (see below), it is not necessary to take this to be an independent assumption: rather, it can be derived from Einstein's other assumptions already presented above.

4.2.5 Linearity of the transformations

Homogeneity implies that the transformations between inertial frames must be linear. Einstein doesn't spell out how this works, but a reconstruction can be found in [14, §2.3]. Generic transformations between frames can be written

$$x'^{\mu} = f^{\mu}\left(x^{\nu}\right). \tag{4.2}$$

Suppose that the transformations encode information on the behaviour of rods and clocks (recall Einstein's operational understanding of coordinate systems). Then such behaviour should not depend on where the rods and clocks find themselves in space or time, on pains of violation of homogeneity. Consider now the infinitesimal version of the above transformation law,

$$dx'^{\mu} = \frac{\partial f^{\mu}}{\partial x^{\nu}} dx^{\nu}. \tag{4.3}$$

Homogeneity implies that the coefficients $\partial f^{\mu}/\partial x^{\nu}$ must be independent of the x^{ν} coordinates, which means that f^{μ} must be linear functions of the coordinates x^{μ}.

4.2.6 Lorentz transformations up to $\phi(v)$

Following Einstein, we now let K be a 'stationary' system, and let (t, x, y, z) be coordinates for K, determined by the conditions of surveyability-using-rods-and-clocks-that-are-stationary-in-K and the Einstein definition of simultaneity applied in K (for t). We let k be a system of coordinates that is moving with speed v along the positive x-direction relative to the 'stationary' system K, and let (τ, ξ, η, ζ) be coordinates for k, determined by the conditions of surveyability-using-rods-and-clocks-that-are-stationary-in-k and the Einstein definition of simultaneity applied in k (for τ). Using Einstein synchrony in k and the linearity of the coordinate transformations, Einstein derives (I'll omit his steps, since they are straightforward)

$$\tau = \phi(v)\,\gamma\left(t - \frac{vx}{c^2}\right). \tag{4.4}$$

Now consider a light ray emitted from the origin in the positive ξ-direction. Using **RP** and **LP** to write down expressions for the relationship between ξ and τ that holds on the path of this ray, and similarly (using **RP** alone) for the relationship between x and t that holds on the path of this ray, Einstein likewise derives therefrom that

$$\xi = \phi(v)\,\gamma\,(x - vt). \tag{4.5}$$

Similarly, by considering rays of light emitted in the η and ζ directions from the perspectives of both K and k, Einstein obtains

$$\eta = \phi(v)\,y, \tag{4.6}$$

$$\xi = \phi(v)\,z. \tag{4.7}$$

(4.4)-(4.7) are the Lorentz transformations, up to a velocity-dependent factor $\phi(v)$.

4.2.7 Final steps

The final steps involve setting $\phi(v) = 1$, and thereby recovering the Lorentz transformations. First, one invokes **RP** and Reciprocity in order to argue that $\phi(v)\,\phi(-v) = 1$. Now, given Einstein's operational understanding of coordinates, $\phi(v)$ can be interpreted physically as the inverse of the *transverse length contraction factor*, i.e., the factor by which setting a body in motion causes that body to shrink in the direction perpendicular to its motion. Given that interpretation, isotropy entails that $\phi(v) = \phi(-v)$, so one has $\phi(v)^2 = 1$. We then argue somehow against the rogue possibility that $\phi(v) = -1$ (using continuity and $\phi(0) = +1$?—Einstein does not discuss this explicitly). It then follows that $\phi(v) = 1$. This yields the by-now familiar Lorentz transformations!

4.3 Einstein versus the trailblazers

Einstein's 1905 paper predicts once and for all the null result of ether wind experiments such as that of Michelson and Morley. Indeed, it does so *trivially*—just by insisting upon **RP** alongside **LP**. As I've already mentioned, one way to understand Einstein is as insisting that the laws of *mechanics* should also be

Poincaré invariant—he is making Poincaré invariance universal, as a *kinematical constraint*. One sometimes finds the claim that Lorentz was not happy with Einstein's approach, as might seem apparent in passages such as the following:

> Einstein simply postulates what we have deduced, with some difficulty and not altogether satisfactorily, from the fundamental equations of the electromagnetic field. [96, p. 230]

To be fair to Lorentz, however, he followed the above passage with this concession:

> By doing so, he may certainly take credit for making us see in the negative result of experiments like those of Michelson, Rayleigh and Brace, not a fortuitous compensation of opposing effects but the manifestation of a general and fundamental principle. [96, p. 230]

As Brown writes, "The full meaning of relativistic kinematics was simply not properly understood before Einstein" [14, p. 68].

It's worth asking oneself how radical Einstein's 1905 approach really was. Arguably, Newton himself was constructing a principle theory—the postulates being his three laws of motion. When combined with the **RP** and the auxiliary hypotheses mentioned in Chapter 2, these imply the Gailiean invariance of physical laws (as a *kinematical constraint*—i.e., independent of the details of the particular dynamics governing matter). This, indeed, was achieved by Albert Keinstein in 1705: see [14, §3.3].[3]

Question: Given the above, was Einstein, in deriving a *different* kinematical constraint (*viz.*, Poincaré invariance, rather than Gailean invariance), really being any more radical than Newton?

4.4 Einstein's later misgivings

Einstein would later voice certain misgivings about his 1905 derivation, in particular regarding:

1. The treatment of rods and clocks as primitive bodies, not "moving atomic configurations". [50, 54]

2. The special role of light. [52, 54]

On (1), here's what Einstein wrote in his 1949 *Autobiographical Notes*:

> One is struck [by the fact] that the theory [of special relativity] ... introduces two kinds of physical things, i.e. (1) measuring rods and clocks, (2) all other things, e.g., the electromagnetic field, the material point, etc. This, in a certain sense, is inconsistent; strictly

[3] Please note that Keinstein is fictional!

> speaking measuring rods and clocks would have to be represented as solutions of the basic equations (objects consisting of moving atomic configurations), not, as it were, as theoretically self-sufficient entities. However, the procedure justifies itself because it was clear from the very beginning that the postulates of the theory are not strong enough to deduce from them sufficiently complete equations ... in order to base upon such a foundation a theory of measuring rods and clocks. ... But one must not legalize the mentioned sin so far as to imagine that intervals are physical entities of a special type, intrinsically different from other variables ('reducing physics to geometry', etc.). [54]

The point is that (as per its being a principle theory) Einstein's approach in 1905 simply assumes that there exist boostable rods and clocks, which when boosted read of intervals as per a Lorentz transformed frame. Ultimately, this is a dynamical assumption, which should be justified rather than assumed: I'll return to this issue in Chapter 10.

On (2), Einstein wrote this:

> The special theory of relativity grew out of the Maxwell electromagnetic equations. But ... the Lorentz transformation, the real basis of special-relativity theory, in itself has nothing to do with the Maxwell theory. [52]

> [T]he Lorentz transformation transcended its connection with Maxwell's equations and had to do with the nature of space and time in general. [14, p. 73]

The point here is that the later Einstein viewed the appeal to Maxwell's electrodynamics in the 1905 paper as a heuristic tool into special relativity (based upon the historical contingency that the first Poincaré invariant laws to be discovered were those of Maxwell), but in fact, once the completed theory of special relativity is in hand, one recognises that it has noting *in particular* to do with electrodynamics. Here's how the later Einstein put the point:

> The content of the restricted relativity theory can accordingly be summarised in one sentence: all natural laws must be so conditioned that they are covariant with respect to Lorentz transformations. [53]

4.5 The Ignatowksi transformations

In 1911, von Ignatowski sought to derive the Lorentz transformations using **RP**, but *without* ***LP*** [79]. This claim should elicit suspicion: which of the remaining assumptions is violated by Newtonian physics (complete with Galilean transformation—cf. again the fable of Keinstein)? Let's delve into this. The Ignatowski transformations (i.e., those derived by von Ignatowski in his 1911 article) read as follows, where K is some hitherto-unspecified universal constant:

$$t' = \left(1 - Kv^2\right)^{-1/2}(t - Kvx)\,, \tag{4.8}$$

$$x' = \left(1 - Kv^2\right)^{-1/2}(x - vt)\,, \tag{4.9}$$

$$y' = y, \tag{4.10}$$

$$z' = z. \tag{4.11}$$

Note now three special cases:

- Setting $K = 0$ yields a Galilean transformation.
- Setting $K = 1$ yields a Lorentz transformation.
- Setting $K = -1$ yields a Euclidean transformation.

Recall that Galilean transformations consist of rigid 3D spatial rotations, Galilean boosts, and rigid translations; Poincaré transformations consist of rigid 3D spatial rotations, Lorentz boosts (together, these are the 'Lorentz transformations'), and rigid translations. We have not seen the 4D Euclidean transformations up to this point, but these consist of rigid 4D rotations, plus rigid translations.

These results vindicate our suspicion: Galilean, Lorentz, and Euclidean transformations are thus *all* special cases of the Ignatowski transformations. So dropping **LP** is not sufficient to derive the Lorentz transformations. Sometimes, authors rule out $K = -1$ as "unphysical" (see e.g. [119])—to this one should also object, for there are plenty of physical applications of theories with Euclidean symmetries—e.g., any theory which uses the Poisson equation.[4]

[4]For further discussion on this point, see [24].

Chapter 5

Spacetime structure from Aristotle to Minkowski

> Geometry is found in mechanical practice, and is nothing but that part of universal mechanics.
> (Newton, 1678)

Up to this point, we've witnessed the crisis in physics which precipitated the advent of special relativity; we've also seen Einstein's derivation of the Lorentz transformations in his 1905 paper, the upshot of which was supposed to be that these transformations (with invariant speed c) constitute a *kinematical constraint* on future physical theorising. So far, however, mention of spacetime has been conspicuously absent: we haven't seen the term since Chapter 1!

In fact, in was only in 1909 that Hermann Minkowski—one of Einstein's old teachers at the *Eidgenössische Polytechnikum* (now ETH Zurich)—showed that theories with Poincaré symmetries can be understood as being set in what has now become known as *Minkowski spacetime*. In his paper, Minkowski introduced the 'world-postulate': the principle that all fundamental physical laws must be conditioned so as to be Poincaré invariant. This, as we have seen, was already to be found in Einstein, but by expressing this notion in four-dimensional geometrical language, Minkowski felt he had shown how "the validity of the world-postulate ... now lies open in the full light of day." [109]

> **Question:** Can what Minkowski suggests here be understood as a precursor to a Friedman-style 'geometrical' approach to physical theories? (Cf. Chapter 1.)

My purpose in this chapter is to explain what this spatiotemporal structure amounts to, as well as to compare this structure with the Newtonian spacetime structures of which we already saw a little in Chapter 1. Before doing so, however, it's worth mentioning Einstein's initial reaction to Minkowski's spatiotemporal reformulation of special relativity. In response to Minkowski's somewhat grandiose claim that, having set theories in his spacetime, "Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality" [109], Einstein accused this work of being "superfluous learnedness" [117]. At the end of Chapter 6, I'll consider what it even *means* for a theory to be 'special relativistic'; this reaction on the part of Einstein will be worth bearing in mind.

5.1 Two conceptions of geometry

Before introducing the specific details of Minkowski spacetime, we need to take a step back. In general, there are two different approaches to understanding geometrical notions: the 'Kleinian approach', and the 'Riemannian approach':[1]

Kleinian conception: Geometry is characterised via the invariance groups of certain structures under coordinate transformations.[2]

Riemannian conception: Geometry is characterised via tensors and other coordinate-independent differential-geometric structures.

In this chapter, I'll focus on the Kleinian approach to geometry, and defer a discussion of the Riemannian approach to the next chapter. The general idea of the Kleinain approach—from a physical point of view—is as follows. We have seen that the inertial frames are those coordinate systems in which dynamical equations governing matter take their simplest form, and in which force-free particles move with uniform velocity. Sometimes, people also think about the inertial frames as those frames which respect spacetime's 'inertial structure' in a certain way. On the Kleinian approach, one can then use the transformations between the inertial frames of a theory to ascertain that theory's spacetime geometrical commitments. The three-point plan is this:

1. Specify the class of coordinate transformations which relate the inertial frames in the theory under consideration.

2. Identify the structures and quantities which are *invariant* under those transformations.

3. Regard these structures and quantities as picking out different kinds of spacetime.

I'm first going to illustrate how the Kleinian approach works in the case of Newtonian theories; only after doing so will I turn to the case of special relativity.

[1]For more detail on the distinction between these two approaches, see [165].

[2]This approach to geometry is the central idea underlying Klein's 'Erlangen programme' for geometry.

5.2 Spacetime structure in Newtonian physics

Perhaps surprisingly, the question of the spacetime structure of Newtonian mechanics turns out to be a very delicate business—in fact, much *more* delicate than in the case of relativistic theories. There is, indeed, a hierarchy of possible spacetime structures in Newtonian mechanics: running from strongest to weakest, this reads (for an extremely elegant summary of this hierarchy, see [35, ch. 2]):

Aristotelian spacetime
↓
Newtonian spacetime
↓
Neo-Newtonian/Galilean spacetime
↓
Maxwellian/Newton-Huygens spacetime
↓
Leibnizian spacetime
↓
Machian spacetime

5.2.1 Aristotelian spacetime

Let's begin with Aristotelian spacetime, as conceived on the Kleinian approach.[3] Suppose that one has a physical theory in which the dynamical equations take their simplest form in coordinate systems related by the following (rather restricted!) set of *Aristotelian transformations*:

$$t \mapsto \pm t + \tau \tag{5.1}$$

$$\mathbf{x} \mapsto \mathbf{R}\mathbf{x} \tag{5.2}$$

One now asks: what is preserved under such transformations? In this case, a great deal! The following structures are all invariants of the above Aristotelian transformations:

1. A notion of spatial distance.

2. A notion of temporal distance.

3. A standard of rotation across time.

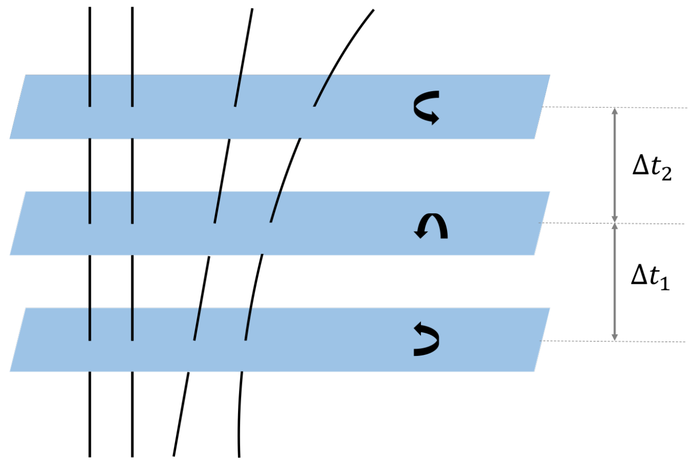
4. A notion of straightness of paths across time.

5. A preferred velocity.

6. A preferred point.

[3]Throughout the following, $\mathbf{R} \in SO(3)$ and any functions of t are smooth.

In every case, the reason is that the structure in question is unaffected by time translations/inversions, and/or spatial rotations—which exhaust the above Aristotelian transformations. Given this, one can proceed to draw a picture of a spacetime which preserves all of these notions—it might look something like this:



In this image, let the vertical line on the left be the preferred point; anything comoving with respect to the preferred point has the preferred velocity (and acceleration—i.e., standard of straightness of paths across time). In the spacetime, there is also a standard of rotation, allowing one to adjudicate on whether or not an object is spinning (this is represented by the curved arrows to the right); there is also a preferred notion of spatial distance at a time (on the blue hypersurfaces), and of temporal distance (between the blue slices). Thus, respectively, absolute position, velocity, acceleration, rotation, temporal distance, and spatial distance are all well-defined in Aristotelian spacetime.

5.2.2 Newtonian spacetime

Suppose now that one liberalises the Aristotelian transformations to the following class of *Newtonian transformations*:

$$t \mapsto \pm t + \tau \tag{5.3}$$

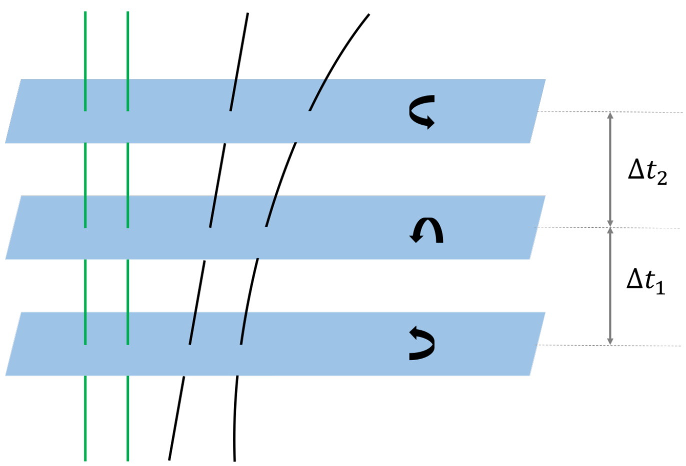
$$\mathbf{x} \mapsto \mathbf{R}\mathbf{x} + \mathbf{a} \tag{5.4}$$

In particular, note that the Newtonian transformations—unlike the Aristotelian transformations—allow for constant translations of the spatial coordinates. This means that a preferred point is no longer well-defined in Newtonian spacetime, for such a point would not be left invariant by spatial translations! Thus, only the following concepts are well-defined in Newtonian spacetime:

1. A notion of spatial distance.
2. A notion of temporal distance.
3. A standard of rotation across time.
4. A notion of straightness of paths across time.
5. A preferred velocity.
6. ~~A preferred point.~~

Exercise: Convince yourself that structures (1)-(5) remain well-defined in Newtonian spacetime.

Accordingly, schematically, a picture of Newtonian spacetime might take the following form:



Here, the colouring green is supposed to indicate that the two trajectories can be mapped into one another using the transformations of the Newton group, so there is no sense in this spacetime structure in which one is 'preferred' over the other. (In later diagrams in this chapter, the same rationale underlying the green colouring applies.) Although here one no longer has a preferred point, one retains the trans-temporal identity of spacetime points, which affords a 'rigging' (i.e., congruence of vertical lines) with respect to which absolute velocity and acceleration can be defined.

5.2.3 Neo-Newtonian/Galilean spacetime

Let's press on, in the same spirit. Suppose that one now liberalises the Newtonian transformations to the *Galilean transformations*:

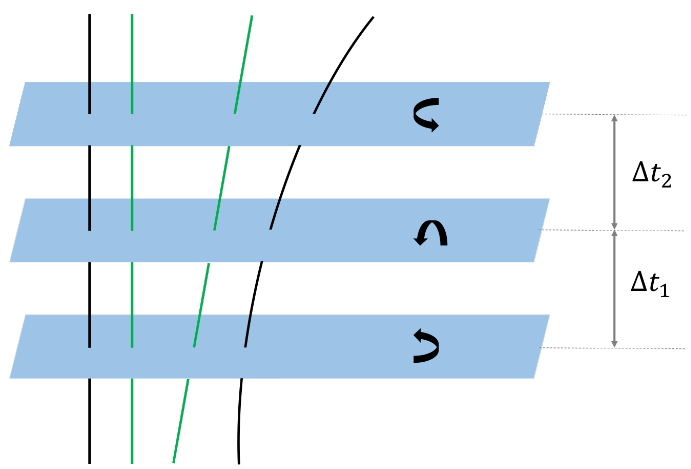
$$t \mapsto \pm t + \tau \tag{5.5}$$

$$\mathbf{x} \mapsto \mathbf{R}\mathbf{x} + \mathbf{v}t + \mathbf{a} \tag{5.6}$$

Galilean transformations—unlike Newtonian transformations—now allow for constant velocity transformations of the spatial coordinates. This means that a preferred velocity is no longer well-defined in Galilean spacetime (sometimes called 'neo-Newtonian spacetime', but it's worth stressing that the terms are completely interchangeable), for such a velocity would not be preserved under Galilean transformations! Thus, only the following concepts are well-defined in Galilean spacetime:

1. A notion of spatial distance.
2. A notion of temporal distance.
3. A standard of rotation across time.
4. A notion of straightness of paths across time.
5. ~~A preferred velocity.~~
6. ~~A preferred point.~~

(Again, one can convince oneself that structures (1)-(4) are well-defined in Galilean spacetime.) Schematically, a picture of Galilean spacetime would look like this:



Here, the curved line is supposed to indicate that there remains a standard of absolute acceleration in Galilean spacetime, even though one can map the first (vertical) green line (i.e. the worldline of a body with some uniform velocity) to the second (non-vertical but straight) green line (i.e., the worldline of a body with some other uniform velocity) by the action of the Galilean group. One might be puzzled by this: how can there be a standard of absolute acceleration, but not of absolute velocity? At this point, suffice it to say that this is a well-defined *mathematical* possibility; I hope to be able to shed further light on this question in the following chapter, when I discuss the Riemannian approach to geometry.

5.2.4 Maxwellian/Newton-Huygens spacetime

Next, suppose we liberalise the Galilean transformations to the *Maxwell transformations*:

$$t \mapsto \pm t + \tau \tag{5.7}$$

$$\mathbf{x} \mapsto \mathbf{R}\mathbf{x} + \mathbf{a}\,(t) \tag{5.8}$$

We now allow for arbitrary time-dependent transformations of the spatial coordinates. In this case, a preferred acceleration (i.e., standard of straightness of paths across time) is no longer well-defined, for it is not preserved under such transformations. Thus, only the following concepts are well-defined in Maxwellian/Newton-Huygens spacetime (again, the terms are completely interchangeable):

1. A notion of spatial distance.

2. A notion of temporal distance.

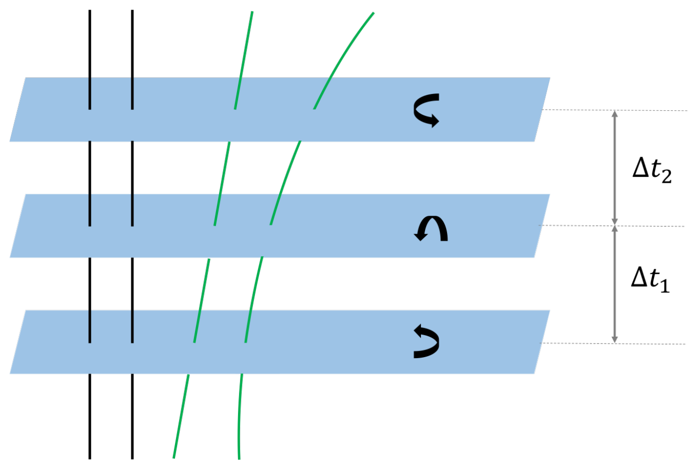
3. A standard of rotation across time.

4. ~~A notion of straightness of paths across time.~~

5. ~~A preferred velocity.~~

6. ~~A preferred point.~~

Schematically, a picture of Maxwellian spacetime might look like this:



In this case, one can no longer distinguish between curved and straight lines through this spacetime structure.

5.2.5 Leibnizian spacetime

Suppose we liberalise the Maxwell transformations to the *Leibniz transformations*:

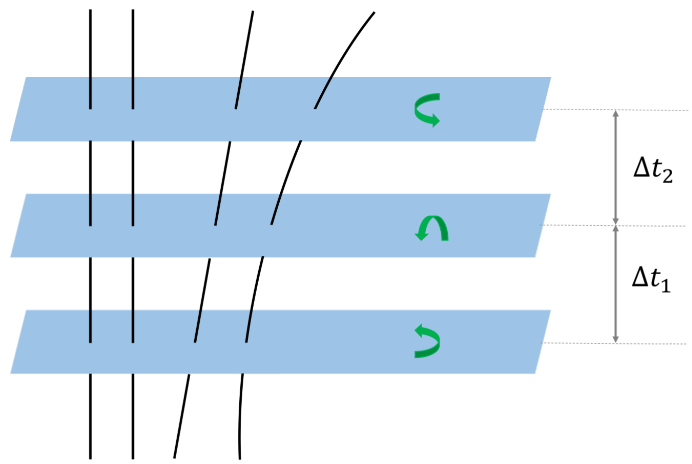
$$t \mapsto \pm t + \tau \tag{5.9}$$

$$\mathbf{x} \mapsto \mathbf{R}(t)\,\mathbf{x} + \mathbf{a}(t) \tag{5.10}$$

In this case, we allow for arbitrary time-dependent *rotations* of the spatial co-ordinates. This means that a standard of rotation is no longer well-defined in Leibnizian spacetime, for rotation rate need not be left invariant under such transformations. Thus, only the following transformations are well-defined in Leibnizian spacetime:

1. A notion of spatial distance.
2. A notion of temporal distance.
3. ~~A standard of rotation across time.~~
4. ~~A notion of straightness of paths across time.~~
5. ~~A preferred velocity.~~
6. ~~A preferred point.~~

Schematically, a picture of Leibnizian spacetime might then look like this:



Here, colouring the curved arrows green is supposed to indicate that there is no standard of rotation in Leibnizian spacetime.

5.2.6 Machian spacetime

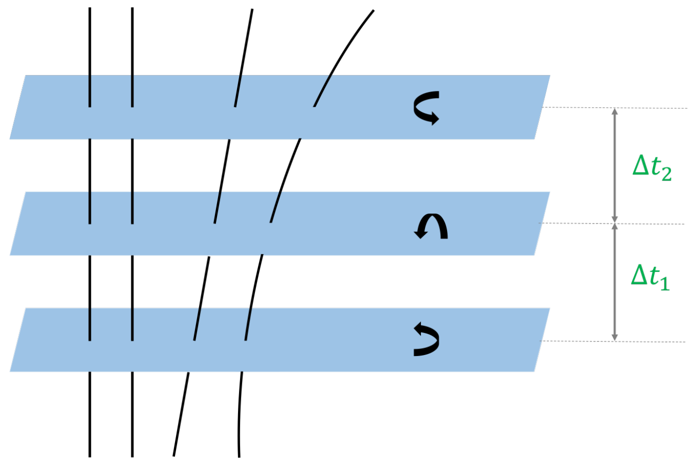
Now suppose that we liberalise the Leibniz transformations to the *Machian transformations*:

$$t \mapsto f(t) \qquad (f \text{ monotonic}) \tag{5.11}$$
$$\mathbf{x} \mapsto \mathbf{R}(t)\,\mathbf{x} + \mathbf{a}(t) \tag{5.12}$$

We now allow for arbitrary rescalings of the temporal coordinates; this means that a preferred notion of temporal distance is no longer well-defined in Machian spacetime, for temporal distance is not an invariant of such transformations. Thus, only the following transformations are well-defined in Machian spacetime:

1. A notion of spatial distance.
2. ~~A notion of temporal distance.~~
3. ~~A standard of rotation across time.~~
4. ~~A notion of straightness of paths across time.~~
5. ~~A preferred velocity.~~
6. ~~A preferred point.~~

Schematically, a picture of Machian spacetime might look like this:



Here, colouring of the temporal intervals between spacetime hypersurfaces is supposed to indicate that such intervals are not invariants of the Machian transformations.

5.2.7 Summary

The above constitutes the standard hierarchy of Newtonian spacetimes, as one will find in e.g. [35, ch. 2]. I think it suffices by now to illustrate the general point: as one liberalises one's class of allowed transformations (which, physically, are to be understood as relating the frames of reference in which one's description of the physics takes its simplest form), the number of invariants of those transformations decreases; thus, one's spacetime geometrical structure (understood as per the Kleinian approach) becomes, in a clear sense, weaker. The general moral here is worth keeping in one's mind:

More symmetries $\iff$ Less structure

Also worthy of mention is that there are other possible elements of the hierarchy of Newtonian spacetimes which I've elided on the grounds that they're not necessary to make the above general conceptual points. First: one might allow reflections of the spatial coordinates, so $\mathbf{x} \mapsto \pm\mathbf{x}$; in this case, spacetime would no longer have a preferred spatial *orientation* (see e.g. [75]). Second: one might allow for *rescalings* of the spatial coordinates: $\mathbf{x} \mapsto \mathbf{\Omega}\mathbf{x}$ (here, $\mathbf{\Omega}$ is a matrix implementing a possibly spacetime-dependent scale transformation); in this case, only spatial *conformal structure* (i.e., angles, but not distances) would be well-defined.[4]

5.3 Spacetime structure in special relativity

By now, I've spent a lot of time presenting possible Newtonian spacetime structures, through the lens of the Kleinian approach to geometry. At this point, we must ask: how does the spacetime structure of special relativity compare with that of the spacetimes we have just seen? To think about and answer this question, it's going to be helpful to switch notation. Consider again the coordinate transformations associated with Galilean spacetime. So far, I've written these in vector notation, as in 5.5 and (5.6). The equivalent expressions in *index notation* would be

$$t \mapsto \pm t + \tau \tag{5.13}$$

$$x^i \mapsto R^i{}_j x^j + v^i t + a^i \tag{5.14}$$

Note that all terms must have the same free indices, and the Einstein summation convention is used (so that indices which appear twice in a term are summed over). By convention, we use Latin indices ($i, j, \ldots = 1, 2, 3$) for spatial indices, and Greek indices ($\mu, \nu, \ldots = 0, 1, 2, 3$) for space*time* indices.

With this in mind, we can present the *Poincaré transformations* as follows:

$$x^\mu \mapsto \Lambda^\mu{}_\nu x^\nu + a^\mu \qquad (\Lambda^\mu{}_\nu \in SO\,(1,3)). \tag{5.15}$$

The spacetime structure which is left invariant under the action of the Poincaré transformations *just is* the spacetime structure which Minkowski introduced in 1909. In this spacetime—predictably, dubbed *Minkowski spacetime*—there is:

1. ~~A notion of spatial distance.~~
2. ~~A notion of temporal distance.~~
3. A standard of rotation across time.
4. A notion of straightness of paths across time.
5. ~~A preferred velocity.~~
6. ~~A preferred point.~~
7. A notion of *spacetime* distance.

Note that what's well-defined and what's not in Minkowski spacetime cuts across the Newtonian hierarchy: in all of our Newtonian spacetimes, there was a well-defined notion of spatial distance; by contrast, this is *not* an invariant

[4] The resulting spacetime has a claim to be the correct spacetime structure for the programme of 'shape dynamics', but I won't go into this further here (see [134] for more). For more on shape dynamics, see [106].

of the Poincaré transformations. By contrast, there *is* a preferred notion of straightness of paths across time (translating into a notion of absolute acceleration), *unlike* (e.g.) Maxwellian, Leibnizian, or Machian spacetime. On the other hand, one very important invariant of the Poincaré transformations is the *interval*—a notion of four-dimensional spacetime distance, which can be written

$$I = -c^2dt^2 + dx^2 + dy^2 + dz^2. \tag{5.16}$$

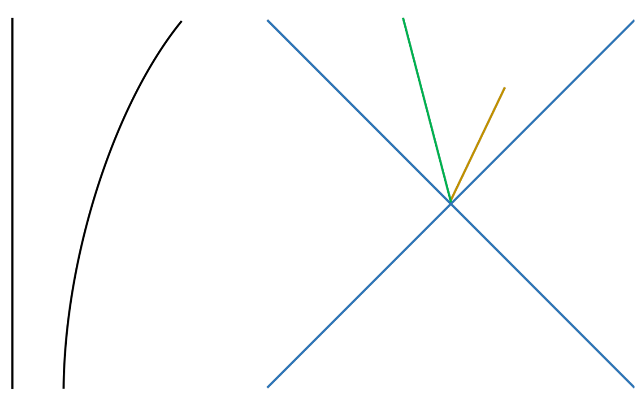
The interval I is preserved in all inertial frames in special relativity—i.e., in all frames related by Poincaré transformations. It can be used to distinguish between three different kinds of trajectory through spacetime:

1. *Timelike* paths (representing the trajectories of massive bodies), which are such that the tangent vector to the path at every point is such that $I < 0$.

2. *Spacelike* paths (representing the trajectories of superluminal bodies), which are such that the tangent vector to the path at every point is such that $I > 0$.

3. *Null* paths (representing the trajectories of massless bodies, such as light rays), which are such that the tangent vector to the path at every point is such that $I = 0$.

Together, these three kinds of trajectory pick out the famous 'lightcone' structure of special relativity. Schematically, then, a picture of Minkowski spacetime might look like this:



What I mean by this image is the following. The two lines on the left indicate that one can still distinguish straight (i.e. non-accelerating) from curved (i.e. accelerating) paths through this spacetime (one also has a standard of rotation in Minkowski spacetime, but I haven't represented that in the diagram). On the right, the blue lines represent the lightcone structure of the theory (the two colored lines represent two distinct timelike vectors).

5.4 Further reflections on spacetime

Up to this point, I've introduced both the standard hierarchy of Newtonian spacetimes, as well as Minkowski spacetime, via the Kleinian approach to (spacetime) geometry. I'll close this chapter with some further philosophical points regarding the nature of spacetime. The first regards the connection between spacetime and dynamical laws.

In Chapter 2, we saw that the laws of Newtonian mechanics are invariant under Galilean transformations. But these are the transformations associated with Galilean spacetime, as we have seen above. It is natural, therefore, to regard Newtonian mechanics as being *set in Galilean spacetime*. Earman [35, ch. 3] makes it a very general principle that the spacetime and dynamical symmetries of a theory should match, by laying down the following two conditions:

SP1: Any dynamical symmetry of T is a spacetime symmetry of T.

SP2: Any spacetime symmetry of T is a dynamical symmetry of T.

(Some have gone further, by saying that these principles are *analytically true*—see e.g. [111]. I'll return to this suggestion in Chapter 10.) The idea here is this: if there are dynamical symmetries which are not spacetime symmetries, then (by our above mantra that 'more symmetries' is equivalent to 'less structure'), there is spatiotemporal structure which is not relevant to the dynamics. In that case, by an Occamist norm, such structure should be expunged (for more on such Occamist reasoning in contemporary physics, see [27]). On the other hand, if there are spacetime symmetries which are not dynamical symmetries, then it seems that one's dynamics adverts to structures which don't exist. It's questionable whether this is even coherent: Belot calls it 'arrant knavery' [9].[5]

In this sense, one might accuse Newton of having made a mistake, in postulating that Newtonian rather than Galilean spacetime is the correct spacetime setting for his theory. The thought here is that we have neither *a priori* nor *direct* empirical access to the structure of spacetime we live in; rather, our guide to which structure obtains is in the dynamical laws: we should postulate as much structure as is required to state (the invariance properties of) the laws of our best physical theories, *and no more*. (To repeat, this is essentially the content of Earman's conditions.) With hindsight, Newton violated this requirement: Newtonian physics can be formulated in (merely) *Galilean* spacetime, not *Newtonian* spacetime (as Newton maintained). Occam's razor thus advises against postulating a standard of absolute rest in addition.[6]

[5]For what it's worth, I disagree with Belot's claims that such approaches are incoherent. For example, Huggett's regularity relationalism [76]—to which I already alluded in Chapter 1—begins with an impoverished spacetime ontology, yet gives a precise prescription for how further spatiotemporal commitments may be secured via dynamical considerations. For more on what Pooley calls 'have-it-all relationalism' (which includes Huggett's approach), see [127].

[6]While this might be true in principle, I agree with Dasgupta [27] that in practice, since Newton didn't have the concept of Galilean spacetime, he was justified in believing in Newtonian absolute space, and thereby in violating Earman's principles.

Raising this point presents the following question: is it indeed the case that *Galilean* spacetime is the correct spacetime setting for Newtonian mechanics (given Earman's conditions), as is by now the standard line? If we follow the methodology of moving from Newtonian to Galilean spacetime as the correct spacetime setting for Newtonian mechanics, then (it seems) the discovery of *further* symmetries of the Newtonian laws would likewise motivate moving to a different spacetime setting again, with even less structure than Galilean spacetime. With this in mind, considers Newton's 'Corollary VI' in the *Principia*:

> If bodies moved in any manner among themselves are urged, in the direction of parallel lines by equal accelerative forces, they will all continue to move among themselves, after the same manner as if they had not been urged by those forces. [19, p. 21]

This points out that there is no standard of *linear* acceleration in Newtonian mechanics—so perhaps the correct spacetime setting for the theory should be *Maxwellian* spacetime? This suggestion was first raised in [148], and continues to be a matter of some controversy—see *inter alia* [86] and [166] for further discussion.

The second philosophical point which I wish to make is this. If we impose extra structure on Galilean spacetime (namely, a standard of rest), we can recover Newtonian spacetime. Perhaps more surprisingly, however, if we impose extra structure on Minkowski spacetime (namely, again, a standard of rest), we can *also* recover Newtonian spacetime. So, as Barrett summarises:

> There is a precise sense in which Newtonian spacetime has more structure than both Galilean spacetime and Minkowski spacetime. But in this same sense, Galilean and Minkowski spacetime have incomparable amounts of structure; neither spacetime has less structure than the other. The progression towards a less structured spacetime therefore does not continue into the relativistic setting. [6, p. 37]

Chapter 6

General covariance

If only I knew more mathematics!
(Schrödinger, 1925)

In this chapter, I'm going to explain how the second of our two approaches to geometry—the Riemannian approach—works. Ultimately, I'll return both to the Newtonian heirarchy and to Minkowski spacetime. Before doing so, however, I need to say a little more on the different ways in which one might present a given set of physical laws.

6.1 Physical laws

In the previous chapter, I introduced briefly the four-dimensional index notation. Let us now consider how to write some familiar physical laws using this index notation. I'll begin with the Klein-Gordon equation, which is a four-dimensional wave equation for a scalar field ϕ:

$$-\frac{1}{c^2}\frac{\partial^2\phi}{\partial t^2}+\frac{\partial^2\phi}{\partial x^2}+\frac{\partial^2\phi}{\partial y^2}+\frac{\partial^2\phi}{\partial z^2}=0. \tag{6.1}$$

Completely equivalently, I can write this equation using a matrix, as follows:

$$\begin{pmatrix} \frac{1}{c}\frac{\partial}{\partial t} & \frac{\partial}{\partial x} & \frac{\partial}{\partial y} & \frac{\partial}{\partial z} \end{pmatrix} \begin{pmatrix} -1 & & & \\ & 1 & & \\ & & 1 & \\ & & & 1 \end{pmatrix} \begin{pmatrix} \frac{1}{c}\frac{\partial}{\partial t} \\ \frac{\partial}{\partial x} \\ \frac{\partial}{\partial y} \\ \frac{\partial}{\partial z} \end{pmatrix} \phi = 0. \tag{6.2}$$

Calling the vector of partial derivatives in the above ∂^μ ($\mu = 0\ldots 3$) and the above matrix $\eta_{\mu\nu}$, I can again write this (completely equivalently!) using the Einstein summation convention (where, recall again, repeated indices are summed) as follows:

$$\eta_{\mu\nu}\partial^\mu\partial^\nu\phi = 0. \tag{6.3}$$

It's important to stress that the content of (6.3) is *exactly the same* as that of (6.1): it still describes the same behaviour of the field ϕ, in the same coordinate system. Yet there are conceptual merits to the latter syntactic formulation: not only does it save ink, but (as we'll see shortly), it also helps us to ascertain the symmetries of this equation (a point to which I alluded in Chapter 2).

Before I get onto this, I'll introduce a couple more examples. Consider the Newton-Poisson equation, which describes the gravitational potential ϕ in the field formulation of Newtonian gravity:

$$\frac{\partial^2\phi}{\partial x^2} + \frac{\partial^2\phi}{\partial y^2} + \frac{\partial^2\phi}{\partial z^2} = 4\pi\rho. \tag{6.4}$$

As before, I can rewrite this equation using a matrix as follows:

$$\begin{pmatrix} \frac{1}{c}\frac{\partial}{\partial t} & \frac{\partial}{\partial x} & \frac{\partial}{\partial y} & \frac{\partial}{\partial z} \end{pmatrix} \begin{pmatrix} 0 & & & \\ & 1 & & \\ & & 1 & \\ & & & 1 \end{pmatrix} \begin{pmatrix} \frac{1}{c}\frac{\partial}{\partial t} \\ \frac{\partial}{\partial x} \\ \frac{\partial}{\partial y} \\ \frac{\partial}{\partial z} \end{pmatrix} \phi = 4\pi\rho. \tag{6.5}$$

Defining ∂^μ exactly as before, and now calling the above matrix $h^{\mu\nu}$, I can write this equation as follows, where the Einstein summation convention is used:

$$h^{\mu\nu}\partial_\mu\partial_\nu\phi = 4\pi\rho. \tag{6.6}$$

Again, it bears stressing that the content of (6.6) is *exactly the same* as the content of (6.6). Moreover, the advantages of this formulation are the same as in the previous case: (i) it's more compact, and (ii) it's easier to use this formulation to ascertain the symmetries of the equation than the first.

The third example is particularly relevant to special relativity: Maxwell's equations. Recall again that, in the usual 3-vector presentation, these equations read:

$$\nabla \cdot \mathbf{E} = \rho, \tag{6.7}$$
$$\nabla \cdot \mathbf{B} = 0, \tag{6.8}$$
$$\nabla \times \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t}, \tag{6.9}$$
$$\nabla \times \mathbf{B} = \mathbf{J} + \frac{\partial \mathbf{E}}{\partial t}. \tag{6.10}$$

If I define the following two objects:

$$F^{\mu\nu} = \begin{pmatrix} 0 & -E_1/c & -E_2/c & -E_3/c \\ E_1/c & 0 & -B_3 & B_2 \\ E_2/c & B_3 & 0 & -B_1 \\ E_3/c & -B_2 & B_1 & 0 \end{pmatrix}, \tag{6.11}$$

$$J^\mu = \begin{pmatrix} \rho \\ J^i \end{pmatrix}, \tag{6.12}$$

then Maxwell's equations can be written:

$$\eta_{\mu\lambda}\partial^\lambda F^{\mu\nu} = J^\nu, \tag{6.13}$$

$$\partial_\mu F_{\nu\lambda} + \partial_\nu F_{\lambda\mu} + \partial_\lambda F_{\mu\nu} =: \partial_{[\mu} F_{\nu\lambda]} = 0. \tag{6.14}$$

As before, at this stage (6.13) and (6.14) are simply a compact and convenient reformulation of our initial version of the equations, (6.7)-(6.10).

> **Exercise:** Plug components into (6.13) and (6.14) in order to recover Maxwell's equations in their 3-vector forms, (6.7)-(6.10).

Having put all these examples on the table, let's think about the second professed advantage: that the latter (more compact) formulations make it easier to ascertain the symmetries of these equations. The Klein-Gordon theory and Maxwell theory both feature explicit coupling to $\eta_{\mu\nu}$. The simplest form of these equations will be preserved under coordinate transformations which preserve the diagonal form of $\eta_{\mu\nu}$, i.e. coordinate transformations such that $\Lambda^\sigma{}_\mu \Lambda^\lambda{}_\nu \eta_{\sigma\lambda} = \eta_{\mu\nu}$. But these are just the Lorentz transformations!

> **Exercise:** Verify that the condition $\Lambda^\sigma{}_\mu \Lambda^\lambda{}_\nu \eta_{\sigma\lambda} = \eta_{\mu\nu}$ picks out Lorentz boosts and/or spatial rotations.

Indeed, the equations are also invariant under translations, making them invariant under the full Poincaré group. One sometimes hears the claim that writing a theory using four-dimensional indices makes the symmetries of one's equations 'manifest'—this can be misleading, but the point is that it's easier to read off the symmetries of equations when they're formulated in this way.

> **Exercise:** Show explicitly that the Klein-Gordon equation (6.3) and Maxwell equations (6.13)- (6.14) are invariant under Poincaré transformations.

We can use exactly the same methodology to demonstrate the Galilean invariance of the Newton-Poisson equation (6.6). This equation features explicit coupling to $h^{\mu\nu}$. The simplest form of this equation will be preserved under coordinate transformations which preserve the diagonal form of $h^{\mu\nu}$, i.e. coordinate transformations such that $M^\mu{}_\sigma M^\nu{}_\lambda h^{\sigma\lambda} = h^{\mu\nu}$. Assuming that the transformations are linear (i.e., assuming that the change-of-basis matrices $M^\mu{}_\sigma$ are not functions of spacetime coordinates), these are just the Galilean transformations (up to a constant rescaling of t[1]), once we also include translations.[2]

[1] If one considers the symmetries of the Newton-Poisson equation only, one in fact finds that the allowed transformations of the temporal coordinate are $t \mapsto \kappa t$ for some constant κ; one can only set $\kappa = \pm 1$ if one assumes that the symmetries in addition preserve a standard of temporal distance, which strictly speaking is not part of the content of the Newton-Poisson equation.

[2] If one liberalises the linearity condition, one finds that (6.6) is in fact invariant under the

> **Exercise:** Show explicitly that the Newton-Poisson equation (6.6) is invariant under Galilean transformations.

The main point which I want to stress here is that so far we have just *repackaged* these dynamical equations—we have not fundamentally changed their symmetry properties. In fact, the index notation makes it pretty easy to transform to an arbitrary (rather than inertial) coordinate system, and see these equations in their general (and ugly!) form: recall, indeed, that I already did this explicitly in the case of **N1L** in Chapter 1.

> **Exercise:** Transform (1.1) to arbitrary coordinates, and thereby reproduce (1.3) from Chapter 1.

One can also show this in the case of e.g. the Klein-Gordon equation: explicitly, the transformation proceeds as follows:

$$\begin{aligned}
\eta_{\mu\nu}\partial^{\mu}\partial^{\nu}\varphi &= 0 \\
\eta_{\mu\nu}\frac{\partial}{\partial x_{\mu}}\frac{\partial}{\partial x_{\nu}}\varphi &= 0 \\
\longrightarrow \eta_{\mu\nu}\frac{\partial x_{\mu'}}{\partial x_{\mu}}\frac{\partial}{\partial x_{\mu'}}\left(\frac{\partial x_{\nu'}}{\partial x_{\nu}}\frac{\partial}{\partial x_{\nu'}}\varphi\right) &= 0 \\
\eta_{\mu\nu}\frac{\partial x_{\mu'}}{\partial x_{\mu}}\left(\frac{\partial^2 x_{\nu'}}{\partial x_{\mu'}\partial x_{\nu}}\frac{\partial}{\partial x_{\nu'}}\varphi + \frac{\partial x_{\nu'}}{\partial x_{\nu}}\frac{\partial}{\partial x_{\mu'}}\frac{\partial}{\partial x_{\nu'}}\varphi\right) &= 0 \\
\eta_{\mu\nu}\frac{\partial^2 x_{\nu'}}{\partial x_{\mu}\partial x_{\nu}}\partial^{\nu'}\varphi + \eta_{\mu\nu}\frac{\partial x_{\mu'}}{\partial x_{\mu}}\frac{\partial x_{\nu'}}{\partial x_{\nu}}\partial^{\mu'}\partial^{\nu'}\varphi &= 0.
\end{aligned}$$

Note the extra term in the non-inertial frame (cf. fictitious forces in (1.3)).

6.2 General covariance

At this point, I want to ask: can we write theories in what's known as a *generally covariant* form—i.e., a form which holds in an arbitrary frame? (Note that the terminology 'general *co*variance' is confusing here—it should really be 'general *in*variance', but to mesh with the literature I'll use the standard term.) Einstein circa 1915 thought that the answer to this question was *no*, and that this is what made his newly-developed general relativity special. But Kretschmann said in 1917 to Einstein: *yes*. Indeed, speaking anachronistically now, there are (at least) two different ways to render a theory generally covariant:[3]

Leibniz group of transformations. This isn't so surprising once one notes that (6.6) is a static, three-dimensional equation, so changes in the temporal direction should leave it unchanged. When one also considers the force equation of Newtonian gravity, the symmetry group of the theory is reduced.

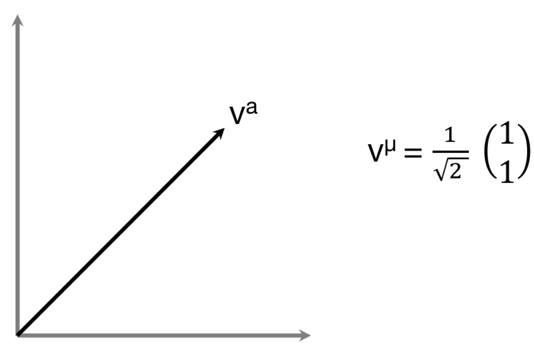
[3]For historical background, see [113].

1. Write its equations in an arbitrary frame, with all extra terms included.

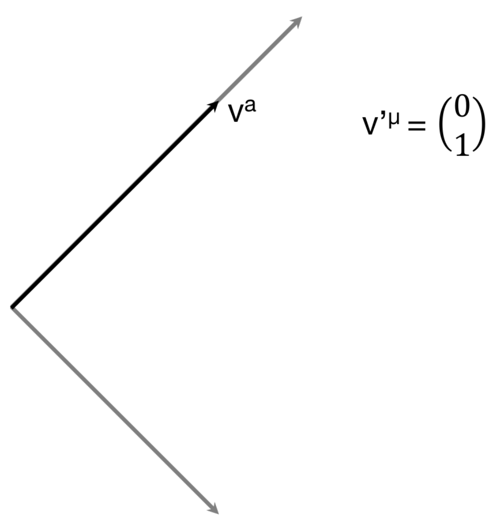
2. Write the theory in a *coordinate-independent* language.

We've seen option (1) both with moving from (1.1) to (1.3) and with our above transformation of the Klein-Gordon equation to an arbitrary frame. Let's now think a bit more about option (2). To do this, we need to be clear about the distinction between (i) geometric objects, versus (ii) the components of those objects in a given coordinate system.

To illustrate this difference, consider a vector which I'll call v^a: the components of this vector in one given (Cartesian) coordinate system could be as follows:



If I now rotate the coordinate system (i.e., do a passive transformation), the the vector will remain unchanged, but its components will differ, perhaps as follows:



One might, in light of this, seek to write down *different* dynamical equations for a physical theory, which are liberated altogether from coordinate systems, and which treat with geometric objects themselves, rather than the representations of those objects in some coordinate system.[4] To write a theory in a coordinate-independent way, we move from using *coordinate indices* $(\mu, \nu, \ldots)$, which represent the *components* of objects in a particular coordinate basis, to *abstract indices* $(a, b, \ldots)$, which directly represent the objects themselves. E.g. in the case of the Klein-Gordon equation, move from (6.3) to

$$\eta_{ab} \nabla^a \nabla^b \phi = 0. \tag{6.15}$$

This involves no reference to a coordinate system at all—so *a fortiori* holds in all coordinate systems. Note, in particular, that in order to make this move, we've introduced two *new* objects: (what's known as) a rank-2 tensor field η_{ab}, and a derivative operator ∇. Suffice it to say that both of these objects can be defined in a coordinate-independent manner. (See [60, 100] for details.)

Such a move is not always metaphysically innocent. Sometimes, one finds the claim that writing our theories in a coordinate-independent language makes manifest the full ontological commitments of those theories. For example, in the case of Klein-Gordon theory, the claim would be that coordinate-independent presentations make manifest the commitment of the theory not merely to the field ϕ, but also to another field, η_{ab}—Minkowski spacetime (along with its compatible derivative operator ∇—'compatible' means that $\nabla_a \eta_{bc} = 0$). But should this be regarded as representing an autonomous entity (i.e., object in our ontology), or just as being a *codification* of the symmetries of the coordinate-based dynamical equations from which we began? I'll return to this issue in detail in Chapter 10.[5]

6.3 The Riemannian conception of geometry

Rather than identifying geometrical structure as the invariants of a given set of transformations (as per the Kleinian approach), the Riemannian approach directly presents and defines such structures, without any reference to coordinate systems (the technical details of how this works are often sophisticated, but see e.g. [60, 100] for explicit presentations of how all of the objects which I will discuss in the remainder of this chapter can be defined on the Riemannian approach).[6] The Kleinian and Riemannian approaches are complimentary, insofar the transformations specified in the Kleinian approach are those transformations which would leave invariant the structures presented on the Riemannian approach, were they to be written in a coordinate basis.

For the time being, however, I'll simply present the Riemannian approach, first to the hierarchy of Newtonian spacetime structures which we saw in Chap-

[4]For some reflections on whether this is always possible, see [122, 137].

[5]Recall also some of the discussion from Chapter 1.

[6]Whether the Riemannian approach *really* makes no appeal to coordinate systems is questionable—see [165]—but I'll set this aside here.

ter 5, and then to special relativity. To begin, recall again that the following structures are well-defined in Aristotelian spacetime:

1. A notion of spatial distance.
2. A notion of temporal distance.
3. A standard of rotation across time.
4. A notion of straightness of paths across time.
5. A preferred velocity.
6. A preferred point.

In order to underwrite the meaningfulness of these notions, on the Riemannian approach we specify Aristotelian spacetime as a tuple of geometric objects, $\langle M, t_{ab}, h^{ab}, \nabla, \sigma^a, \xi\rangle$. Here, M is a four-dimensional differentiable manifold representing the points of spacetime; t_{ab} is a temporal metric field of signature $(1,0,0,0)$ which represents temporal distance relations between spacetime points; h^{ab} is a spatial metric field of signature $(1,1,1,0)$ representing spatial distance relations between spacetime points; ∇ is a derivative operator affording standards both of straightness of paths and of rotations; σ^a is a timelike (in the sense that $t_{ab}\sigma^b = 0$) vector field representing trans-temporal identities of spacetime points, and affording a standard of rest; and ξ is a scalar field identifying the preferred point in this spacetime. (As I say, I won't go further into the technical details here, but interested readers should consult e.g. [35, 60, 100, 128].)

As one weakens the spacetime structure in the Newtonian hierarchy, fewer and fewer geometrical notions become meaningful, as we have already seen. This is captured easily in the Riemannian approach: one simply defines fewer and fewer geometrical objects in one's spacetime models. Using the same objects as before, the entire Newtonian hierarchy, indeed, can be captured as follows:

Aristotelian spacetime: $\langle M, t_{ab}, h^{ab}, \nabla, \sigma^a, \xi\rangle$
Newtonian spacetime: $\langle M, t_{ab}, h^{ab}, \nabla, \sigma^a\rangle$
Galilean spacetime: $\langle M, t_{ab}, h^{ab}, \nabla\rangle$
Maxwellian spacetime: $\langle M, t_{ab}, h^{ab}, [\nabla]\rangle$
Leibnizian spacetime: $\langle M, t_{ab}, h^{ab}\rangle$
Machian spacetime: $\langle M, h^{ab}\rangle$

There are a couple of further points to make at this stage. First: as already mentioned, it is now Galilean spacetime which (for better or worse) is regarded as being the 'correct' spacetime setting for Newtonian mechanics. It is for this reason that authors such as Malament [100] simply present Newtonian gravity in this setting, without identifying Galilean spacetime by name. Second: one might wonder what the square brackets in the above presentation of Maxwellian spacetime are doing. Typically when one sees such notation in mathematics, what is meant is an *equivalence class* of the relevant object (within the brackets). In this case, $[\nabla]$ denotes the equivalence class of derivative operators ∇ which differ on their standards of linear acceleration (i.e., differ on the adjudications of which one-dimensional paths through spacetime are bent—i.e., accelerating), but which agree on their standard of rotation (i.e., agree in there adjudications of whether bodies are or are not rotating). Thus, by taking this equivalence class, we secure exactly the structure which we already defined in the previous chapter to be implicated in Maxwellian spacetime—and no more.

As another example in which the same notation appears, typically conformal structure—which encodes facts about angles but not facts about distances—is written in the Riemannian approach using square brackets. For example, one might yet further weaken Machian spacetime to encode only conformal structure on the spacelike hypersurfaces: in this case, one could write the models of the theory as $\langle M, [h^{ab}] \rangle$. Now, in all such cases, one might complain that it would be better (in the sense of: more physically perspicuous) to define geometric objects such that exactly as much structure as required is introduced from the outset, rather than by (a) introducing something with too much structure, then (b) telling us to forget about some of it. I agree![7] Indeed, Weatherall [167] has shown recently that it is possible to write Machian spacetime using a 'standard of rotation' $\circlearrowleft$ which meets the above desiderata. Thus, in fact, it would arguably be better—and more physically/metaphysically perspicuous—to write the models of Maxwellian spacetime as $\langle M, t_{ab}, h^{ab}, \circlearrowleft \rangle$.[8]

6.4 What is special relativity?

By now, we understand (i) the genesis of special relatvity, (ii) the content of Einstein's 1905 paper, and (iii) the different senses in which one might understand the spatiotemporal commitments of physical theories, including special relativity. But, having achieved all this, the following question arises naturally: just *what is* special relativity? In fact, there are at least three different options on the table:

1. Special relativity consists of the **RP**, the **LP**, whatever supplementary principles are needed to derive the Lorentz transformations therefrom, and the said derivation of the Lorentz transformations.

2. Special relativity is the statement that the laws of physics (in standard formulation) are Poincaré invariant.

3. Special relativity is the statement that spacetime structure (over and above topological and differentiable structure) is exhausted by Minkowski spacetime.

[7] Here, there are connections to a recent philosophical debate about 'sophistication': see [29, 101].

[8] In the case of conformal structure, one can use a tensor density—see e.g. [92].

In the coming chapters of this book, we'll see how different views on the nature of special relativity can have substantial impacts upon one's preferred resolution to certain philosophical puzzles which arise in that theory (however construed).

> **Question:** Which of the above do you think best captures the 'essence' of special relativity? Or is this a wrong-headed question, and if so why?

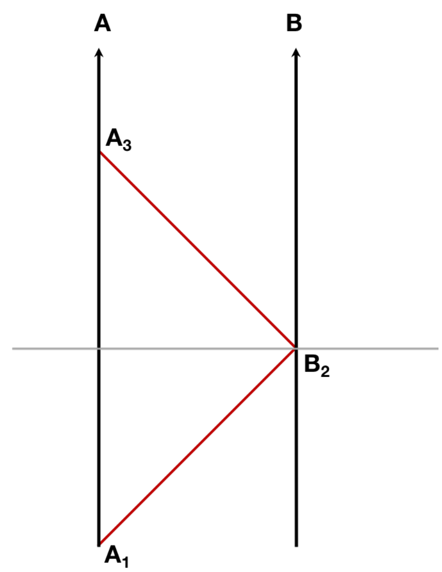
Chapter 7

The conventionality of simultaneity

> First, it is impossible to ascertain whether two distant clocks are set "correctly" in their indication of time; second, they can be set arbitrarily and yet no contradiction will arise.
> (Reichenbach, 1956)

Having in the first half of this book presented the genesis of special relativity, as well as several different ways in which one might understand the content of the theory, in the coming chapters I'll introduce, and chart the space of possible responses to, several important special relativistic paradoxes and conceptual conundrums. I'll begin with one of the most long-standing and vexed: the question of whether simultaneity is *conventional* in special relativity. We have already seen in Chapter 4 some hints as to what this might mean; before explaining this in full detail, however, I must remind the reader of a better-known special relativistic phenomenon: the *relativity* of simultaneity.

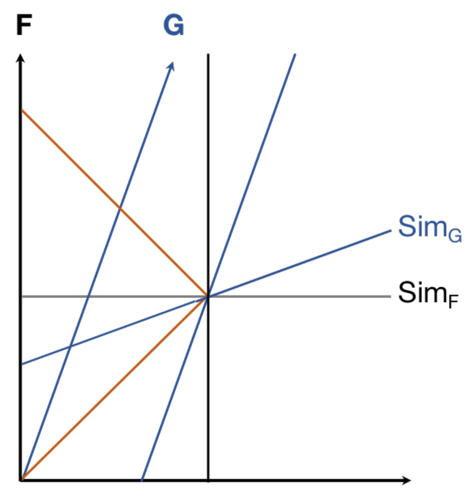
7.1 The relativity of simultaneity



Recall again the setup introduced in Einstein's discussion of distant simultaneity in his 1905 article. Suppose one bounces a light ray from mirror A to mirror B, then back again to mirror A, as per the above diagram. Which point on the worldline of mirror A is simultaneous (according to a clock at A) with point B_2 on the worldline of mirror B (according to a clock at B)? As we have already seen in Chapter 4, a natural answer to this question was stipulated by Einstein (following the earlier writings of Poincaré) to be the following:

$$t_B\left(B_2\right) = t_A\left(A_1\right) + \frac{1}{2}\left(t_A\left(A_3\right) - t_A\left(A_1\right)\right). \tag{7.1}$$

This is the Einstein-Poincaré clock synchrony convention. If we apply this in all frames, then the *relativity of simultaneity*—which means that adjudications on simultaneity will vary from inertial frame to inertial frame, so that simultaneity is not an invariant of the relevant transformations—follows, as can be seen in the following diagram:



Here, we consider a new coordinate system G in which our above-described Langevin clock setup (consisting of the two mirrors A and B and a bouncing light ray) is moving uniformly; by applying the Einstein-Poincaré synchrony convention in this frame, one finds *tilted* simultaneity hyperplanes. So, if we understand simultaneity *à la* Einstein, then the frame-relativity of simultaneity follows. But could it be that, *even in one particular frame*, there is no fact about which point on the worldline of mirror A is simultaneous with point B_2 on the worldline of mirror B? One who thinks this would have to say that that there are no facts about simultaneity *even in one frame*—and thus that these can be fixed by convention only. This is the *conventionality of simultaneity*, which is conceptually distinct from the relativity of simultaneity.

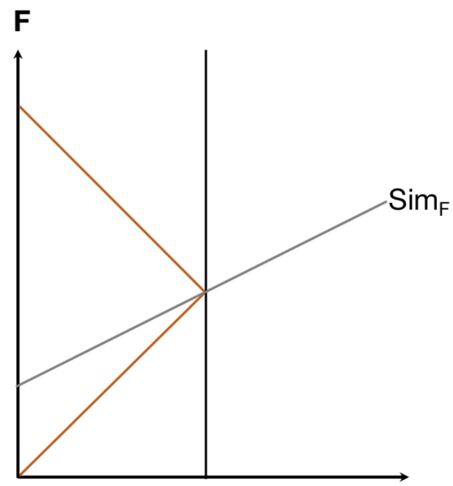
7.2 The conventionality of simultaneity

One of the first authors to explore systematically the possibility of other simultaneity convenions was Hans Reichenbach, in his *The Philosophy of Space and Time* [141]. Reichenbach maintained that we are free to make stipulations about which point on the worldline of mirror A is simultaneous with event B_2 on the worldline of mirror B different from those of the Einstein-Poincaré convention. To reflect this, he generalised Einstein's simultaneity relation by replacing the factor of $1/2$ in (7.1) with an ϵ-factor, such that $\epsilon \in [0,1]$:

$$t_B(B_2) = t_A(A_1) + \epsilon\left(t_A(A_3) - t_A(A_1)\right), \qquad 0 < \epsilon < 1. \qquad (7.2)$$

Reichenbach's underlying thought was this: nothing in the formal structure of special relativity fixes which synchrony convention we must use; it is, rather, *an additional input choice*. This, indeed, squares with the way in which we have already seen that Einstein understood the matter of distant clock synchrony in special relativity.

How would the description of physical goings-on change one were to deploy a non-standard (i.e., $\epsilon \neq 1/2$) simultaneity convention in the rest frame of the above-described Langevin clock? The answer is illustrated below: simultaneity hyperplanes in this convention will be *tilted*. Moreover, if we choose e.g. $\epsilon = 1/4$, *simultaneity is still frame-relative*—i.e., simultaneity hypersurfaces will still shift on transforming to frames comoving with the original frame (assuming the same convention is used in the moving frame). Finally, any $\epsilon \neq 1/2$ will mean that the one-way speed of light is *not* isotropic.[1]



Why did Reichenbach bound ϵ by 0 and 1? Here's Brown on this question:

> I will have more to say about this Reichenbach factor ϵ shortly, but note that it is widely assumed that ϵ must be restricted to the closed set $[0, 1]$ ... This is to ensure that in one direction light does not propagate backwards in time. It is often claimed that such a possibility would violate the fundamental canons of causality, but it is a hum-drum experience for airline travellers flying East across the International Date Line.
>
> ... I can testify, having flown from New Zealand to both North and South America, that arriving before you left is survivable! ... Come to think of it, every telephone call from, say Australasia to the UK, involves a signal arriving before it left, and no one seems the worse for it. [14, p. 97]

What Brown is stressing here is that we are free to coordinatise space and time in

[1]The conventionality of simultaneity as discussed here is closely related to the fact that it is not possible to measure the one-way speed of light: see [145] for further discussion.

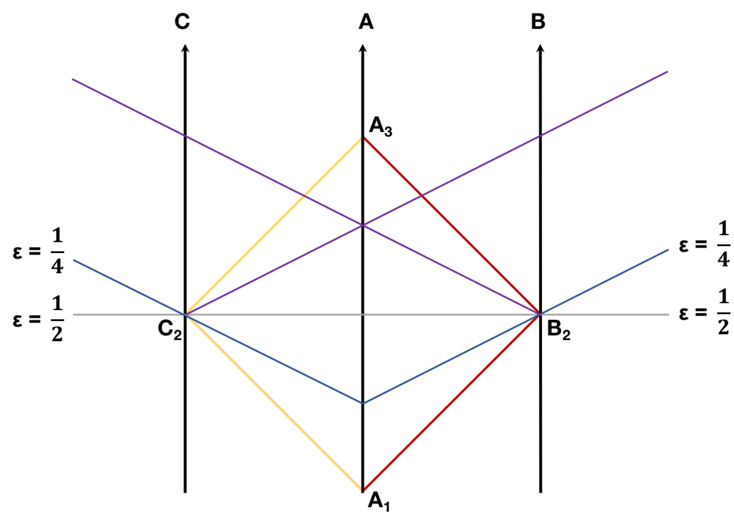
any way that we please; even if a particular coordinatisation yields descriptions of physical events according to which there is (say) communication backwards in time, this will not lead to logical catastrophe. Therefore, although choosing $\epsilon \notin [0, 1]$ might yield just such descriptions, this is not *per se* problematic. This point is surely correct—and yet, one might feel that Brown has missed something. In my opinion, Huggett hits the nail on the head when he writes:

> Now of course we are logically free to coordinatize as we please, and so we can assign, in principle, the same 'time' coordinate to any pair of points we wish. Indeed, in the sense that coordinates are just labels for points, we could attach absolutely any numbers to any points we liked. At certain points (e.g., *PR*, 20, 97) Brown seems to mean nothing more by 'convention', but surely this sense has little philosophical import.
>
> A more weighty issue that motivates conventionalism is that of the status of spacetime geometry. The realist-minded about geometry will evaluate different choices of coordinates according to how well they express the geometric properties of the spacetime manifold. Of course, even if the manifold were a substance, with intrinsic geometric structure, then we could still assign coordinates as we chose without affronting logic; but if there are intrinsic facts of the matter about the geometry of spacetime then some coordinates are 'better' than others. [77, p. 410-411]

Here's how I would put the point. It's of course uncontroversial that we can coordinatise space and time in any way we please, and that descriptions of physical events may be counter-intuitive or unnatural in some such coordinatisations. However, theories come endowed with laws with certain symmetries, and the question is: to what extent do such symmetries fix (i.e., leave invariant, in the sense of Chapter 5) certain notions—most relevantly for us in this chapter, simultaneity? Note that, in fact, it does not matter whether one has a 'dynamics-first' view such as that of Brown (according to which spacetime structure is a codification of dynamical symmetries—see Chapter 10 for further discussion) or a 'geometry-first' view such as that of Friedman (according to which spacetime structure in some sense constrains dynamical symmetries—again, see Chapter 10) in order to make this point: in both cases, the issue is: given those symmetries, which notions are or are not well-defined? (Cf. [76, p. 411].)[2]

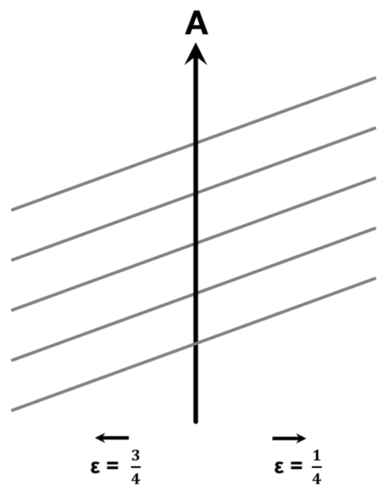
Moving on from these issues, let's explore the ramifications of choosing non-standard (i.e., $\epsilon \neq 1/2$) simultaneity conventions. In fact, once one recognises the possibility that $\epsilon \neq 1/2$ (however one takes the quantity to be bounded), an array of *different* possible means of 'spreading time through space' arise. I'll focus on two, which I'll call the 'Reichenbach-I' and 'Reichenbach-II' synchrony conventions, respectively.

[2] Brown [14, p. 20] also claims that simultaneity is conventional in Newtonian mechanics—however, the same criticisms as above would apply to that claim.



Let's begin with the former. Suppose we send a light ray out in both directions, with an $\epsilon = 1/4$ convention. Simultaneity surfaces won't be flat, and there will be a preferred position in the reference frame. This is represented in the above figure by the blue line, bent at A. About A, the description of the one-way speed of light is isotropic, but highly non-homogeneous due to the preferred point. (On one natural understanding, C_2 and B_2 are simultaneous from the point of view of A but not from the point of view of C, so what counts as simultaneous is not just frame-dependent, but position-dependent. A second natural understanding would have it that C_2 and B_2 are simultaneous *tout court*—but then there is something metaphysically privileged about A, which might seem mysterious.)

One objection to the Reichenbach-I synchrony convention is due to Torretti [159, ch. 7]. Call a timescale (i.e. an assignment of temporal coordinates to spacetime points) *inertial* just in case, relative to that timescale, free bodies have (or would have) constant velocities. Then, an assignment of temporal coordinates as per the Reichenbach-I convention does not define an inertial timescale. To see this, consider a free body which crosses A's worldline. As the particle moves from one side of this worldline to the other, it (according to this way of spreading time through space) accelerates instantaneously—in spite of the fact that no force is acting on it. Given this, we can say that, when one adopts non-standard synchrony on the Reichbach-I convention, the resulting frames of reference are not inertial frames (recalling Knox's functional definition of inertial frames given in Chapter 1), for they implicate free bodies in arbitrary accelerative motions.



Turn now to the Reichenbach-II synchrony convention. In this case, we set coordinated values of ϵ on either side of (in our example) A's worldline, such that no 'bend' in the simultaneity hypersurfaces arises. Suppose, for example, that we set $\epsilon = 1/4$ on one side, then we set $\epsilon' := 1 - \epsilon = 1 - 1/4 = 3/4$ on the other side. This will yield flat simultaneity surfaces. (See the above figure.) Around A, space will be anisotropic but homogeneous: light travels faster in the rightwards direction. Note that Torretti's objection does not apply in this case.

We've already seen that the description of the selfsame physical events can change, depending upon one's choice of simultaneity convention. Indeed, the derivation of the Lorentz transformations assumes standard ($\epsilon = 1/2$) synchrony; adopting non-standard synchrony would require changing, *inter alia*: [2, 172]

- The form of the Lorentz transformations.
- Length contraction and distances in a frame (typically a rod will contract differently when moving in different directions).
- Time dilation.
- How fast something moves relative to a reference frame.

Of course, though, empirically-accessible quantities will have to stay the same (otherwise our synchrony convention would make an observable difference, and so no longer be a convention!). For example, the time read by two clocks when reunited after a 'twin paradox' journey will have to be the same, given any synchrony convention (see Chapter 9).

7.3 Arguments against conventionality

Since the possibility of the conventionality of simultaneity in special relativity was first raised, a number of different arguments have been presented to the effect that, in fact (and in spite of the above discussions), simultaneity is *not* conventional in this theory. These arguments intimate that if one attends sufficiently carefully to the conceptual architecture of the theory, one will find that only one simultaneity convention is permitted (typically, this is argued to be 'standard' $\epsilon = 1/2$ synchrony). Here, I'll focus on two of the best-known such arguments:

1. Arguments from slow clock transportation.

2. Malament's 1977 (purported) proof of non-conventionality.

Let's begin with the former.

7.3.1 Slow clock transport

The thought underlying the idea of synchrony by slow clock transport is this. Take two clocks, A and B, which are initially spatiotemporally coincident and synchronised. Now transport B infinitesimally slowly away from A. In such a scenario, the internal workings of the clock should not change,[3] so the clocks (the thought goes) should continue to tick in step after B has been transported away from A. In turn, this recovers standard synchrony.

The idea of using slow clock transport to establish a privileged simultaneity convention goes back (at least) to Eddington in the 1920s [38] (although *nota bene*: Eddington did not actually endorse this proposal—see below). There are, however, a number of concerns with the approach, which have since been articulated. One is that the whole idea is question-begging (so the thought goes), because until the clocks are synchronized, there is no way of measuring the one-way velocity of the transported clock. In order to tackle this concern, Bridgman [12, p. 26] used the 'self-measured' velocity, determined by using the transported clock to measure the time interval. However—in fact, like Eddington—he did not see this scheme as contradicting the conventionality thesis:

> What becomes of Einstein's insistence that his method for setting distant clocks—that is, choosing the value $1/2$ for ϵ—constituted a 'definition' of distant simultaneity? It seems to me that Einstein's remark is by no means invalidated. [12, p. 66]

The point is that using the slow clock method to synchronise distant clocks *is itself just another synchrony convention.* It's also, of course, completely irrelevant for clocks which are *not* originally transported away from one another in this way.

[3] Note that here the 'clock hypothesis'—which I'll discuss in detail in Chapter 9—is invoked implicitly.

7.3.2 Malament's 1977 theorem

I'll now dedicate some attention to a theorem proven by Malament in 1977 [99], which was (and continues to be) interpreted by many as demonstrating unequivocally that simultaneity is *not* conventional in special relativity, and that only the $\epsilon = 1/2$ simultaneity convention is allowed. As Brown puts it, Makament's proof is

> a result which virtually single-handedly managed to swing the orthodoxy within the philosophy literature from conventionalism to anticonventionalism. [14, p. 98]

And here's Norton:

> Contrary to most expectations, [Malament] was able to prove that the central claim about simultaneity of the causal theorists of time was false. He showed that the standard simultaneity relation was the only nontrivial simultaneity relation definable in terms of the causal structure of a Minkowski spacetime of special relativity. [112, p. 222]

The content of Malament's result is this. He claims to prove that the simultaneity relation $S(\cdot, \cdot)$ picked out by the standard ($\epsilon = 1/2$) convention is the only such relation

(a) which is invariant under all *O-causal automorphisms* (i.e., maps from Minkowski spacetime to itself preserving the lightcone structure and mapping the worldline of some observer O to itself).

(b) which is an equivalence relation (i.e., which is symmetric, transitive, and reflexive).

(c) for which there exist world points p and q, one of which is on O's worldline and one of which is not, such that $S(p, q)$.

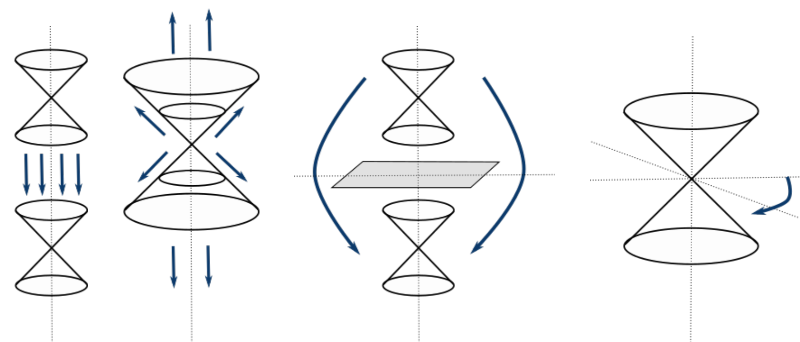
(d) which is not the universal relation.

That is, Malament considers a world with only one inertial observer O, along with the causal (i.e. lightcone) structure of special relativity. He then considers the simultaneity relations which can be defined from this structure—i.e., which respect the symmetries of this structure, which are known as the 'O-causal automorphisms' (if a symmetry relation were not to respect the symmetries of this structure, then it would—by the mantra of Chapter 5—presuppose implicitly further structure, which is *ex hypothesi* prohibited), and shows that, subject to the further above (supposedly innocuous—but see e.g. [69, 82] for for discussion and criticism) constraints, this picks out uniquely the standard synchrony relation as the simultaneity relation which O would be able to use in order to 'spread time through space.'

What exactly *are* the O-causal automorphisms? These are maps from the worldline O to itself which preserve the worldline (hence 'automorphism') and the lightcone structure on O (hence 'O-causal'). They include all and only:

1. Translations along O.
2. Scale expansions.
3. Reflections about a hypersurface orthogonal to O.
4. Spatial rotations.

Visually, from left to right, these transformations are presented in the following diagram (based upon [112, p. 226]):



The idea is this: given an inertial worldline O in Minkowski spacetime, there is only one simultaneity relation which an observer represented by the wordline could define—namely, standard synchrony. Any other simultaneity relation would not be invariant under O-causal automorphisms, and so (to repeat) would imply a commitment to further spatiotemporal structure beyond that of Minkowksi spacetime. One prominent author who gives exactly this line of argument is Friedman:

> So we cannot dispense with standard simultaneity without dispensing with the entire conformal structure of Minkowski space-time. Second, it is clear that if we wish to employ a nonstandard [simultaneity] ... we must add further structure to Minkowski space-time. ... This additional structure has no explanatory power, however, and no useful purpose is served by introducing it into Minkowski space-time. Hence the methodological principle of parsimony favors the choice of Minkowski space-time, with its 'built-in' standard simultaneity, over Minkowski space-time plus any additional nonstandard synchrony.
>
> These considerations seem to me to undercut decisively the claim that the relation of [simultaneity] ... is arbitrary or conventional in the context of special relativity. [60, p. 312]

Friedman's point is that, in order to articulate non-standard synchrony conventions in a given frame in special relativity, one must introduce extra structure.

But, just as the extra structure in Newtonian spacetime (i.e., persisting points of absolute space—see Chapter 5) is unnecessary to state the laws of Newtonian mechanics, so too is this extra structure otiose in the relativistic case. Thus, Friedman is stating that while we *could* articulate non-standard synchrony conventions in a given frame, this would involve introducing extra structure, and we have an Occamist norm to not do so (cf. [27]). *This* is the import of Malament's result, for Friedman.

Not all authors agree with Friedman. Brown's response, perhaps predictably, is very different:

> Why should we consider defining simultaneity just in terms of the limited structures at hand in the Grunbaum-Malament construction, namely an inertial world-line W and the causal, or light-cone structure of Minkowski space-time? [14, p. 100]

The thought is this: in the real world, there are *multiple* observers, each with an associated worldline. What's wrong with saying that O is to use the standard simultaneity relation of O'—which need not be a standard simultaneity relation for O? Malament's proof, the thought goes, would have relevance only in the impoverised (and utterly counterfactual!) case in which only one inertial observer exists in a background Minkowksi spacetime. (Cf. [82].)

In fact, however, Brown's qualms run deeper than just this: in the Malament world, it's not obvious that we have enough physical structure to set up coordinates *at all* (how, operationally, is one to 'spread time through space' with only one worldline—that of O?). There would, for example, be no way to set up 'radar coordinates' in such a world. (Not only this, but in fact stronger: it's not obvious that Brown—with his views that spacetime geometry is ultimately to be regarded as a codification of dynamics—will regard the Malament world as being coherent to begin with!) So, given an operational understanding of coordinates, it's not clear that it is legitimate to speak of simultaneity relations *at all* in that world. And in the actual world, there are many observers and much physical structure, which should afford *ample* opportunity to define non-standard simultaneity relations for O. Either way, Malament's proof seems to fail to show what is claimed.

For what it's worth, I find Brown's reasoning here convincing. But it's helpful to recall the different possible understandings of the content of special relativity (adumbrated at the end of the previous chapter) in order to understand why the issue of the conventionality of simultaneity continues to propel authors in different directions. If one understands (as on the third option) special relativity to *just be* a theory of Minkowski spacetime and what's derivable therefrom, then the Malament-Friedman line that simultaneity is not conventional in special relativity (because only standard synchrony is definable using only one observer and said structure) looks more plausible. But if one has the second understanding, according to which special relativity has to do with Poincaré invariant material laws, then arguably Brown's position becomes the more plausible (here, there is no limit to the number of material bodies involved). Interestingly, if one takes the *first* understanding, according to which special relativity essentially amounts to the content of Einstein's 1905 paper, then there's a sense in which simultaneity is *not* conventional in the theory, for standard synchrony is baked into its axioms! This highlights that there can be both theory-*external* notions of conventionalism—which additional, super-empirical, assumptions to insist upon when building a theory?—and theory-*internal* notions of conventionalism—having fixed a theoretical edifice, what's definable uniquely therefrom, and what's not?

Let me close this chapter with one broader thought. Famously, Quine, in his critique of the analytic/synthetic distinction, maintained that "the lore of our fathers is ... a pale grey lore, black with fact and white with convention. But I have found no substantial reasons for concluding that there are any quite black threads in it, or any white ones" [132]. If correct, this would imply that there is no clean distinction between the (supposedly) empirically-motivated inputs in Einstein's 1905 derivation of the Lorentz transformations (e.g. his two postulates) and the (supposedly) conventional inputs (e.g. standard synchrony).

Question: How plausible is Quine's position, in the context of special relativity?

Chapter 8

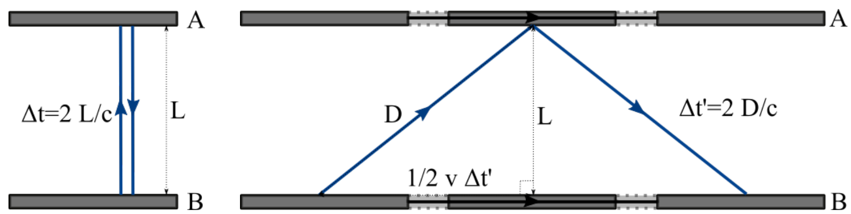
Frame-dependent effects

> The presentation of space-time theory found here has slowly evolved over many classes. At first I followed standard presentations, making extensive use of coordinates and coordinate transformations. Bit by bit, class after class, reference to coordinates dropped away, leaving the fundamental geometry open to inspection. (Maudlin, 2012)

The phenomena of time dilation, length contraction, and the relativity of simultaneity are often presented as some of the defining and canonical results of special relativity. However, there are, as we will see in this chapter, at least some good reasons for doubting the physical reality of these phenomena, for they are *frame-dependent* effects, which do not admit of a description liberated from coordinate systems. So: are these truly physical effects or not? This is the question upon which I will focus in this chapter.

8.1 Time dilation

I'll begin with time dilation: the famous special relativistic result that 'moving clocks run slow'. It's easy to demonstrate time dilation directly from Einstein's two postulates using the example of a Langevin clock: in a frame moving uniformly with respect to the light clock setup presented by Einstein at the beginning of his 1905 paper (see Chapter 4), the light will still travel with velocity c, but will now have to traverse the hypotenuse of a triangle—meaning that the time between ticks will thereby be slower.



Exercise: Derive a formula describing the rate of ticking of a Langevin clock in terms of the velocity of that clock in the direction orthogonal to the alignment of the mirrors.

The result can also be derived directly—and very straightforwardly—from the Lorentz transformations. Considering two coordinate systems related by a Lorentz boost in the positive x-direction, we have, where $\beta := {}^{v}\!/_{c}$,

$$c\Delta t' = \gamma \left(c\Delta t - \beta \Delta x \right), \tag{8.1}$$

$$\Delta x' = \gamma \left(\Delta x - \beta c \Delta t \right), \tag{8.2}$$

$$\Delta y' = \Delta y, \tag{8.3}$$

$$\Delta z' = \Delta z. \tag{8.4}$$

Setting $\Delta x = 0$ in the first of the above Lorentz transformations, we have $\Delta t' = \gamma \Delta t$. Thus, given a clock stationary in one frame, that clock will tick more slowly in a Lorentz-boosted frame.

But here's the rub: time dilation seems to arise because the time elapsed between ticks on a clock is *frame-relative*. So it seems that one 'gets a clock to slow down' merely by changing one's own frame of reference; but, in so doing, one clearly does nothing at all to the clock itself. (In other words, one need only perform a passive rather than an active transformation in order for time dilation to manifest itself—recall Chapter 2.) This line of thought seems to suggest that time dilation is not a real *physical* effect, but is a 'merely perspectival' one. Moreover, whether or not a clock moving in a given direction runs slow relative to any given frame depends upon how distant clocks are synchronised in that frame. Hence, conventionalists about simultaneity should also, for consistency, be conventionalists about time dilation—and this might reasonably further undercut any thought that time dilation is a 'real' phenomenon.

8.2 Length contraction

Le me turn now to length contraction. Like time dilation, this phenomenon can be derived from Einstein's two postulates, as well as directly from the Lorentz transformations. This time, I'll skip directly to the second. Consider again a boost in the positive x-direction. Combining (8.1) and (8.2), we have

$$\Delta x' = \gamma\Delta x - \beta c\Delta t' - \beta^2\gamma\Delta x. \tag{8.5}$$

Setting $\Delta t' = 0$, we have

$$\Delta x' = \gamma\Delta x \left(1 - \beta^2\right). \tag{8.6}$$

But $\gamma^{-2} = 1 - \beta^2$, so

$$\Delta x' = \frac{1}{\gamma}\Delta x. \tag{8.7}$$

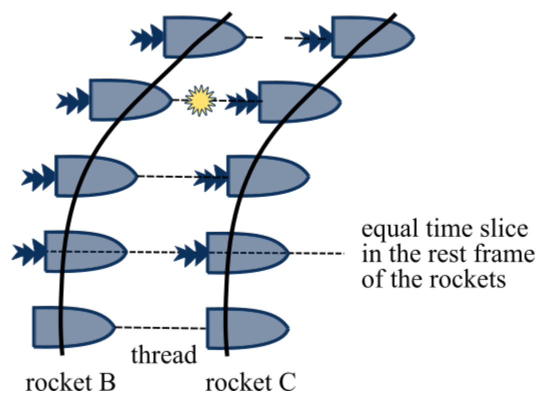
So, given a rod stationary in one frame, the distance between the ends of that rod at a given time will be smaller in a Lorentz-boosted frame.

Once again, there are worries here regarding perspectivalism and conventionalism. Length contraction seems to arise because the length of a rod is *frame-relative*. So it seems that one 'gets a rod to contract' merely by changing one's own frame of reference; but, in so doing, one clearly does nothing at all to the rod itself. This line of thought seems to suggest that length contraction is not a real *physical* effect, but is a 'merely perspectival' one. Moreover, note that the length of a given object in a given frame depends upon the synchrony scheme for distant clocks in that frame—if (and only if) the object is moving relative to the frame in question. Hence, conventionalists about simultaneity should also, for consistency, be conventionalists about lengths of moving bodies—and this might reasonably further undercut any thought that length contraction is a 'real' phenomenon.

8.3 Bell's rockets

We've already seen the relativity of simultaneity in the previous chapter, so I'll skip over an explicit discussion of that phenomenon here. Rather, I'll turn now to the question of whether frame-dependent *explanations* in special relativity are—or can be—legitimate. (As contrasted with the question of whether frame-dependent *phenomena* are physically real.) One of the most famous places in which frame-dependent explanations come to the fore is a thought experiment due to John Bell, regarding two rockets:

> Three small spaceships, A, B and C, drift freely in a region of space remote from other matter, without rotation and relative motion, with B and C equidistant from A.
>
> On reception of a signal from A, the motors of B and C are ignited and they accelerate gently.
>
> Let the ships B and C be identical, and have identical acceleration programmes. Then (as reckoned by the observer in A) they will have at every moment the same velocity, and so remain displaced one from the other by a fixed distance. Suppose that a fragile thread is tied initially between projections from B and C[, and that] it is just long enough to span the required distance initially. [7, p. 67]



Question: Does the string in Bell's rocket thought experiment break? Why, or why not?

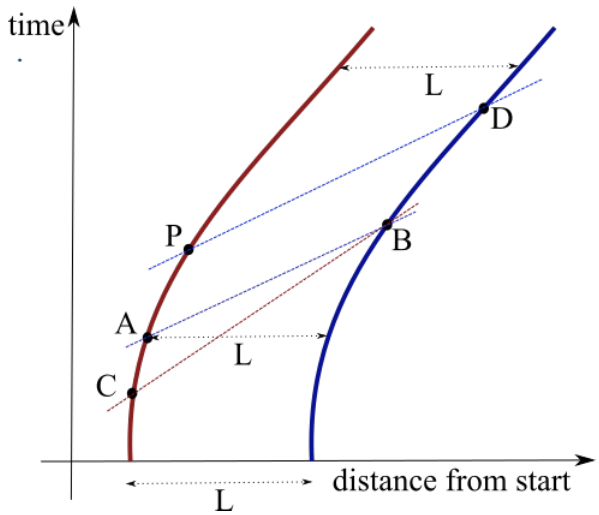
Take a couple of minutes to think about the above question before proceeding. As Bell explains, the answer to the question is this:

> If [the rope] is just long enough to span the required distance initially, then as the rockets speed up, it will become too short, because of its need to Fitzgerald contract, and must finally break. It must break when, at a sufficiently high velocity, the artificial prevention of the natural contraction imposes intolerable stress.
>
> Is it really so? This old problem came up for discussion once in the CERN canteen. A distinguished experimental physicist refused to accept that the thread would break, and regarded my assertion, that indeed it would, as a personal misinterpretation of special relativity. We decided to appeal to the CERN Theory Division for arbitration, and made a (not very systematic) canvas of opinion in it. There emerged a clear consensus that the thread would **not** break! [7, pp. 67-68]

So, the string breaks, as illustrated also in the above diagram (based upon that found in [103]). But let's think about the different explanations for *why* the string breaks, which might be offered from different frames of reference:

- From the point of view of the control tower A, the breakage happens as a result of length contraction of the string.

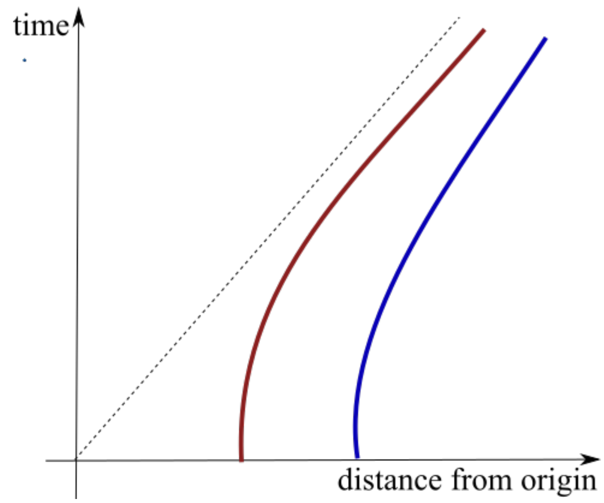
- From the point of view of the first rocket B, the breakage happens as the second rocket moves progressively further away (due to the relativity of simultaneity—draw a spacetime diagram!).



- From the point of view of the second rocket C, the breakage happens as the first rocket lags further behind (due to the relativity of simultaneity—draw a spacetime diagram!).

All of the above should make sense (though I'll return shortly to the question of whether frame-relative explanations in general are legitimate). So why so much confusion in the CERN theory division about whether the string would snap? The Bell rocket scenario is peculiar, in the following sense. If one were to begin with two rockets stationary with respect to one another, and boost to a uniformly accelerating frame in special relativity (a 'Rindler frame'), one would find that the rockets do *not* have the same accelerations in this frame, at any given time. This difference in accelerations would mean that the rockets move closer to one another as they accelerate, thereby implementing the length contraction effects. This does *not* happen in the Bell rocket scenario—so the rest frame of A is *not* a Rindler frame. This difference is illustrated in the diagrams on this page and the next (which are based upon those found in [170]): the first represents the Bell rocket setup; the second represents two rockets in a Rindler frame. Clearly, these two physical setups are different!

In other words, the point is this: many presented with this puzzle assume that, as the rockets accelerate, the rocket-string-rocket system length contracts (from the point of view of the control tower A), so that the string does not snap. However, *by stipulation*, in the Bell rocket scenario, the rockets maintain at all times equal spatial distance between them, in A's frame. This means that the rockets exert an ever-greater force on the string, ultimately meaning that the latter will snap. In Bell's scenario, the string connecting the rockets is weak: it breaks under only a small applied force, and is unable to keep the rockets together. If, however, the string were infinitely strong, then it would contract as the rockets accelerate, thereby pulling the rockets together: they would form



a *Rindler pair*.

Having settled what can be so confusing about the Bell rocket example, let's return to our three frame-dependent accounts of why the string breaks, in Bell's original scenario. Maudlin repudiates such explanations:

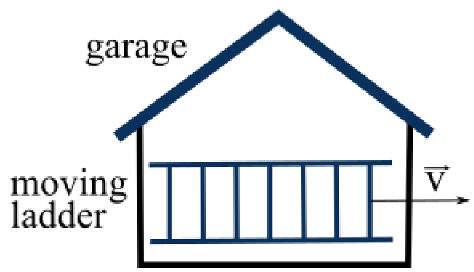
> The surface contradiction between these three account of why the thread breaks illustrates that frame-dependent narrations of events in Relativity can be misleading. There is one set of events, governed by laws that are indifferent to which coordinate system might be used to describe a situation. In each frame-dependent account, the interatomic forces in the thread play a role in determining exactly when the thread breaks. But how that role is described in a particular reference frame depends critically on which frame is chosen. [103, p.120]

Question: What, exactly, is misleading about frame-dependent accounts of special relativistic phenomena?

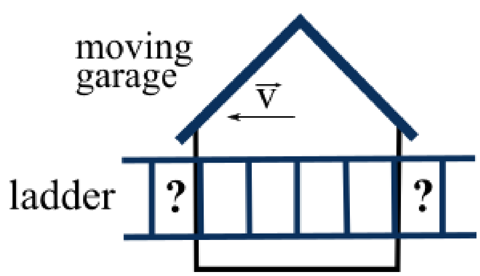
8.4 The ladder paradox

The Bell rocket thought experiment is one place in which frame-dependent explanations as to why a certain (invariant!) phenomenon occurs (in that case, the snapping of the string tethering the two rockets) can differ. I now want to illustrate the same point with another example: what's known as the *ladder paradox*.

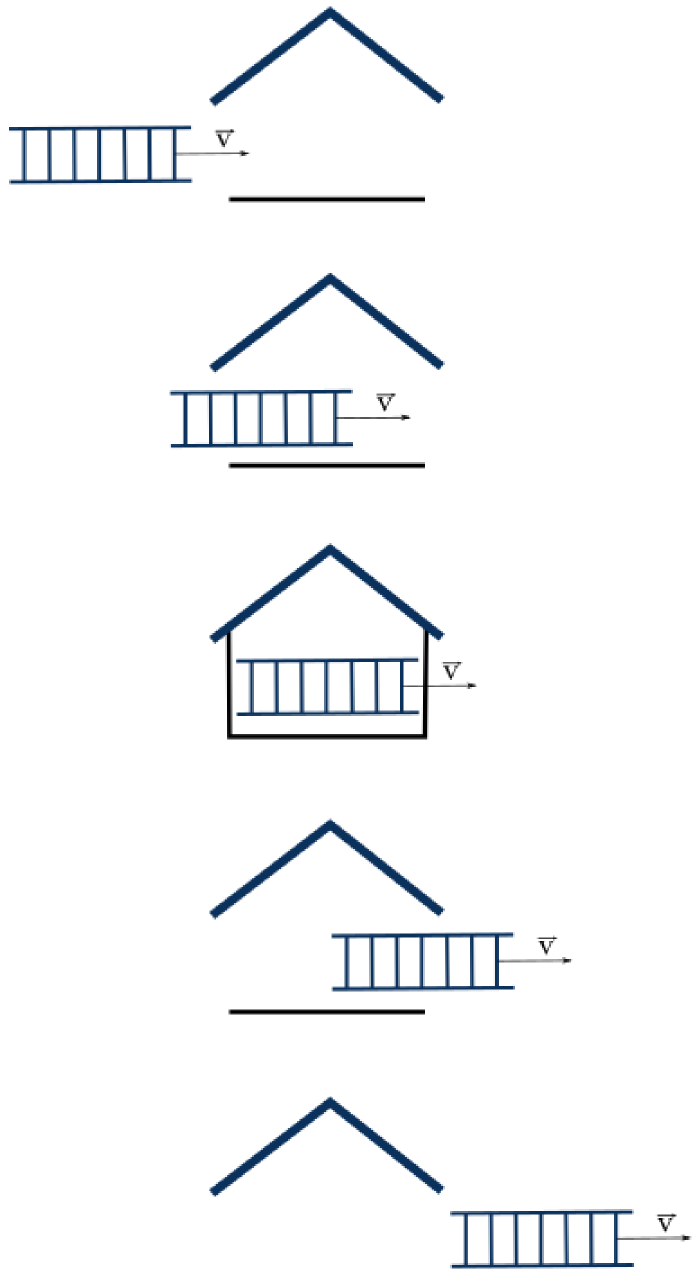
Consider a garage with a front and back door which are open, and a ladder which, when at rest with respect to the garage, is too long to fit inside. Now move the ladder at a high horizontal velocity through the stationary garage. The ladder undergoes length contraction, meaning that it can fit inside the garage, at a particular time. We could, if we liked, simultaneously close both doors for a brief time, to demonstrate that the ladder fits, as illustrated below:



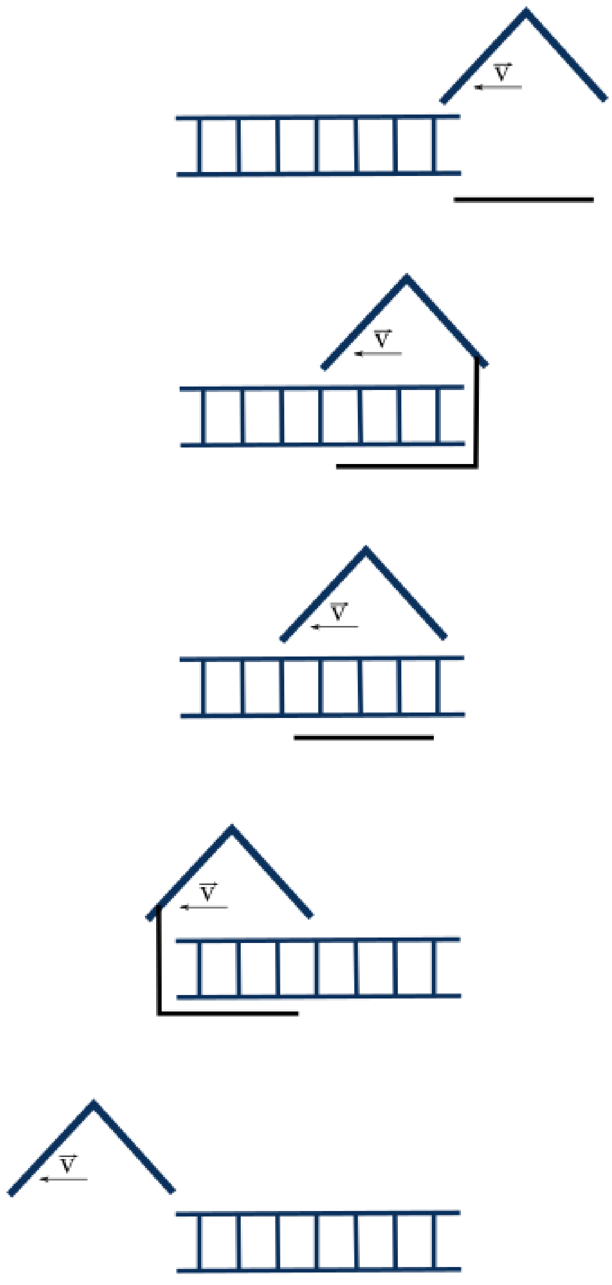
But now consider an observer co-moving with the ladder. From this perspective, the ladder is stationary, and the garage is moving at high velocity. So the garage is now length contracted—so how can the ladder fit inside the garage, and how can the doors close to contain the ladder?



The solution to the riddle is this: in the frame co-moving with the ladder, we need to properly take into account the relativity of simultaneity in the ladder's rest frame: the doors of the garage no longer close at the same time! This can be brought out by considering again the situation in the barn rest frame versus ladder rest frame. The barn-frame description runs as follows:



The ladder-frame description, by contrast, runs as follows:



What's wrong with frame-relative explanations like these? Don't they help us to get a handle on what's invariant and what's not in special relativity, and thereby help us to understand the architecture of the theory? Presumably, however, authors such as Maudlin would deride these frame-dependent explanations of the ladder paradox, for the same reasons as in the case of Bell's rocket thought experiment.

> **Question:** What kind of explanation do you think would satisfy Maudlin here? And is it as physically insightful as these frame-dependent explanations?

I'll go some way to answering the above question in the next section.

8.5 Assessing frame-dependent effects

Up to this point in this chapter, we've both witnessed frame-dependent effects such as time dilation and length contraction, and seen arguments to the effects that these phenomena are 'merely perspectival', or conventional. We've also seen, in the context of the Bell rockets and ladder paradoxes, that one can find in the literature different attitudes towards the legitimacy of frame-dependent explanations. This means that there are really two questions in play:

1. Are frame-dependent *explanations* of physical phenomena legitimate?
2. Are frame-dependent *effects*—e.g., length contraction and time dilation—'physical'?

As we have seen, Maudlin disavows frame-dependent explanations (of e.g. the Bell rocket result), for different explanatory accounts will be offered in different frames. But what exactly is wrong with availing oneself of such explanations? Why does a lack of univocity imply illegitimacy? Maudlin instead prefers *geometrical* explanations, as is evident in the epigraph to this chapter. Note, in particular, that in that passage Maudlin is:

1. Committing to a geometrical understanding of special relativity.
2. Disavowing frame-dependent explanations.

The thought is that only invariant structures—e.g. the structure of Minkowski spacetime in special relativity—should feature in genuine explanations. Whatever one makes of this, it is clearly going to be anathema to e.g. Brown, for whom such invariant spacetime structures are just a codification of the symmetry properties of the dynamical equations governing matter, written in coordinate bases. (See Chapter 10.)

Let's turn now to the second question: are frame-dependent effects 'physical'? To make progress in answering this question, let me say provisionally that a phenomenon associated with a coordinate transformation is *physical* just in case that transformation relates physically distinct states of affairs. So:

- Global Galilean boosts are physical in Newtonian spacetime.

- Global Galilean boosts are not physical in Galilean spacetime.
- Global Lorentz boosts are not physical in Minkowski spacetime. (Recall: Minkowski spacetime has no standard of rest.)
- *Local* Galilean boosts are physical in Galilean spacetime. (Consider Galileo's ship.)
- *Local* Lorentz boosts are physical in Minkowski spacetime. (Consider a constant-velocity-transformation version of Bell's rockets—this is what Maudlin calls 'physical length contraction'.)

The moral which I think we can take here is this. The physicality of a coordinate effect (by the preceding definition of 'physicality') is crucially dependent upon

(a) the amount of spacetime structure presupposed, and

(b) whether the associated coordinate transformations are applied globally (i.e., to the whole universe) or locally (i.e., to subsystems of the universe).

Local transformations can effect genuine physical change, even if the particular *mode of description* of that change is frame-dependent (cf. again Bell's rockets, or the ladder).

8.6 Fragmentalism

Within the metaphysics literature, there's a stronger view than that articulated at the end of the previous section, to the effect that *all* frame-dependent effects can (in principle) be regarded as being physically real. This view is known as 'fragmentalism', and was first articulated by Fine in the context of the philosophy of time [57]. According to this view, "the world is inherently perspectival", and "the overall collection of facts, 'über reality', includes pairs of mutually incompatible facts" [94, p. 23]. So, on this view in the context of special relativity, the totality of facts about the universe includes frame-dependent facts about (e.g.) lengths of rods and periods of clocks, which are mutually inconsistent.

It's important to be clear on the fragmentalist's commitments. As Lipman writes,

> The importance is that of marking a metaphysical realism about those variant matters. The relevant question is whether realism or antirealism is true about the frame-relative facts, that is, whether consideration of the special theory of relativity removes all frame-relative facts from one's metaphysical conception of reality: the Minkowskian answers yes, the fragmentalist answers no. [94, p. 31]

That is, the fragmentalist doesn't deny the existence of coordinate-independent facts to do with (say) Minkowski spacetime; they simply admit further, frame-dependent facts into their ontology. I'll leave it to the reader to decide what to make of fragmentalism in the context of special relativity;[1] here, however, are two questions which the fragmentalist must address:

Question: How to make sense of a 'disunified reality', according to which 'the totality of facts is incoherent'?

Question: What does fragmentalism add to the considerations of physicality and subsystem-environment decompositions introduced already above?

[1] For my own take, see [137].

Chapter 9

The twin paradox

> If we placed a living organism in a box ... one could arrange that the organism, after any arbitrary lengthy flight, could be returned to its original spot in a scarcely altered condition, while corresponding organisms which had remained in their original positions had already long since given way to new generations. For the moving organism, the lengthy time of the journey was a mere instant, provided the motion took place with approximately the speed of light. (Einstein, 1911)

From *Planet of the Apes* to *Ender's Game*, the twin paradox is by now a mainstay of 20th Century science fiction. Qualitatively, the idea is this: consider two identical twins, at rest on the Earth. One twin takes an interstellar journey before returning to Earth, while the other remains at home on Earth; on reunion, our twins find that they have aged by different amounts. So far, this is just a *feature* of special relativity (which I'll derive quantitatively below)—the *paradox* is supposed to consist in the fact that, if one considers the same situation in the rest frame of the travelling twin, then it seems that it should be the *Earthbound* twin who ages less (the situations are entirely symmetrical, or so it seems). So, how to resolve this paradox? This is the question which I'll address in this chapter.

9.1 The clock hypothesis

Before I discuss the twin paradox any further, I need to introduce a crucial device in the foundations of spacetime theories: what's known as the *clock hypothesis*. Suppose that we have two identical clocks built from Poincaré invariant matter fields, with one clock moving with uniform velocity with respect to the first. Will these clocks function identically in their rest frames? *Yes*, by the relativity principle. Now suppose we have two identical clocks built from Poincaré invariant matter fields, with one clock accelerating with respect to the first. Will *these* clocks function identically in their rest frames? *Not necessarily*—for the relativity principle holds for systems related by *Poincaré* transformations.

Another (more geometrical) way to make the point is this. Given two clocks A and B, if B moves at uniform velocity with respect to A, then if A correctly reads off the Minkowski spacetime interval $\int_{\gamma_A} ds$ along its worldline γ_A, then so too will B correctly read off the interval $\int_{\gamma_B} ds$ along its worldline γ_B, by the relativity principle. However, if B accelerates with respect to A, then the fact that A correctly reads off the Minkowski spacetime interval $\int_{\gamma_A} ds$ along its worldline γ_A does not guarantee that B correctly reads off the interval $\int_{\gamma_B} ds$ along its worldline γ_B. That this is so is an additional input assumption, which is the clock hypothesis. As Maudlin puts it, the clock hypothesis amounts to this:

> The amount of time that an accurate clock shows to have elapsed between two events is proportional to the Interval along the clock's trajectory between those events, or, in short, clocks measure the Interval along their trajectories. [103, p. 76]

One should not, however, simply assume that the clock hypothesis is foundationally unproblematic. In fact, to suppose that *any* clock satisfies the clock hypothesis is misleading, for all clocks have a breaking point. As Eddington said nicely of an accelerating clock,

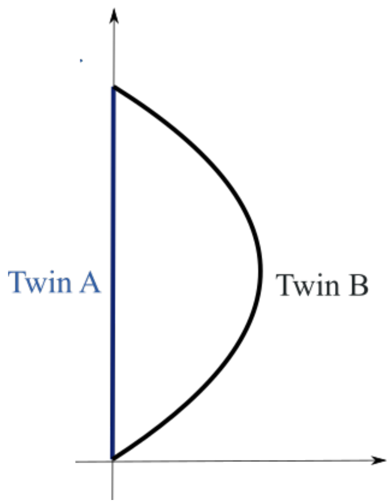
> We may force it into its track by continually hitting it, but that may not be good for its time-keeping qualities. [39, p.64]

The point is this: whether a particular clock ticks in accordance with the spacetime metric is not a matter of stipulation or luck, but depends crucially on the constitution of the clock. For any given clock, no matter how ideal its performance when inertial, there will in principle be an acceleration-producing external force, or even tidal effects inside the clock, such that the clock 'breaks,' in the sense of violating the clock hypothesis. Might it therefore not be more appropriate to speak of the clock *condition*? (Cf. [17, §III.C].)

Regardless of what one thinks of this, what's uncontroversial is that, whenever we have accelerating clocks, the clock hypothesis/condition must be brought into consideration: is it satisfied or not? And what upshots does this have for the discussion at hand? In much of this chapter, in order to render the contours of philosophical discussion of the twin paradox as crisp as possible, I'll simply *assume* the clock hypothesis—but it's important to remember that this principle lurks beneath the hood. I'll flag it again explicitly where relevant.

9.2 The twin paradox

Without further ado, then, let's turn to a more quantitative presentation of the twin paradox. Consider two identical twins A and B, who are spatiotemporally coincident on the Earth at some time. Twin B decides to make an out-and-back trip away from the Earth—perhaps to our closest star, Alpha Centauri—while Twin A stays at home. The situation is illustrated on a spacetime diagram as follows:



It's a basic feature of special relativity that, on returning to the Earth, Twin B will have aged less than Twin A. This is easy to see, by computing the proper time (which is the time read off by a clock in the rest frame of the observer under consideration, which will corresponding to the integral of the metric interval along that observer's worldline, on the assumption of the clock hypothesis) along the worldline of each twin:

$$T_A = \int_o^p d\tau_A \tag{9.1}$$

$$\begin{aligned} T_B &= \int_o^p d\tau_B \\ &= \int_o^p \left(1 - \left(\frac{dx}{d\tau_A}\right)^2 - \left(\frac{dy}{d\tau_A}\right)^2 - \left(\frac{dz}{d\tau_A}\right)^2\right)^{\frac{1}{2}} d\tau_A \\ &< T_A. \end{aligned} \tag{9.2}$$

(Here, o and p are, respectively, the departure and reunion events of the two twins.) Note that the result of this computation is not relative to a particular frame—it's a *frame-independent fact* that Twin B has aged less than Twin A when they are reunited.[1] There's a temptation to appeal to time dilation in order to explain the twin paradox result, but (at least in the first instance) this should be resisted: we've already seen in the previous chapter that whether it's appropriate to appeal to time dilation will depend upon the frame of reference with respect to which one is describing the physical situation under consideration; moreover, there are choices of simultaneity convention which *eliminate* time dilation effects. Thus—again, as stressed previously—at the very lest such accounts cannot be fundamental.

The above result is certainly unexpected, but it's not yet a paradox. (Recall Quine's famous characterisation of a paradox: an apparently successful argument having as its conclusion a statement or proposition that seems obviously false or absurd [133].) But we can generate the paradox in the following way. We've seen that $T_A > T_B$—and this is a *frame-independent result.* But if we were to boost to B's rest frame, the situation would look (it seems) exactly analogous. In that case, we would surely expect $T_B > T_A$. Assuming that $T_A \neq T_B$, this leads to a contradiction—and so a something more unavoidably classified as a paradox. So the challenge is this: *what breaks the symmetry between A and B?*

9.2.1 Inertial frames

As a first response to the twin paradox, it is natural to appeal to inertial versus non-inertial frames (or, if one prefers language expunged of reference to frames, inertial versus non-inertial *trajectories*). Recall that Minkowski spacetime has the resources to distinguish straight ('inertial') from bent ('accelerating') trajectories. Suppose that A is following an inertial trajectory relative to Minkowski spacetime structure; then (the thought would go), B is *not* following an inertial trajectory relative to the selfsame spacetime structure. Therefore, to boost to B's rest frame would involve moving to a non-inertial frame, in which case, we should not expect the same laws of physics to apply. Thus, consideration of the structure of Minkowski spacetime allows us to break the symmetry between A and B, and thereby resolve the paradox.

This reasoning on the basis on inertial frames is a plausible first reaction to the paradox—although ultimately we'll see that it's not problem-free. Before I get onto that, though, we should recall from Chapter 1 that different authors have very different views on the nature of inertial frames. In particular, authors such as Brown might well be unhappy with the appeal to Minkowksi spacetime in the above discussion of inertial frames. In light of this, we should ask: what role is Minkowski spacetime playing in the above explanation? Could we excise it, and just appeal to the inertial frames as picked out by the dynamics, rather

[1] Aside: this is a nice illustration of the sense in which drawing spacetime diagrams can be misleading—for B's path looks *longer* on the diagram presented above, but is in fact *shorter*, when we do the computation.

than cashed out using geometrical notions? Indeed we can do this—here's how the account might go.

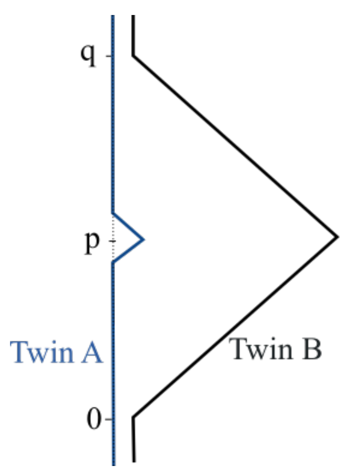
Suppose that A is following an inertial trajectory—that is, it travels with uniform velocity in the inertial frames, as picked out by the dynamics (in one way or another—see Chapter 1). Then B is *not* following an inertial trajectory, for B accelerates with respect to A. Therefore, to boost to B's rest frame would involve moving to a non-inertial frame, in which case, we should not expect the same laws of physics to apply. Thus (again, the thought might go), consideration of the inertial frames allows us to break the symmetry between A and B, and thereby resolve the paradox.

My point here is really a simple one: one can appeal to inertial frames in order to attempt to account for the twin paradox time differential, on both a 'geometrical' and 'dynamical' understanding of inertial frames. Fair enough—but is the account actually any good to begin with? One should be careful about making too much of the inertial/non-inertial distinction, for one can formulate twin paradoxes with

(i) equal accelerations, or

(ii) no accelerations at all!

Let me begin with the first case (here, I'll drawing on Maudlin's very elegant discussion of the twin paradox [103, p. 82]). One can envisage a case where Twin A undertakes a 'mini-journey', but with the same acceleration profile, as per the following diagram:



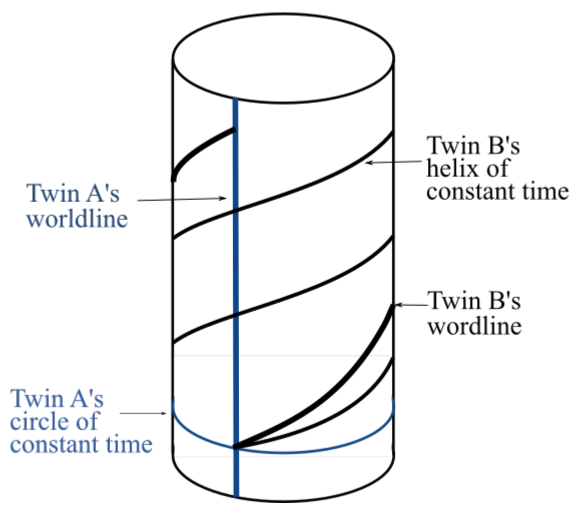
In this case, neither Twin A nor Twin B find themselves in inertial frames—nevertheless, or recombination, Twin B has still aged less than Twin A. Thus, it seems that it cannot be non-inertial motion *alone* which accounts for this result. On this issue, Maudlin writes the following:

> Both Rindler and Feynman point out that acceleration is objective in Relativity, just as it is in Newtonian absolute space and time and in Galilean space-time. This is true but irrelevant: the issue is how *long* the world-lines are, not how *bent*. [103, p. 83]

Let's turn to the second potential problem for inertial frame-based attempts to explain the twin paradox: the cases in which one has no accelerations at all. There are two such cases. The first involves not twin but *triplets*, A (the stay-at-home twin), B (whose clock is initially synchronised with that of A, and who travels away from the Earth with constant velocity), and C (who travels towards the Earth with constant velocity, and who synchronises her clock with that of B on passing the latter). In this case, the time displayed by C's clock will still be less than that displayed by A's clock on recombination. Moreover, here, all three triplets are moving inertially—so can one really appeal to inertial versus non-inertial motion to account for this result?

Question: How physical is this case, given that (presumably) some energy/momentum must be exchanged between Twin B and Twin C?

The second 'no acceleration' case is particularly intriguing. Imagine that our twins A and B find themselves on a spacetime of cylindrical topology, as per the following diagram:[2] (Cf. [169, p. 587].)



In this case, Twin A stays at home as before, whereas Twin B travels with constant velocity around the cylinder, before rejoining Twin A on the Earth. Again in this case, on recombination, Twin B will have aged less than Twin A.

[2] There's a broader literature on twin paradoxes in spacetimes of different topologies: see [98].

Since both twins are (it seems) moving inertially in this case, it would again seem to be the case that one cannot appeal to the distinction between inertial and non-inertial motion in order to account for the time discrepancy between the clock readings of the twins.

In neither of the above cases is there a straightforward way of appealing to the inertial/non-inertial distinction in order to account for the twin paradox time differential. That said, in the latter (i.e., the cylindrical spacetime case), perhaps there is still a difference between A and B—for only A's worldline is aligned with the principal axis of the cylinder. So claim: in this case, at least, there *is* a preferred frame, thereby allowing us to account for the cylindrical twin paradox time differential.

> **Exercise:** Assess the above response to the case of the cylindrical twin paradox.

What should we take the lessons from these cases to be? Taken together, they suggest that the twin paradox result can't be accounted for solely in terms of the accelerations of the twins. So, at this point—as we've already seen in the above quote from Maudlin—exploring some other possible explanations of the paradox is apposite.

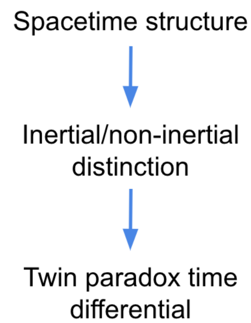
9.2.2 Geometrical and dynamical explanations

On the twin paradox, Maudlin writes:

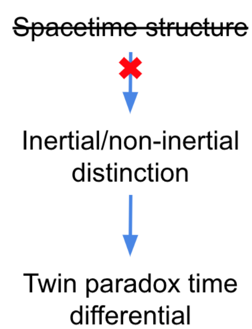
> The Twins "Paradox" has inspired more confusion about Relativity than any other effect. The explanation of the phenomenon, in terms of the intrinsic geometry of Minkowski space-time and the Clock Hypothesis is exquisitely simple: clocks measure the Interval along their world-lines, and B's world-line between o and q is longer than A's. Period. There is nothing more to say. [103, p. 79]

It's certainly true that this kind of geometrical account of the twin paradox time differential faces no apparent counterexamples, as with the previously-countenanced appeals to inertial frames. But how illuminating is it? Presumably, a 'dynamicist' (e.g. Brown) would find the spacetime explanation of the cylindrical twin paradox (and the equal-acceleration twin paradox) similarly otiose, and would say that, even if it's not an (operationalised) notion of inertial frames which accounts for the time differential, it's still *facts about the matter out of which the twins are built*, more generally construed, which account for the difference, rather than anything to do with spacetime geometry.

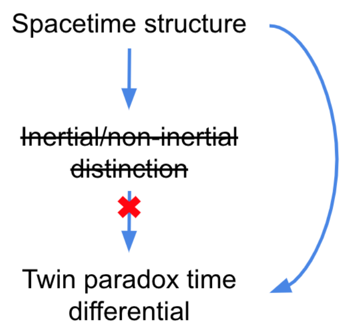
To summarise, the dialectic here between the 'geometrical' camp *à la* Maudlin and the 'dynamical' camp *à la* Brown proceeds as follows. An initial 'geometrical' thought might be that it is spacetime which grounds the distinction between inertial and non-inertial motion, and it is this distinction which can be appealed to in an explanation of the twin paradox time differential. Such a line of thought could be represented thus:



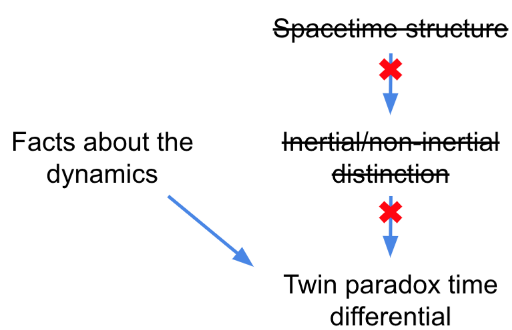
On the other hand, an initial 'dynamical' though, as we have seen, would be that the appeal to spacetime in the above is redundant, and one can appeal directly to the inertial/non-inertial distinction (as, ultimately, given by facts about the dynamics) in order to account for the twin paradox time differential. Such a line of thought could be represented thus:



In light of our problem cases, however, we've seen that it's difficult to maintain that appeal to the inertial/non-inertial distinction can account completely for the twin paradox result. In light of this, a revised 'geometrical' understanding (again, *à la* e.g. Maudlin) would appear thus:



By contrast, a revised 'dynamical' thought would maintain that it's facts about dynamics which *directly* explain the twin paradox time differential; appeal to the inertial/non-inertial distinction is likewise recognised to be unnecessary here:



Question: Which of the 'geometrical' or 'dynamical' approaches to the twin paradox is to be preffered, and why?

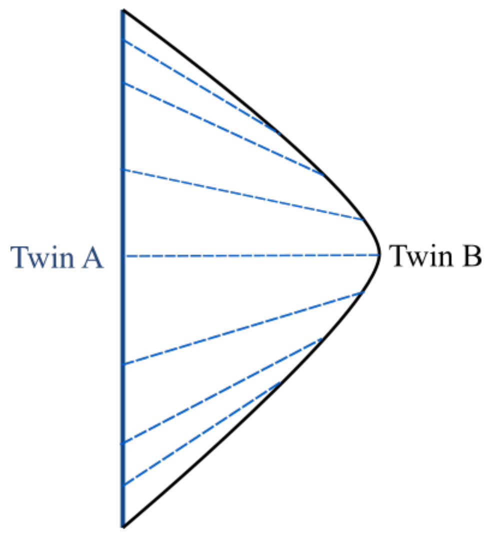
9.3 Frame-relative accounts

There are many purported 'explanations' of the twin paradox which make appeal to *frame-relative structures*. (The situation is very similar to that of e.g. Bell's rockets, discussed in the previous Chapter.) Here, I'll present one of the most prominent of these, which appeals to simultaneity hypersurfaces in B's rest frame.[3] I'll then consider (in a continuation of the discussion presented in the

[3]This particular proposal was first made by von Laue in 1913 [108]; in [110], it is described as a 'complete solution to the twins paradox'. At the very least, that latter claim is misleading,

previous chapter) the more general question of the legitimacy of these accounts.

The account of the twin paradox time differential which appeals to the relativity of simultaneity proceeds like this. Consider the ($\epsilon = 1/2$) simultaneity hyperplanes from the point of view of B's rest frame. At the turnaround point, there is a sudden swing in the hyperplanes, leading to 'lost time' relative to A's worldline. The situation would be illustrated on a spacetime diagram as follows:



Then claim, then, is that it's this 'lost time' which accounts for the time differential between A and B. This seems fine (at least if one is Brown—not if one is Maudlin!), but is the account a fundamental one? Here's Brown on this question:

> *[E]xplanations of synchrony-independent phenomena in SR that rely crucially on the relativity of simultaneity are not fundamental.* (A common example concerns the clock retardation effect, or 'twins paradox', where it is claimed that at the point of turn-around of the travelling clock, the hyperplanes of simultaneity suddenly change orientation and the resulting 'lost time' accounts for the fact that the clocks when reunited are out of phase. It is worth bearing in mind that the clock retardation effect, like any other synchrony-independent phenomenon in SR, is perfectly consistent with all the non-standard transformations ..., including those which eliminate relativity of simultaneity.) [14, p. 105]

I agree with Brown (who, on this front, would also agree with Maudlin). There are, indeed, three reasons why such accounts of the twin paradox result should be regarded as being non-fundamental:[4]

given the appeal of this account to frame-relative structures.

[4]Those who don't accept that simultaneity is conventional in special relativity—recall

1. They are frame-relative.

2. They are convention-relative. (For more on this, see [28].)

3. They only apply to certain versions of the paradox—e.g., not to the cylindrical case.

To repeat: Maudlin *agrees* that such accounts are non-fundamental, but also (as we have seen in the previous chapter) regards such accounts as thereby illegitimate. Thus, the difference between authors such as Brown on the one hand, and such as Maudlin on the other, *vis-à-vis* such frame-dependent accounts, can be summarised thus:

Brown-style: They are legitimate, but non-fundamental.

Maudlin-style: They are illegitimate and non-fundamental.

(Of course, we've already seen that these authors have profoundly different views as to what would count as a *fundamental* explanation of the effect—Maudlin appeals to spacetime structure; Brown appeals to dynamics.)

9.4 General relativity

It is sometimes claimed that, since the twin paradox scenario involves accelerations, we must appeal to general relativity to explain the result (at various stages, Einstein and Born made such claims: see [81, p. 165]). Recall that general relativity is Einstein's theory of gravitation, completed in 1915, according to which spacetime structure is dynamical, and can vary in the presence of matter. As I will discuss in detail in Chapter 12, consideration of accelerations afforded a crucial way into the theory for Einstein; my conjecture is that it's *this* role of the consideration of accelerations—as an heuristic for the *construction* of general relativity—which ultimately has led to the confused and incorrect claims that discussion of accelerations *requires* recourse to general relativity—which it emphatically does not! Any such claim, indeed, is confused, for:

1. Accelerations are *not* an essential feature of the twin paradox—as we have already seen above.

2. Special relativity *has the resources to distinguish accelerating from non-accelerating trajectories.* (Recall Chapter 5.)

Still, it's worth dissecting this reasoning a bit more, to see what's really wrong with it (as I say, I'll have more to say on connections with general relativity in Chapter 12).

Chapter 7—would not accept (2). This, however, would not prevent them from still accepting the conclusion, in light of (1) and (3).

Consider the fictitious force terms which one obtains by writing one's theories of physics in non-inertial frames of reference (we've seen explicit examples of these terms in Chapter 1 and Chapter 6). Call these terms 'inertial effect' terms. Einstein in 1907 [44] had an insight—now known as 'Einstein's equivalence principle' (see [90])—that such inertial effect terms are to be *identified conceptually* with terms representing gravitation (for further discussion here, see [89]). I'll discuss the significance of this move—and why Einstein later declared it to be "the happiest thought of my life"—in Chapter 12, but for now let's just consider its ramifications in the context of the twin paradox.

One *could* appeal to Einstein's equivalence principle to explain (accelerating versions of) the twin paradox: the accelerating twin is subject to a gravitational force. But—crucially!—note that this is really no better than the original (bad!) appeal to accelerations! Moreover, this approach is also in tension with a widespread methodology in the philosophy of physics: try to understand effects which arise in a given theory *in terms of that theory itself*—i.e., without introducing notions which transcend that theory. Thus, in my view, claims that one has to appeal to general relativity in order to account for the twin paradox result implicate one in a misunderstanding of (a) the equivalence principle, (b) the representational and descriptive capacities of special relativity (to repeat again: accelerations are perfectly meaningful here!), and (c) the necessity of accelerations for twin paradox effects. Best, then, to avoid such appeals, when one is engaging in the philosophical and conceptual ramifications of the special theory.

Chapter 10

Dynamical and geometrical approaches to spacetime

> There is no intention here to make any reservation whatever about the power and precision of Einstein's approach. But in my opinion there is something also to be said for taking students along the road made by Fitzgerald, Larmor, Lorentz and Poincaré. The longer road sometimes gives more familiarity with the country. (Bell, 1976)

One of the central and recurring themes of this book has been the profound differences between 'dynamical' and 'geometrical' approaches, both to articulating the content of physical theories (e.g. Newtonian mechanics—see Chapter 1) and to the explanations of physical phenomena (e.g. the twin paradox time differential—see Chapter 9). In this chapter, I'll address head-on the differences between authors in these two camps (while also recognising that there are substantial differences *internal* to each of these camps).[1]

10.1 Bell's Lorentzian pedagogy

Bell, in his 'How to Teach Special Relativity' [7], considers an atom as modelled by classical Maxwell theory. He shows that, when such an atom is gently ac-

[1] I should flag at the outset that this debate has become very subtle and involved—in other recent work (in particular [18, 74, 135, 136]), I approach the same issues from other angles, so those articles can be considered complimentary to this chapter.

celerated up to some constant velocity, its moving state will be contracted with respect to its stationary state, in accordance with the length contraction of subsystems under active Lorentz boosts. The moral—what he calls he *Lorentzian pedagogy*—is that we can *explain* the behaviour of macroscopic systems via appeal to the micro-dynamical underpinnings of those systems. In particular, we can do so without "premature philosophizing about space and time" [7].

Brown and Pooley takes inspiration from Bell's Lorentzian pedagogy: they maintain that appeal to the fundamental physical laws governing the systems under consideration can explain the behaviour of those systems [15]. I'll come back to this in a minute, but for the time being note that Bell himself stresses that there are some limitations to his particular electron model as a means of illustrating the Lorentzian pedagogy:

> Can we conclude then that an arbitrary system, set in motion, will show precisely the Fitzgerald and Larmor effects? Not quite. There are two provisos to be made.
>
> The first is this: the Maxwell-Lorentz theory provides a very inadequate model of actual matter, in particular solid matter. It is not possible in a classical model to reproduce the empirical stability of such matter. ...
>
> The second proviso is this. Lorentz invariance alone shows that for any state of a system at rest there is a corresponding 'primed' state of that system in motion. But it does not tell us that if the system is set anyhow in motion, it will actually go into the 'prime' of the original state, rather than into the 'prime' of some other state of the system at rest. In fact, it will generally do the latter. A system set brutally in motion may be bruised, or broken, or heated, or burned. [7, pp. 74-75]

Here, Bell is stressing that, in order for the Lorentzian pedagogy to go through in full detail, we had better

1. appeal to the fundamental laws governing the physical systems under consideration, and
2. hope that we can actually build stable bodies (such as rods and clocks) from matter governed by such laws. (Recall again the clock hypothesis, discussed in Chapter 9.)

In a sense, this point isn't novel to Bell. Here's the young Wolfgang Pauli, writing on the selfsame issues in his seminal 1921 textbook on general relativity:

> Should one, then, completely abandon any attempt to explain the Lorentz contraction atomistically? We think that the answer to this question should be No. The contraction of a measuring rod is not an elementary but a very complicated process. It would not take place except for the covariance with respect to the Lorentz group of the

> basic equations of electron theory, as well as of those laws, as yet unknown to us, which determine the cohesion of the electron itself. [118, p. 15]

So, if one takes certain macroscopic, phenomenological special relativistic effects, e.g.—canonically—the contraction of rods and dilation of clocks (at least relative to some synchrony convention: see Chapter 7), the thought is that it would be legitimate to explain those effects in terms of the micro-constituents of the systems under consideration. As a matter of practical fact, however, we might lack an understanding of the physics of such micro-constituents, or it might be that to work which such physics is experimentally intractable (consider e.g. the number of degrees of freedom in statistical mechanics, often thereby requiring recourse to thermodynamics). For this reason, Brown and Pooley [16] advance what they call a *truncated Lorentzian pedagogy*:

> In order to predict, on dynamical grounds, length contraction for moving rods and time dilation for moving clocks, Bell recognised that one need not know exactly how many distinct forces are at work, nor have access to the detailed dynamics of all of these interactions or the detailed micro-structure of individual rods and clocks. It is enough, said Bell, to assume Lorentz covariance of the complete dynamics—known or otherwise—involved in the cohesion of matter. We might call this the truncated Lorentzian pedagogy. [16, p. 7]

The suggestion is that we can offer a *partial* explanation of special relativistic effects via appeal to the Poincaré invariance of the dynamical laws. A full (untruncated) explanation is deferred to a later date.

10.2 Constructive and principle theories, reprise

Recall from Chapter 4 that a *constructive theory* attempts to provide a detailed dynamical picture of what is microscopically going on, from which predictions for observable phenomena can be derived. A *principle theory*, by contrast, takes certain phenomenologically well-grounded principles, raises them to the status of postulates, and derives from them constraints on what the underlying detailed dynamical equations could be like, without attempting to give a fully detailed account of what those equations *are*. The Lorentzian pedagogy suggests (straightforwardly) that the detailed microdynamics associated with special relativistic systems would provide the constructive account of the behaviour of those systems. Here, indeed, is Bell *circa* 1992 writing on precisely this matter:

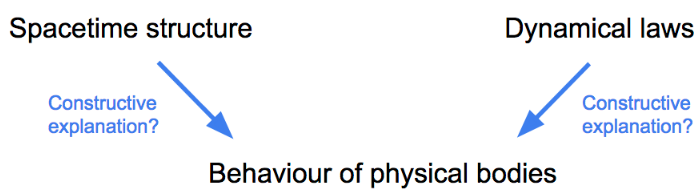
> If you are, for example, quite convinced of the second law of thermodynamics, of the increase of entropy, there are many things that you can get directly from the second law which are very difficult to get directly from a detailed study of the kinetic theory of gases, but you have no excuse for not looking at the kinetic theory of gases to see how the increase of entropy actually comes about. In the same way,

> although Einstein's theory of special relativity would lead you to expect the FitzGerald contraction, you are not excused from seeing how the detailed dynamics of the system also leads to the FitzGerald contraction. [8, p. 34]

Clearly, Bell is suggesting that the fundamental microdynamics governing physical systems can provide a constructive underpinning of (macroscopic) special relativistic effects. Brown and Pooley are fully onboard with this lesson, but others—certain geometricians—have a very different story to tell.[2] To be concrete, here is Janssen's very different take on the constructive theory associated with Einstein's 1905 special relativity:

> Minkowski (1909) did for special relativity, understood strictly as a principle theory, what Boltzmann had done for the second law of thermodynamics. It turned special relativity into a constructive theory by providing the concrete model for the reality behind the phenomena covered by the principle theory. [83, p.40]

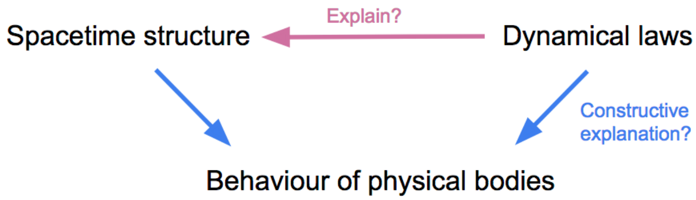
The idea is that it is Minkowski spacetime structure which affords the constructive underpinning of special relativity. The state of play at this point, then, can be summarised as follows:



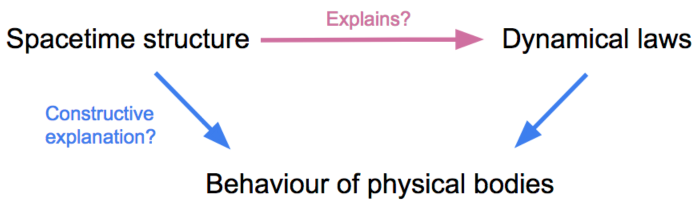
10.3 Arrows of explanation

In order make progress in this dispute regarding the constructive underpinnings of special relativistic phenomena, authors change focus: to whether spacetime structure *explains* the form of the dynamical laws governing the matter out of which our physical systems are constructed, or vice versa. Proponents of a 'dynamical' view *à la* Brown maintain something like this:

[2]As we'll see below, the story is subtle when it comes to some advocates of a geometrical view, e.g. Maudlin [103]. As we'll see, Maudlin is also completely on board with the above lesson from Bell, yet nevertheless maintains that geometry has a significant role to play in the explanation of physical effects and phenomena.



On the other hand, proponents of a 'geometrical' view *à la* Friedman, Janssen, or Maudlin maintain something like this:



Our authors, indeed, recognise explicitly their disagreements as such. Here, again, is Janssen:

> Our central disagreement ... is a dispute about the direction of the arrow of explanation connecting the symmetries of Minkowski spacetime and the Lorentz-invariance of the dynamical laws governing systems in Minkowski spacetime. I argue that the spacetime symmetries are the *explanans* and that the Lorentz invariance of the various laws is the *explanandum*. Brown argues that it is the other way around. [83, p. 29]

Brown agrees on the nature of this dispute, but (by now predictably!) does not think that spatiotemporal geometrical explanations hold together:

> Here we are at the heart of the matter. It is wholly unclear how this geometrical explanation is supposed to work. [14, p. 134]

> As a matter of logic alone, if one postulates spacetime structure as a self-standing, autonomous element in one's theory, it need have no constraining role on the form of the laws governing the rest of the theory's models. So how is its influence supposed to work? Unless this question is answered, spacetime cannot be taken to explain the Lorentz covariance of the dynamical laws. [16, p. 84]

Rather, Brown and Pooley propose to *reverse* the arrow of explanation, as follows (and as we've already seen in e.g. Chapter 1 and Chapter 9):

> [T]he appropriate structure is Minkowski geometry *precisely because* the laws of physics ... are Lorentz covariant. [16, p. 80]

There are three points to note on the proposal which is being adumbrated here by Brown and Pooley. First, inasmuch as the position seeks to *reduce* spacetime structure to facts about the dynamical laws, arguably it is best understood as being a modern-day form of *relationalism* (according to which spacetime is derivative—in some way or other—on material bodies and their behaviours)—see [127].[3] Second, arguably, the view renders the connection between spacetime and dynamical symmetries *analytic*: spacetime structure *just is* an expression of dynamical symmetries [111].[4] Third, if this view can indeed be made to hold together, then there is a clear sense in which spacetime symmetries (and structure) are *explained* by dynamical facts.[5]

To summarise so far, then: certain 'geometrical' authors such as Janssen maintain that spacetime structure *explains* the behaviour of matter, and the symmetries of the associated laws. For Brown and Pooley, this is mysterious; they propose to reverse the arrow of explanation, by *ontologically reducing* spacetime structure to an expression of the symmetries of the dynamical laws for material bodies, which (for them) are to be regarded as being conceptually prior.[6]

10.4 Geometrical sub-views

In order to better understand the geometrical position, I now want to distinguish several different possible versions of this view:

Version A: Spacetime structure (e.g. the Minkowski metric field η_{ab} in special relativity) is ontologically autonomous and primitive, and (in some sense to be articulated) constrains the dynamical behaviour of matter.

Version B: Spacetime structure is not necessarily to be construed as being ontologically autonomous and primitive, but is, rather, a *universal kinematical constraint* on possible physical theorising. (This position is close to that explicitly stated by Janssen [83]). This kinematical constaint could be, e.g.,

1. a 'meta-law', in the sense of Lange [87], or
2. a pragmatic restriction (more on which below).

Versions A and B.1 are both what I referred to in [135] as 'unqualified geometrical views', in the sense that both are subject to Brown and Pooley's

[3] It hasn't escaped notice that this position is not neutral on the metaphysics of laws of nature—see [16, 77, 136] for discussion.

[4] Brown is broadly on board with this claim—see [18] for his engagement with the analyticity claim.

[5] For further discussion of issues of explanation in this debate, see [136] and references therein.

[6] Several authors have reasonably asked whether one can articulate these laws without presupposing spacetime structure. I touched on this question in Chapter 6, but see e.g. [30] for further discussion.

challenge: *how is this geometrical explanation supposed to work?*[7] Version B.2 is, by contrast, a 'qualified geometrical view', in the sense that this charge does not apply to it: we can use (e.g.) η_{ab} to explain the behaviour of matter (including the symmetry properties of the laws governing matter), once we have restricted to a certain allowed class of laws (namely, those which are Poincaré invariant). We'll see this view expliclty in the quote from Maudlin below—so there's little doubt that Maudlin counts as a 'qualified geometrician'.

Before I get to Maudlin's views in more detail, though, I want to ask the following question: in what sense can a qualified geometrical approach offer a *constructive* explanation of the behaviour of the physical bodies under consideration? This is a good question, since not all proponents of a geometrical view profess to hypostatise spacetime (Janssen, for example, explicitly does not do this: see [83]). Since constructive explanations (i.e., explanations in terms of constructive *theories*: see [136]) make appeal to physical entities and goings-on, it seems to me that Janssen occupies an unstable position in both refusing to hypostatise spacetime yet nevertheless imputing that spacetime can offer constructive explanations of physical phenomena: in my view, the former is a necessary condition for the latter. Of course, though, this isn't to say that a non-hypostatised spacetime can't offer *other* kinds of explanations of physical goings on—perhaps unificatory explanations, in the manner of Friedman [59].[8]

What, then, of Maudlin? The following passage is revealing:

> Complete physical understanding of an equilibrium state would require a complete account of the internal structure of the rigid system, both its composition and the forces among its parts. But even absent such a detailed account, we can make some general assertions about rigid bodies in any Special Relativistic theory. The fundamental requirement of a relativistic theory is that the physical laws should be specifiable using only the relativistic space-time geometry. For Special Relativity, this means in particular Minkowski space-time. It is the symmetry of Minkowski spacetime that allows us to prove our general result. [103, p. 117]

Note that the first sentence here is completely consistent with the Lorentzian pedagogy, so Maudlin wholly concurs with Brown and Pooley on this point. When Maudlin then writes that "[t]he fundamental requirement of a relativistic theory is that the physical laws should be specifiable using only the relativistic space-time geometry", this is also something to which the advocate of the dynamical approach should be able to assent (as a mathematical claim, at least). The remaining issues are (a) whether this spacetime structure is ontologically autonomous, and (b) whether it can offer a constructive explanation of the above effects. Advocates of the dynamical approach will assent to neither (a) nor (b), whereas Maudlin, I take it (albeit not in the above quote!), *will* assent to both (a) and (b). Once one recognises Maudlin as a 'qualified geometrician', however,

[7] For Brown on Version B.1, see [18, p. 76].

[8] For further discussions on all these issues, see [1, 136].

there does not seem to be anything profoundly problematic in his position (for further discussion, see [135]).

10.5 Norton's challenge

Having clarified the different forms that a geometrical view might take, I now want to turn to a different issue. Norton claims that the whole idea of a 'dynamical approach' to spacetime in the style of Brown is question-begging:

> Constructivists, such as Harvey Brown, urge that the geometries of Newtonian and special relativistic spacetimes result from the properties of matter. Whatever this may mean, it commits constructivists to the claim that these spacetime geometries can be inferred from the properties of matter without recourse to spatiotemporal presumptions or with few of them. I argue that the construction project only succeeds if constructivists antecedently presume the essential commitments of a realist conception of spacetime. [114, p. 821]

Recall from Chapter 6 that, when constructing spacetime theories (on the Riemannian approach, at least), we begin by writing down a differentiable manifold M, before writing down certain additional (e.g.) metrical structure on that manifold. For example, recall that the spacetime structure of special relativity (on the Riemannian approach) is $\langle M, \eta_{ab} \rangle$; the (Galilean) spacetime structure of Newtonian mechanics is $\langle M, t_{ab}, h^{ab}, \nabla \rangle$. Norton's claim, amongst other things, is that Brown must presuppose the manifold structure M in order to write down dynamical equations for matter fields (for these equations hold at spacetime *points*), and so to get his relationalism about metric structure off the ground. So Brown's approach fails, according to Norton, for it implicitly makes certain spatiotemporal presuppositions.

Is this fair? Let's consider two responses to Norton. The first is issued by Pooley, who accuses Norton of misunderstanding the scope of the dynamical project:[9]

> The advocate of the dynamical approach need not be understood as eschewing all primitive spatiotemporal notions (*pace* Norton, 2008). In particular, one might take as basic the "topological" extendedness of the material world in four dimensions. [127, p. 55]
>
> [T]he project was to reduce chronogeometric facts to symmetries, not to recover the entire spatiotemporal nature of the world from no spatiotemporal assumptions whatsoever. [127, p. 57]

Others have argued that it's unreasonable to say that Brown does not have a relational account of the manifold, as indeed seems to be exhibited in the following passages:

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[9]For more on this, see [154].

> In pre-quantum physics then, space-time points are perhaps best viewed not as entities in their own right, but as correlations or links between the individual degrees of freedom of distinct physical fields. [13, p. 68]
>
> The simplest (and to my mind the best) conclusion, and one which tallies with our usual intuitions concerning the gauge freedom in electrodynamics, is that the space-time manifold is a non-entity. [14, p. 156]

One might, however, regard the above as mere promissory notes: how exactly is Brown to eliminate his apparent commitment to manifold points? Menon [105] takes up this challenge, using the machinery of 'algebraic fields' to *show* that manifold points can be understood as 'structural properties of matter', in line with the above quote from Brown. This work has very recently been developed further in e.g. [22]—but for a more sceptical response, see [93]. One concern expressed by Linnemann and Salimkhani in the latter of these articles is this: how does demonstrating the existence of a mapping between (i) theories in their traditional manifold setting, and (ii) these theories formulated in terms of algebraic fields, actually resolve Norton's challenge? For this, one would surely need to argue that the formulation in (ii) is *metaphysically prior* to the formulation in (i)—but how would any such argument proceed?

These debates are ongoing. But what we can say, in light of the recent writings of *inter alia* Pooley and Menon, is that it's not clear whether Norton's charges against the dynamical approach find their mark.

Chapter 11

Presentism and relativity

> Following up the consequences of the strange state of affairs one is led to conclusions about the nature of time which are very far-reaching indeed. In short, it seems that one obtains an unequivocal proof for the view of those philosophers who, like Parmenides, Kant, and the modern idealists, deny the objectivity of change and consider change as an illusion or an appearance due to our special mode of perception. (Gödel, 1949)

In this chapter, I'll consider the bearing of special relativity upon certain long-standing debates and views in the philosophy of time. In particular, I'll focus on *presentism*: the view that only the present exists. Most presentists take it that reality is three dimensional (below, I'll discuss heterodox presentist views which deny this), but one can see immediately the potential problems for such a view raised by the relativity and conventionality of simultaneity in special relativity: if there is no objective way of identifying which events are simultaneous with a given event, then how can we identify the objectively present events, and so (for the presentist) the existent events?

11.1 The philosophy of time

I'll start by introducing three distinct but related debates in the classical philosophy of time. In the next section, I'll then discuss how positions in these debates interact with special relativity.

11.1.1 Static versus dynamic conceptions of time

The following intuition will be familiar to all: far-future moments become near-future moments, which become present moments, which in turn become near-past and far-past moments, in an endless 'flow of time'. But is temporal passage—becoming—truly an objective feature of reality? Here, answers fall into one of two camps: *dynamic* views hold that temporal passage is fundamental, whereas *static* views deny this. This debate goes back to the pre-Socratics: Heraclitus maintained that temporal passage was a fundamental feature of reality, whereas Paremenedes held a static conception of time. These ancient philosophers continue to find respective counterparts in the modern literature: for example, Maudlin embraces a dynamic conception of time [102], whereas Barbour's 'Machian relationalist' approach to physics adheres to a static view [4].

11.1.2 The A-series and B-series

In his (in)famous 1908 paper, 'The Unreality of Time', J. E. M. McTaggart distinguishes between what he calls the *A-series* and the *B-series* [104]:

A-series: That ordering of events according to whether they are past, present, or future.

B-series: That ordering of events according to whether they are earlier or later.

Those who maintain that the universe contains irreducible A-series ('tensed') facts are known as *A-theorists*. Those who maintain that the universe does not contain irreducible A-series facts, but only B-series facts, are known as *B-theorists*. (One way to think about this is that A-theorists maintain that events in spacetime have the additional, primitive tensed properties of being past/present/future; B-theorists deny the existence of primitive tensed properties.) To connect this up with the previous debate: those who believe in a 'moving present'—i.e., those who believe that temporal passage is fundamental—will be A-theorists.

Question: Does the reverse of this implication hold?

11.1.3 The ontology of events

The third by-now classic debate in the philosophy of time regards the ontology of events in spacetime: which moments *really exist*? There are three main positions taken in response to this question:[1]

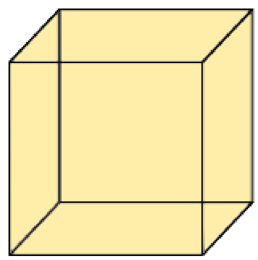
[1] Note, though, that these are not exhaustive: for example, one might embrace a 'shrinking block' view [21]. Since such positions are neither terribly popular nor essential for my purposes, I won't say more about them here.

1. The 'block universe' view.

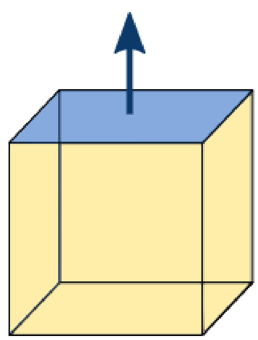
2. The 'growing block' view.

3. Presentism.

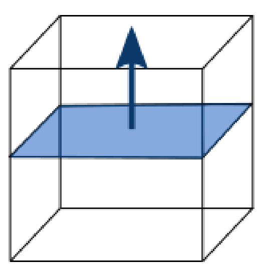
I'll go through each of these in turn.



The *block universe* view (sometimes: *eternalism*) holds that past, present and future events are all equally real. Schematically, then, the view is represented in the image above (take the spatial dimensions to run along the horizontal axis, and the temporal dimension to run along the vertical axis).



The *growing block* view holds that the past and present are real, but the future is not. Reality, then, is four-dimensional, but the four dimensional block grows over time, as represented in the image above.



Finally, according to (the most standard version of—again, see below) *presentism*, reality is three-dimensional; the past and future are unreal, as represented in the image above. In fact, there are several different varieties of presentism (here, I follow the terminology of Dainton's excellent discussion [26, ch. 6]):

1. *Solipsistic presentism*: Nothing exists that is not present, and only one present ever exists—*this* one. (A static view.)

2. *Many-worlds presentism*: Reality as a whole includes many momentary presents that are *not* temporally related to one another, and so do not succeed each other in any way. (A static view.)

3. *Dynamic presentism*: Reality takes the form of a succession of instantaneous (or near-instantaneous) presents; no sooner has one present come into existence than it will depart from it, to be replaced by another.

Question: How does many-worlds presentism differ from the block universe view?

Question: Can Barbour's 'shape dynamics' be classified as a many-worlds presentist view?

11.1.4 Connections between the debates

It's natural to group, e.g., presentism with dynamic conceptions of time with an A-series, and eternalism with static conceptions of time with a B-series. However, one should be wary of thinking that these connections are stronger than they in fact are. They need not necessarily hold—for example, as pointed out above, solipsistic and many-worlds presentism are presentist *static* views; moreover, eternalism is compatible with events having auxiliary A-series properties. (Consider e.g. the 'moving spotlight view', according to which reality is four dimensional, but events in spacetime have primitive tensed properties, and these properties change—specifically with respect to which events are 'illuminated' as the present events.)

Thus, in my view, the most appropriate strategy is to regard the above three debates as being distinct, albeit very closely related. That said, there's a sense in which the A-theory/B-theory debate is more fundamental than the other two, for commitments there (specifically: a commitment to the A-series) is (so it seems) required if one is to endorse certain views in the other two debates (e.g., a dynamic conception of time, and presentism).

11.2 Presentism and relativity

In order to understand better the challenges presented to presentism by relativity, it's helpful to rehearse why the same issues (supposedly) do not arise in the context of Newtonian mechanics. Newtonian mechanics looks to be an hospitable environment for presentism, because:

1. Time in Newtonian mechanics is absolute. (In either Newtonian or Galilean spacetime.)

2. Simultaneity in Newtonian mechanics is also absolute. (In either Newtonian or Galilean spacetime.)

So there exists sufficient spacetime structure to identify the class of spacetime points which might qualify as 'the present'.[2]

By contrast to the (standard line on the) Newtonian case, special relativity does not appear to be an hospitable environment for presentism, because:

1. The relativity of simultaneity tells us that how we 'spread time through space' depends upon the frame from which the physics is described.

2. The conventionality of simultaneity tells that, even within a frame, there's no fact of the matter about the simultaneity of spatially-separated events.

Even if they do not accept the conventionality of simultaneity—see Chapter 7—the presentist will have to contend with (1). In any case, focusing for now on the relativity of simultaneity, Putnam constructed the following formal argument against the possibility of presentism in special relativity [131]:

I. All events that I consider to be simultaneous with me-now are real. (Remember, the presentist thinks only these things are real.)

II. Some of these events involve other observers, so I should believe that these other observers are real. Some of them are in motion relative to me.

III. There are no privileged observers, so if one of the other observers thinks something's real, then I should think it's real too.

IV. Special relativity tells me that the events moving observers consider to be simultaneous will be different from those that I think are simultaneous.

V. Therefore, some events are real that are not simultaneous with me—so presentism is false!

By running this argument repeatedly, the presentist seems forced to concede that *all* events in the four-dimensional block are real—a *reductio* on the view. In light of Putnam's argument, then, is there any way of saving presentism within the framework of relativity theory?

[2] Although by far the mainstream view, it's worth noting that the second of the above two points has been questioned by Brown, as already mentioned (and called into question) in footnote 2 of Chapter 7. Thus, Brown might very likely maintain that presentism faces difficulties not only in special relativity, but also—and for the same reasons—in Newtonian mechanics.

11.3 Presentist fallbacks

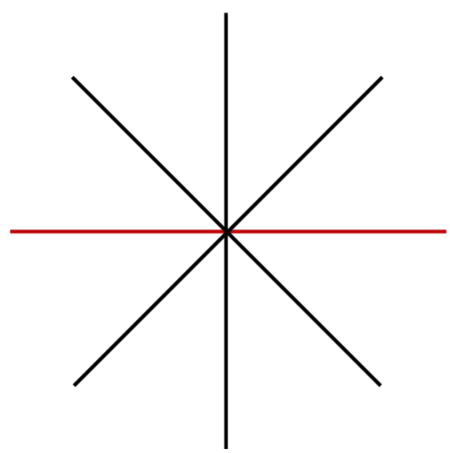
I'll discuss three different presentist fallbacks within the context of special relativity, following the discussion of Hinchliff [73]. These fallbacks are:

1. Introduce a privileged simultaneity slicing.
2. 'Point presentism'.
3. 'Cone presentism'.

Let me go through each of these in turn.

11.3.1 Privileged simultaneity slicing

The first of our three fallbacks involves designating some simultaneity surface as being metaphysically privileged, in the sense of picking out the present—and so existent, for the presentist—events. Clearly, this is tantamount to choosing a preferred frame, for the designated simultaneity surface will be that associated to some observer (idealised as a timelike trajectory), given standard synchrony (see Chapter 7). Sometimes, this view is accordingly dubbed 'neo-Lorentzianism', since the existence of a preferred frame was, of course, one of the commitments of the ether theorists such as Lorentz (see Chapter 3 and Chapter 4). Schematically, the commitments of this view might be represented thus:



(Here, the red horizontal line represents the objective present; the vertical worldline represents the idealised observer associated with this designation of simultaneous events (given standard synchrony), and the diagonal lines represent the lightcone structure of special relativity.) Importantly, note that a privileged simultaneity slicing is not *incompatible* with special relativity—although it does involve adding extra structure which, one might argue, we have an Occamist norm to expunge (see Chapter 5).

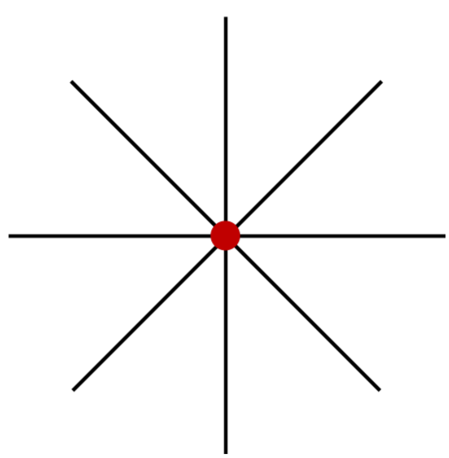
Here is one way to implement this position in terms of Reichenbach's ϵ factors: (See Chapter 7.)

- Suppose simultaneity is *not* conventional, but that the correct ϵ factor changes from frame to frame.
- Then, we can *eliminate* the relativity of simultaneity.
- Moreover, since simultaneity is not conventional, there is a fact about simultaneity in each frame.
- In this way, we can introduce a privileged slicing into special relativity—though, of course, it won't be empirically accessible.

As already indicated above, however, the central concern with this position is that the extra, privileged simultaneity structure is otiose, and a throwback to Lorentz. In addition, there is a worry about observer-dependence: why did a particular observer get lucky, in the sense that only *their* simultaneity slicing picks out the objective present? Without good answers to these questions, the position is in danger of seeming *ad hoc*.

11.3.2 Point presentism

Let's turn next to point presentism. The point presentist says that only the present exists, but the present is not a simultaneity surface, but rather *a single point*. Thus, reality is in fact zero-dimensional, on this view! The major benefit of this position is supposed to lie in the fact that it is the relativity of simultaneity which plays havoc with presentism—but if reality is in fact a *point*, one can cease to worry about the different possible ways of spreading time through space which are consistent with special relativity. Schematically, then, this view might be represented as follows:



As with the previous case, however, point presentism faces a number of problems; here I'll mention three which I take to be the most pressing. The first is that it is lonely or solipsistic—it implies that anything which is spatially separated from the privileged point does not exist! Hincliff is unmoved by this objection, writing that it is "just a restatement of the view" [73, p. 579]. However, if other presentist (or, indeed, eternalist) views do not have this radical consequence, one might well regard that as being *ceteris paribus* a mark in their favour. The second concern is closely related: not only is the view lonely, but it is in fact (the charge might run) *empirically incoherent*, for it denies the existence of the (spatially extended!) measuring devices which are used in order to confirm the very theory itself! (For more on the issue of empirical incoherence, see [5, 78].)

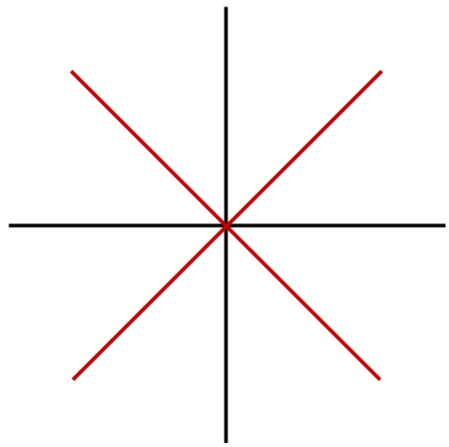
The third concern is that—perhaps contrary to initial appearances—the view does not evade the issue of observer-dependence after all, for there remains the question: upon *whose* worldline does the privileged point lie? One version of point presentism might appeal to the idea of there being multiple, *observer-relative* point presents in order to avoid this concern; the challenge, however, remains to make good metaphysical sense out of such proposals.[3] Perhaps the fragmentalist views discussed in Chapter 8 can help here—but again, the devil is in the details of how exactly these issues are worked out.

Exercise: Develop and defend what you take to be the most plausible point presentist view.

11.3.3 Cone presentism

The final presentist fallback which I'll discuss here is cone presentism. The idea underlying this position is to use the structures that *are* held to be invariant by special relativity—namely, the lightcone structure—and identify the objectively present events with (at least some of) such structures. Specifically, cone presentists identify the surface of a light cone as picking out the objective present, as represented thus:

[3] For more on the idea of 'personal A-series', see [25]. Maudlin also discusses there idea of there being multiple A-series in [102].



On the merits of cone presentism, Hinchliff writes:

> One virtue of [cone presentism] is that it captures the idea that what is present is what I am seeing now. A second virtue is that it identifies the present with an invariant feature of the special theory. A third virtue is that we are not alone. [73, p. 500]

I don't dispute the second and the third of these points. But it's worth flagging that the first of these consequences, although intuitive, is certainly highly revisionary: since high school physics, we're used to the idea that the light reaching us now depicts the state of the universe so-and-so many (millions of) light years ago. On cone presentism, this is false: the time of emission *just is* the time of reception: the astrophysical events which I'm observing are happening *right now*!

I'll very shortly return to an assessment of cone presentism. Before doing so, however, I want to draw attention to the fact that there are two distinct sub-views within cone presentism, depending upon whether one takes it that the present should be identified as[4]

1. the 'past' lightcone ('backward-cone presentism'), or

2. the entire lightcone ('double-cone presentism').

(Technical aside: backward-cone presentism presupposes that the manifold be temporally orientable, such that a future/past lobe of the lightcone can be identified consistently across spacetime.[5]) Which one of these is to be preferred? Here's Savitt on backward-cone presentism:

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[4]Again, these positions aren't exhaustive: one could, in principle, be a 'forward-cone presentist'. To my knowledge, nobody has endorsed such a position—but perhaps it shares some of the virtues of the 'shrinking block' view which I mentioned in footnote 1.

[5]For further recent philosophical discussion regard the orientability of spacetime and its testability, see [11].

> [Backward-cone presentism] seems to rest on the idea that events on the past lightcone of E have a lightlike separation from E and hence the spacetime interval from E to (say) E' (on the past lightcone of E) is 0. But then it seems arbitrary to exclude from the present events on the future lightcone of E, which are also light like separated from E. [149, p. 6]

And here's Hinchliff's response to this charge of arbitrariness:

> The surface of E's past lightcone is the set of events *from* which a light signal or ray could be sent *to* E. The surface of E's future lightcone is the set of events *to* which a light signal or ray could be sent *from* E. The difference between the cones is due to the asymmetry built into the nature of a light *ray* or *signal*. And that asymmetry arises from the asymmetric nature of causation itself, which is a non-arbitrary foundation on which to rest the distinction between cone and double-cone presentism. [73, p. 582]

Question: Is Hinchliff introducing extra structure in the form of a primitive causal relation here? (Cf. Chapter 7.)

Note also that one needn't appeal to causal notions *à la* Hinchliff here—one could appeal to a primitive spacetime orientation (cf. [102]), or to some other means of identifying an asymmetry in time (e.g., an entropic gradient).[6]

As before, there are various worries regarding cone presentism. First, again, there is a worry about with respect to *whose* worldline the present is meant to be defined—in the absence of a response to this, the view still appears to be observer-relative in a way which renders it *ad hoc*. Second, as already indicated to some extent in the foregoing, radiation is currently reaching us from the cosmological decoupling period—does that mean we're simultaneous with the 'early' universe? (Note that this is only a worry for the double-cone presentist—but even here, one anticipates that Hinchliff will respond that this is not an *argument* against the view.)

11.4 Presentism and cosmology

So far, we've seen that while there are attempts to save presentism from concerns regarding the relativity/conventionality of simultaneity in special relativity, none of these responses are problem-free. Does anything change when we move from special relativity to the *general* theory? (Again, for background on general relativity, see Chapter 12.) Some authors, e.g. Swinburne [155], have suggested that the prospects for presentism in fact *improve* when one moves from special relativity to general relativity. Here's the reasoning. Minkowski

[6] For an assessment of these different possible approaches to grounding an arrow of time, see [164].

spacetime is just one solution of the Einstein equations of general relativity (the flat spacetime solution). Another solution is the Friedman-Lemaître-Robertson-Walker (FLRW) cosmological ('big bang') solution—and it's *this* spacetime which cosmologists use to model our universe. Unlike Minkowski spacetime, there is a preferred choice of temporal coordinate (i.e., foliation; i.e., frame) in which (the coordinate description of) FLRW spacetime simplifies. Thus (the thought goes), there *is* a preferred frame, once one moves to general relativity. This gives a notion of *cosmic simultaneity*.[7]

Be the above as it may, one can identify are a number of concerns about this strategy (see [139] for further discussion). Two notable such concerns are:

1. FLRW spacetime is an idealisation: it assumes perfect homogeneity and isotropy. The actual universe would be better represented by a 'perturbed FLRW' spacetime. Can cosmic simultaneity be defined in such spacetimes? (As far as I know, this is as-yet an open question—albeit one which should be tractible to the willing.)

2. There are other solutions of general relativity in which the spacetime cannot be foliated into hypersurfaces *at all*—e.g., Gödel's famous time travel solution. There are no good prospects for presentism there.[8] But since metaphysics cannot be contingent (or so the argument now countenanced here goes), there are no prospects for presentism in the actual world, either. (This is Gödel's 'modal argument'—see [66].)

These debates are ongoing. What we can say, though, is that there are *prima facie* serious problems for presentism in *special* relativity, which the proponent of the view must address; the extent to which these issues carry over to general relativity remains to be settled.

11.5 The growing block and relativity

In this final section of the chapter, I want to move away from presentism, and consider the growing block theory: does this fare any better in the context of relativistic physics? The first thing to say is that the growing block view is necessarily (definitionally!) a dynamic A-theoretic view: events have primitive tensed properties, which change, and as they change, more events get added to the total stock of existent events. Given its status as a dynamic theory of time, the growing block theory faces problems associated with any such theory, such as: "How fast is the block growing? With respect to what is the block growing—some additional dimension?", etc. [36, p. 138]

In addition to these problems, insofar as the growing block theory presupposes an objective past/present/future distinction, it *also* inherits all of the

[7] For recent elegant discussions of such arguments in the context of presentism and the A-theory, see [91].

[8] *Nota bene*: such a claim ignores point presentism. (But why should the point presentist concern themselves with different solutions when in every case they take the reality represented by such solutions to be a single unstructured point?)

problems for presentism which we have already discussed. While the growing block seems to have better prospects in Newtonian theories than in special relativity, due to the absolute simultaneity structure,[9] one again faces the following challenge in special relativity: how to define the hypersurfaces into which the block is growing?

Thus, the quick verdict on the growing block theory is that its prospects are *at least* as bad as those for presentism in the relativistic context. That said, it might be that the growing block views finds a more natural home in certain approaches to *quantum gravity*. Here's Sorkin on one such approach, known as *causal set theory*:

> Think of the causal set as an idealized growing tree (in the botanical sense, not the combinatorial one). Such a tree grows at the tips of its many branches, and these sites of growth are independent of one another. Perhaps a cluster of two leaves springs up at the tip of one branch (event A) and at the same moment a single leaf unfolds itself at the tip of a second branch (event B). To a good approximation, the words "at the same moment" make sense for real trees, but we know that they are not strictly accurate, because events A and B occur at different locations and distant simultaneity lacks objective meaning. If the tree were broad enough and the growth fast enough, we really could not say whether event A preceded or followed event B. [152, p. 4]

To go into causal set theory and the philosophy of time any further here would take me too far afield—but see [173] for further discussion.

[9] Although note again Brown's views on simultaneity in Newtonian physics—see footnote 2 in Chapter 7.

Chapter 12

Acceleration and gravitational redshift

> Gravity and inertia are the same in their very essence. (Einstein, 1918)

One sometimes hears that it is not possible to model accelerating systems in special relativity, and that to do so one must move to the framework of general relativity.[1] As already mentioned towards the end of Chapter 9, any such claim is badly confused. For consider e.g. Bell's accelerating rockets—what was incoherent in this setup? (Answer: nothing!) Or: what is wrong with Rindler frames in special relativity? (Answer: again, nothing!) Indeed, such a claim should be especially perplexing, given that we've already seen (in Chapters 5 and 6) that special relativity *retains* a standard of absolute acceleration!

My main purpose in this chapter is to present and resolve one significant confusion involving acceleration in relativity theory—*viz.*, that regarding *gravitational redshift*. The claim to be tacked has it that to account for the results of gravitational redshift experiments mandates recourse to general relativity—but, again, my response is going to be that consideration of accelerations in special relativity (plus one other input, to be discussed below) suffices. Before getting into this, though, I must say something on what's known as Einstein's *equivalence principle*.

12.1 The Einstein equivalence principle

In 1907, when thinking about someone unfortunate enough to be falling off a roof, Einstein had "the happiest thought of my life" ("*glücklichste Gedanke*

[1]See [63] for discussion.

meines Lebens") [51]. He realised that in the immediate vicinity of such an observer, gravity would seem to disappear. Of course, this would have been known since Newton, but Einstein's revolutionary insight was that the gravitation field itself "has only a relative existence" [51]. Here's what he wrote:

> [F]or an observer falling freely from the roof of a house there exists—at least in his immediate surroundings—no gravitational field ... The observer therefore has the right to interpret his state as "at rest". [117, p. 178]

The point is that, for Einstein after 1907, gravitational effects and inertial effects (i.e., perceived accelerative effects which arise in a non-inertial frame, as seen in Chapters 1 and 6) are not just empirically equivalent, but *conceptually identical*. As Einstein put it in 1918 in the epigraph to this chapter, gravity and inertial are *in essence* the same thing. And as was nicely summarised by Pauli:

> In Einstein's theory, gravitation is just as much a *fictitious* force as the coriolis and centrifugal forces are in Newton's theory. (However, it is equally justified to say that in Einstein's theory neither of these two forces is a fictitious force.) [118, p. 709]

So, given Einstein's equivalence principle, gravitational effects *just are* inertial effects. If one is in a freely-falling frame in which one doesn't feel any gravitational effects, that's because *there literally are no gravitational effects*.

Why was it so important to Einstein that he make this move? As Lehmkuhl writes in his excellent recent survey article on on the equivalence principle,

> Einstein himself stressed again and again the heuristic importance of the [Einstein equivalence principle] in his search for what came to be [general relativity]. This role of the principle is intimately connected to Einstein thinking of it as a relativity principle. He clearly saw it as extending the special principle of relativity, that states that all inertial motions, including rest, are empirically indistinguishable and thus equivalent in an important sense. [90, p. 7]
>
> For Einstein, seeing the presence of gravitational fields as a coordinate-dependent state of affairs was not a price to be paid but a major achievement of the theory. [90, pp. 13-14]

For Einstein, the idea was that by writing one's physics in arbitrary coordinate systems, and subsequently identifying what were previously regarded as inertial effect terms in one's equations as terms pertaining to the gravitational field, one has thereby constructed a theory which is generally covariant—that is, a theory which (recall from Chapter 6) holds in all coordinate systems. One has thus liberated physics from its dependence upon the inertial system. It was this which Einstein regarded as being a major conceptual leap forward in his quest for the general theory of relativity.

12.2 Inertial frames, reprise

Suppose, then, that one embraces the Einstein equivalence principle (henceforth **EEP**). What are the consequences for one's understanding of the nature of inertial frames? Recall Knox's functional definition of inertial frames (presented explicitly in Chapter 1): they are those coordinate systems in which the laws of physics take their simplest form, and in which force-free bodies move with uniform velocities. Given **EEP**, the frames which qualify as inertial are *not* (as we might previously have thought) the frames stapled to the surface of the Earth (say, the rest frame of my office), for in such frames there are gravitational effects (if I drop my cup, it is accelerated to the floor by gravity). Rather, the inertial frames are the *freely falling frames*, in which (on the assumption of **EEP**!) it is not simply that inertial effects cancel the effects of the gravitational field (as on the Newtonian, pre-**EEP** account[2]), but that there *just is* no gravitational field in such frames. Thus, given **EEP**, it is the freely falling frames which satisfy Knox's functional definition of the inertial frames—it is *these* frames which qualify as inertial in any theory which embraces the **EEP**, including general relativity.

This point will be crucial to securing a proper understanding of the reasons underlying the results of gravitational redshift experiments. Before I explain this, however, there is one further piece of conceptual apparatus to introduce: what's known as the *strong* equivalence principle.

12.3 The strong equivalence principle

Unlike **EEP**, the strong equivalence principle (henceforth **SEP**) is not (at least in the way in which I'll understand it in this chapter) an heuristic tool used in the construction of general relativity. Rather, it's a principle which holds in the *completed* theory of general relativity.[3] To explain **SEP**, then, I first need to say a little more about the structure of general relativity.

Let's begin with models of the theory.[4] In general relativity, spacetime is represented not by $\langle M, \eta_{ab} \rangle$ (as in special relativity), but rather by $\langle M, g_{ab} \rangle$. Like the Minkowski metric η_{ab}, the metric field g_{ab} of general relativity encodes spatiotemporal distances and (via its compatible derivative operator) a notion

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[2]See [140] for a discussion of Newtonian equivalence principles, which uses the same terminology as this chapter.

[3]It's worth flagging that the status of the equivalence principles in the completed theory of general relativity—especially that of **SEP**—is controversial. For example, Synge declared infamously in 1960 that

> The Principle of Equivalence performed the essential office of midwife at the birth of general relativity, but ... I suggest that the midwife be now buried with appropriate honours ... [156, pp. ix-x]

I don't accept this understanding of the equivalence principle in general relativity, and will proceed accordingly.

[4]I'll present things in the language of the Riemannian approach. It's a bit harder to present general relativity in the language of the Kleinian approach, as solutions of the theory in general have no global symmetries. For relevant discussion, see [165].

of straightness of paths through spacetime. The central differences between η_{ab} and g_{ab} are these. First, although one can find a coordinate system such that *globally* η_{ab} takes the form $\text{diag}(-1, 1, 1, 1)$, one can only do this *locally*—i.e., in the neighbourhood of each point $p \in M$—in the case of g_{ab}. Second, g_{ab} is *dynamical*, obeying the *Einstein equation*,[5]

$$G_{ab}(g_{ab}) = 8\pi T_{ab}(g_{ab}, \Phi). \tag{12.1}$$

This says that the spacetime curvature associated with g_{ab} is proportional to the amount of matter (energy-momentum) content T_{ab} in the relevant region.

Although visually the above idea might be clear enough, it's worth giving the technical definition of curvature (for further details, see e.g. [100, ch. 1]). Take two vectors, initially at the same point $p \in M$ and pointing in the same direction. Transport these vectors along two different paths to some other point $q \in M$.[6] If the vectors cease to point in the same direction, then *by definition* the spacetime is curved. The point is nicely illustrated on the surface of the Earth: consider two vectors initially at the equator; transport one around the equator by some amount, then transport both vectors to the North pole. Generically, the vectors will no longer point in the same direction—this, mathematically, is why the surface of the Earth is curved.[7]

Mathematically, the extent to which vectors cease to point in the same direction when transported as above is quantified by an object known as the Riemann tensor, written ${R^a}_{bcd}$. The Einstein tensor G_{ab}, which appears on the left hand side of (12.1), is related to the Riemann tensor by

$$G_{ab} := {R^c}_{acb} - \frac{1}{2} g_{ab} g^{de} {R^c}_{dce}. \tag{12.2}$$

We needn't get into the weeds here any further—for my purposes, it suffices to note that G_{ab} also expresses facts about the curvature of spacetime.

With all of this preamble in mind, let me turn now to **SEP**. The idea underlying this principle is that, locally (i.e., in sufficiently small neighbourhoods of any given $p \in M$), the curved spacetime of general relativity (and its associated physics) should approximate the flat spacetime of special relativity (and its associated physics). Here's how Einstein put the idea:

> [L]et us now introduce the following premise: For infinitely small four-dimensional regions the theory of relativity in the restricted sense [i.e., special relativity] holds, if the coordinates are suitably chosen. [46, p. 777]

[5]Here's a fussy point: if one is using abstract notation, as in (12.1), one has a *single* equation, hence 'Einstein equation'. But in coordinate indices, one has a set of partial differential equations, hence 'Einstein equations'.

[6]Specifically, *parallel* transport the vectors. I won't go into the definition here; see e.g. [163] for the details.

[7]There are differences between 'intrinsic' and 'extrinsic' curvature, but I won't go into them here—see e.g. [67].

Intuitions underlying **SEP** can be pumped by the following obvious analogy: the surface of the Earth, in spite of being curved (as discussed above), is effectively flat in sufficiently small regions. Making **SEP** precise, however, is a delicate and ongoing business—see *inter alia* [14, 62, 85, 138, 168] for further discussion.

Although in one sense the conceptual status of **EEP** versus **SEP** is very different—as we've seen, the former is an heuristic device used in the construction of a geometrised theory of gravity such as general relativity (cf. [140]), while the latter is a principle taken to obtain within such a theory, once completed—it's helpful, following Lehmkuhl, to see both principles as 'bridges' with other physical theories:

> [O]ne might look at the EEP as a bridge principle, a principle forming a bridge from GR to Newtonian theory, a bridge that allows us to see the shadows of Newtonian theory in GR. But this bridge is not just about accommodating our "physical habits of thinking" in allowing us to keep operating with the terms 'gravity' and 'inertia', it also implies that a curvature-free spacetime is just as 'gravitational' as a strongly curved spacetime. [90, p. 25]
>
> While the Einstein equivalence principle can be seen as a bridge from GR to Newtonian theory, the strong equivalence principle can be seen as a bridge from GR to SR. [90, p. 25]

12.4 Gravitational redshift

Finally, with all of this background in hand, we can turn our attention to experiments designed to detect gravitational redshift, such as the famous Pound-Rebka experiment of 1959 (discussed in detail below) [130]: do the results of these experiments provide—as is often claimed—direct evidence for general relativity, and in particular for the curved spacetime structure of that theory? (Such claims are common in the literature—see e.g. [20, 70].) I'll cast doubt on this claim: special relativity in accelerating frames, together with the equivalence principle(s), should suffice.[8]

Before going further, we should be clear about the nature of gravitational redshift. The phenomenon amounts to this: clocks situated deeper in a gravitational well tick more slowly than those further outside of the well. Equivalently: the wavelength of a photon is longer when observed from further out of a gravitational well. Here, the 'clock' is the frequency of the photon and a lower frequency is the same as a longer ('redder') wavelength.

Gravitational redshift has been *experimentally confirmed*—most famously by Pound and Rebka in the above-mentioned experiment. Such experiments made use of the 'Mössbauer effect': γ-rays in a certain narrow frequency range are emitted and absorbed by two solid samples containing radioactive Fe^{57}. When two such samples are placed vertically with a height difference h, the photons emitted from one sample will no longer be absorbed by the other. But if the

[8]This argument is presented in greater depth in [17].

absorber is put into a certain degree of vertical motion relative to the source, the resulting Doppler effect can restore absorption.

Contrary to the orthodoxy, here's how an explanation of the results of such experiments based upon only special relativity and the equivalence principle(s) would work. First, **SEP** states that the local neighbourhood of a gravitational redshift experiment should look approximately special relativistic (i.e., like Minkowski spacetime).[9] Second, by **EEP**, the experimental setup on the surface of the Earth is in an accelerating frame of reference (which, recall, is a perfectly legitimate notion in special relativity). So considering this setup in an accelerating frame in special relativity should allow us to derive the correct results—and indeed we do! Quantitatively, we find

$$\Delta t_B \simeq \left(1 - \frac{gh}{c^2}\right) \Delta t_A, \tag{12.3}$$

where Δt_A is the coordinate interval between successive electromagnetic wave crests being emitted by A, and Δt_B is similarly defined for reception at the lower sample B. This is in agreement with the experimental results![10]

I suggest, then, that the moral is this. The results of a single gravitational redshift experiment of Pound-Rebka type do not provide direct evidence for spacetime curvature, for spacetime curvature is not required to explain these results. That said, there is a role for curvature in the results of redshift experiments, albeit of a more subtle kind. We can explain the results of a gravitational redshift experiment using the fact that the inertial frames are the freely falling frames, as captured by **EEP**. But now consider *multiple* such experiments, at different points on the Earth's surface. Doesn't this mean have certain inertial frames moving *non-inertially* with respect to one another? The solution (familiar from the completed theory of general relativity) is to recognise that the inertial frames are to be re-conceptualised as being *local*, not *global*. Ultimately, this motivates the introduction of a curved 'affine connection'—this is the true place for spacetime curvature in discussions of gravitational redshift.[11]

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[9] Note that **SEP** is required only if one is working within the framework of the completed theory of general relativity—otherwise it too is redundant, and merely special relativity alongside **EEP** will suffice.

[10] Note also that these calculations make certain assumptions—e.g., the clock hypothesis (see Chapter 9).

[11] To present all the mathematical details here would take me too far afield, but see [17] for further discussion on this point.

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