CL 120 Intermediate philosophy of physics: Philosophy of special relativity
Dr Hilary Greaves, Hilary Term 2011

Wednesday week 1: Definitions and empirical significance in Newtonian mechanics

1 Newton's laws of motion

Newton's laws of motion, as every high school physics student knows, are something like the following:

Newton's first law (N1L). Force-free particles travel with uniform velocity.

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

Newton's third law (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.

But these laws are supposed to hold only relative to an inertial frame. (Cf. Newtonian mechanics on a playground carousel.) What is an inertial frame? Come to that, what is the mass of a body, and what is the force exerted by one body on another? If we don't have definitions of these notions, we don't seem (?) to know what we're talking about when we assert Newton's laws of motion.

2 Defining 'inertial frame'

1. We could try to make Newton's *First* Law 'true by definition' by stipulating that an inertial frame *just is* a frame with respect to which all force-free particles travel with uniform velocity. But there are a few problems with this:
    (a) Q: If N1L is true by definition, does it still have any empirical content?
        i. A: no, but it may be an empirically nontrivial claim that *there are* any inertial frames as thus defined (provided the coordinate system is required to be continuous, and provided we are empirically given facts about the instantaneous spatial distance between two bodies that the coordinate system is required to recover as coordinate distance).
        ii. Specifically: the claim becomes nontrivial as soon as we have two or more particles.
    (b) The definition only has a determinate meaning insofar as 'force-free particle' has been given a determinate meaning.

   i. But using the First Law to define ‘inertial frame’ precludes using it to define ‘force-free body’.
   ii. This would be a bad idea anyway …
      A. Eddington’s sarcastic comment: ‘Every body continues in its state of rest or uniform motion in a straight line, except insofar as it doesn’t.’
   iii. One move is to appeal to the fact that *all known forces fall off with spatial distance*, and say that a body is *approximately* force-free if it is ‘sufficiently far away’ from all other bodies.
   iv. But this is unsatisfactory: such approximateness is not tolerable in the *foundations* of a theory (as opposed to: in its practical applications).

(c) A worse problem is that *however* one defines ‘force-free particle’, if the definition succeeds in coinciding extensionally with existing usage, *there are no force-free particles.*
   i. The consequence of this is that on the suggested definition of ‘inertial frame’, *every* frame would vacuously count as inertial.

(d) It is tempting at this point to revise the definition so that it explicitly invokes counterfactuals: ‘an inertial frame is a frame such that, *if there were* any force-free bodies, they would travel with uniform velocity relative to the frame in question’.
   i. But this revision violates the (very plausible?) requirement that the truth of counterfactuals should be *grounded* in truths about what the world is *actually* like – counterfactuals should not be ‘bare truths’. (Consider, for example, what might *make true* the counterfactual conditional ‘If you ask her whether or not she stole the sweets, she will lie.’)

2. In view of these problems, a better approach is to recognise that *Newton’s first law is actually a special case of Newton’s second law*, and to see whether the strategy of using Newton’s *second* law to define ‘inertial frame’ is any better-fated than the above.

   (a) If we do this, we no longer have the above problem: there certainly are particles being acted on by forces.
   (b) But we still have to supply the complementary definitions of ‘force’ and ‘mass’, in order for the whole to have empirical content.
      i. Plausibly, force may be definable via the particular force laws (Newton’s law of gravitational repulsion, Coulomb’s law of electrostatic repulsion, etc.) … [Exercise: Is it, or does this too lead to problems?]

3 Defining mass

(More detail in: Ch. 1 of Jammer, *Concepts of mass in contemporary physics and philosophy*, Princeton University Press (2000))

1. Newton wrote that mass is 'quantity of matter'. But this is just metaphor.

2. Weyl's approach: Use the principle of conservation of momentum to define mass

   (a) Suppose that two particles A and B with velocities u_A, u_B respectively collide inelastically and coalesce to form a compound particle of zero velocity. Then define their 'mass-ratio' $m_{A/B}$ as: $m_{A/B} := \frac{|u_B|}{|u_A|}$.

   (b) Empirically, we find that thus-defined mass-ratios obey the following transitivity condition: for all particles A, B, C, we have $m_{A/B}m_{B/C}m_{C/A} = 1$.

   (c) As a consequence of the preceding, *it is possible to assign* numbers to particles in such a way that the mass-ratio between any given pair of particles is just the ratio of the corresponding pair of numbers.

   (d) Relative to an arbitrary choice of one particular particle to have 'unit mass', this system of mass ratios *uniquely* fixes the assignment of masses to particles.

3. Objections to Weyl's approach: This does provide a definition of mass that is independent of Newton's second law, but:

   (a) It is in-principle-impossible to *completely* screen off the influences of all the particles except the intended partner. So these approaches requires us to allow some approximation. But again: approximation is not allowed in fundamental definition.

   (b) Either the 'definition' is to be construed as requiring that the pairwise interactions in question *have* been carried out, or it is talking about what *would* happen *if they were* carried out. But in the first case not many particles have masses (we haven't carried out such a procedure for many, if any), and in the second case both approaches are based on apparently ungrounded counterfactuals (cf. the complaints above about such counterfactuals).

   (c) It doesn't enable us to define the masses of very large or very small objects, with which we could not carry out such procedures. (Carnap)

4 An holistic approach: Simultaneous implicit definition

Lewis, 'How to define theoretical terms', in his *Philosophical papers: Volume I*, Oxford University Press (1983); and references therein.

1. In light of messes such as the above, many theorists conclude that we have been asking for 'definitions' of theoretical terms in too narrow a sense of 'definition'. An alternative account is that of *simultaneous implicit (or functional) definition*.

2. Step 1: Implicit vs explicit definition

    (a) An *explicit definition* of a term X gives necessary and sufficient conditions for the term X to be applied. Such a definition has a logical form similar to $X := \ldots$; this sentence is held to be true as a matter of definition of X.

    (b) Example: a bachelor is an unmarried man. This is held true as a matter of definition of 'bachelor'; the term 'bachelor' is applicable in a given context iff the term 'unmarried man' is applicable in that same context.

    (c) An *implicit definition* of X does not directly state the extension and intension of the term. Instead, it is a statement (or collection of statements) containing X but having some logical form *other* than 'X is . . . ', which is/are asserted to be true as a matter of the definition of X.

        i. Example: A person has a *legal right* to X iff the system of law in her society requires others to provide her with X. This (or something like it) might be an implicit definition of 'legal right'.

3. Step 2: *Simultaneous* definition

    (a) A suggestion: Can we take e.g. 'inertial frame' to be merely *implicitly* defined by Newtonian theory?

    (b) A perhaps-natural thought: If Newtonian mechanics is an implicit definition of (say) 'inertial frame', surely we require some *other, independent* definition of 'mass', 'force' etc?

    (c) Wrong! Newtonian mechanics could be a *simultaneous* implicit definition of *all* its 'theoretical terms'. (Inertial frames, masses, forces etc are those entities/quantities, if any such there be, that *collectively* make *all* of Newton's laws true.)

    (d) Other examples:

        i. 'Point', 'line' etc as implicitly defined by the axioms of geometry? (Hilbert)

        ii. 'Number', 'Zero', 'successor' as implicitly defined by the Peano axioms? (*Contra* Frege and Russell.)

4. Empirical content again

    (a) On this proposal, Newtonian mechanics has empirical content iff the claim that there *exists* any assignment of coordinates, mass-numbers etc to particles that makes the whole of Newtonian mechanics true itself has empirical content.

   (b) Question: *Does* Newtonian mechanics have empirical content, then, on this proposal?
   (c) Tentative answer: Newton's laws of motion *on their own* do not, but Newton's laws of motion *together with a complete set of force laws* plausibly do. (Exercise: check this!)

5. General philosophy of science implications of this account
   (a) If Newton's laws of motion have empirical content only in conjunction with particular force laws, the laws of motion are not *on their own* falsifiable. So (since Newtonian mechanics clearly counts as good science!) a naive falsifiability criterion of demarcation of science is in danger of branding paradigm cases of science as pseudo-science.
   (b) It is not in general an objection to a given theory that individual and/or explicit definitions of key theoretical terms have not been provided. (Cf. Sen on justice.)
   (c) The meaning of a theoretical term is determined *holistically*, by the *entire theory* in which the term is embedded. This will have implications when we come to discuss theory change, e.g. the nature of the disagreement between pre- and post-relativistic physics: if it is impossible in principle for the two theories to mean the same thing as one another by e.g. 'mass' or 'force', they cannot be making contradictory claims about the behaviour of one and the same quantity, and the sense (if any) in which the two theories contradict one another must be relatively subtle.

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Friday week 1: Galilean relativity and Galilean covariance

1 Galilean relativity

In his classic treatise 'Dialogue concerning the two chief world systems', Galileo pointed out that if one is confined to the inside of a ship's cabin, one *cannot tell the difference (by means of experiments confined to the ship's cabin) between the case in which the ship is stationary and the case in which the ship is moving with constant speed in a straight line:*

> 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 throw it no more strongly in one direction than another, the distances being equal; jumping with your feet together, you pass equal spaces in every direction. When you have observed all these things carefully (though doubtless when the ship is standing still everything must happen in this way), 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.

This phenomenon is called 'Galilean relativity'.

2 Galilean covariance

The empirical phenomenon of Galilean relativity is explained if the laws of motion governing the processes involved in Galileo's experiments are *Galilean covariant.*

1. **The Galilean group.** 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:

Spatial translation	$g_{\mathbf{a}}(\mathbf{a} \in \mathbb{R}^3)$:	$g_{\mathbf{a}}(t, \mathbf{x}) = (t, \mathbf{x} + \mathbf{a})$.
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	$g_{\mathbf{v}}(\mathbf{v} \in \mathbb{R}^3)$:	$g_{\mathbf{v}}(t, \mathbf{x}) = (t, \mathbf{x} - \mathbf{v}t)$.

2. **Covariance.** There are two (equivalent) ways of defining what it means for a given set of laws to be 'covariant' under a given group of transformations:

   (a) Space-of-solutions version:

   i. Toy example of covariance

   A. Equation of motion
   $$\frac{dr}{dt} = -kr. \tag{1}$$

   B. General solution
   $$r(t) = Ae^{-kt}, A \in \mathbb{R}. \tag{2}$$

   C. For any such r and any time translation g_b, we can form the transformed structure $g_b r$:
   $$\begin{aligned} (g_b r)(t) &= r(t - b) && (3)\\ &= ae^{-k(t-b)} && (4)\\ &= (ae^{+kb})e^{-kt}. && (5) \end{aligned}$$

   This is another solution of the same equation (1).

   D. Because *time translations always take solutions of (1) to solutions*, we say that the equation (1) is *time translation covariant.*

   ii. Summary of the general method

   A. Identify the set Θ of equations to be investigated.

   B. 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.

   C. Identify the group G of transformations whose effects on Θ we will be interested in investigating.

   D. For general $g \in G$, identify the action of g on S.

   E. Ask whether this action of G preserves the subset $D \subset S$ of solutions to Θ.

   iii. Toy example of non-covariance

   A. Let the equation of motion (and hence Θ and S) be as before.

   B. Let G be the group B_1 of (one-dimensional) *boosts* $g_v : x \mapsto x - vt$.

   C. Action of any such g_v on S:
   $$(g_v r)(t) = r(t) - vt. \tag{6}$$

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

$$(g_v r)(t) = Ae^{-kt} - vt, \tag{7}$$

which is *not* identical to Be^{-kt} for any $B \in \mathbb{R}$, i.e. is not a solution of (1).

E. So this toy theory is *not* covariant under the boosts (6). (This conclusion should be intuitive . . .)

iv. Galilean covariance of Newtonian gravitation

A. Newtonian gravity for two particles is given (combining N2L and the law of gravitation) by

$$\ddot{\mathbf{r}}_i = \frac{Gm_1m_2}{|\mathbf{r}_1 - \mathbf{r}_2|^3}\left(\mathbf{r}_i - \mathbf{r}_{i+1}\right), i = 1, 2. \tag{8}$$

B. A *structure* for this pair of equations is a pair of functions $(r_1 : \mathbb{R} \to \mathbb{R}^3, r_2 : \mathbb{R} \to \mathbb{R}^3)$.

C. Let G be the group B_3 of *three*-dimensional boosts $\{(g_{\mathbf{v}} : \mathbf{r} \mapsto \mathbf{r} - \mathbf{v}t) : \mathbf{v} \in \mathbb{R}^3\}$.

D. A general such boost g_v acts on a structure $(\mathbf{r}_1, \mathbf{r}_2)$ by

$$g_v\mathbf{r}_i(t) = \mathbf{r}_i(t) - \mathbf{v}t. \tag{9}$$

E. Now, suppose the original pair $(\mathbf{r}_1, \mathbf{r}_2)$ satisfies the equations (8) (Θ). Clearly $(\mathbf{r}_1, \mathbf{r}_2)$ and $(g_v\mathbf{r}_1, g_v\mathbf{r}_2)$ agree with one another on the accelerations of both particles at all times; since they also agree on the displacement of particle 2's position from that of particle 1 at all times, they give the same values as one another for both the LHS and the RHS of (8) at all times, and so if the untransformed structure satisfies that equation then the transformed structure must satisfy it also.

F. So Newtonian gravity [for two particles] is *Galilean (boost) covariant.* (Exercise (easy): rewrite the argument for arbitrary N.)

(b) Form-invariance of equations version

Here we consider the equations themselves directly, rather than their solutions.

i. Toy example of covariance (method 2)

A. Consider again

$$\frac{d}{dt}r(t) = -kr(t). \tag{10}$$

B. This equation is built from a number of entities, including $\frac{d}{dt}$, r and k.

C. Under a time translation g_b,

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

D. The transformed *equation* is therefore

$$\frac{d}{dt}r(t-b) = -kr(t-b). \tag{11}$$

E. But asserting that (11) holds for all t is equivalent to asserting that (10) holds for all t. We conclude that the original (untransformed) equation (10) is time translation covariant.

ii. Method for form-invariance of equations version

A. Identify Θ.

B. Identify G.

C. Identify an action of G on each of the ingredients of each equation in Θ.

D. Write down the equations with transformed ('primed') quantities in place of untransformed ones.

E. Iff the result is a set of equations equivalent to the original Θ, then Θ is G-covariant.

iii. Toy example of non-covariance (method 2)

A. Consider again (1) in conjunction with ...

B. ... the group B_1 of one-dimensional boosts g_v.

C. The ingredients of our equation transform as

$$g_v : \frac{d}{dt} \mapsto \frac{d}{dt}; \tag{12}$$

$$g_v : k \mapsto k; \tag{13}$$

$$g_v : r(t) \mapsto r(t) - vt. \tag{14}$$

D. So the transformed equation is

$$\frac{d}{dt}(r(t) - vt) = -k(r(t) - vt). \tag{15}$$

E. But this is equivalent to (10) only if $-v = vkt$, which clearly cannot hold for all t. This *non*equivalence of the untransformed and transformed equations means that the original equation (10) is not boost invariant.

iv. Galilean covariance of Newtonian gravitation (method 2)

A. Our equation is

$$\ddot{\mathbf{r}}_i = \frac{Gm_1m_2}{|\mathbf{r}_1 - \mathbf{r}_2|^3}\left(\mathbf{r}_i - \mathbf{r}_{i+1}\right), i = 1, 2. \tag{16}$$

B. Our group is the group B_3 of three-dimensional boosts.

C. Five quantities appear in this equation: the particle positions $\mathbf{r}_1$ and $\mathbf{r}_2$, the masses m_1 and m_2, and the acceleration $\mathbf{r}_i$ of the ith particle. These transform as

$$\mathbf{r}'_i(t) \equiv (g_{\mathbf{v}\mathbf{r}_i})(t) = \mathbf{r}_i(t) - \mathbf{v}t. \quad (17)$$

$$\ddot{\mathbf{r}}'_i(t) \equiv (g_{\mathbf{v}}\ddot{\mathbf{r}}_i)(t) = \ddot{\mathbf{r}}_i(t). \quad (18)$$

$$m'_i \equiv g_{\mathbf{v}} m_i = m_i. \quad (19)$$

D. The transformed equation is

$$\ddot{\mathbf{r}}'_i = \frac{Gm'_1 m'_2}{|\mathbf{r}'_1 - \mathbf{r}'_2|^3}\left(\mathbf{r}'_i - \mathbf{r}'_{i+1}\right); \quad (20)$$

i.e., eliminating the primes,

$$\frac{d^2}{dt^2}(\mathbf{r}_i - \mathbf{v}t) = \frac{Gm_1m_2}{|(\mathbf{r}_1 - \mathbf{v}t) - (\mathbf{r}_2 - \mathbf{v}t)|^3}\left((\mathbf{r}_i - \mathbf{v}t) - (\mathbf{r}_{i+1} - \mathbf{v}t)\right).$$

E. But clearly this is equivalent to our original equation. So the latter is Galilean boost covariant.

Note that in this last example, the argument for covariance was somewhat cleaner using method 2 than using method 1. But the two methods are (extensionally) equivalent.

3 Historical aside: Newton on Galilean relativity

1. Newton claims to infer this phenomenon 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 forwards in a right line without any circular motion

   — i.e., that Galilean relativity holds.

2. Newton's *argument* for 'Corollary V' is as follows:

   > For the differences of the motions tending towards the same parts, and the sums of those that tend towards contrary parts, are, at first (by supposition), in both cases the same ; and it is from those sums and differences [i.e. those vectorial 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 the 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.

In calling it a 'corollary', Newton is claiming that Galilean relativity follows from his three laws of motion *alone*. But this is not the case; there are two *non sequitur*s in Newton's argument.

3. 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 one upon 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 $\mathbf{F} = -k\mathbf{v}$.)

4. 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.)

5. *With* these two auxiliary assumptions in place, Galilean relativity can indeed be derived from Newton's Second Law (by essentially Newton's argument).

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Wednesday week 2: The 'ether wind' search and Lorentz's program

Lecture handouts are posted (sometime) on Weblearn: See www.philosophy.ox.ac.uk → Lectures → Lecture Materials → (Undergraduate) Lecture Resources (HT11) → CL 120 Intermediate Philosophy of Physics.

For more detail on this lecture's material, see: Janssen and Stachel, 'The optics and electrodynamics of moving bodies', to appear in Stachel, *Going Critical*, Dordrecht: Springer. Available from Weblearn.

1 Ether theory up to 1880

1.1 The expectation of an ether wind

1. Around the beginning of the 19th century, the wave theory of light was gaining acceptance.
2. In such a theory (as opposed to a particle theory of light), presumably,
   (a) There must be a *medium* in which the waves propagate (the ether);
   (b) The speed with which light propagates is independent of the speed of the source (it will just be some given speed with respect to the ether, regardless of how the source is moving).
3. But in that case the Earth's movement through the ether should be detectable. 19th century experiments persistently and spectacularly failed to detect the 'ether wind'.

1.2 Initial null results: refraction

1. Expectations based on the wave theory
   (a) Refraction is normally thought to proceed via Snell's Law ($n_1 \sin\theta_1 = n_2 \sin\theta_2$).
   (b) But in a wave theory, if this is true in the ether's rest frame, then in any frame that is *moving* with nonzero velocity with respect to the ether, one expects (precisely calculable) deviations from Snell's Law.
2. Arago's experiment (1810)
   (a) Arago studied the refraction of starlight from a given star over the course of a year.
   (b) Contrary to expectation, *no* deviations from Snell's Law were observed.

1.3 The Fresnel drag coefficient (1818)

1. Fresnel noticed that one could explain Arago's null result on the hypothesis that the ether is not *exactly* stationary: it is *generally* stationary, but it is *partially dragged* by moving refractive media ($n > 1$).

2. Quantitatively: if a refractive medium is moving with velocity $\mathbf{v}$ [with respect to the general state of rest of the ether], the ether inside that refractive medium will be moving with velocity $f\mathbf{v}$, where the 'Fresnel drag coefficient' $0 \leq f < 1$ depends on refractive index n.

3. Question: What functional dependence of f on n, if any, would lead to the prediction that Snell's Law is valid also in moving frames?

4. Answer: If $f = 1 - \frac{1}{n^2}$ then Snell's Law will be valid in all frames.

5. Thus the Fresnel drag theory, armed with the formula $f = 1 - \frac{1}{n^2}$, successfully retrodicts that no *refraction* experiment will detect the ether wind.

1.4 The Fizeau water experiment (1851)

1. This experiment was designed to test the Fresnel drag hypothesis.

2. The experiment: Light from a source is split into two parallel beams, which are then made to traverse a given 'circular' path through a water device in opposite directions, in such a way that one beam is always travelling with the flow of water, while the other is always travelling against the flow of water. On completing the path, the two beams are recombined and the interference pattern observed. Fizeau wished to determine whether the interference pattern varied with the speed of water flow.

3. Fresnel prediction: The Fresnel drag theory predicts that there *will* be variation in the interference pattern with water speed.

4. Result: The result was exactly as predicted by Fresnel.

1.5 Problems with the Fresnel theory

1. Despite the experimental success of the Fresnel drag coefficient, it was difficult to make physical sense of what might be going on. There seemed to be two possibilities:

   - Fresnel's own suggestion: There are two kinds of ether: an 'undraggable ether' filling all space with some constant density, and a 'draggable ether' present only in refractive media. When a refractive medium moves, *all* the draggable ether inside that medium is *totally* dragged; the quantity $(1 - \frac{1}{n^2}\mathbf{v})$ is the average velocity of the draggable and undraggable ethers.

- Stokes' suggestion: There is just one kind of ether, and it is carried along with a fraction of the velocity of the moving refractive medium.

But neither of these accounts seems able to account for the fact that the index of refraction *depends on the frequency of the light being refracted.* The ether cannot be dragged with one velocity relative to one frequency of light, and with another velocity relative to another ...

2. The Fresnel drag hypothesis does not suffice to account for the null result of the Michelson-Morley experiment ...

2 The Michelson-Morley experiment (1887)

1. This experiment is carefully designed so that even the Fresnel drag theory would predict that the ether wind should be detected via this experiment.

2. The experimental setup

   (a) The Michelson interferometer sends a beam of light towards a half-silvered mirror. Here the beam is split into two components that continue at right angles to one another: one down 'arm A' of the interferometer, the other down 'arm B'. A short distance later each half-beam encounters a second (but fully silvered) mirror, and is reflected back. The beams are recombined, and the resulting interference pattern observed on a screen.

   (b) The observed pattern will depend on (i) the lengths of the arms A and B, and (ii) *the speed of travel of the light along each arm in each direction.*

3. Qualitative rationale

   (a) More precisely: In a laboratory 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 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.

   (b) *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 is not technologically feasible ...

   (c) ...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 change as a signature of the ether wind.

4. Quantitative calculation of the expected fringe shift on rotating the apparatus

   (a) Suppose (for simplicity) that the two arms are of equal length, L.

   (b) Then, the out-and-back time for light to travel along the arm that is parallel to the ether drift should be

   $$\Delta t_{\parallel} = \frac{L}{c-v} + \frac{L}{c+v} = \frac{2Lc}{c^2 - v^2}. \quad (1)$$

   (c) Meanwhile, the out-and-back time for light to travel along the arm that is *perpendicular* to the ether drift should be

   $$\Delta t_{\perp} = \frac{2L}{\sqrt{c^2 - v^2}}. \quad (2)$$

   (d) To second order in $\frac{v^2}{c^2}$, these quantities can be approximated as follows:

   $$\Delta t_{\parallel} \approx \frac{2L}{c}\left(1 + \frac{v^2}{c^2}\right); \quad (3)$$
   $$\Delta t_{\perp} \approx \frac{2L}{c}\left(1 + \frac{1}{2}\frac{v^2}{c^2}\right). \quad (4)$$

   (e) Hence, the delay time is

   $$\Delta t_{\parallel} - \Delta t_{\perp} = \frac{L}{c}\frac{v^2}{c^2}; \quad (5)$$

   the corresponding number of periods for light of wavelength λ and hence frequency $\frac{c}{\lambda}$ is then $n = \frac{2L}{\lambda}\frac{v^2}{c^2}$.

   (f) After rotating the apparatus, the roles of the two arms are switched. We thus have a phase *shift* given by

   $$\Delta n = \frac{2L}{\lambda}\frac{v^2}{c^2}. \quad (6)$$

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

5. The result

   (a) The result of the Michelson-Morley experiment, infamously, was *null* — rotating the apparatus did not lead to any fringe shift.

   (b) Michelson and Morley concluded that 'if there be any relative motion between the earth and the luminiferous ether, it must be small'; 'small' here means 'probably less than one-sixth of the earth's orbital velocity, and certainly less than one-fourth.'

   (c) This null result is a mystery: this 'small relative motion' could easily obtain by luck at any given instant, but it is difficult to see how it could obtain *throughout* the Earth's orbit.

3 Lorentz's program (1890s)

Lorentz sought to explain *both* the results that had previously been explained via Fresnel drag *and* the Michelson-Morley experiment, on the basis of a new theory according to which the ether is *completely* immobile (i.e. not dragged at all, even by refractive media).

1. Deriving the Fresnel drag coefficient
   (a) Lorentz postulates small particles ('ions'/'electrons') that generate electric and magnetic fields in the ether satisfying Maxwell's equations, and that in turn are acted on by these fields according to the Lorentz force law.
   (b) The propagation of light through a medium will be the *sum* of the original primary wave and various 'secondary waves' generated by 'electrons' that are set into simple harmonic motion by the primary wave.
   (c) It follows from this model that an electromagnetic wave passing through an electron-rich medium will effectively be slowed down, and this by a factor n that depends on the density and properties of the charged particles in question.
   (d) We can also calculate what will happen if the electron-rich medium is moving. *Lorentz finds that such waves will, if his theory is correct, travel with speed* $\frac{c}{n} - v\left(1 - \frac{1}{n^2}\right)$ — i.e., that their propagation is modified by precisely the Fresnel 'drag' coefficient, but with no need for any 'ether drag' to explain this.
   (e) The first major triumph of Lorentz's theory over a 'partial ether drag' hypothesis is that frequency-dependence of a medium's index of refraction is no longer a problem.
2. The theorem of corresponding states
   (a) Lorentz seeks to give a general argument for the claim that *no* optical experiment, within a broad class (broader than 'refraction experiments'), will detect the ether wind.
   (b) To this end, Lorentz introduces the following auxiliary functions of the space and time coordinates and of the electric and magnetic fields

at a given point:

$$
\begin{aligned}
t' &= \left(\frac{t}{\gamma} - \gamma\frac{v}{c^2}(x - vt)\right); \\
x' &= \gamma(x - vt); \\
y' &= y; \\
z' &= z; \\
E'_x &= E_x \\
E'_y &= \gamma\left(E_y - \frac{v}{c}B_z\right) \\
E'_z &= \gamma\left(E_z + \frac{v}{c}B_y\right) \\
B'_x &= B_x \\
B'_y &= \gamma\left(B_y + \frac{v}{c}E_z\right) \\
B'_z &= \gamma\left(B_z - \frac{v}{c}E_y\right).
\end{aligned}
$$

(c) Using these quantities and the known equations of electromagnetic theory, however, Lorentz is able to prove the following striking result:

Lorentz's (exact) theorem of corresponding states. Let S be a configuration of charged particles and electric and magnetic fields in space and time satisfying the equations of electrodynamics. Let S'_v be the configuration obtained from S by first putting primes on all appearances of space and time coordinates and expressions for the electric and magnetic fields, and then eliminating the primes using the definitions of the primed in terms of the unprimed coordinates. Then, the constructed configuration S'_v itself satisfies the same equations of motion.

(d) On the basis of this theorem, Lorentz is able to sketch a *general* argument for the claim that *no* optical experiment whose result boils down to observation of a stationary pattern of brightness and darkness will ever detect the ether wind:

i. What is perceived as darkness is the vanishing of the electric and magnetic fields. Similarly, what is perceived as brightness is a large value of the time-averages of the absolute values of such quantities.

ii. Let S be the configuration of electric and magnetic fields that would be produced by a given experimental setup at rest with respect to the ether. Let S_v be the 'corresponding state' moving with velocity v with respect to the ether, in the sense of the TCS; so S_v is the configuration that would be produced by the given experimental apparatus, if *it* were moving with velocity v with respect to the ether.

iii. The ‘corresponding state’ S_v has the *auxiliary* fields vanishing (resp. being large) in the spacetime regions that ‘correspond’ (in the sense of the auxiliary spatial coordinates and the ‘local time’) to the regions in which the real fields vanished (resp,. were large) in the original field configuration.

iv. But since the auxiliary fields are linear combinations of the real fields, if the auxiliary fields vanish (resp are large) then the real fields must also vanish (resp be large), and thus darkness and brightness must be observed in those corresponding regions — so the *pattern* is unchanged.

v. Hence, observation of the pattern of brightness and darkness will not enable the observer to distinguish between the case in which he is at rest with respect to the ether, and the case in which he is moving with velocity v with respect to the ether.

(e) There are, however, two holes in this argument as it stands:

i. In the ‘corresponding state’, the darkness-brightness pattern is *shrunk* in the longitudinal direction. So we might be able to detect the ether wind by e.g. *measuring* the distance between successive bright fringes.

ii. The argument assumes that the experiment in question takes place ‘in a vacuum’ — i.e. that the electromagnetic goings-on are not interacting with any *other matter* located at independently fixed points of space and time. This assumption, of course, is false in the Michelson-Morley experiment. Thus Lorentz’s theorem of corresponding states has as yet done nothing to remove the expectation of a positive result for that experiment.

3. ‘Completing’ the account: The contraction hypothesis

(a) The two ‘holes’ in the above argument can be repaired if we suppose that *matter deforms when it is set in motion*: specifically, if a body moving with speed v contracts in length by a factor γ in the longitudinal direction.

(b) In that case, any ruler that we tried to use to measure the distance between successive brightness fringes would itself contract in such a way as to mislead us into thinking that the pattern had not been shrunk . . .

(c) . . . and the arms of the Michelson-Morley interferometer would contract in just such a way as to compensate for the differences in the speed of light in the longitudinal and transverse directions . . .

(d) . . . So empirical adequacy is recovered. But the ‘contraction hypothesis’ that was required to recover it may appear hopelessly *ad hoc* (?).

Friday week 2 (and Wednesday week 3): Einstein 1905

Einstein, 'The electrodynamics of moving bodies' (1905). Available from Weblearn.

1 Introduction

Preview: The distinctive things about Einstein's approach in 1905 include that

1. it eliminates 'asymmetries which do not appear to be inherent in the phenomena'.
2. It accounts in one shot (?) for *all* null ether-wind results.
3. It does not postulate a luminiferous ether at all, or a standard of absolute rest.
4. It insists on (and makes liberal use of) an *operational* understanding of the meaning of the coordinates in any given frame of reference: if it is a physical fact that setting a rod in motion causes it to shrink, this has implications for the transformations relating the coordinates of different inertial frames. ('Insufficient consideration of this circumstance lies at the root of the difficulties which the electrodynamics of moving bodies at present encounters'.)
5. It is a 'principle theory', rather than a 'constructive theory'.

2 Principle theories and constructive theories

1. Einstein (later — first in 1919) draws a distinction between 'principle theories' and 'constructive theories' in physics.
2. A 'constructive theory' is a theory that attempts to provide a detailed dynamical picture of what is microphysically going on, from which predictions for observable phenomena can be derived. (Most theories in physics are like this.)
3. A 'principle theory' is a theory that takes certain 'phenomenologically well-grounded principles' as *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*.

4. Paradigm example: Thermodynamics is a principle theory; the 'phenomenologically well-grounded postulates' in this case are the 1st/2nd/3rd laws of thermodynamics. From this we are supposed to derive e.g. the existence of an entropy function that never decreases, and various relationships between various functions of state. The corresponding constructive theory would be the (statistical) kinetic theory of gases.

5. 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.

6. Einstein in 1905 sees himself as being in this situation: Lorentz has been pursuing a constructive approach, but Einstein is bothered by deep suspicions that the true equations governing intermolecular forces are very far from being known.

3 Summary of Einstein's 1905 paper

1: 'Definition of simultaneity'

1. Einstein's operational understanding of coordinates means that he will require space coordinates to 'match' the lengths of 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. But even to set up *one* coordinate system, we need more than this: we need to *decide* how to sychronise clocks that are spatiall separated from one another.

2. Einstein *stipulates* that clocks are to be synchronised (i.e. simultaneity is to be defined) in such a way that the one-way speed of light is isotropic.

   (a) I.e.: Let O_A, O_B be inertial worldlines that are stationary in a given frame F. Let A_1, A_3 be events on O_A, and let B_2 be an event on O_B. Let a light signal leave O_A at the event A_1, reaching O_B at the event B_2; let the signal then be reflected immediately back to O_A, arriving at the event A_3. Then say that the clocks t_A, t_B are *Einstein-synchronous relative to the frame F* iff

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

   (b) Note well that there is no such thing as a one-way speed, until and unless we have defined simultaneity.

3. We *assume* that Einstein synchrony is a symmetric and transitive relation. [Exercise: what does this assumption amount to, physically?]

2: 'On the relativity of lengths and times'

Einstein helps himself to a 'stationary' coordinate system K, defined to be one in which 'the laws of Newtonian mechanics hold good'.

In this section Einstein lays down the two 'phenomenologically well-grounded principles' that he intends to 'raise to the status of postulates':

The relativity principle 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.

The light postulate 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?]

$$\text{velocity} = \frac{\text{light path}}{\text{time interval}}, \tag{2}$$

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

3: 'Theory of the transformation of coordinates and times from a stationary system to another system in uniform motion of translation relatively to the former'

The game now is to derive coordinate transformations from these [and a couple of other] principles. In addition to (RP) and (LP), Einstein will need to assume

- The homogeneity of space and time;
- The isotropy of space;
- 'Reciprocity': if two inertial coordinate systems S, S' in standard configuration 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'.

Let k be a system of coordinates that is moving with speed v along the positive x-axis relative to the 'stationary' system K. Let ξ, η, ζ, τ be coordinates for k, determined by the conditions of surveyability-using-rods-and-clocks-that-are-stationary-relative-to-k and the Einstein definition of simultaneity applied in k (for τ). Einstein then argues as follows:

1. From Homogeneity, infer that the coordinate transformations relating inertial coordinate systems are linear.
2. From Einstein synchrony in k, derive expressions for $\frac{\partial\tau}{\partial x'}, \frac{\partial\tau}{\partial y}$ and $\frac{\partial\tau}{\partial z}$ in terms of $\frac{\partial\tau}{\partial t}$, and hence (given the linearity of the transformations) obtain

$$\tau = \phi(v)\frac{1}{\gamma}\left(t - \frac{v}{c^2 - v^2}(x - vt)\right), \tag{3}$$

where $\gamma := \left(1 - \frac{v^2}{c^2}\right)^{-\frac{1}{2}}$.

3. Consider a light ray emitted from the origin in the positive ξ direction. Use 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 LP alone) for the relationship between x and t that holds on the path of this ray, Einstein can derive the expression

$$\xi = \phi(v)\gamma(x - vt); \tag{4}$$

furthermore, although this expression has initially been derived only for points on the light ray, the fact that 'a point at rest in the system k must have a system of values $x'[\equiv x - vt]$ independent of time' ensures that the same relationship must then be true for *all* points.

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

$$\eta = \phi(v)y; \tag{5}$$
$$\zeta = \phi(v)z. \tag{6}$$

4. We now have the Lorentz transformations *up to* an as yet undetermined function $\phi(v)$. To fix this function:

   (a) Invoke RP and Reciprocity to argue that $\phi(v)\phi(-v) = 1$.

   (b) Note that given Einstein's operational understanding of the coordinates, $\phi(v)$ can be interpreted physically as inverse of the *transverse length contraction factor*, i.e. the factor by which setting a body in motion causes it to shrink in the direction perpendicular to its motion.

   (c) Given that physical interpretation, Spatial Isotropy entails that $\phi(v) = \phi(-v)$.

   (d) We argue somehow against the rogue possibility that $\phi(v) = -1$ (using continuity and $\phi(0) = +1$?).

   (e) It follows that $\phi(v) \equiv 1$. We now have the Lorentz transformations.

4: 'Physical meaning of the equations obtained in respect to moving rigid bodies and moving clocks'

1. Length contraction:

   (a) Consider a rigid sphere at rest in k. Points on its surface must satisfy the equation

$$\xi^2 + \eta^2 + \zeta^2 = R^2. \tag{7}$$

(b) But applying the coordinate transformations shows that this is equivalent to the condition

$$\gamma^2(x-vt)^2+y^2+z^2=R^2. \tag{8}$$

(c) This shows that *a moving sphere contracts* by a factor $\gamma \equiv \frac{1}{\sqrt{1-\frac{v^2}{c^2}}}$ in the direction of its motion.

2. Time dilation

   (a) Consider a clock that is at rest in k, hence moving with speed v in the $+x$ direction relative to K.

   (b) From the Lorentz transformations, we have the general formula

$$\tau=\frac{1}{\gamma}\left(t-\frac{v}{c^2-v^2}(x-vt)\right). \tag{9}$$

   (c) Points on the worldline of the moving clock in addition obey the equation $x=vt$.

   (d) Thus (9) reduces to $\tau=\frac{t}{\gamma}$.

   (e) That is, the 'moving clock runs slow', *in the sense that*: if our moving clock A is initially synchronised with a clock B that is at rest in S at the common spatial origin at $t=\tau=0$, and is later compared with a third clock C that is at rest in S and that is synchronised with B by Einstein-Poincare synchrony, then A will fail to read the same time as C when they pass one another.

5: 'The composition of velocities'

1. Einstein considers a point-sized object moving with velocity $(w_\xi, w_\eta, 0)$ with respect to the 'moving' system k. The question is what the velocity of this point will be relative to the 'stationary' system K.

2. By writing down expressions relating the k-coordinates of this object to one another and invoking his transformations between the k-coordinates and the K-coordinates, Einstein derives the now-standard 'relativistic velocity addition law': the coordinates satisfy

$$x = t\frac{(w_\xi+v)}{\left(1+\frac{w_\xi v}{c^2}\right)}; \tag{10}$$

$$y = t\frac{w_\eta}{\gamma\left(1+\frac{w_\xi v}{c^2}\right)}; \tag{11}$$

$$z = 0. \tag{12}$$

3. In particular, for the case of parallel velocities ($w_\eta = 0$), the speed V of the object according to the 'stationary' system K is given by

$$V = \frac{v + w}{1 + \frac{vw}{c^2}}. \tag{13}$$

4. So the 'Galilean law for the composition of velocities', $V = v + w$, is valid in the limit $\frac{v}{c} \to 0$, but *only* in that limit.

4 Einstein 1905's account of Fizeau 1851 and MM 1887

1. Einstein on Fizeau:

   (a) The outcome of the Fizeau experiment can be predicted using Einstein's new formula for the (relativistic) addition of velocities.

   (b) This predicts that light travelling (resp.) with/against the water flow in Fizeau's device will be travelling with speed

   $$V = \frac{\frac{c}{n} \pm v}{1 \pm \frac{v}{cn}}. \tag{14}$$

   (c) Expanding this to first order gives a formula that agrees with Fresnel's prediction. Expanding it to higher order gives corrections to Fresnel's prediction.

   (d) Thus Einstein's theory can explain why Fresnel's prediction was correct to within the experimental error, but also predicts that if we were able to look closely enough (which we probably can't, in this experiment) we would see small discrepancies between Fresnel's prediction and experimental reality.

2. Einstein on Michelson-Morley ...

   (a) Short version: Einstein's 1905 theory predicts null results for all ether-wind experiments at once, trivially (i.e. from the relativity principle alone).

   (b) Long version: Einstein's 1905 theory entails the truth of a fully general Lorentz-style 'theorem of corresponding states' and contraction hypothesis, and thus vindicates Lorentz's explanation of the null results.

   (c) You decide which of these versions is 'better'!

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Friday week 3: Variations on Einstein's theme

References to Brown are to his *Physical relativity*, Oxford University Press (2005).

1 More on Reciprocity

1. A Reciprocity-violating scenario:
   (a) *Suppose* that the following conditions held:
      i. the Light Postulate;
      ii. clocks are synchronised according to the Einstein procedure;
      iii. rods that are moving with respect to the 'stationary' frame S contract longitudinally by a factor γ; *but*
      iv. clocks that are moving with respect to S *do not* slow down (or speed up) — i.e., contrary to special relativity, there is no time dilation.

   (b) We wish to determine whether, if S' is moving with velocity v *according to* S in the positive x direction relative to S, it follows that S is moving with velocity $-v$ *according to* S' in the positive x' direction according to S'. Well ...
      i. Consider an object that is at rest in S', and that passes through the common (spatiotemporal) origin O of S and S'. Let A be some event that is later than the origin, and on the worldline of our object. Let B be the event that is at the spatial origin of S and is simultaneous with A according to S.
      ii. Reciprocity would require $\frac{x_A}{t_A} = -\frac{x'_B}{t'_B}$.
      iii. Consider a rod at rest in S', and whose ends pass through A and B respectively ... Our length contraction assumption tells us that $x_A < -x'_B$ (specifically, it tells us that $x_A = -\frac{x'_B}{\gamma}$).
      iv. Consider a clock at rest at the spatial origin of S' ... Our assumption of no time dilation (with respect to S) tells us that $t'_A = t_A$. But by construction, $t_A = t_B$, and by considering Einstein synchrony in S' we can see that $t'_B > t'_A$. Hence we have $t'_B > t_A$.
      v. This does not yet tell us whether or not Reciprocity holds: when we try to evaluate that condition we have two inequalities 'pointing in different directions', and we do not know whether they will 'cancel' one another. But we can see that it would require

a *coincidence* — a precise coordination between the (logically independent) length contraction and time dilation factors — to give us Reciprocity.

(c) To go further, we can proceed quantitatively, as follows

i. The argument that Einstein uses to derive his τ transformation assumes only 'Einstein synchrony' and the light postulate. Thus the same argument gives us the same result:

$$t' = \frac{\phi(v)}{\gamma}\left(t - \frac{v}{c^2 - v^2}(x - vt)\right). \tag{1}$$

ii. Imposing the condition 'no time dilation' forces $\phi(v) = \gamma$. Hence the above reduces to

$$t' = \left(t - \frac{v}{c^2 - v^2}(x - vt)\right) \tag{2}$$

$$= \gamma^2 t - \frac{v}{c^2 - v^2}x. \tag{3}$$

iii. But x' must be a function of x alone. To say that we have longitudinal length contraction by a factor γ is to say that in particular,

$$x' = \gamma(x - vt). \tag{4}$$

iv. Hence our velocity transformation law is given by

$$\frac{\Delta x'}{\Delta t'} = \frac{\gamma(\Delta x - v\Delta t)}{\gamma^2 \Delta t - \frac{v}{c^2 - v^2}\Delta x} \tag{5}$$

$$= \frac{\gamma(\frac{\Delta x}{\Delta t} - v)}{\gamma^2 - \frac{v}{c^2 - v^2}\frac{\Delta x}{\Delta t}} \tag{6}$$

v. But this means that when $\frac{\Delta x}{\Delta t} = 0$, we have

$$\frac{\Delta x'}{\Delta t'} = -\frac{v}{\gamma}, \tag{7}$$

which is not equal to $-v$. This is a failure of Reciprocity.

2. Reciprocity and the Relativity Principle

(a) The above is also a scenario in which the Relativity Principle fails: for example, while there is (by construction) no time dilation *from the point of view of S*, a clock that is stationary with respect to S 'runs slow' according to S'.

(b) So is Reciprocity derivable from RP?

(c) No. Reciprocity *is* (apparently!) derivable from RP and Isotropy together . . . (Brown pp.106-7)

(d) But (our moral): the condition *is non-trivial.* So one must either have such a derivation of it, or explicitly assume it.

2 Ignatowsky's approach to deriving the Lorentz transformations

Brown pp. 105–110

1. Ignatowski seeks to derive the Lorentz transformations using Einstein's assumptions *minus the light postulate.*

2. This claim should elicit healthy suspicion: which of the remaining assumptions is violated by Newtonian physics (complete with Galilean transformations)?

3. What you can get from Ignatowsky's assumptions: the 'Ignatowsky transformations',

$$
\begin{aligned}
t' &= (1 - Kv^2)^{-\frac{1}{2}}(t - Kvx) && (8) \\
x' &= (1 - Kv^2)^{-\frac{1}{2}}(x - vt) && (9) \\
y' &= y && (10) \\
z' &= z, && (11)
\end{aligned}
$$

   where K is an unknown constant. (It follows from these transformations that $K^{-\frac{1}{2}}$ is an invariant speed.)

4. *Putting* $K = \frac{1}{c^2}$ reduces these to the Lorentz transformations. But this value for K cannot be obtained from Ignatowsky's assumptions: it must be assumed separately.

5. Putting $K = 0$ reduces the Ignatowsky transformations to the Galilean transformations. (So, as our healthy suspicion guessed, Ignatowsky's assumptions indeed did not suffice to rule out the Galilean case.)

3 The experimental approach to deriving the Lorentz transformations

Brown pp. 26–31, 46–46 and 82–87

One might worry that RP and LP are themselves fairly theoretically buried assumptions. Can we start from something closer to experiment, and therefore more epistemologically secure?

1. A very general argument (assuming only Homogeneity and Isotropy) forces the coordinate transformations between inertial frames (in 'standard configuration') to take the form

$$
\begin{aligned}
t' &= \frac{1}{\mathcal{D}(1-\alpha v)}(t-\alpha x) && (12)\\
x' &= \frac{1}{\mathcal{C}_{\parallel}}(x-vt) && (13)\\
y' &= \frac{1}{\mathcal{C}_{\perp}}y && (14)\\
z' &= \frac{1}{\mathcal{C}_{\perp}}z. && (15)
\end{aligned}
$$

2. Michelson-Morley (1887)

   (a) The Michelson-Morley experiment is naturally interpreted as telling us

      i. that the speed of light within any single inertial frame is isotropic, and

      ii. that the longitudinal and length contraction factors are related to one another by $\mathcal{C}_{\perp} = \gamma \mathcal{C}_{\parallel}$.

   (b) This narrows down our transformations to

$$
\begin{aligned}
t' &= k\gamma \frac{c}{c'}(t - \frac{vx}{c^2}) && (16)\\
x' &= k\gamma(x-vt) && (17)\\
y' &= ky && (18)\\
z' &= kz, && (19)
\end{aligned}
$$

   where c is the speed of light in S, and c' is the speed of light in S'.

3. The Kennedy-Thorndike experiment (1932)

   (a) From MM we know that the light speed is isotropic *within any one* inertial frame, but we do not know that the (isotropic) light speed in S is the same as the (isotropic) light speed in S' (i.e., we do not know that the speed of light is *invariant*). Enter Kennedy-Thorndike.

   (b) The KT experiment is a variant on the Michelson-Morley experiment. Instead of *rotating* the interferometer within a given inertial frame, Ives and Stilwell investigate whether the interference pattern changes during the course of the year, i.e. from one inertial frame to another.

   (c) Provided the interferometer arms are of unequal length, one expects a positive result unless the light-speed is the same in all inertial frames.

   (d) The result was null.

   (e) Imposing the invariance of the light-speed narrows down our transformations further: putting $c' = c$ in the above gives us the 'k-Lorentz' transformations.

4. The Ives-Stilwell experiment (1938)

   (a) As yet we have no *experimental* evidence for the undetermined function k (Einstein's $\phi(v)$). Enter Ives-Stilwell.

   (b) The Ives-Stilwell experiment is based on the fact that this undetermined factor affects time dilation.

   (c) A particular case of this: from the k-Lorentz transformations one can derive the relativistic Doppler effect, up to modifications that depend on the factor k. Thus, by observing the apparent frequency of light emitted by a fast-moving source, Ives and Stilwell can obtain an experimental determination of k.

   (d) The result gives $k = 1$, and hence the Lorentz transformations.

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Wednesday week 4: Spacetime structure from Aristotle to Minkowski

Earman, J. *World enough and space-time*, MIT Press (1989), Chapter 2.

1 Key ideas of the 'spacetime structure' approach

1. Some coordinate systems are made better than others not by the way matter behaves, but by the *structure of space and time/spacetime* [which in turn also, via the dynamical laws, affects how matter behaves].
   (a) Thus the inertial frames (e.g.) can be defined as those frames that match spacetime's 'inertial structure' in a preferred way (to be clarified!), rather than e.g. those relative to which 'all free particles' have constant velocity.
2. We distinguish between 'meaningful' and 'meaningless' questions (in a loose sense of these terms): a question is 'meaningful' iff
   (a) It can be posed in a coordinate-free way, by reference to the real structures of space and time themselves; or (equivalently)
   (b) Its answer is one that all 'good' coordinate systems (i.e. coordinate systems that bear a common privileged relationship to the underlying spacetime structure) agree on. (This is the idea of 'objectivity as invariance'.)
3. Many of the key assertions that have been made throughout history can be rephrased (thoroughly anachronistically) as differing postulates about *what structure spacetime has.*

2 Spacetime structure from Aristotle to Newton

1. 'Machian spacetime'
   (a) For any two points a, b, there is a fact about whether or not a and b are simultaneous.
   (b) For any two points *that are simultaneous*, there is a fact about their spatial separation.
   (c) There are *no* facts about
      i. whether or not a given particle is at rest;
      ii. whether or not a given particle is moving with constant velocity;

   iii. whether or not a given configuration of particles is rotating;
   iv. the spatial distance between two events that are non-simultaneous;
   v. the relative speed of two given particles.

2. Leibnizian spacetime
   (a) All the structure of Machian spacetime is still present. In addition:
   (b) There is a *temporal metric*: for any two points a, b, there is a fact about the time interval between a and b.

3. Maxwellian spacetime
   (a) All the structure of Leibnizian spacetime is still present. In addition:
   (b) There is a *standard of rotation*: for any two points a, b on one timeslice and points c, d on a second timeslice, and given a rod whose ends at the first time coincide with a and b respectively, there is a fact of the matter about whether one would have to *rotate* the rod in order to make it the case that at the second time its ends coincided with c and d.

4. Galilean/Neo-Newtonian spacetime
   (a) All the structure of Leibnizian spacetime is still present. In addition:
   (b) The spacetime has *affine structure*: i.e. for any line through spacetime, there is a fact about whether or not that line is 'straight'.

5. Newtonian spacetime
   (a) All the structure of Galilean spacetime is still present. In addition:
   (b) There is a *standard of absolute rest*: i.e. for any two points, there is a fact about whether or not they are in 'the same (spatial) place as' one another.

6. Aristotelian spacetime
   (a) All the structure of Newtonian spacetime is still present. In addition:
   (b) There is a *preferred spatial location* (the centre of the universe).

3 Minkowski spacetime

1. Forget the previous structures ... There is a *spatiotemporal metric*: For any two points a, b, there is a fact about their *spatiotemporal distance* from one another.

2. The Minkowski spacetime interval:

(a) Coordinate-dependent expression (in a Lorentz coordinate system):

$$d(a,b) = \sqrt{(\Delta t(a,b)^2 - \Delta x(a,b)^2 - \Delta y(a,b)^2 - \Delta z(a,b)^2)}, \quad (1)$$

where $\Delta t(a,b) := t(a) - t(b)$, and *mutatis mutandis* for $\Delta x, \Delta y, \ \Delta z$.

(b) The Minkowski interval between two points A, B of spacetime may be

i. 'timelike': $d(a,b)$ real ('hyperboloid of two sheets')

ii. 'spacelike': $d(a,b)$ pure imaginary ('hyperboloid of one sheet')

iii. 'lightlike': $d(a,b) = 0$ (double cone)

3. Q: What other facts are there?

(a) A: Just those that are *definable in terms of* the spacetime interval.

(b) How to get a handle on which facts are so definable: just those on which all Lorentz coordinate systems agree. (E.g. We do have a standard of straightness.)

4 Newton's mistake: Newtonian vs neo-Newtonian spacetime

1. We have neither a priori nor *direct* empirical access to the structure of the spacetime we live in.

2. Our guide to which structure we have is in the dynamical laws: we should postulate as much structure as is required to state the laws of our best physical theories, *and no more*. ('The covariance group of the dynamical equations should equal the invariance group of the spacetime structure.')

3. With hindsight, Newton violated this requirement: Newtonian physics (with no dependence of forces, masses etc on absolute velocity ...) can be formulated in (merely) *neo*-Newtonian spacetime. Occam's Razor thus advises against postulating, in addition, a standard of absolute rest.

4. A question to ponder: Why did Newton make this mistake (if indeed it is a mistake)?

CL 120 Intermediate philosophy of physics: Philosophy of special relativity
Dr Hilary Greaves, Hilary Term 2011

Friday week 4: Generally covariant theory-formulations

J. Norton, 'Philosophy of space and time', sections 5.4 – 5.7. Available online from `http://www.pitt.edu/ jdnorton/papers/PST-2.pdf`, or from Weblearn.

1 Motivation for the general-covariance approach

1. We usually formulate physical laws in such a way that *the laws are true only relative to a privileged class of coordinate systems* (this is their 'standard formulation'). For instance, Newton's laws are valid only in *inertial* frames.

2. On reflection, there is something odd about this:

   (a) Suppose we have two (or more) free Newtonian particles, widely separated from one another in space.

   (b) In order for there to *exist* any frame in which both particles are moving with constant velocity, it seems that the 'last' particle must somehow 'know' how the 'first' particle 'decided' to move, and behave accordingly.

   (c) But this seems to amount to a 'cosmic conspiracy'.

3. How to remove the sense of conspiracy?

   > *[S]omething real has to be conceived as the cause for a preference of an inertial system over a noninertial system.* (Einstein, 1924)

4. Realists about spacetime structure claim that the relevant pieces of spacetime structure supply this 'something real'.

5. The point of formulating theories in a 'generally covariant' way is that everything the theory in question is in fact ontologically committed to, *including* spacetime structure, is then represented explicitly in the equations. In contrast, in standard formulation, spacetime structure is presupposed but not explicitly represented.

2 Standard vs generally covariant formulations: a toy theory

1. Consider first a 'toy theory': we will just model a temporal continuum, T.

   (a) T is a continuum of points. For any two points a, b, there is a fact about the *temporal displacement* $D_t(a, b)$ of b from a.

(b) We model T via a coordinate system, $t : T \to \mathbb{R}$.

(c) In this special coordinate system, the temporal displacement of any point from any other is given by the corresponding difference in temporal coordinates: $D_t(a, b) = t(b) - t(a)$.

(d) But there is some arbitrariness in our choice of coordinate system: If t is an adequate coordinate system, so also is $t' := t + a$ $(a \in \mathbb{R})$.

(e) 'Objectivity = invariance': the physically real features are the features that all allowed coordinate-dependent descriptions agree on.

2. A generally covariant formulation of this theory

   (a) A generally covariant formulation (by definition) is one in which the facts can be represented just as adequately in *any* coordinate system.

   (b) How is this possible?

   (c) To construct a generally covariant formulation of our toy theory:

      i. Postulate a 'scale factor', $w : T \to \mathbb{R}$.

      ii. If t is a standard coordinate system, then, in t, $w = 1$ everywhere.

      iii. But in an arbitrary coordinate system, in general, $w \neq 1$.

         A. To transform the 'scale factor' between coordinate systems t and t', we need to use the formula

$$w' = \frac{dt}{dt'} w. \tag{1}$$

      iv. In *any* coordinate system, temporal displacements can be recovered via the formula

$$D_t(a, b) = \int_a^b w \cdot dt. \tag{2}$$

3 Euclidean space in standard and generally covariant formulation

In Euclidean space, there is a fact about the distance between any two points. The space is 'homogeneous' (every point is 'like' every other) and 'isotropic' (every direction is 'like' every other).

1. The standard formulation

   (a) Distance facts are recovered from coordinates via

$$D_E(a, b) = \sqrt{(x(a) - x(b))^2 + (y(a) - y(b))^2 + (z(a) - z(b))^2}. \tag{3}$$

   (b) If (x, y, z) is a preferred coordinate system, so also is any $\mathbf{x}' : S \to \mathbb{R}^3$ that is related to $\mathbf{x}$ by a transformation of the form

$$\mathbf{x}' = \mathbf{R}\mathbf{x} + \mathbf{a}. \tag{4}$$

2. The generally covariant formulation

   (a) We postulate a *matrix field*, $\gamma_{\mu\nu} : S \to \mathbb{R}^9$.

   (b) Distances are recovered from this matrix field via the formula

$$l = \int \sqrt{\gamma_{\mu\nu} dx^\mu dx^\nu}. \tag{5}$$

   (c) In the 'Cartesian' coordinate systems of our standard formulation, this two-form becomes a diagonal matrix:

$$\gamma_{\mu\nu} = \begin{pmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{pmatrix}. \tag{6}$$

      i. Exercise: What is $\gamma'_{\mu\nu}$, if the 'primed' coordinates are spherical polars?

   (d) The *general* transformation law for $\gamma_{\mu\nu}$ is $\gamma'_{\mu\nu} = \gamma_{\alpha\beta} \frac{\partial x^\alpha}{\partial x'^\mu} \frac{\partial x^\beta}{\partial x'^\nu}$.

   (e) With this transformation law for γ, *all* coordinate systems agree with one another about the distance between a given pair of points.

4 Special-relativistic theories (in standard and in generally covariant formulation)

4.1 Minkowski spacetime

1. The spacetime structure is exhausted by the Minkowski spacetime interval.

2. Standard formulation

   (a) The spacetime interval between two points a, b is given, in terms of preferred coordinates, by

$$d(a,b) = \sqrt{(\Delta t(a,b)^2 - \Delta x(a,b)^2 - \Delta y(a,b)^2 - \Delta z(a,b)^2)}, \tag{7}$$

   where $\Delta t(a,b) := t(a) - t(b)$, and *mutatis mutandis* for $\Delta x, \Delta y, \ \Delta z$.

   (b) If $x^\mu \equiv (t, x, y, z)$ is a preferred coordinate system, so also is any $\mathbf{x}' : S \to \mathbb{R}^4$ that is related to $\mathbf{x}$ by a Lorentz transformation.

3. Generally covariant formulation

   (a) The spacetime interval between spacetime points a and b is given by an integral along any curve joining them:

$$d(a,b) = \int_a^b \sqrt{\eta_{\mu\nu} dx^\mu dx^\nu}. \tag{8}$$

(b) η has the general transformation law

$$\eta'_{\mu\nu} = \eta_{\alpha\beta} \frac{\partial x^\alpha}{\partial x'^\mu} \frac{\partial x^\beta}{\partial x'^\nu}. \quad (9)$$

(c) With this transformation law for η, the expression (8) gives the correct answer for the spacetime interval in an *arbitrary* coordinate system.

4.2 Dynamical theories formulated in Minkowski spacetime (i.e. special-relativistic theories)

1. Content of the theory, in standard formulation: Dynamical laws must be Lorentz covariant. (= 'The special principle of relativity', according to Friedman)

   (a) Example 1: The law of inertia, in standard formulation

   $$\ddot{\mathbf{x}} = 0 \quad (10)$$

   (b) Example 2: Maxwell's equations, in standard formulation[1]

   $$\begin{aligned} \frac{\partial F_{\mu\nu}}{\partial x^\nu} &= J_\mu; & (14) \\ \frac{\partial F_{\mu\nu}}{\partial x^\sigma} + \frac{\partial F_{\nu\sigma}}{\partial x^\mu} + \frac{\partial F_{\sigma\mu}}{\partial x^\nu} &= 0. & (15) \end{aligned}$$

1. Content of the theory, in generally covariant formulation: there is a symmetric 4×4 matrix field $\eta_{\mu\nu}$ (properly: a tensor field of type $(0, 2)$) of 'Lorentz signature', and there are no other 'spacetime structure' fields.

   (a) Example 1: The law of inertia, in generally covariant formulation

   $$\frac{d^2 x^\mu}{d\lambda^2} + \Gamma^\mu_{\nu\sigma} \frac{dx^\nu}{d\lambda} \frac{dx^\sigma}{d\lambda} = 0. \quad (16)$$

[1] Explanatory note: This is the four-dimensional or 'manifestly Lorentz covariant' (standard) formulation of Maxwell's equations. $F_{\mu\nu}$ is a 4×4 antisymmetric matrix field [properly, a two-form] encoding both $\mathbf{E}$ and $\mathbf{B}$,

$$F_{\mu\nu} = \begin{pmatrix} 0 & E_x & E_y & E_z \\ -E_x & 0 & -B_z & B_y \\ -E_y & B_z & 0 & -B_x \\ -E_z & -B_y & B_x & 0 \end{pmatrix}; \quad (11)$$

J_μ is the 'charge-current density' $(\rho, -\mathbf{J})$. The Maxwell equation (14) (for example) is then equivalent to the two possibly-more-familiar '3D' equations

$$\begin{aligned} \nabla \cdot \mathbf{E} &= \rho, & (12) \\ \nabla \times \mathbf{B} - \frac{\partial \mathbf{E}}{\partial t} &= \mathbf{J}. & (13) \end{aligned}$$

(b) Example 2: Maxwell's equations, in generally covariant formulation

$$
\begin{aligned}
F_{\mu\nu;\nu} &\equiv \frac{\partial F_{\mu\nu}}{\partial x^\nu} - \Gamma^\lambda{}_{\mu\nu} F_{\lambda\nu} - \Gamma^\lambda_{\nu\nu} F_{\mu\lambda} && (17)\\
&= J_\mu; && (18)\\
F_{[\mu\nu;\sigma]} &\equiv \frac{1}{3}\Big(\frac{\partial F_{\mu\nu}}{\partial x^\sigma} - \Gamma^\lambda{}_{\mu\sigma} F_{\lambda\nu} - \Gamma^\lambda{}_{\nu\sigma} F_{\mu\lambda} && (19)\\
&\quad + \frac{\partial F_{\nu\sigma}}{\partial x^\mu} - \Gamma^\lambda_{\nu\mu} F_{\lambda\sigma} - \Gamma^\lambda_{\sigma\mu} F_{\nu\lambda} && (20)\\
&\quad + \frac{\partial F_{\sigma\mu}}{\partial x^\nu} - \Gamma^\lambda_{\sigma\nu} F_{\lambda\mu} - \Gamma^\lambda_{\mu\nu} F_{\sigma\lambda}\Big) && (21)\\
&= 0. && (22)
\end{aligned}
$$

(c) Here, in each case, Γ is the 'Christoffel symbol', which is a fixed function of the matrix field $\eta_{\mu\nu}$; *it is a function that vanishes in Lorentz coordinate systems, and not in other coordinate systems.*[2]

5 A bit more on general covariance

1. Two ways of modelling a 'billiard table with a dip':

   (a) 'Standard formulation':

      i. The only physical quantity explicitly represented in the theory is the ball trajectory, $f : T \to \mathbb{R}^2$

      ii. The equations of motion encode the instructions 'accelerate near the coordinate point $x = y = 3$'

      iii. In this formulation, the theory is (obviously) not translation covariant

   (b) 'Generally covariant formulation':

      i. We represent the ball trajectory via a function $f : T \to \mathbb{R}^2$ as before, *but also* we represent the shape of the table via a 'table height function' $\phi : \mathbb{R}^2 \to \mathbb{R}$

      ii. The equations of motion encode the instructions 'accelerate near the dip in ϕ'

      iii. This theory *is* translation covariant

2. Basic idea of generally covariant formulations: *If you explicitly model (by means of additional fields etc.) the spacetime structure that your theory presupposes, as well as the more directly 'observable' quantities, then you can use whatever coordinate system you like* — coordinates are just labels.

[2] For the aspiring cognoscenti:

$$\Gamma^\rho_{\mu\nu} = \frac{1}{2}\sum_\rho \eta^{\rho\sigma}\left(\frac{\partial \eta_{\nu\sigma}}{\partial x^\mu} + \frac{\partial \eta_{\mu\sigma}}{\partial x^\nu} - \frac{\partial \eta_{\mu\nu}}{\partial x^\sigma}\right) \tag{23}$$

— so clearly Γ vanishes in any coordinate system in which η is constant.

3. The relationship between standard and GC formulations
   (a) In the generally covariant formulation of SR, *there exist* coordinate systems in which η takes the same simple form, $diag(-1,1,1,1)$, everywhere in spacetime.
   (b) In *those* coordinate systems, the 'Christoffel symbols' $\Gamma^{\sigma}_{\mu\nu}$ all vanish.
   (c) If we promise to work only in coordinate systems that bear this special relationship to η, we can replace expressions like Γ, in our (generally covariant) equations, with the numerical values that they take in these special coordinate systems.
   (d) The resulting equations will (of course) pick out the intended set of models only relative to coordinate systems in which η and Γ *do* take the special numerical values we have substituted for them.
      i. This just is the standard formulation.
      ii. It is now utterly unmysterious why the standard-formulation equations are valid only in a certain 'privileged' subset of mathematically possible coordinate systems.

6 'What is special relativity?'

1. Special relativity as a principle theory
   (a) 'Special relativity consists of the Relativity Principle, the Light Postulate, whatever supplementary principles are needed to derive the Lorentz transformations therefrom, and the said derivation of the Lorentz transformations.'
2. Special relativity as a statement about transformations between privileged coordinate systems
   (a) 'Special relativity is the statement that the laws of physics (in standard formulation) are Lorentz covariant.'
3. Special relativity as a statement about the structure of spacetime
   (a) 'Special relativity is the statement that spacetime structure (over and above topological and differential structure) is exhausted by the Minkowski metric.'

Q(?): Which of the above captures the essence of special relativity?

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Dr Hilary Greaves, Hilary Term 2011

Wednesday week 5: Relativity and conventionality of simultaneity (Part I)

John Norton, 'Philosophy of space and time', section 5.3 (in Part I). Available from Weblearn.

1 Introduction and background

1. Preamble 1: Simultaneity in pre-relativistic physics
   (a) Before the advent of relativity theory, it was assumed without question that there was a matter of fact about which events are simultaneous with which others.
   (b) Theorising about the nature of the simultaneity relation often connected simultaneity with causation.
      i. Kant's "positive and negative causal criteria": two events are simultaneous iff one of the following conditions holds:
         A. Neither is a cause of the other (NCC); or,
         B. Each is a cause of the other (PCC).
   (c) In *prerelativistic* physics, we are supposed to allow causal influences to travel with any speed (including infinite). Then, according to Kant's definitions, simultaneity is an equivalence relation on the class of events.
   (d) But special relativity's 'speed limit' for causal signals changes all this.
2. Simultaneity in special relativity: initial moves
   (a) In SR, Kant's criteria would have us identify the 'is simultaneous with' relation with the 'is spacelike separated from' relation.
   (b) But the latter is not an equivalence relation.
   (c) This is a problem if simultaneity is supposed to correspond to 'same time coordinate in the good coordinate systems'. [Q: *Need* it so correspond?]
   (d) The standard solution to this problem (cf. lecture 4) is Einstein's 'definition' of simultaneity in terms of light signals.
   (e) Note that Einstein-simultaneity is *frame-relative*: that is, two events that are Einstein-simultaneous relative to one frame will in general be non-Einstein-simultaneous relative to another.

(f) Our question now is whether the Einstein synchrony criterion is itself a *convention*, as opposed to giving the One True Way of synchronising clocks in special relativity.

3. Preamble 2: Fact vs convention

   (a) One of the most difficult tasks in the foundations of physics is distinguishing between those aspects of a given theory that should be taken to represent (or to purport to represent) aspects of physical reality, and when, on the other hand, a given apparent amendment of theory amounts merely to a change of convention.

   (b) A trivial example at each extreme

      i. Example 1: Jones formulates classical mechanics using the letter x to represent a particle's spatial position. Smith uses r to represent spatial position, but otherwise her formulation of classical mechanics is identical to Jones's.

      ii. Example 2: Davies has a theory that predicts that a cannonball fired horizontally in a uniform gravitational field will describe a parabolic path. Evans has a theory that predicts that such a cannonball will traverse a straight-line path.

   (c) Question: is *simultaneity* a matter of fact, or a matter of convention?

2 Alternatives to Einstein synchrony

1. Alternative synchrony schemata: Reichenbach (I)

   (a) We proceed as in Einstein's definition, but replace the factor of $\frac{1}{2}$ in Einstein's equation (**??**) with a parameter ϵ:

$$t_B(B_2) - t_A(A_1) = \epsilon \left(t_A(A_3) - t_A(A_1) \right). \tag{1}$$

   ϵ is then permitted to take any value in the interval $(0, 1)$.

   (b) Example: setting $\epsilon = \frac{1}{4}$ has the consequence of 'tilting' the lines of simultaneity between the worldlines O_A and O_B.

   (c) Objection: the resulting assignment of temporal coordinates does not define an *inertial* timescale.

      i. Definition: A timescale (i.e., an assignment of time coordinates to spacetime points) is said to be *inertial* iff relative to that timescale, free particles have (or: would have!) constant velocity.

2. Alternative synchrony schemata: Reichenbach (II)

   (a) The Reichenbach-1 synchrony scheme resulted in a non-inertial timescale because we used the *same* non-$\frac{1}{2}$ value of ϵ for every spatial direction.

(b) The idea of the Reichenbach-2 scheme is to retain the feature $\epsilon \neq \frac{1}{2}$, but nevertheless to end up with an intertial timescale, by allowing ϵ to vary with spatial direction.

(c) The scheme:

i. For each frame F, choose a direction $\mathbf{r}^F_{max}$ and a value $\epsilon^F_{max} \in (0, 1)$.

ii. For events that are a positive distance from O_A in the spatial direction $\mathbf{r}_{max}$, apply the Reichenbach-1-(ϵ_{max}) scheme as above.

iii. Extend the simultaneity surfaces to events in other spatial directions from O_A by imposing the requirements that

A. the simultaneity surfaces be (hyper)planes, i.e. that the resulting timescale be inertial;

B. $\mathbf{r}^F_{max}$ is the direction of maximum ϵ for the frame F.

(d) The resulting simultaneity relation will be that given by Einstein synchrony for *some* inertial frame, but not necessarily the frame F whose notion of simultaneity we are defining.

(e) Question: Does this 'Reichenbach-II' schema synchronise distant clocks correctly?

3. Interlude: Synchrony by slow clock transport

(a) Q1: How have we synchronised clocks for the past 2000+ years, in practice?

i. Usually: by *clock transport.*

(b) Q2: But special relativity entails [doesn't it?] that the rate at which a clock ticks changes when we set the clock into motion. (Pan Am.) So how have we got away with this practice, without running into inconsistencies?

i. If clock transport is *slow enough*, we have *approximate* path-independence.

ii. It can be shown (Eddington) that, in the limit $\frac{v}{c} \to 0$, synchrony by slow clock transport leads to the same (frame-relative) standard of simultaneity as does the Einstein-Poincare convention.

(c) Q3: But isn't 'slowness' itself a synchrony-relative matter?

i. A: Yes. But we can define the 'self-measured speed' in the absence of any synchrony schema, and understand 'slow' in self-measured terms.

3 Arguments for the conventionality thesis

1. Reichenbach's argument for the conventionality of simultaneity

(a) Reichenbach's argument is based on the empiricist/positivist idea that the only facts are observable facts. (Motivation: underdetermination.)

P1: Empirical equivalence. Versions of SR that differ only on the standard of simultaneity are observationally equivalent.

P2: Criterion of conventionality. If two sets of statements are observationally equivalent, then they agree on all matters of fact, and the choice between them is a choice of convention.

Conclusion: Conventionality of simultaneity. The choice between versions of SR that differ only on standard of simultaneity is a choice of convention.

(b) Objections:

i. Empirical claim: P1 is false (? TBC)

ii. Anti-positivism: P2 is false (? See Phil of Science course!)

2. Grunbaum's argument for conventionality of simultaneity

(a) Grunbaum rejects positivism, but thinks that conventionalism about simultaneity is true nonetheless.

P1: Basic quantities. The basic spatiotemporal quantities are the topology of the spacetime manifold, and the facts about which pairs of spacetime points are causally connectible.

P2: Criterion of factuality. A spatiotemporal relation is factual iff it is definable in terms of the basic quantities; otherwise it is conventional.

P3: Indefinability of simultaneity. Simultaneity is (a spatiotemporal relation that is) not definable in terms of topology and causal-connectibility facts.

Conclusion: Conventionality of simultaneity. Simultaneity is conventional, not factual.

(b) Objections:

i. Question-begging: P1 is unmotivated (?)

ii. *Does* Grunbaum's P2 capture what we (all!) mean by 'conventional'?

iii. Malament's theorem: P3 is false (? TBC)

CL 120 Intermediate philosophy of physics: Philosophy of special relativity
Dr Hilary Greaves, Hilary Term 2011

Friday week 5: Relativity and conventionality of simultaneity (part II)

1 Phenomenological counterarguments to the conventionality thesis

1. Overview
   (a) In response to Reichenbach and Grunbaum's conventionality thesis, it was sometimes claimed that simultaneity was empirically accessible (i.e. that convention-free phenomena together with the laws of physics could establish the holding of simultaneity relations).
   (b) If true, this would show Reichenbach's P1 to be false, and (presumably) would also show that Grunbaum's argument is unsound.
   (c) Examples:
      i. Argument from clock transport
      ii. Arguments from the measurability of the one-way speed of light (Rømer 1676, Bradley 1726, Fizeau 1849)
      iii. Argument from Maxwell's equations
      iv. Argument from the conservation of momentum
   (d) None of these anti-conventionalist arguments is sound. But it is very instructive to see where each of them fails.
2. Argument from clock transport
   (a) Claim: We can discover facts about simultaneity by transporting clocks. What we discover is that the Einstein-Poincare means for establishing synchrony is correct.
   (b) Reply: Synchrony by clock transport is just another synchrony schema (one that happens to coincide with Einstein-Poincare synchrony in the limit of slow clock transport).
3. Measurements of the speed of light (I): The eclipses of Io (Rømer 1676)
   (a) Astronomical tables suggested that Io, one of Jupiter's moons, should move into the shadow of Jupiter on November 9, 1676, at 45 seconds after 5:32am.
      i. These tables were based on numerous observations of previous eclipses, from which the average orbit time etc of Io had been calculated.

(b) Rømer noticed that certain irregularities in the recorded intervals eclipse times — irregularities that other astronomers had been treating as random — were actually systematic, and that they could be explained by the hypothesis that light travelled with a finite speed.

(c) Rømer correctly predicted that the November 9 eclipse would occur exactly 10 minutes later than the accepted prediction.

(d) One can calculate the *magnitude* of the one-way speed of light from the *amount* of time by which eclipses are delayed/advanced. (Result: $\sim 2.12 \times 10^8 ms^{-1}$(1676); $\sim 3 \times 10^8 ms^{-1}$(1809).)

4. Measurements of the speed of light (II): Stellar aberration (Bradley 1729)

   (a) To view a star through a telescope, one cannot *quite* point the telescope straight at the star: one needs to aim the telescope slightly 'off' the line along which light is arriving, due to the fact that the Earth has (in general) a non-zero velocity in the plane perpendicular to the arriving light ray, and the fact that the speed of light is finite. (It's like catching rain in a straw.)

   (b) This effect is small, but noticeable. By measuring the 'angle of aberration' — the angle by which the telescope must be aimed 'off' in order to 'catch' light from a given star — one can, again, calculate the speed of light. (Result: $3.01 \times 10^8 ms^{-1}$.)

5. Measurements of the speed of light (III): Fizeau's cog-wheel apparatus (Fizeau 1849)

   (a) Fizeau's apparatus: a cog-wheel and mirror

   (b) By determining which wheel-speeds allowed a light pulse to pass through the gaps between cogs on both outward and return journeys, and which did not, Fizeau was able to calculate the speed of light. (Result: $\sim 3.15 \times 10^8 ms^{-1}$.)

6. Light-speed measurements and synchrony

   (a) The Fizeau measurements are of the *average round-trip* speed of light. But the Rømer and Bradley measurements are of the *one-way* light speed.

7. The (anti-conventionalist) argument from the measurability of the speed of light:

   P1. The one-way speed of light has been measured experimentally, and found to be isotropic.

   P2. The one-way speed of light is isotropic according to the Einstein-Poincare synchrony convention, but not according to any rival convention.

C. Synchrony schemata that fail to agree with the Einstein-Poincare scheme are inconsistent with experimental results.

Is this argument valid? Is it sound?

8. Synchrony presuppositions in the Rømer measurements

   (a) To calculate the one-way speed of light from the Rømer measurements: Let ΔT be the time interval between two particular successive eclipses (as recorded by clocks on Earth) that we would expect on the assumption that light travelled at infinite speed. Let the measured interval between those eclipses be $\Delta T + \delta t$. Let r be the distance between the positions of the Earth when the two eclipses are observed (in, say, a frame in which the Sun is stationary; for present purposes we can regard this frame as inertial). Then, the one-way light speed is given by $\frac{r}{\delta t}$.

   (b) But δt is the time lapse recorded by a clock that is *moving relative to the frame we are using* (viz., a frame in which the two observation events are a distance r apart). Thus, to presuppose that it records the 'true' time lapse between these events (relative to the frame in question) is to presuppose synchrony by slow clock transport.

9. Argument from Maxwell's equations

   (a) Anti-conventionalist argument:

   P1 Maxwell's equations form part of a well-confirmed physical theory.

   P2 Maxwell's equations entail that the one-way speed of light is isotropic (specifically, that the speed is c in all directions).

   C1 Well-confirmed physical theory entails that the one-way speed of light is isotropic.

   P3 Only the Einstein synchrony scheme agrees with the isotropy of the one-way speed of light.

   C2 Only the Einstein synchrony scheme is consistent with well-confirmed physical theory.

   (b) Reply: Maxwell's equations entail the isotropy of the one-way speed of light *in Lorentz charts* (and also the anisotropy of the one-way speed of light in 'Reichenbach-2' charts).

10. Argument from the conservation of momentum

   P1. It is well-confirmed (both theoretically and experimentally) that momentum is conserved in all interactions.

   P2. Momentum is not conserved according to 'Reichenbach-2' synchrony.

   C. Reichenbach-2 synchrony is inconsistent with theory and experiment.

Reply: What is well-confirmed (theoretically and experimentally) is that $m\mathbf{v}$ is conserved *relative to Lorentz charts* ...

11. General counterargument: from the possibility of a generally covariant formulation

    (a) All phenomenological arguments against nonstandard synchrony *must fail*.

    P1 **Any special-relativistic theory can be given a generally covariant form.**

    P2 A generally covariant theory gives correct empirical predictions relative to one coordinate system iff it gives correct predictions relative to all coordinate systems.

    C1 Any phenomena that can be correctly predicted by a special-relativistic theory can be correctly predicted using any coordinate system.

    P3 Non-standard synchrony conventions are just non-standard coordinate systems.

    C2 Any phenomena that can be correctly predicted by a special-relativistic theory can be correctly predicted by a descriptive scheme that includes a non-standard synchrony convention.

2 Malament's theorem

1. A preliminary consensus

    (a) Circa 1977, the orthodox view was that Reichenbach and Grunbaum were correct: that simultaneity in special relativity is conventional.

2. Malament's theorem (1977)

    (a) Malament proved that the Einstein simultaneity relation for a given inertial frame F is the only nontrivial equivalence relation that is definable from (a) the lightcone structure of Minkowski spacetime and (b) the frame F.

    (b) This resulted in 'one of the most dramatic turns in the debate over the conventionality of simultaneity' (Norton).

3. Sketch of Malament's result

    (a) 'Causal automorphism': A map from Minkowski spacetime onto itself that preserves lightcone structure.

    (b) 'O-causal automorphism': A map from Minkowski spacetime onto itself that both preserves lightcone structure, and takes all points on the worldline O to (possibly, but not necessarily, distinct) points on O.

(c) Claim (about definability): a relation [on Minkowski spacetime] is 'definable in terms of causal structure and the worldline O' iff it is invariant under all O-causal automorphisms.

(d) Claim: The only relation on Minkowski spacetime that

  i. is invariant under all O-causal automorphisms,
  ii. is 'non-trivial',
  iii. is not the universal relation, and
  iv. relates some point on O to some point not on O

  is the relation of Einstein-Poincare synchrony in the rest frame of O.

  i. This should not be particularly surprising. A Reichenbach-2 synchrony relation, for instance, required the specification of a direction $\mathbf{r}_{max}$, and (as a result) clearly is not invariant under spatial rotations leaving O fixed (and such rotations are of course O-causal automorphisms).

4. The (ir?)relevance of Malament's theorem to the debate

(a) As we saw above, some such definability claim was an essential premise for *Grunbaum's* argument for the conventionality thesis. So Malament *may* have shown that Grunbaum's argument is unsound on technical grounds. But

  i. It's not obvious that Malament's result establishes even this.
    A. For Grunbaum, the issue was supposed to be 'definability in terms of topological and causal structure'. Malament discusses instead 'definability in terms of causal structure and worldline O.' Insofar as this is permissible, [why] isn't 'definability in terms of worldline O and spatial direction r^F_{max}' equally permissible?
  ii. Grunbaum's argument isn't the only pro-conventionality argument we have.
  iii. Even showing that *all arguments we have for conventionality are unsound* does not/would not show that *simultaneity is not conventional.*
  iv. Grunbaum's notion of conventionality isn't the only notion in town, and it's not obvious that it's the most interesting notion of conventionality.

CL 120 Intermediate philosophy of physics: Philosophy of special relativity
Dr Hilary Greaves, Hilary Term 2011

Wednesday week 6: Length contraction and time dilation

1 Length contraction

1. Length contraction: the basics
   (a) Length contraction from the Lorentz transformations
      i. (Ideal) rods aligned with the x-axis measure the Lorentz x coordinate in their own rest frame.
      ii. The Lorentz transformation for the x coordinate is
      $$x' = \gamma\,(x - vt)\,. \tag{1}$$
      iii. Since $\gamma > 1$, this means that at $t = 0$ we have $x' > x$.
      iv. So, for a rod whose left-hand end passes through the common origin of S and S' at $t = 0$, we have $l' > l$ (where l' is the length of the rod relative to the 'moving' system S', and l is the length of the same rod relative to the 'stationary' system S).
      v. But l' must be the rest length of the rod.
      vi. So, the length l (relative to S) of a rod that is moving (relative to S) is *less than that rod's rest length* — i.e., a moving rod contracts.
   (b) Clarification: doesn't the same argument work equally well (or badly!) in reverse, to show that a moving rod *expands*?
      i. The reverse argument: the inverse Lorentz transformation is
      $$x = \gamma(x' + vt'). \tag{2}$$
      ii. From this we see that at $t' = 0$ we have $x = \gamma x' \ldots$
      iii. ... hence $x > x'$ ...
      iv. ... hence the length of the rod relative to S is *greater than* its length relative to S', i.e. is greater than its rest length???
   (c) Resolution of this puzzle: the difference in x coordinate between the intersections of the worldlines of the left-hand and right-hand ends of the rod and the $t' = 0$ hyperplane (as opposed to: the $t = 0$ hyperplane) is *not* 'the length of the rod relative to S': to find that length, we must consider a simultaneity hyperplane *of* S.

(d) Side-remark: note that the length of a given object in a given frame depends on 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, for consistency, also be conventionalists about lengths of moving objects.)

2. 'Mere perspectivalism' about length contraction

   (a) Length contraction seems to arise because the length of a rod is *frame-relative*.

   (b) So it seems (?) that one 'gets a rod to contract' merely by changing one's own frame of reference; but, in doing so, one clearly does nothing at all to the rod itself.

   (c) This line of thought seems (?) to suggest that length contraction is not a real *physical* effect, but a 'merely perspectival' one.

3. Bell's 'three-spaceships' puzzle

   (a) 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. (Bell, 1976)

   (b) Q: Will the string break?

4. Comoving frames analyses of length-contraction phenomena

   (a) Basic idea: Use the postulate that the object takes on its rest length in its own instantaneous rest frame, together with the Lorentz transformations relating the object's instantaneous rest frame to the 'lab frame' in question, to deduce the description of the phenomenon in question in terms of the 'lab frame'.

   (b) 'Comoving frames' analysis of the rockets problem

      i. Let l be the original length of the string, in the original rest frame of the rockets.

      ii. After being boosted, the string must have natural length l in its new rest frame.

      iii. So what we have to work out is: whether the separation of the rockets, in the rockets' rest frame after being boosted, is smaller than, greater than or equal to l.

iv. From the Lorentz transformations, we can deduce that it is greater than l.

v. It follows that the string will be stretched (and presumably will eventually break).

5. Homework exercise 1: What is the description of the three-rocket scenario *from the point of view of the eventual rest frame of spaceships B and C?*

6. Homework exercise 2: Car in a garage

   (a) You have a fancy new car. It is 5m long. Unfortunately, your garage is only 4m long. It seems you have a parking problem. But then, having learnt special relativity, you have a brainwave: if you drive the car into the garage *fast enough*, you've been taught, the car will contract lengthwise. If you then slam on the brakes *really hard* and have a friend close the garage door *really fast*, you'll be able to shut the door with the car inside the garage.

   (b) Q: Is this (in principle) possible?

2 Time dilation

1. Time dilation: the basics

   (a) Time dilation from the Lorentz transformations

   i. (Ideal) clocks measure the Lorentz time coordinate in their own rest frame.

   ii. The Lorentz transformation for the time coordinate is

$$t' = \gamma\left(t - \frac{vx}{c^2}\right) \tag{3}$$

$$\equiv \frac{1}{\gamma}\left(t - \frac{v}{c^2 - v^2}(x - vt)\right). \tag{4}$$

   iii. Hence, on the worldline of a clock that passes through the origin at $t = 0$ and moves in the direction of increasing x with speed v, we have ($x = vt$, and hence) $t' = \frac{t}{\gamma}$.

   iv. This means that at any given time (as judged by the 'stationary' frame), the reading on the moving clock is less than the reading on a stationary clock with which it was synchronised at the common origin at $t = 0$. In this sense 'a moving clock runs slow'.

   (b) Clarification: doesn't the same argument work equally well in reverse, to show that a moving clock speeds up?

   i. If we consider the inverse transformation and set $x' = -vt'$, then we get $t = \frac{t'}{\gamma}$ ...

   ii. Response: Yes, but this only shows that the moving clock registers (at a given point on its worldline) a higher reading than does the stationary clock at a *simultaneous-according-to-S'* point on the worldline of the stationary clock. This does *not* amount to the conclusion that a moving clock speeds up *according to the (stationary) frame in which it is moving.* To make that mistake is again to neglect the relativity of simultaneity.

(c) Side-remark: whether or not a clock moving in a given direction runs slow relative to any given frame depends on how distant clocks are synchronised in that frame. (Hence conventionalists about simultaneity should be conventionalists about time dilation — up to a point!)

2. The twins 'paradox'

   (a) The (would-be) paradox:

      i. The stay-at-home twin reasons that, since the rocket twin is moving throughout, the rocket twin will age more slowly, and be younger at their reunion.
      ii. But the rocket twin can just as well reason likewise, and predict that the stay-at-home twin will be younger at the reunion.
      iii. It cannot be that both twins are correct!

3. Defusing the 'paradox'

   (a) An explanation based on acceleration?

      i. Claim: The rocket twin cannot use this reasoning, because her trajectory is not inertial throughout.
      ii. This is not a relevant disanalogy: acceleration is not required for the 'twins effect'.
         A. An acceleration-free version of the 'twins paradox'

   (b) The claim that GR is required in order to dissolve the paradox

      i. Claim: This is indeed a paradox within special relativity. It demonstrates the need for general relativity: special relativity cannot reason correctly about non-inertial frames.
      ii. This is *badly* confused.
         A. Special relativity can underpin correct reasoning using *any* coordinate system (since it has a generally covariant formulation).
         B. As above, non-inertial frames are anyway not required to set up the 'paradox'.

   (c) Correcting the rocket twin's reasoning

- The rocket-twin's reasoning neglects the relativity of simultaneity: specifically, the fact that his pre-turn and post-turn frames do not agree regarding which point on the stay-at-home twin's worldline is 'simultaneous with' the rocket twin's turn point.
- As a result of this neglect, there is a section of the stay-at-home twin's worldline that the rocket twin fails to 'count'.

(d) The spacetime-structure explanation

i. Dead simple: the two twins traverse curves of different proper time. The stay-at-home's worldline is longer (in the Minkowski metric!) than the rocket twin's worldline.

A. Each's ageing process is a clock surveying proper time along its own worldline.

(e) A spatial analogy: The 'wheel paradox'

i. Suppose you and I live in a Euclidean space. We start at a common point of space, and we each walk in a straight line away from this point. Each of us uses a coordinate system such that we are walking in our own positive z-direction.

A. I report that you are travelling further for each unit gain in z-distance than I am. You report that I am travelling further for each unit gain in z-distance than you are.

ii. After some distance, I turn through 90 degrees and walk until my path once again intersects yours. As I turn, I change the coordinate system I am using, so that it will still be the case after my turn that I am walking in my own positive z-direction.

iii. When our paths intersect for a second time, I will have walked further than you. This can be verified by e.g. having had us each roll a wheel of the same diameter along our path, and count the number of times the wheel turns.

A. You could explain this fact by noting that throughout your journey, I was walking further per unit gain in z-distance than you were.

B. But (the 'wheel paradox') couldn't I say the same about you?

4. Advanced homework exercise: the 'twin paradox' on a cylinder

(a) Suppose that space is closed (and has one dimension), so that the topology of spacetime is a cylinder.

(b) Then, we can set up a 'twin paradox' in which *each twin stays at rest in a single inertial frame throughout.*

(c) How can the 'paradox' be defused in this case??

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Friday week 6: Bell's 'Lorentzian pedagogy'

Basic idea of the Lorentzian pedagogy

1. Bell's central point: while one *can* explain phenomena such as length contraction and time dilation via comoving-frames accounts (i.e. always reasoning fist in terms of the rest frame of the object of interest and using the Lorentz transformations to transform back to the 'lab' frame), it is not *necessary* to switch between frames in order to see what will happen in such puzzle cases in special relativity. A correct and comprehensible story can always be told from within a single frame.
2. Also: understanding is increased by seeing how.
3. The more general point is (perhaps) that while 'principle' theories are good for (some) predictions, there is always insight to be gained by also understanding the underlying 'constructive' story:

> If you are, for example, quite convinced of the second law of thermodynamics . . . , 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. (Bell 1992)

1 Step 1: Electric and magnetic fields generated by a moving charge

1. We know, from electromagnetism, the electric and magnetic fields that are generated by a particle of charge Z moving with speed V along the positive z-axis:

$$\begin{aligned}
E_z &= Zez'\left(x^2+y^2+z'^2\right)^{-\frac{3}{2}} \\
E_x &= Zex\left(x^2+y^2+z'^2\right)^{-\frac{3}{2}}\left(1-\frac{V^2}{c^2}\right)^{-\frac{1}{2}} \\
E_y &= Zey\left(x^2+y^2+z'^2\right)^{-\frac{3}{2}}\left(1-\frac{V^2}{c^2}\right)^{-\frac{1}{2}} \\
B_x &= -\left(\frac{V}{c}\right)E_y \\
B_y &= +\left(\frac{V}{c}\right)E_x \\
B_z &= 0,
\end{aligned} \tag{1}$$

where $z' := (z - z_N(t))\left(1 - \frac{V^2}{c^2}\right)^{-\frac{1}{2}}$.

2. In the special case $V = 0$, these fields are (of course) spherically symmetrical. But for $V \neq 0$, *they are not.*

3. We should therefore *expect*, on theoretical grounds, that matter in rapid motion will change shape.

2 Step 2: An electron orbiting the moving charge

Consider now an electron orbiting a moving nucleus.

1. The nucleus (since it has a net positive charge) generates fields as described above.

2. The equation of motion for an electron moving in an external electromagnetic field is given by

$$\frac{d\mathbf{p}}{dt} = -e\left(\mathbf{E} + \frac{\dot{\mathbf{r}}_e}{c} \times \mathbf{B}\right), \tag{2}$$

where $\dot{\mathbf{r}}_e = \frac{\mathbf{p}}{\sqrt{m^2 + \frac{\mathbf{p}^2}{c^2}}}$.

3. It follows that (if the nucleus is accelerated gradually enough not to e.g. tear apart the atom) the initially circular orbit deforms into an ellipse.

4. Also: If the period of the orbit when the nucleus is stationary is T, it follows from the above equations of motion that the period of orbit around the moving nucleus is $T\left(1 - \frac{V^2}{c^2}\right)^{-\frac{1}{2}}$.

3 Step 3: A change of variables

1. Consider the following change of variables:

$$\begin{aligned} z' &:= \left(1 - \frac{V(t)^2}{c^2}\right)^{-\frac{1}{2}} (z - z_N(t)), \\ x' &:= x, \\ y' &:= y, \\ t' &:= \int_0^t d\tau \sqrt{1 - \frac{V(t)^2}{c^2}} - \frac{1}{c^2} V(t) z'. \end{aligned} \tag{3}$$

2. In terms of *these* variables, the orbit is 'circular' with 'period T', and has 'constant angular velocity'. *Note that*

> *The description of the orbit of the moving atom in terms of the primed variables is identical with the description of the orbit of the stationary atom in terms of the original variables ...*

(a) And note that we can read off length contraction and time dilation from the truth of this statement, together with the form of our change of variables $(x, y, z, t) \mapsto (x', y', z', t')$.

3. Further: consider the following change of variables for the fields:

$$E'_x = \left(E_x - \frac{V}{c}B_y\right)\left(1 - \frac{V^2}{c^2}\right)^{-\frac{1}{2}} \tag{4}$$

$$E'_y = \left(E_y + \frac{V}{c}B_x\right)\left(1 - \frac{V^2}{c^2}\right)^{-\frac{1}{2}} \tag{5}$$

$$E'_z = E_z \tag{6}$$

$$B'_x = \left(B_x + \frac{V}{c}E_y\right)\left(1 - \frac{V^2}{c^2}\right)^{-\frac{1}{2}} \tag{7}$$

$$B'_y = \left(B_y - \frac{V}{c}E_x\right)\left(1 - \frac{V^2}{c^2}\right)^{-\frac{1}{2}} \tag{8}$$

$$B'_z = B_z. \tag{9}$$

4. Then, we can further add:

> *. . . [And T]he expression of the field of the uniformly moving charge in terms of the primed variables is identical with the expression of the field of the stationary charge in terms of the original variables.* (Bell, 1976; emphasis in original)

(a) Example: Compare e.g. E_y for the stationary atom with E'_y for the moving atom.

4 Step 4: Moving observers

1. Above, we introduced the 'primed' coordinates x', y', z', t' merely for mathematical convenience, without any suggestion that e.g. t' was a 'time' coordinate.

2. However, it is easy to see that these primed coordinates '*are precisely those which would naturally be adopted by an observer moving with constant velocity who imagines herself to be at rest*' (Bell, *ibid.*, p.75; emphasis in original).

3. *If* we regard our original ('stationary') observer as being 'really' at rest, we will regard the moving observer as subject to certain systematic illusions:

   (a) Her measuring rods are contracted in the z direction. But she doesn't realise this, because e.g. the retinas of her eyes are contracted in the z direction also.

(b) Her clocks run slow. But she doesn't realise this, because e.g. her thinking runs slow too.

(c) Her moving charge generates a nonzero magnetic field. But she doesn't notice this field, because all test-particle accelerations are equally consistent with the alternative hypothesis that (she is stationary and) the electric and magnetic fields are those given by the 'primed' expressions ...

5 Step 5: Generalising the lesson: Lorentz covariance

1. Above, we proceeded by studying the specific dynamical laws (viz., Maxwell's equations and the relativistic Lorentz force law) for the phenomenon we were interested in.

2. But (*almost*) the only feature of these laws that we actually needed, in order to see that moving objects behave the same way in terms of the 'primed' coordinates (3) as stationary objects behave in terms of the 'unprimed' coordinates, was their *Lorentz covariance.*

3. 'Law L is Lorentz covariant': this means (cf. our discussion of Galilean covariance, in lecture 2)

   (a) Form-invariance of equations version: If we replace both x, y, z, t and the other dynamical quantities (e.g. $\mathbf{E}, \mathbf{B}, \mathbf{p}$) in law L with their 'primed' counterparts, and then we eliminate the primes using the expressions for the Lorentz transformations that relate primed to unprimed quantities, we recover the same laws we started with.

   (b) Space-of-solutions version: For any solution of the dynamical equations that is expressed in terms of the original coordinates x, y, z, t, one can construct a new solution by putting primes on all the variables and then eliminating these primes by means of the expressions relating primed to unprimed quantities (cf. the italicized quotes in Step 3). I.e.

   > 'Given any state of motion of the system, there is a corresponding 'primed' state which is in overall motion with respect to the original[. And it follows from the form of the Lorentz transformations that this primed counterpart] shows the Fitzgerald contraction, and the Larmor dilation.' (Bell, *ibid.*, p.73)

6 The upshot

What follows from all this?

1. *Not* that there is a standard of absolute rest. (The 'Lorentzian philosophy')

2. *Not* that one *cannot* predict, or that one cannot explain, length contraction and time dilation by first considering the description of each object in its own rest frame and using the Lorentz transformations to work out deritatively how that object will appear to observers in other frames.

3. Rather, that (as advertised at the outset) it is always *possible* to tell a correct and comprehensible story from within a single frame. (The 'Lorentzian pedagogy')

CL 120 Intermediate philosophy of physics: Philosophy of special relativity
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Wednesday week 7: The arrow of explanation between dynamics and geometry

Harvey Brown, 'Physical Relativity', esp. chapter 8.
Brad Skow's review of Brown's book, online at http://ndpr.nd.edu/review.cfm?id=6603.

1 Introduction: What's the issue?

1. We have seen, so far, two distinct styles of 'explanation' of length contraction and time dilation:
   (a) In terms of spacetime structure
      i. The spacetime geometry is as given by the Minkowski metric. This metric induces separate spatial and temporal metrics relative to each frame. Rods/clocks at rest in a given frame just are devices that survey (resp.) the spatial/temporal metrics in their own rest frame. It follows from the Lorentz transformations (which themselves follow from the Minkowski geometry) that such rods and clocks must (resp.) contract/slow down when set in motion.
   (b) In terms of dynamics
      i. Lorentz-pedagogically: on the basis of the details of a particular Lorentz-covariant dynamics (e.g. Bell 1967)
      ii. Truncated-Lorentz-pedagogically: on the basis of Lorentz covariance of the dynamical laws *alone* (again, see Bell 1967)
         A. Explanation in general: adding irrelevant details doesn't improve an explanation
      iii. Two notes of caution on the truncated Lorentzian pedagogy: As Bell noted (but did not particularly emphasise), Lorentz covariance *alone* does not entail that (hence, does not explain why) either
         A. A given system in a given state *does go into* the corresponding 'primed' state when the system is boosted. ('The boostability of rods and clocks'.)
         B. There are any systems that render 'length/duration in frame F' empirically accessible in the first place. (The *existence* of rods and clocks.)
      iv. But it seems innocuous to add these two conditions as auxiliary assumptions.
   (c) In terms of *both*

    i. We explain length contraction and time dilation in terms of Lorentz covariance (as in the truncated Lorentzian pedaogy), *but* we then to on to explain Lorentz covariance *itself* by appeal to Minkowski geometry.
    ii. This latter position probably represents the 'orthodox' ('explanationist') perspective on explanation in special relativity.

2. Disputed questions
    (a) Does postulating Minkowski geometry for spacetime *explain*
        i. the Lorentz covariance of the dynamical laws?
        ii. Phenomena such as length contraction and time dilation?
    (b) Insofar as special relativity is empirically adequate, should this lead us to believe in Minkowski geometry as an *independent* real feature of the world?
    (c) Two views of Minkowski geometry: Insofar as we believe Special Relativity ...
        i. 'Explanationism': ... we should believe in an independent Minkowski geometry, and (perhaps: precisely because) postulating this geometry enables us to explain various things that we can't otherwise explain.
        ii. 'Codificationism': ... we should believe that the geometry of spacetime is Minkowskian, but this latter statement is a mere codification of certain facts about the [standard-formulation] dynamical laws (namely, the brute fact that they are all Lorentz covariant). As such, it cannot *explain* those facts.
            A. 'Opium sends people to sleep because it has a dormitive virtue'. This is not an explanation.

2 Two preambles from the philosophy of science

1. Preamble 1: (Scientific) antirealism
    (a) Scientific realism (roughly): The empirical success of a scientific theory gives us a good reason to believe in the entities postulated by that theory/to believe that the theory is (approximately) true.
    (b) Scientific antirealism (roughly): The empirical success of a scientific theory gives us a good reason to believe *that that theory will continue to be empirically successful*, but not that what it says about goings-on beyond the observable level is true, or approximately true, or that (anything like) the unobservable entities postulated by the theory exist.
        i. Examples: antirealism about
            A. electrons,

B. heliocentric astronomy (Osiander's preface);

C. unobservable goings-on in quantum mechanics.

(c) Antirealism *across the board* vs antirealism about particular entities

i. An antirealist-about-(say)-electrons would just say that *electrons* don't exist (or that we have no good reason to believe that they do exist). A blanket (scientific) antirealist would say that we have no good reason to believe in the existence of *any* of science's 'theoretical'/'unobservable' entities.

ii. Our 'codificationist' is an antirealist about the Minkowski metric, but is *not* a blanket scientific antirealist.

2. Preamble 2: Inference to the best explanation

(a) Nobody thinks that scientific theories are *deductively proved* from experimental data.

(b) Nobody thinks that scientific theories proper (as opposed to: phenomenological models) are obtained by simple *inductive generalisation* from experimental data.

(c) The methodology is better described as *hypothetico-deductive*: one *postulates* or *hypothesises* a theory, deduces predictions from that theory (what one would expect to see in experiments if that theory were true), and tests those predictions against experiment. If the predictions do match experiment, this is, in some sense, a strike in favour of the theory. (Here agreement ends as to what *exactly* is going on.)

(d) Scientific realists are often fans of *inference to the best explanation (IBE)*:

i. Advocates of IBE think that inferences of the form

P1. We have obtained data D.

P2. Theory T is the best available explanation of data D.

C. Theory T is [approximately] true,

while (of course) not deductively valid, are reasonable (i.e. that it is reasonable to assign high probability to their conclusions on the basis of their premises).

ii. The link between our disputed questions: if postulating Minkowski geometry facilitates the best available explanation of Lorentz covariance, then IBE recommends believing that postulate.

3 Claim 1: 'Minkowski geometry entails Lorentz covariance'

1. More carefully: There are *two* claims here:

(a) Claim 1a: The claim that there is a Minkowski metric (explicit representation of which has been suppressed in our 'standard formulation') entails that standard-formulation laws will in general *not* be covariant under *non*-Lorentz transformations.

(b) Claim 1b: The claim that there is *no suppressed structure other than the Minkowski metric* entails that standard-formulation laws *will* be covariant under *Lorentz* transformations.

2. Spelling out the argument for, e.g., Claim 1b:

   (a) Suppose that we start with a theory in generally covariant form, and that one of the fields is the Minkowski metric field.

   (b) Suppose that we move to a 'standard formulation' by replacing the components of the Minkowski field, *but nothing else*, with their numerical values in Lorentz coordinate systems.

   (c) It follows immediately that *the standard-formulation laws will be Lorentz covariant* (since the values of (all) the suppressed fields are the same in all Lorentz charts).

3. Example:

   (a) Some of Maxwell's equations are written in generally covariant form as

$$\frac{\partial F_{\mu\nu}}{\partial x^{\nu}} - \Gamma^{\lambda}{}_{\mu\nu} F_{\lambda\nu} - \Gamma^{\lambda}_{\nu\nu} F_{\mu\lambda} = \eta_{\mu\nu} J^{\nu}. \tag{1}$$

   (b) In Lorentz coordinate systems (and not in other coordinate systems), all the components of Γ are zero, and $\eta_{\mu\nu}$ is everywhere just the diagonal matrix $diag(-1, 1, 1, 1)$.

   (c) Hence, in a (NB: in *any*) Lorentz coordinate system, and not in any other coordinate systems, our equations reduce to the 'familiar' standard-formulation Maxwell equations

$$\frac{\partial F_{\mu\nu}}{\partial x^{\nu}} = (-J^0, \mathbf{J}). \tag{2}$$

4 Entailment vs explanation

1. Suppose all parties agree [as they should] that Claim 1 is true. We can still ask: Does Minkowski geometry *explain* Lorentz covariance?

2. To answer this further question, we need to get clearer on the issue of *what exactly an explanation is.*

3. The 'deductive-nomological' (DN) model: Theory T (the 'explanans') explains data D (the 'explanandum') iff there is a deductively valid argument with D as conclusion, and with the following features:

(a) The argument's premises include T.

(b) The argument would no longer be valid if T were deleted from the list of premises.

(c) The argument's premises are all true.

4. Example:

   (a) Q: Why did the string break?

   (b) A: The string had a tensile strength of 10N. A 20N force was applied to it. Any time a force exceeding a string's tensile strength is applied to that string, the string will break. [Therefore, the string broke.]

5. *According to the D-N model*, our derivation of Lorentz covariance from Minkowski geometry [specifically: from the statement that the Minkowski geometry is the *only* background structure] is an explanation.

6. The problem is that the D-N account of explanation is quite generally too permissive: it is generally recognised that not every 'D-N explanation' is really an explanation.

   (a) We need (at least) a further condition that the explanans is in some sense *more fundamental than* the explanandum. (Cf. the example of the flagpole and its shadow.)

   (b) In the present debate, this leads to stalemate, when that debate is conducted under the banner of IBE.

   (c) We need a *separate* discussion of whether the Minkowski metric is more, or less, fundamental than the Lorentz covariance of the dynamical laws.

5 Explanationism vs codificationism

5.1 Against explanationism: Brown's objections (in 'Physical relativity', chapter 8)

1. Objection 1: The explanation of Lorentz covariance in terms of Minkowski geometry is 'wholly unclear' (Brown, *ibid.*, p.134)

   (a) Reply: No, it isn't ...

2. Objection 2: Spacetime structure in SR violates the action-reaction principle (Brown, *ibid.*, section 8.3.1)

   (a) Reply: The action-reaction principle is neither an [ontological] criterion of reality, nor an [epistemological] criterion of legitimate postulation.

3. Objection 3: Geometry doesn't always explain. Why would the present case be any different? (Brown, *ibid.*, sections 8.2.1–8.2.3)
   (a) Short reply: Well, why would the present case be the *same*?

5.1.1 Against codificationism/in favour of explanationism

1. Science (according to the scientific realist) normally proceeds by postulating more fundamental entities behind the appearances, and taking those entities to be (i) real and (ii) explanatory.
2. Applying this general strategy to the present case seems to lead straightforwardly to explanationism concerning the Minkowski metric.
3. Hence a codificationist must specify in what *relevant* respect(s) the case of the Minkowski metric is *unlike* other cases of postulation in science. Otherwise codificationism about Minkowski geometry seems no better (and no better motivated) than codificationism about electrons, planets, cats, the external world . . . ??

6 Claim 2: 'Saying that the Minkowski structure *is geometrical/spatiotemporal/etc* does not *explain* (as opposed to codify) anything.'

1. Consider 'bimetric' theories (Cf. Brown, *ibid.*, section 9.5.2):
   (a) In certain alternatives to GR, there are *two* fields that, mathematically, could be regarded as 'metric fields'. But rods and clocks 'survey' only one of them — the other plays a more theoretically buried role in the theory.
   (b) Question: Is the non-surveyed field 'geometrical'?
2. An analogy:
   (a) Q: Can I identify an arbitrary collection of particles in Newtonian mechanics, and *postulate* that they compose a *billiard ball*?
   (b) A: Of course not: they have to behave like a billiard ball in order to deserve the name.
   (c) Q: Can I *explain* why they behave like a billiard ball by saying that they are one?
   (d) A: Of course not. (That would be like explaining why opium makes one sleepy by saying that it has a dormitive virtue.)
3. Suggested methodology: *postulate* the physical reality of certain mathematically specified structures; then *argue*, on the basis of how they behave, that they deserve certain names.

4. But this must sharply be separated from the issue of whether or not the Lorentz covariance of the dynamical laws gives us reason to believe in *the Minkowski tensor field*, specified via its mathematical structure.

CL 120 Intermediate philosophy of physics: Philosophy of special relativity
Dr Hilary Greaves, Hilary Term 2011

Friday week 7: Relationism in special relativity

Tim Maudlin, 'Buckets of water and waves of space', *Philosophy of Science* 60 (1993), pp. 183–203. Available from Weblearn.

1 Relationism vs substantivalism, in general

Substantivalism is roughly the thesis that

1. Points of space/instants of time/points of spacetime exist, independently of any material objects (or fields etc) that may occupy them;
2. Spatial/temporal/distance relations hold most fundamentally between these points of space/instants of time/etc;
3. Distances between material objects are derivative on (i) facts about which points of space (etc) those material objects (etc) *occupy*, and (ii) distances between points of space (etc).

Relationism, in contrast to this, holds roughly that

- points of space/instants of time/points of spacetime do not fundamentally exist (talk of space is in some sense a mere *facon de parler*);
- in place of spatial/temporal/spatiotemporal relations between such points of space/instants of time/points of spacetime, we can make do with an ontology of material objects/material points/similar, and spatial/etc distance relations holding directly between *those*.

2 Leibnizian relationism

The original relationist was 'Leibnizian': he took the relational facts to be *instantaneous spatial distances*, and temporal distances, between 'material points' [i.e., in modern parlance, points on the worldlines of material particles]. (Cf. 'Leibnizian spacetime', from lecture 7.)

Newton's famous two-globes thought experiment generates an argument against Leibnizian relationism:

1. Consider the following two scenarios:
   - Scenario 1: A pair of iron spheres, joined to one another by a string that is just pulled tight, floating at rest in space.
   - Scenario 2: as before, but the whole configuration is spinning about the halfway point along the string.

2. Clearly there is a physical difference between Scenarios 1 and 2: for instance,

   (a) in Scenario 2 the string will be under tension, but in Scenario 1 it will not;

   (b) if the string is cut, in Scenario 2 the distance between the two spheres will steadily increase, whereas in Scenario 1 it will not.

3. But spinning an object makes no difference to the Leibnizian relations between its parts ...

4. Why this is a *problem* for the Leibnizian relationist:

   (a) Maudlin's account: the Leibnizian relationist 'lacks the explanatory resources to account for the variation in the tension of the cord'.

   (b) Huggett's account: given the additional assumption that 'inertial effects' supervene on a scenario's Leibnizian relations, the Leibnizian relationist cannot agree with the standard Newtonian theorist about the dynamically possible histories of Leibnizian relations.

3 Newtonian relationism

But perhaps it is unsurprising that *Leibnizian* relationism is empirically inequivalent to *Newtonian* substantivalism! If we want a relationist counterpart to Newtonian substantivalism, we should instead consider *Newtonian* relationism.

- A Newtonian relationist admits relations between material points corresponding to *Newtonian* (rather than Leibnizian) spacetime structure. That is, she admits a spatial distance and a temporal displacement between *every* pair of material points (i.e. including spatial distances between *non-simultaneous* material points.)

- The *Newtonian* relationist *can* account for the two-globe phenomenon: spinning an object *does* change the *Newtonian* relations between its (non-simultaneous) parts.

- This suggests that the two-globes-based objection is actually an objection to *Leibnizian structure* (whether relationistically or substantivalistically conceived), not to relationism.

4 Minkowskian relationism

1. Our question: What form of relationism is appropriate in the context of special relativity?

2. Obvious answer: Admit material points (as before), but replace the Newtonian's separate spatial and temporal distance relations with a single Minkowski *spatiotemporal-distance* relation, holding between pairs of such material points.

3. This 'Minkowskian relationist' can successfully account for the two-globe phenomenon, just as the Newtonian relationist can.

This is perhaps the most obvious way to be a relationist in the context of special relativity. Pooley thinks that the 'dynamical approach' suggests a *different sort* of special-relativistic relationism:

> [Huggett] sees Newton's globes thought experiment as illustrating that no theory has the following three characteristics. (i) Its spatiotemporal ideology is restricted to Leibnizian relations; (ii) its dynamically allowed histories of such relations are exactly those predicted by Newtonian theory; (iii) inertial effects supervene on the specified spatiotemporal relations between bodies. (Pooley, 2011)

The above sort of relationism amounts to dropping (i). But one could maintain relationism by dropping instead (ii) or (iii). In particular, 'the dynamical approach to special relativity, defended [in various joint papers by Brown and Pooley, and by Brown in *Physical Relativity*]' can be seen as relationism-by-dropping-(iii) . . .