Review of *Accelerating Expansion: Philosophy and Physics with a Positive Cosmological Constant*, by Gordon Belot

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For *Foundations of Physics*

Gordon Belot
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The self-declared focus of Gordon Belot's new book, *Accelerating Expansion: Philosophy and Physics with a Positive Cosmological Constant*, is de Sitter spacetime. Belot discusses its mathematical structure, the central role which it plays in contemporary relativistic cosmology, and—perhaps most importantly for the readers of this journal—the philosophical and conceptual puzzles that arise from taking this central role seriously. The book aims to be a graduate-student-friendly invitation to all things de Sitter, and the main text is accompanied by mathematical exercises and more philosophically-oriented open questions.

Before we continue, let's set some notational conventions and recall the definition of de Sitter spacetime. The Einstein equation of general relativity (GR) with cosmological constant is

$$G_{ab} + \Lambda g_{ab} = 8\pi T_{ab}, \tag{1}$$

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where G_{ab} is the Einstein tensor, g_{ab} is the metric tensor, T_{ab} is the stress-energy tensor associated with material fields (that is, matter and radiation), and Λ is the cosmological constant. For solutions with constant curvature, $G_{ab} = -\frac{1}{4}Rg_{ab}$, where R is the Ricci scalar.[1] So, if one sets $\frac{1}{4}R = \Lambda$ and $T_{ab} = 0$, one can treat such solutions as vacuum solutions to (1). In this setting, the familiar Minkowski spacetime is the maximally symmetric vacuum solution to (1) with $R = 0$ (and so $\Lambda = 0$), whereas *de Sitter spacetime* is the maximally symmetric vacuum solution to (1) with $R > 0$ (and so $\Lambda > 0$).[2]

From a slightly different point of view, closer to Belot's own presentation, an n-dimensional de Sitter spacetime can be defined as as a submanifold of an $(n+1)$-dimensional Minkowski spacetime with global coordinates $(t, x_1, ..., x_n)$: it is characterised by the equation $-t^2 + x_1^2 + ... + x_n^2 = 1$, and is equipped with the induced metric. It's worth mentioning that the development of this curved vacuum solution in 1917 by Willem de Sitter was the first of three blows to the cosmological constant that led Einstein to denounce it from relativistic field equations.[3]

Belot identifies a number of reasons, both from physics and from philosophy, to study de Sitter spacetime (pp. 4–5).[4] From the physics point of view: insofar as we now believe that we live in a universe with positive cosmological constant,[5] we anticipate that de Sitter will be a 'powerful attractor' for late-time cosmology, and therefore worthy of study in physics (more on that in our discussion of Chapter 7 below). And from the philosophy point of view: de Sitter spacetime can be treated as a 'toy' spacetime in the $\Lambda > 0$ regime, just as Minkowski is for $\Lambda = 0$, and thus conducive for debates regarding, for example, the nature of time and causality in light of relativity theory.

This review is structured as follows. First, we'll present a chapter-by-

[1] See Hawking and Ellis (1973, p. 124). For further background on GR, see, for example, Wald (1984).

[2] Let us note that concepts such as 'constant curvature' and 'maximally symmetric space' are carefully expounded by Belot in Chapter 3.

[3] For a fascinating account of the history of cosmological constant's role in relativity theory, see Earman (2001). The other two blows—discussed by Earman (2001, §5)—were: Eddington's proof of the instability of the Einstein static universe (mentioned in our discussion of Chapter 7 below), and Hubble's redshift observations that indicated an expanding universe.

[4] Throughout this review, unless otherwise stated, page references are to Belot's book.

[5] See, for example, Peebles (2022, ch. 9). Note, however, that recent results from the DESI Collaboration weaken the empirical support for a purely cosmological constant-based model for dark energy, as Belot himself acknowledges (p. 4, n. 15).

chapter survey, picking up on themes raised by Belot which we regard as being of particular interest or importance. The book has nine chapters: all them blend exposition of relevant physics and mathematics with some philosophical and conceptual reflections, although in varying proportions. Chapters 1 (on de Sitter spacetime), 3 (on curvature and symmetry), and 5 (on anti-de Sitter spacetime) are almost completely expository, whereas chapters 8 (on underdetermination of cosmic topology) and 9 (on Boltzmann brains) are more philosophically rich. The other chapters—2 (on time in de Sitter spacetime), 4 (on elliptic de Sitter spacetime), 6 (on asymptotically de Sitter spacetimes), and 7 (on stability and genericity)—are somewhere in between. After our survey, we'll provide some more general reflections on the book, its style, and its ambitions. To break the narrative tension: overall, our verdict will be very positive.

In **Chapter 1**, Belot introduces the technical background to de Sitter spacetime. Belot's approach is to present de Sitter spacetime as a submanifold of a higher-dimensional Minkowski spacetime (as we did above). He thereby emphasises that n-dimensional de Sitter spacetime (dS_n) is related to $(n+1)$-dimensional Minkowski spacetime (M_{n+1}) in a way that resembles a more familiar relationship between an n-dimensional sphere and $(n+1)$-dimensional Euclidean space: geodesics of dS_n arise from intersections of dS_n with hyperplanes of M_{n+1}, points of dS_n are said to be antipodal when they lie on a straight line in M_{n+1} passing through the origin, and the isometry group of dS_n is a subgroup of isometries of M_{n+1} that fix the origin. (There are also differences, acknowledged by Belot, such as the fact that, unlike the round n-sphere, dS_n is not geodesically connected.)

Belot also introduces the notions of homogeneity and stationarity of spacetime, and notes the consequences of the fact that de Sitter is not stationary (even though homogeneous), and so does not possess a timelike Killing field: in de Sitter, it is difficult to make sense of the notion of conserved total energy for the stress tensor of a matter field (such as Klein-Gordon), and the notion of a particle for quantum field theories. This establishes the first significant disanalogy with Minkowski spacetime. Finally, Belot introduces Einstein's static universe E_n,[6] and notes that de Sitter spacetime is conformally equivalent to a subset of this (this relationship is discussed further in Chapter 5). Overall, the chapter doesn't go beyond quite standard material,

[6] Recall that the n-dimensional Einstein static universe E_n is a spacetime with topology $\mathbb{R}\times S^{n-1}$ and metric $ds^2 = -dt^2 + d\Omega^2_{n-1}$, where $d\Omega^2_n$ is the metric of the round n-sphere.

although it's worth noting that the presentation is deft, intuitive, and sufficiently thorough for Belot's discussion in subsequent chapters (and ample references are provided for those who wish to cover this material in greater depth).

Next, in **Chapter 2**, Belot considers how philosophical-foundational issues of time and simultaneity play out in de Sitter spacetime. Recall that in Minkowski spacetime, assuming the Einstein–Poincaré clock synchrony convention, simultaneity is frame-relative. Moreover, there is a sustained and long-running debate about whether simultaneity is also conventional (which amounts to the question of whether the Einstein–Poincaré clock synchrony convention is mandated in Minkowski spacetime).[7] How, Belot asks, do these issues transfer over to de Sitter spacetime? On this, he writes that

> [...] since only events that can both signal and be signalled by events on [the worldline of a freely falling observer] l can be considered to be Einstein-simultaneous with points on l, only events in the causal diamond $J^+(l) \cap J^-(l)$ of l will be eligible for this honour.
>
> In Minkowski spacetime, all events are in the causal diamond of the worldline of an eternal freely falling observer and Einstein-simultaneity is an equivalence relation. The picture is different in de Sitter spacetime. The causal diamond of a freely falling observer covers only a fraction of the spacetime [...]—and since only points in the interior of the causal diamond of an observer can be Einstein-simultaneous with points on the observer's worldline, the surfaces of Einstein simultaneity of a de Sitter observer do not partition de Sitter spacetime. (p. 27)

After discussing the Einstein–Poincaré method of defining simultaneity relation on a causal diamond in de Sitter (resulting in the so-called 'static patch'), Belot presents three alternatives based on, respectively: (i) the set of points that are invariant under all symmetries of dS_n that fix a given point on l and leave l invariant as a set,[8] (ii) flat spacelike de Sitter hypersurfaces

[7] See, for example, Janis (2018) for recent discussion.

[8] This resembles the construction known from the 'proof' of the non-conventionality of simultaneity offered by Malament (1977a). However, concerns about operationalising the simultaneity relation in the world where there is one inertial observer in an otherwise empty spacetime would seem to just as relevant, locally in de Sitter as globally in Minkowski spacetime.

orthogonal to any point in l ('the cosmological patch'), and (iii) the intersection between dS_n and level surfaces of t in ambient M_{n+1} equipped with inertial coordinates ('the global patch'). None of these procedures allows one to define the class of simultaneity slices in de Sitter spacetime that both (a) covers the whole spacetime and (b) yields slices of minimum volume. Consequently, Belot claims, "the transition from Minkowski spacetime to de Sitter spacetime opens a new front [in philosophical and conceptual investigations of time]" (p. 39).

The presentation of the technical material here is careful and highly pedagogical, but we don't fully agree with Belot's philosophical conclusion. In particular, Belot's dialectic proceeds as if it is the move from Minkowski spacetime to de Sitter spacetime specifically (that is, from the $\Lambda = 0$ regime to the $\Lambda > 0$ regime), rather than merely from special to general relativity, that reveals new aspects of the philosophical debate about time and simultaneity in light of contemporary physics. We are not convinced by this.

For, in many general relativistic spacetimes (not just in de Sitter), there is no timelike curve whose causal diamond covers the whole spacetime, so that the Einstein–Poincaré convention cannot be applied globally. This has been duly noted in the literature on the philosophy of time, and its implications have been explored by, for example, Savitt (2015) and Aames (2022). Also, there are many spacetimes which do not admit *any* global spatial slicing that yields slices of minimal volume, and this has nothing to do with the cosmological constant. So, it's hard to see what is so special about de Sitter in this regard.

Moving on: in **Chapter 3**, Belot provides some further technical background on spaces of constant curvature, of which de Sitter, anti-de Sitter, and their elliptic variants, are just special cases. First, he presents the definition of constant sectional curvature and discusses its relationship with the Riemann tensor. This is followed by a classification of other properties of metric manifolds—maximal symmetry, isotropy, and homogeneity (as well as their local versions)—in both Riemannian and Lorentzian settings. Finally, Belot zooms into paradigmatic cases of maximally symmetric manifolds with constant curvature (again: first Riemannian, then Lorentzian), treating them as subsets of scalar product spaces $\mathbb{R}^n_k$ (that is, copies of $\mathbb{R}^n$ equipped with a natural non-degenerate symmetric bilinear form $\langle\ ,\rangle$ of signature $(k, n-k)$) with the induced metric, thereby following the classical treatment of O'Neill (1983). In this manner, he introduces elliptic de Sitter ($\overline{dS}$), anti-de Sitter (AdS, defined as the universal cover of the anti-de Sitter hyperboloid), and elliptic anti-de Sitter ($\overline{AdS}$) spacetimes (and re-introduces de Sitter spacetime).

Since the chapter is focused solely of the technical material, there is nothing objectionable here on philosophical grounds. From a more technical point of view, the systematic introduction of de Sitter and anti-de Sitter spacetimes and of their elliptic counterparts via subsets of scalar product spaces, is pedagogically very helpful—especially given that it is preceded by, and compared with, an analogous discussion of the more familiar Riemannian case. Belot's presentation of the relationships between homogeneity and isotropy in different settings is also comprehensive and insightful, and some of the relationships he describes are invoked in subsequent chapters.

Chapter 4 continues along these lines, by undertaking a specific study of elliptic de Sitter spacetime ($\overline{dS}$), which stands to de Sitter spacetime as the elliptic plane stands to the sphere (recall that the elliptic plane results from identifying antipodal points of the sphere, whereas $\overline{dS}$ results from identifying antipodal points of dS). First, Belot investigates these relationships in greater detail, discussing, among other things, an alternative way of defining $\overline{dS}$, analogous to that of defining the elliptic space by identifying antipodal points on the boundary of a hemisphere.

Then, Belot notes that there are two main properties that distinguish de Sitter spacetime from its elliptic counterpart. First, in elliptic de Sitter, but not in de Sitter, pairs of distinct points always lie on exactly one geodesic (pp. 73–4). Second, elliptic de Sitter, unlike de Sitter, is temporally *non*-orientable (although it does *not* contain closed causal curves, as opposed to the standard example of a temporally non-orientable spacetime given by, for example, Wald (1984, p. 189)).

The first of these differences, which signifies the mathematical elegance of $\overline{dS}$, speaks in its favour among the geometers. But, perhaps more interestingly, Belot also invokes a passage from Schrödinger's *Expanding Universes* (1956, p. 12) which turns this geometric fact into an argument for the *physical* superiority of $\overline{dS}$ over dS. The argument, as Belot remarks, is quite idiosyncratic, but—we think—it is also philosophically rich and worthy of further scrutiny.

Schrödinger notes that in dS (but not in $\overline{dS}$, nor in Minkowski spacetime for that matter) there are worldlines of observers that at some point share their causal past, but after some point their causal futures become, and forever remain, disjoint. He takes this to represent an impossible situation where these observers share their possible experience at some point, but it is impossible that they *will* have shared their possible experience sometime in the future. Let's take for granted that this is indeed what is represented by appropriate worldlines in dS. The key question then is: what justifies Schrödinger in regarding such a situation as being impossible?

It's difficult to extract Schrödinger's own reasoning from this passage (Belot glosses it as "a bracing Hegelian verificationism" (p. 76), but that is hardly helpful), so we will proceed anachronistically. One possible justification could be that our best understanding of the interplay between modal and temporal operators renders such a situation dubious on purely logical grounds. Consider the following reasoning: suppose it's possible that two observers share their experience. Then, by the widely-held S5 principle of modal logic (that is: what is possible is necessarily possible), this is necessarily so. Some philosophers argue forcefully that necessary truths are always true.[9] So, it should always be true that it's possible for these observers to share their experience. But this is not what happens in dS, so worlds with spacetime structure of dS are not even *metaphysically*, let alone physically, possible. (Of course, with some additional care in specifying what kind of modality is at stake here, one could take this to be an argument against S5, or against the claim that necessary truths are always true, but let's set this aside.)

One can quite easily identify a significant gap in this reasoning. For it begins with an assumption that it's possible that the two observers have the same experience, which is different from the claim that these two observers have the same possible experience (which is what happens in dS). Still, under some interpretation of 'possible' relevant to this situation, these two propositions can be considered equivalent. Suppose that to say that two observers share their experiences at a point p (in a time-orientable spacetime) is to say that their worldlines coincide at least up to p. And suppose that to say 'at p, possibly: observer o_1 is F', is to say that F is true, at p, of some observer o_2 passing through p, whose velocity at p is equal to that of o_1's (and, consequently, whose worldline lies in the union of causal future and causal past of p). So, to say that something that happened at a point q is a possible experience of o_1 at p is to say that at p, it's possible that o_1 traverses q. In that case, if, at p, it's possible that o_1 and o_2 share their experience, then their causal pasts must coincide. Conversely, if they have the same possible experiences at p (and so their causal pasts coincide there),

[9]See, in particular, Dorr and Goodman (2020).

then it's possible that they share the same experience.

Admittedly, this line of reasoning is *far* from watertight: it's not clear why the past should be 'modally open' (that is, why o_2 that witnesses 'at p, possibly o_1 is F' only shares velocity with o_1 at p, but not necessarily its past worldline), nor—more importantly—how invoking this spacetime-relative meaning of 'possible' coheres with the metaphysical notion of modality that underpins the acceptance of modal principles invoked before. Our aim, however, is just to flag that Schrödinger's argument, although idiosyncratic, is not wildly implausible or completely incoherent.

Moving on to the second pertinent difference between dS and $\overline{dS}$, Belot states as a mark against the latter that "to this day, it is often taken for granted that temporal orientability is a necessary condition for physical admissibility" (p. 74), and so—Schrödinger's argument notwithstanding—$\overline{dS}$ cannot be considered a physically reasonable spacetime. Let's look at this remark in a little more detail.

The main motivation behind the claim that a physically reasonable spacetime must be time-orientable is that it seems difficult to make sense of time-asymmetric laws in a temporally non-orientable world.[10] And since some physical laws are arguably time-asymmetric, and—moreover—we should expect any physically reasonable spacetime to be able to uphold such laws (perhaps by definition of "physically reasonable"), this constitutes a defect of temporal non-orientability. Admittedly, Belot notes that this is not a completely uncontroversial argument: he cites discussions by Sklar (1974) and Earman (2002) (p. 74, n. 23), and invites the reader to ponder the question whether time-asymmetric laws make sense in $\overline{dS}$ specifically (Question 4.3, p. 78).

Indeed, a closer look at these sources, and some reflection upon the structure of $\overline{dS}$ as presented by Belot, indicates that one might be more sympathetic toward temporal non-orientability that the main text suggests. Thus, one might try to defend time-non-orientability of spacetime as follows: the time-asymmetric laws in question can be determined only locally, so the global structure of spacetime is irrelevant to the question of upholding such laws. Moreover, locally, every relativistic spacetime is temporally orientable (because every point has a simply connected neighbourhood that is always

[10] This argument has a somewhat 'transcendental' flavour, and one might wonder whether time-orientability of spacetime can be tested empirically. The answer is far from clear: see Lemos et al. (2023) and Bielińska and Read (2022) for further discussion from the perspectives of, respectively, physics and philosophy.

temporally orientable).[11] So, there can be peaceful co-existence between time-asymmetric laws and temporal non-orientability of spacetime.[12]

Earman (2002) responded to this kind of argument with "a more sophisticated kind of induction" (p. 257) which attempts to show that, under certain reasonable assumptions about the universality of physical laws, the co-existence of temporal non-orientability and time-asymmetric laws leads to a contradiction. His argument, however, relies on the existence of closed timelike curves in such spacetimes, and—as we've seen above—these do not occur in $\overline{dS}$. So, the possible stance that we take Belot to hint at (but, of course, not necessarily endorse) by posing his Question 4.3 is this: one might try to establish a peaceful co-existence between temporal non-orientability and time-asymmetric laws by an appeal to the local character of laws, and the induction method invoked by Earman to counter such claims cannot be applied to $\overline{dS}$. Consequently, it is even less obvious than one might have initially supposed why the temporal non-orientability of elliptic de Sitter spacetime should be treated as a mark against its physical admissibility.

Having by now spent quite some time considering de Sitter spacetime and its elliptic cousin, in **Chapter 5**, Belot moves on to consider *anti*-de Sitter spacetime: that is, the maximally symmetric vacuum solution to (1) with *negative* cosmological constant ($\Lambda < 0$). (Perhaps it's worth mentioning that while the so-called anti-de Sitter *hyperboloid* ($\underline{AdS}$) can be defined as a subset of $\mathbb{R}^{n+1}_2$ obeying $\langle x, x\rangle = -1$ that is also equipped with the induced Lorentzian metric, we take anti-de Sitter *spacetime* (AdS) to be the universal cover of that hyperboloid. The pertinent difference is that the topology of $\underline{AdS}$ is $S^1 \times \mathbb{R}^{n-1}$, while AdS has the topology of $\mathbb{R}^n$; and whereas the former admits closed timelike curves, the latter does not.) Then, Belot discusses the fact that AdS has a timelike conformal boundary homeomorphic to an $(n-1)$-dimensional Einstein static universe, E_{n-1}, and explores various oddities in the behaviour of causal geodesics in AdS associated with this fact.[13] This is followed by a proposal to capture the geometry of the conformal boundary

[11] And elliptic dS is "locally time-orientable" even in a stronger sense: every proper open subset of it is time-orientable, since it's isometric to some proper subset of dS.

[12] For further discussion on this point, see Bielińska (2021), Earman (1991), Roberts (2022), and Visser (1996).

[13] The existence of this boundary follows from the fact—neatly expounded by Belot (pp. 81–2)—that AdS is conformally equivalent to a portion of E_n where the spherical part of its topology is replaced by an open hemisphere (and the boundary of this portion is E_{n-1}).

of *AdS* through the equivalence class of conformally related metrics, and a short discussion of the symmetries of this object.

There are two aspects of Belot's presentation of this material that invite some philosophical comments. First, as the name suggests, the geometry of the conformal boundary is determined up to a conformal transformation. In light of this, Belot claims that a "more natural, intrinsic, and revealing way" (p. 88) of describing the object 'conformal boundary of spacetime' is as a manifold equipped with an equivalence class of conformally related metrics, which "may seem like a funny sort of object" (p. 88). We note that there is a different meaning of an 'intrinsic description (or formalism)' of the theory's models (or objects) present in the literature, according to which an intrinsic description is one where symmetry-variant structure of an object is reformulated as symmetry-invariant by means *other* than forming an equivalence class under the symmetry transformation.[14] So, in particular, an equivalence class of conformally related metrics does *not* amount to an intrinsic description of conformal boundary's geometry in this sense. (Such a description could possibly be given using conformal metric tensor densities (which are invariant under conformal isometries): a more serious kind of object, for some tastes at least.[15])

Second, the fact that any open proper subset of the anti-de Sitter hyperboloid $\underline{AdS}$ is isometric to some open subset of any of its various covers (including its universal cover, that is *AdS*) raises questions regarding the underdetermination of spacetime's topology—see, for example, the classic philosophical discussion by Sklar (1974, ch. 2)—which is also pursued in greater detail in Chapter 8. In particular, in his Question 5.1 (p. 84), Belot draws attention to this possibility, and suggests both a "Reichenbachian" conventionalist response (according to which there is no fact of the matter about spacetime's topology in this case) and a "Leibnizian" strategy of plumping for the simplest amongst these spacetimes (clearly, one could also think of this as an Occamist strategy). As Belot himself notes, these two options map very well onto a recent catalogue of possible responses to cases of underdetermination presented by Le Bihan and Read (2018), but we would add that this discussion also invites further integration with recent philosophical

[14]This sense of 'intrinsicality' (inspired by, but distinct from, that of Field (1980)) was recently discussed systematically by Jacobs (2022). For further discussion, see also March (2024).

[15]For a discussion of conformal metric densities, and their role as an absolute object in GR, see Pitts (2006).

writings on spacetime conventionalism.[16]

In the final parts of the chapter, Belot introduces the idea (which will play a more prominent role in Chapter 8) of 'observer complementarity'. This is the view that there is no objective way of adjudicating between different observers' descriptions of what happens in their own causal patches (even when these patches overlap, and the descriptions seem *prima facie* jointly inconsistent), on the grounds that such an objective adjudication would presuppose "the unphysical [and thus ill-founded] perspective of a global observer".[17] Observer complementarity is a generalization of 'black hole complementarity', which is an attempted resolution of the information loss paradox proposed by Susskind et al. (1993) along similar lines.[18] Indeed, Belot weaves his introduction of observer complementarity into a turbo-accelerated tour of the Hawking effect, information loss paradox, and the AdS/CFT correspondence. The tour is, as Belot acknowledges, "highly superficial [...] and highly selective" (p. 89, n. 19), and a reader who's not already familiar with these issues won't learn them from here. (To get a grip on them, additional to the readings suggested by Belot (p. 89, n. 19), let us recommend—especially for philosophers—Sections 5 and 6 of Curiel (2023).)

The chapter ends with a brief discussion of some physicists' hope that a suitable gauge-gravity correspondence will resolve the information loss paradox, followed by a sober reflection that finding such a correspondence in the positive Λ regime has proven extraordinarily difficult, partly due to the fact that the physical significance of disconnected conformal boundaries (which arise from, for example, dS) hasn't been sufficiently understood.

Chapter 6, which deals with *asymptotically* de Sitter spacetimes, begins with some pedagogical remarks about the potential impact of a constant

[16]See, for example, Ben-Menahem (2006), Duerr and Ben-Menahem (2022), and Dürr and Read (2024).

[17]Parikh et al. (2003, p. 1), quoted on pp. 92–3.

[18]Admittedly, one might adopt a weaker reading of black hole complementarity, namely as an operational principle to the effect that no observer will be able to conduct an experiment resulting in a violation of quantum theory. Read in this way, black hole complementarity might well be plausible, and able to assuage the most dangerous threat brought about by the information loss paradox (or its Page-time version to be precise; see Wallace (2021) for the relevant distinction), namely a detectable violation of quantum theory. But those who want something more from physical theories than observational adequacy, and do not wish to resort to some controversial metaphysical doctrines resembling fragmentalism (see Fine (2005)), would most likely remain dissatisfied. For further discussion of these issues, see Muthukrishnan (forthcoming).

term on the qualitative behaviour of solutions to partial differential equations (with an eye on the impact of Λ on the solutions to (1)). Then, Belot invokes passages from Geroch (1977) and Penrose (1983) to illustrate the importance of, as well as the difficulties connected with, the study of 'isolated bodies' and the asymptotic behaviour of spacetimes in general relativity. The situation in GR is contrasted with that in Newtonian gravitational physics, where an 'isolated body' is one such that "its gravitational potential [...] falls off as $1/r$ [approaches zero]" (p. 107). The discussion is vivid and sufficiently detailed for the book's purposes, but—before we move on—let us note that the Newtonian picture is more complicated than Belot portrays it. For Newtonian gravity admits a formulation that dispenses with the gravitational potential and encodes gravitational effects in spacetime curvature (namely, Newton-Cartan theory), and it's likely that the relativistic difficulties with the notion of an 'isolated body' will carry over to that setting.[19] Since some philosophers argue that it's the Newton-Cartan theory which 'gets things right' (as far as Newtonian gravity goes), the issue need not be merely technical.[20]

Setting Newtonian gravity aside, Belot's main methodological point—following Geroch (1977)—is that an adequate definition of asymptotic structure should play a specific theoretical role: in particular, its associated symmetry group should be able to recover conserved quantities necessary to study physical phenomena that one wishes to model with such spacetimes. This point is illustrated (pp. 109ff.) by two examples: a positive one, and a negative one—both intended to be used to model gravitational radiation.

The positive example is the now-standard definition of (strongly) asymptotically Minkowski spacetimes, which are used to study gravitational radiation. They are defined as solutions to (1) with $\Lambda = 0$ that admit a conformal completion suitably similar to that of Minkowski's own (p. 109, n. 20) and with appropriate fall-off conditions for the stress-energy tensor. The symmetry group of the equivalence class of conformal boundaries of such spacetimes is the infinite-dimensional BMS (Bondi-Metzner-Sachs) group, which extends the Poincaré group by the so-called 'BMS-supertranslations' (which are translations along integral curves generated by a distinguished null vector field on the conformal boundary). The BMS group is the correct fit for the

[19] For an exposition of the Newton-Cartan theory, see Malament (2012, ch. 4). We elaborate on this point further in footnote 21.

[20] For a defense of the superiority of the Newton-Cartan formulation, see Knox (2014).

job: unlike the larger diffeomorphism group, it is sufficiently structured to determine an algebra of conserved quantities necessary to study the relevant physical phenomena. And, unlike the smaller Poincaré group, it allows for a flux of conserved quantities to go through the boundary, as one would expect with gravitational radiation.[21]

The negative example consists in the natural definitions of asymptotically *de Sitter* spacetimes ('natural' in the sense that they mirror definitions for the Minkowski case), which—Belot argues—are not suited to model gravitational radiation. For the equivalence class of conformal boundaries of *weakly* asymptotic de Sitter spacetimes is too broad: it has the diffeomorphism group as its symmetry group. And the equivalence class of conformal boundaries of *strongly* asymptotically de Sitter spacetimes is too narrow: its symmetry group implies that no flux can pass through the boundary. A related problem with those definitions, in contrast to the asymptotically Minkowski case, is that the lack of global time translation prevents a natural definition of mass (*qua* a quantity conserved under those translations). Even though for compact binary systems that are usually modelled by astrophysicists, one can safely brush off the complications arising from the positive cosmological constant, there are cases of physical interest when this will not be possible, such as the gravitational wave memory effect, or mergers of supermassive black holes.[22] So, how to make sense of mass and gravitational radiation in the $\Lambda > 0$ regime is, indeed, a pressing question. On a more philosophical note, we remark that the disparity between these regimes invites questions about the extent to which one could carry over the analogies between gravitational and electromagnetic radiation drawn in the asymptotically Minkowski sector by, for example, Gomes and Rovelli (2024).

In **Chapter 7**, Belot discusses the concepts of stability, instability, and (to a lesser extent) genericity, with a particular focus on the role they play in different cosmological constant regimes, and hints at philosophical and conceptual questions arising from these observations. After a brief, history-oriented introduction and a quick review of the notions of 'equilibrium', 'stability', and 'asymptotic stability' of solutions to ODEs (with elementary ex-

[21] Prompted by the parallel between GR and Newton-Cartan theory from the previous paragraph, one might ask whether there is a Newton-Cartan version of the BMS group. The answer is 'yes' (see Batlle et al. (2017) and Delmastro (2017)), and—we think—it shows that the parallels between these theories run deeper than one might initially expect.

[22] See Ashtekar et al. (2015) for further details.

amples), Belot turns to PDEs (with, of course, a specific focus on (1)—which, in any given coordinate system, has the form of a system of second-order non-linear PDEs).

The proper discussion begins with a distinction between stability of a system of equations (so-called 'Cauchy stability') and stability of a solution to some particular system of equations (so-called 'global stability'). In the former case, the equations are Cauchy stable just in case they have a well-posed initial value problem—that is, when solutions to these equations vary continuously whenever initial data uniquely generating them do. In the latter case, a solution to some system of equations is globally stable whenever, roughly, any initial data set similar to the initial data for the original solution yields solutions sufficiently similar to the original one. Belot notes that both definitions make essential use of the notion of initial data sets, as well as full solutions, being "similar" or "varying continuously", which presupposes a particular choice of topology on the space of initial data sets, and the space of full solutions. Yet, different choices can lead to different outcomes, and—moreover—the prospects for selecting a single, most appropriate, all-purpose topology are dim.[23]

Importantly, the question of choosing an appropriate topology for such spaces is relevant not only to those definitions. As Belot notes (pp. 122–3, Question 7.1), some physicists are tempted by the idea that physically relevant properties of spacetimes are exactly those that are 'stable' under perturbations of either initial data sets, or full solutions. We'll call this idea the 'stability-physicality link'. But to make sense of these remarks, one has to first settle the question of choosing an appropriate topology (or, perhaps, turn the tables and say that the choice of topology depends on what properties are taken to be the physically relevant ones, or—differently still—that the arrow of dependency goes both ways). Moreover, different rationales for such a stability-physicality link lead to different puzzles.[24] Another issue raised by

[23] For a further discussion of this issue, see Fletcher (2016).

[24] For example, if one treats the stability-physicality link as an epistemological, or a methodological, remedy for the intractability of unstable inverse problems, then there is a question to what extent this might be a matter of absence of sufficiently powerful mathematical techniques, and, as Belot points out (pp. 127–8, Question 7.6), there are various regularization methods that allow one to overcome such problems in some domains. This is true, but we would like to point out that this still does not address the main epistemological/methodological motivation for the stability-physicality link, namely the imprecision of measurements and observations. For further discussion of the principle of stability (understood as an epistemological principle), see Fletcher (2020).

Belot is whether there are interesting physical theories whose equations are not Cauchy stable (p. 127, Question 7.5), and how such theories mesh with a counterfactual similarity-based account of causation *à la* Lewis (2000).[25]

After posing this chain of philosophical questions, Belot goes on to report that the Einstein static universe is globally unstable (as shown by Eddington in 1930), whereas Minkowski and de Sitter spacetime are globally stable (he also provides some insightful comments and useful references along the way).

Then, Belot turns to the 'Cosmic No-Hair Conjecture' (p. 133), which explicates the sense in which de Sitter spacetime is a 'powerful attractor' for late-time cosmology. More precisely (but still rather informally), the conjecture states that, for reasonable matter sources, generic geodesically complete solutions for (1) with $\Lambda > 0$ are such that certain distinguished spatial slices for late-time observers will become more and more similar to flat spatial slices in the cosmological patch of de Sitter (discussed, recall, in Chapter 2). The restriction to 'generic' (or 'typical') solutions is necessary: the Einstein static universe, as well as Nariai spacetimes, do not meet the conjecture's criteria, but are not treated as counterexamples to it due to their alleged 'non-genericity'. Of course, there's the question of how to explicate such notions formally. Genericity can be treated measure-theoretically (where 'generic' solutions will form a set of measure one in a given measure space of solutions) or topologically (where 'generic' solutions will form a set that is open and dense in a given topological space of solutions). But, even setting aside the fact that these treatments need not be equivalent, there are serious technical obstacles facing either approach.[26] Belot also remarks that the notion of genericity is employed heavily in the Cosmic Censorship conjectures (pp. 134–5, Question 7.7), although he does not connect this issue explicitly with another major related philosophical chestnut, namely determinism. The chapter closes with a report on the state-of-the-art results on the global (in)stability of AdS.[27]

In **Chapter 8**, Belot turns his attention to a slightly more well-trodden issue in the contemporary philosophy of spacetime: namely, the putative un-

[25]We'd like to remark that the first part of this question has been explored (and answered in the affirmative) by, for example, James (2022), and Read and Cheng (2022).

[26]Topological obstacles are discussed above. For measure-theoretic obstacles, additional to the references cited by Belot (p. 135, n. 51), we recommend consulting Curiel (2016).

[27]We add that there are some exciting recent results that relate the Strong Cosmic Censorship to the negative value of Λ, which Belot does not discuss. For a review of those results, see Kehle (2021).

derdetermination of spacetime's topology by all possible observations. This question goes back at least to Glymour (1972), who writes that

> [...] for each of a class of fashionable cosmological models there is another (unfashionable) model different from the first in the topology it ascribes to space-time, and there are good reasons to think that any two such cosmological models are, both in fact and in principle, experimentally indistinguishable. Any bit of evidence which we can account for with one model, we can account for with another, and conversely.[28]

This remark can be supported by various formal definitions of observational (or empirical) indistinguishability together with formal results by Malament (1977b) and Manchak (2009), which show that (rather broad) classes of spacetimes would count as indistinguishable (in the relevant sense) from some other spacetimes with different topological structures.[29] As Belot notes (pp. 146–8), different precise definitions of 'observational (or empirical) indistinguishability' are not equivalent (and, consequently, differ as to how pernicious the resulting form of underdetermination is). Still, since the underdetermination is quite pernicious in all cases, this is not so relevant for the broader philosophical significance of Glymour's remark, namely that we supposedly have no epistemically secure grounds for inferring what is the topological structure of our own universe. But there are two things to note here, and both have to do with the fact that an epistemic warrant for believing a proposition about empirical subject matter (such as one about the topology of spacetime) might go beyond mere observability.

First, the techniques involved in establishing the indistinguishability results that we mentioned produce observationally indistinguishable counterparts of well-known spacetimes that are "irrelevant monstrosities by the standards of working cosmologists" (p. 147), and one might be tempted to dismiss them as 'physically unreasonable'. Yet it is notoriously difficult to precisify and defend this claim. First, Manchak (2011) pointed out that what counts as 'physically reasonable' cannot be justified by merely inductive reasoning. Second, as argued by Butterfield (2014, §3.2), the usual auxiliary principle that attempts to cut down on underdetermination in the case of the actual

[28] See Glymour (1972, p. 195). This passage is quoted by Belot on p. 138.

[29] Manchak's result only aims at producing indistinguishable non-isometric spacetimes, but it is clear from the construction in his proof (2009, p. 55) that these spacetimes will also be non-homeomorphic.

world (namely, the so-called 'Cosmological Principle', which will return below) has a dubious epistemic status itself. Finally, an appeal to an otherwise plausible idea that every physically reasonable feature of a system must be accounted for by some physical process—championed recently by Cinti and Fano (2021)—might not cohere so well with a relativity-friendly thought that spacetime does not 'come into being' through any dynamical physical process, but simply is there just the way it is.[30] Perhaps one might try to save this remark by arguing that simplicity or elegance are the guide to physical reasonability, or by responding to some counterpoints mentioned above. But we'll leave it here.

Second, and perhaps more importantly, one might be warranted in believing some proposition over its empirically indistinguishable counterpart through considerations about the typicality (or genericity) of the observations that confirm these propositions (recall the discussion in Chapter 7). More concretely, if I have the same evidence for mutually exclusive propositions p and q, but I know that this evidence counts as 'typical' for p, but 'atypical' for q, it seems that I am epistemically justified in believing p rather than q. Belot illustrates this point with an example of two observationally indistinguishable two-dimensional spacetimes whose geometries are composed of strip-like regions with de Sitter metric g_0 and strip-like regions with spatially-deformed de Sitter metric g_1 that agrees with g_0 on the boundary of the strip (pp. 143–4). In one of these spacetimes, there is only one strip-like region with g_0 and infinitely many regions with g_1, whereas in the other the proportions are inverted. Even though these spacetimes are observationally indistinguishable, it seems that measuring local geometry to be g_1 should count as evidence of being in the former spacetime, where such geometry is 'typical'.

We are hedging our claims here on purpose: the intuitions tell us that it 'seems' that we're justified to do so, but known attempts to precisify this intuition yield some troublesome cases, and there are some detractors to this overall view as well.[31] One might wonder, however, to what extent this example is even relevant to the main focus of this chapter, namely the underdetermination of spacetime's *topology*. For whereas it's well-established that we can locally measure the geometry of spacetime in a variety of ways—using, for example, a 'gravitational gradiometer' (see Misner et al. (1973,

[30]This extends a point made by Curiel (2009, p. 26) against the demand that physically reasonable spacetimes be inextendible since no physical process can 'cut them short'.

[31]For further references, see (p. 205, n. 77).

pp. 400–3))—and thus engage in such worries about typicality of our measurements, the possibility of locally measuring spacetime's global topology remains unclear.[32] In any case, the question of typicality will recur more prominently in Chapter 9.

Still, even though it's difficult to find a principled reason for dismissing somewhat convoluted observationally indistinguishable alternatives to our own universe, some might be sceptical of the fruitfulness of such debates insofar as they don't directly relate to contemporary cosmology. As noted by Belot, Glymour's concerns about spacetime's topology were originally animated by the thought that, in universes sufficiently quickly expanding (that is, in universes with $\Lambda > 0$ sufficiently large), there will always be parts of the universe inaccessible to us.[33] Thus, Belot declares that he "seek[s] to institute a back to Glymour movement by redirecting attention towards varieties of indistinguishably endemic to cosmology" (p. 139). To do this, he draws attention to the work of Ringström (2013), which shows that we "can find interesting examples of underdetermination of topology by all possible evidence without straying beyond possibilities taken seriously in contemporary cosmology" (pp. 148–9).

The exposition begins with a highly enjoyable crash course on relativistic cosmological models. Belot dissects the well-known FLRW solutions into a kinematic-geometric part and a dynamical part. The former is given by the so-called "Robertson-Walker manifold", that is, a product manifold of an open interval I of the real line and a space form (that is, a complete Riemannian 3-manifold (Σ, h_{ab}) of constant curvature), with a Lorentzian metric of the form $g_{ab} := -dt^2 + a(t)h_{ab}$, where $t : M \to I$. The latter is given by the standard perfect fluid stress-energy tensor that satisfies the Einstein-Euler equations. Only when a Robertson-Walker manifold satisfies such dynamics is it said to be a "Friedman-Lemaître" solution. And only when $a(t)$ behaves appropriately at early and late times is such a solution said to be a "standard cosmological model" (p. 153). Then, Belot shows that, for Robertson-Walker manifolds based on Euclidean space forms, the existence of observationally indistinguishable counterparts (that are themselves Robertson-Walker manifolds based on a flat, but non-Euclidean, space form) depends on the $a(t)$ factor (pp. 155–8), and reports some facts about the topology of space forms

[32] One will find some discussion of this in Bielińska and Read (2022) and Lemos et al. (2023), which were mentioned already in footnote 10.

[33] For further discussion of this issue, see, for example, Ellis (2014).

(pp. 160–6).

This is followed by a discussion of Ringström's own results, which are motivated by the following insight: the restriction to space forms in the construction of Robertson-Walker manifolds is motivated by the so-called 'Cosmological Principle', which states (roughly) that each observer sees an isotropic and homogeneous space (and, since local isotropy is equivalent to constant curvature for Riemannian 3-manifolds, one should restrict one's attention to space form). This principle, however, is not as well-motivated as it might seem, since it does not follow from our own observations, even jointly with some plausible assumptions about typicality. For we only see that space is *approximately* isotropic and homogeneous, and if we conjoin this fact with the so-called 'Copernican Principle', which states (roughly) that we are not special as observers, all we can infer is that our universe should be spatially *approximately* locally isotropic and homogeneous. And, then, it no longer follows that we should restrict our attention to spatial geometries given by space forms. Moreover, we can relax (or perhaps just alter—Belot isn't clear on this) our assumptions about how to model the matter content of the universe: instead of treating it as a perfect fluid, we can treat it as a collision-free gas. Then, the relevant dynamics will be specified by the Einstein-Vlasov equations (which Belot presents on pp. 167–71).

Now, if we follow this line of thought—that is, liberate ourselves from space forms and adopt the Einstein-Vlasov perspective—Ringström has shown that, for any standard cosmological model (M, g_{ab}) for non-trivial matter with $\Lambda > 0$ and a given Cauchy surface $\Sigma \subset M$, and any compact and oriented Riemannian 3-manifold K, and any $\epsilon > 0$, one can find a solution (M', g'_{ab}) of the Einstein-Vlasov equations with topology $I \times K$ ($I \subseteq \mathbb{R}$), and a Cauchy surface $\Sigma' \subset M'$ such that the causal future $J^+(\Sigma)$ of Σ and the causal future $J^+(\Sigma')$ of Σ' are *approximately* observationally indistinguishable (that is, for any observer in $J^+(\Sigma)$, their causal past will—in a certain technical sense—"come ϵ short of being isometric" (p. 173) to the causal past of some observer in $J^+(\Sigma')$). Belot notes—following Ellis (1971)—that these Cauchy surfaces can be the surfaces of 'last scattering' of light for a given observer, where that observer would not be able to obtain any direct information about the causal past of such a surface, because all light emitted before it would be scattered or absorbed by plasma (p. 159). This motivates the conclusion that Ringström's results illustrate a genuine and highly interesting case of underdetermination of cosmic topology.

We think it would be useful to connect this kind of underdetermination with other discussions of underdetermination in cosmology. First, let us note that Ringström's result might constitute an example of what Pitts (2010) has dubbed 'permanent underdetermination'—that is, a situation in which, for any spacetime model (here, in a given sector of a theory), there will always exist another model which is empirically distinguishable from it, but which is also arbitrarily close in terms of its empirical content, so that the underdetermination is more than merely transient.[34] Going forward, it would be interesting to compare this case of permanent underdetermination (if it is indeed so) with the various cases considered by Pitts. Second, we would like to flag that there is another case of underdetermination is contemporary $\Lambda > 0$ cosmology, which invites a host of philosophico-foundational question, (and so is highly relevant to Belot's project!), namely the underdetermination of dark energy physics.[35] How this kind of underdetermination relates to the underdetermination of geometry and topology treated by Belot seems to us to be an issue worth exploring.

In **Chapter 9**, which is the final chapter of the book, Belot considers the epistemological-sceptical challenge posed by so-called 'Boltzmann brains'. The challenge is this (we won't quite follow Belot's presentation here): it seems that contemporary cosmology, in conjunction with quantum statistical mechanics, implies that there are (or will be and have been, if one prefers tensed language even in light of relativity theory) numerous distinct microphysical histories of some systems in the universe that instantiate the macrophysical history of my brain (plus, possibly, some extra things necessary for mental activity to take place) over a relevant period of time. If the mental supervenes on the (macro)physical, such systems, called 'Boltzmann brains', will have the same mental states as I do over the relevant period of time, which include: beliefs about the age of the universe and adequacy of current scientific theories, sensation of pleasure in the fingertips when typing this review, and memories of a failed attempt to make an omelette for breakfast.[36]

[34]As Pitts (2010, p. 264) writes in the context of models of different competing theories, "it is helpful to observe that the physics literature suggests by example several slightly weaker notions of empirical equivalence that, being weaker, are immune to the strategy of being identified as one and the same theory and hence not rivals, yet strong enough that there is no realistic prospect for distinguishing the two theories empirically."

[35]See, in particular, Wolf and Ferreira (2023) and Wolf et al. (2024).

[36]In Remark 9.2, Belot rightly criticises various possible responses to the Boltzmann brain puzzle that rely on extending the necessary period of time for a macroscopic system to count as "having mental activity" in the relevant sense. This section also contains a

For such Boltzmann brains, however, these mental states would not be veridical, because—among other things—they would not be causally connected to their contents in the salient sense (for example, there was no microstate instantiating the 'failed omelette' macrostate in the relevant time-period), and thus *cannot* constitute 'evidence' or 'knowledge'. This invites two interrelated questions. First, how do I know that *I* am not a Boltzmann brain and that (at least most of) my mental states are veridical? The second question (less pronounced by Belot until p. 197 and Question 9.9 on pp. 203–4) is this: if a scientific theory gives me reasons to believe that my mental states are not veridical—because, for example, it predicts numerous Boltzmann brains—then it seems to be self-undermining, for any evidence for the theory is classified as 'non-evidence' by the theory's own lights. Should I always reject such theory?

After some opening remarks, Belot gives a lightning tour of Boltzmannian statistical mechanics (pp. 181–4) which underpins the discussion of Boltzmann brains, including the notions of thermodynamic equilibrium, entropy, Poincaré recurrence, ergodicity, effective ergodicity, the relationships between them, and their consequences for the behaviour of macrophysical systems (all in the classical setting so far). Unlike the earlier discussion of black hole thermodynamics in Chapter 5, this lightning tour is quite self-contained and seems sufficient to give a grasp of what's going on to a reader unfamiliar with the theory. A particularly illuminating part of Belot's discussion, the significance of which re-emerges in the later part of the chapter, is that Poincaré recurrence is compatible with periodic dynamics, which—Belot claims—is, in turn, compatible with the lack of Boltzmann brains.[37] So, to establish the threat of Boltzmann brains, one needs something else, namely effective ergodicity.

In what follows, Belot describes how physicists such as Eddington and

prima facie puzzling remark that Boltzmann brains would still pose an epistemological challenge even if the mental did not supervene on the physical (p. 190). However, as Belot has clarified in personal correspondence, the phrasing of the remark is misleading: its aim was merely to reinforce the point that Boltzmann brains would still pose a problem even if the mental states did not supervene on *brain* states, but only on more spatiotemporally extended physical systems.

[37]Belot justifies his claim with an observation that inhabitants of universes with periodic dynamics might not be "in the grip of surprisingly many false beliefs about the past and the future" (p. 182). What he means by this, presumably, is that inhabitants of such periodic trajectories will share sufficiently robust parts of macroscopic histories that sufficiently many of their beliefs will be veridical (and thus we avoid any interesting sceptical scenario).

Feynman invited us to take the Boltzmann brain hypothesis seriously, and then how Eddington accepted a resolution of the puzzle with the help of the expanding universe. For that kind of cosmological model vindicates the possibility that no given physical system must return to its initial state, since a sufficiently rapid expansion of space can simulate the scenario where the system evolves in an infinite space (p. 194). As Belot notes, this resolution was widely accepted until the work of Dyson et al. (2002) who argued, on the basis of some assumptions about the viability of observer complementarity and the significance of the holographic principle, that the salient spatial slicing of de Sitter is given by the static patch, in which case the quantum version of Poincaré recurrence applies, and the possibility of Boltzmann brains recurs (or so it seems).

There are two things to note here, at least initially. The first is that—just as in the classical setting—quantum Poincaré recurrence might be insufficient for securing the existence of Boltzmann brains, and Belot offers a highly illuminating discussion of what this possibility hinges on in his Remark 9.3 (pp. 197–9). The second is that the idea of observer complementarity (mentioned in our discussion of Chapter 5), which Dyson et al. (2002) use to justify the restriction of the quantum state to the static patch of dS, is peppered with an operationalist, observer-dependent outlook on quantum theory that many (most?) researchers in quantum foundations do not find appealing. Moreover, there is a live and interesting question to what extent less speculative physics/philosophy, such as the standard ΛCDM model, gives rise to Boltzmann brains. This, as pointed out by Carroll (2017, sec. 4), to large extent depends on the interpretation of the quantum vacuum state of dS (and, more generally, of a stationary quantum state). We think that focusing on that better-understood regime would be welcome.

Belot closes off this chapter, and the whole book, with a slew of questions (thirteen!) which relate to some of the points we raised above, as well as much more, including: various alternative routes to the problem of Boltzmann brains, whether it is rational to believe in being a Boltzmann brain given one's total experience, and the question of dealing with self-undermining scientific theories mentioned above.

At this point, then, let's step back a bit and make some general points on our overall impression of the book.

First, let's return to the main stated aim of the book stated in its Preface—that is, to present, in a compact and comprehensible form, a series of technical results from contemporary theoretical physics that Belot regards as being of philosophical and foundational interest. This is, most definitely, done extraordinarily well. The relevant results are introduced with Belot's characteristic ability to maintain a good balance between rigour and insight, and his pointers at philosophical and conceptual questions that arise from such results are often non-obvious, and always intriguing.

On the other hand, one can question whether Belot was quite right to set those aims as he did. In particular, there is very little attempt to answer the questions that Belot sets out: usually they are stated but not returned to, and there is little philosophical or conceptual through-line to the piece. We agree that limiting oneself to teaching and stimulating the reader is a noble aim and not so easy to achieve.[38] But given Belot's philosophical acumen (Belot (2006, 2011, 2018) are just a few highlights) and his ability to put his points crisply and tersely (as witnessed by, for example, his Remark 9.3), the reader would certainly like to hear his takes on at least some of the philosophical questions which he poses. We hope that our review evinces that, sometimes, first-stab philosophical explorations of the questions raised in the book are not so difficult to provide. So, in our view, five or ten more pages at the end of each chapter, with a brief discussion of some of the philosophico-conceptual issues raised, would have been very welcome.

We have a couple of further, although rather minor, points to make. First, in the Preface, Belot pitches the book to graduate students in philosophy. This undersells the technical level of the book, which realistically will be appropriate only for technically-minded graduate students who have already studied general relativity and differential geometry roughly at the level of the first half of Wald (1984), and have a significant degree of mathematical maturity (at least for philosophy students). Second, some editorial aspects of the book aren't fully satisfactory. In particular, the Index contains some amusing, although rather miscellaneous entries, but lacks a number of highly relevant ones, for example: 'curvature', 'stability', 'ergodicity', 'cosmic censorship', or even 'cosmological constant'. This is not to criticise the occasional wit of the book, which—in addition to all the virtues we've described already—makes it a fun read. But we think that it shouldn't come at the expense of usefulness to the reader.

[38] And it has great predecessors: recall the Preface to Wittgenstein's *Philosophical Investigations*: "I should not like my writing to spare other people the trouble of thinking. But, if possible, to stimulate someone to thoughts of his own" (Wittgenstein 1968).

In any case, none of this should detract from the fact that Belot's book will surely become the go-to resource, both for philosophers of physics and (ideally) philosophically-minded physicists, on all things de Sitter. We very much hope that the book will promulgate an expanding research programme in the philosophy of physics, and we look forward to seeing its fruits in the years to come.

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