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1 Overview

Although the thesis consists of 4 papers published independently of each other, they jointly form an integrated piece of work, with a coherent argumentative arc and a unified philosophical point of view. In particular, I am impressed by the way that the elaboration and defense of the central themes common to all the papers evolves and strengthens over the course of time, while remaining true to the spirit at the start. This shows a maturity and integrity of philosophical vision rare in one at the start of his career. His analysis and arguments, even when not entirely on the mark,

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always evidence a clarity of thought, a sober reflectiveness, a capacity to critically reflect on his own thoughts, and an enthusiasm for questioning what may seem obvious that I think of as four of the most necessary traits of a good philosopher.

Doboszewski has an enviable command of the technical material. His work, evidenced by these four papers, shows as well a great sensitivity to the complex interplay between philosophical and technical issues one must master to do good work in the philosophy of physics. That is especially rare in a young researcher who is so technically gifted as Doboszewski is. Too often, I find, technically gifted students lose themselves in dry formal detail and rigor, losing sight of the philosophical meat. That is definitely not Doboszewski, much to the benefit of his work as a philosopher.

Having said that, I must temper my praise with two global criticisms, albeit, ones that are minor in comparison with the virtues of the work. First, although his command of the technical material is superb, Doboszewski can be sloppy in his exposition of it. (I will give examples of this in my review of the individual papers below.) This is problematic for two reasons: a reader who does not know the material so well as Doboszewski may get misled into believing something that is not true; and it demonstrates that he has not taken the time to read his written work with a careful and critical enough eye. The second criticism is that, although his discussion of the more traditionally and overtly philosophical questions is rich, insightful and illuminating, it is sometimes too brief and sketchy, more suggestive of general ideas than completed arguments. I hope and expect to see more sustained and in-depth treatment of some of the philosophical issues in future work of his.

I list here a few overarching questions I have, common to all the papers, before discussing the virtues and problems of the individual papers below, and questions I have particular to each.

1. What are the relations, technical and conceptual, among all the different characterizations of determinism and indeterminism that Doboszewski discusses in the 4 papers?

2. Given that general relativity is not a fundamental theory, what is the status of the conclusions about determinism and physical reasonableness in general relativity? In other words, what exactly are we learning about the theory, about the structure and content of our knowledge in physics, and about the actual world from the arguments?

3. What are the possible lessons of this work to an adequate understanding of modality in physical theory in general, and in general relativity in particular? In what senses, if any, are spacetimes concluded to be “physically unreasonable” possible or not possible? Since one of the lessons of the dissertation as a whole is that “physical reasonability” is perhaps best understood as a context-dependent notion, does that mean that possibility ought to be as well?

4. What are possible applications of the work on determinism and indeterminism to semi-classical gravity and quantum gravity, in particular to the black hole Information-Loss Paradox?

5. Can Doboszewski cleanly separate the methodological, the epistemological, and the metaphysical lessons of his arguments and conclusions?
Some of these questions are addressed, either directly or indirectly, in some of the papers, but I feel that Doboszewski can have much more to say about all of them, and I would like to hear it.

2 “Non-uniquely Extendible Maximal Globally Hyperbolic Spacetimes in Classical General Relativity: A Philosophical Survey”

This is an excellent review of the state of the art for the nexus of problems the paper addresses, the existence of non-unique extensions to maximally developed globally hyperbolic spacetimes, with regard both to the physics and to philosophical questions that the existence of such extensions naturally raise. It is not a review, however, but makes a positive and valuable philosophical contribution in its own right, in several ways. In particular, it brings to the attention of a wider philosophical audience a class of phenomena appearing in general relativity that is manifestly important for an appropriately comprehensive discussion of problems of determinism in current physical theory, and also of what counts as physically reasonable with regard to the models a theory provides for the family of physical systems it purportedly treats. Over and above that, and more importantly, it provides a sustained discussion of how the existence of such spacetimes ought to inform our conceptions of determinism and physical reasonability in general relativity. It also makes contact with and extends in important ways the debates in the previous literature on singularities, time machines and cosmic censorship in general relativity.

In particular, I think that Doboszewski’s suggestion that we use Butterfield’s schema for expressing a generalized notion of Laplacian determinism, in conjunction with using maximal globally hyperbolic developments (‘MGHDs’) as the relevant spacetime region to make Butterfield’s schema concrete, constitutes real progress in the debate about determinism in general relativity. Still, I would have liked to have seen a more extensive and detailed discussion of exactly why MGHDs are appropriate for playing this role. I also think that the list, in §11.3.1, of common features of known non-unique extensions of MGHDs is excellent. I would like to see more discussion of it.

These are the questions I have about it. (All page numbers refer to the published version.)

1. I am troubled by the distinction Doboszewski introduces early in the paper (p. 194), between “determinism in a world” and “determinism in a theory”, and the use he puts it to later. Although this is a popular and influential distinction made in discussions of determinism, I am skeptical that it does any real work. The given definition does not adequately capture an interesting notion of “determinism in the world”, at least not as baldly stated. What it rather captures is an idea of determinism with respect to a fixed class of worlds serving as a standard of comparison. In other words, it depends from the start on a fixing of the class of possibilities we are permitted to countenance, so the property being characterized is not an intrinsic property of the world \( W \) at all, but rather a property of the class of possible worlds \( W \). If \( W \) is trivial—say, containing only \( W \)—then \( W \) is trivially deterministic. How are we to guarantee that \( W \) is not trivial? The obvious—and only reasonable—answer is that we do so by making reference to our best physical theories. If this class is not determined by a physical theory, then it is not clear to me why it is relevant in an analysis of determinism.
that purports to make substantive contact with physics. If it is determined by a physical
theory, then I do not see how it differs from Definition 2, except trivially, in that the state
description a theory gives may not be usefully thought of as an “entire world”. Even that
last difference is not one that matters, however, since in considering the import of Definition
1 and its possible relation to physics, we presumably restrict attention to theories whose
models are appropriately thought of as “entire worlds”. The definition of “determinism in
a world” is clearly trying to get at a “metaphysical” notion of determinism, as opposed to
the “epistemological” notion that the definition of “determinism in a theory” attempts to
capture. Unless the metaphysics can guarantee that \( W \) is both large enough to make the
comparison class interesting and at the same time grounded in and informed by real physics,
it is difficult to see why anyone would care about the definition. If it is grounded in and
informed by real physics, however, then it is difficult to see why it will not simply collapse
into the definition for theories. In particular, I cannot see that it supports the work he wants
it to do, distinguishing propositions about determinism in the actual world from propositions
about the conceptual and modal structure of general relativity as a theory. Restricting \( W \)
to be a class of “physically reasonable” spacetimes, moreover, won’t solve the problem: it is just
a restriction of the definition of “determinism in a theory”, and, without a characterization
of possibility independent of the proposed notion of “physically reasonable”, tells us nothing
about the “metaphysics” that the definition of “determinism in a world” was intended to
capture.

2. The characterization of indeterminism on p. 195 is restricted to temporally orientable space-
times. Does this mean that the form of determinism being characterized here does not
apply to purportedly “atemporal” theories, such as certain programs of quantum gravity? or
ordinary thermodynamics, on some construals?

3. On p. 197, Doboszewski suggests that the existence of singularities may be thought of as
some form of indeterminism in general relativity. It is not clear to me why anyone would be
tempted to think of the bare existence of singularities (incomplete, inextendible curves) in a
relativistic spacetime as a form of indeterminism. Ought we also think of the formation of
singular caustics and cusps in geometrical optics, or the divergence of kinetic energy in some
models of Navier-Stokes fluids, as indeterminism? Except in examples where determinism
fails for more straightforward and standard reasons (failure of the initial-value formulation,
formation of a Cauchy surface, etc.), every case of a singularity in a relativistic spacetime I
know is determined by initial data. I strongly endorse a pluralistic attitude to conceiving of
determinism (as I approvingly discuss with regard to the paper “Relativistic spacetimes and
definitions of determinism”, in §4 of this review below), but, still, there are limits to what
applying the label to a given family of phenomena can profitably bear.

4. What about a distinction about “local” and “global” notions of determinism? General relativ-
ity, I claim, is locally deterministic, in a wide class of models, in the following sense: in every
relativistic spacetime satisfying some causality condition \( C \), every point has a neighborhood
such that:

a. an achronal surface in the neighborhood, of co-dimension 1, passes through the point;
b. the domain of dependence of that surface lies entirely in that neighborhood, and it has a unique extension lying entirely in the neighborhood.

That such a \( \mathcal{C} \) exists can be seen by the fact that the claim (trivially) holds for globally hyperbolic spacetimes, and not for spacetimes that violate strong causality. (I conjecture that \( \mathcal{C} \) is strong causality, but the “obvious” proof doesn’t work; one may need to weaken strong causality so that given a point and a neighborhood, there is a non-zero infimum on the minimal spacelike distances between the point and all timelike curves in the neighborhood that re-enter it, except that timelike curves passing though the point and re-entering the neighborhood have the distance calculated on the part of the curve not passing through the point.) Would the worries about indeterminism be assuaged if we restrict attention to local determinism, and not worry about global phenomena?

5. On p. 196, Doboszewski claims, “models most useful for the description of our world (FRLW spacetimes) are (at least in the classical theory) inextendible beyond the maximal globally hyperbolic region, which in turn strongly suggests that the world itself is deterministic.” That is not correct. If one allows “exotic” (but still classical!) equations of state for the cosmic fluid, then one can find FLRW spacetimes that are extendible (Harada, Carr, and Igata 2018). Even putting aside the issue that the claim in the quoted text relies on the problematic definition of “deterministic in a world”, this seems to contradict the spirit, if not the letter, of the claim.

6. On p. 199, Doboszewski mentions the initial-value formulation with initial data posed on a null rather than a spacelike hypersurface. It is known that these two formulations are inequivalent, in the sense that there are spacetimes with a well posed initial-value formulation in 3+1 that are not well posed in the 2+2 double null formalism (Geroch 1978). (To the best of my knowledge, the contrary is not known.) I would like to hear his thoughts on how this discrepancy may bear on issues of deciding whether a given spacetime is deterministic in something like a Laplacian sense.

7. Throughout the discussion starting on p. 199, of whether general relativity is deterministic in Butterfield’s sense, using MGHDs as the relevant spacetime regions, Doboszewski speaks of the existence of a definite number of non-isometric extensions to a given extendible maximally globally hyperbolic spacetime. Strictly speaking, every extendible spacetime has an uncountable number of non-isometric extensions, so long as one allows the extensions to be at most smooth: just smoothly deform the metric past the Cauchy horizon any old way one likes. One gets the “definite number” Doboszewski refers to only by imposing further collateral conditions (analyticity, satisfying certain symmetries, smoothly extending previously inextendible geodesics, etc.). What role do these collateral conditions one must assume to get control over the family of possible extensions play in an adequate analysis of determinism? (This fact also contradicts the claim, on p. 205, that “choice of a differentiability class does not seem to influence whether spacetime has non-isometric extensions”—if one demands analyticity, then, e.g., Kerr has exactly one extension, but if one requires only smoothness then there are an uncountable number of them.)
8. On p. 203, Doboszewski says, “one could conjecture that non-uniqueness of a solution requires either that the solution is ‘in both ways’ indeterministic, or that it needs to have curvature singularity in some (but not all) regions close to the Cauchy horizon.” Is there a physical argument in favor of this conjecture, or is it just a generalization suggested by the list of common features shared by non-unique extensions to MGHDs?

9. In the discussion (§11.3.2) of whether diffeomorphisms spacetimes are physically equivalent, it is not clear to me how the non-uniqueness of extensions under different conditions of isometry relates to the physical equivalence of two given diffeomorphic spacetimes simpliciter. I would like clarification on this.

Many of these questions arise in almost exactly the same form in some of the later three papers. For the purpose of brevity, I will not repeat them below in the sections on those papers. Doboszewski should recognize which questions are relevant to which papers.

The following are places where I feel that Doboszewski is sloppy in the exposition.

1. The classic Choquet-Bruhat and Geroch theorem applies only to vacuum spacetimes. Doboszewski, however, frequently refers to it in contexts that are ambiguous between vacuum and non-vacuum spacetimes. (He does this in the later papers as well.) Since there are no similarly comprehensive results known about the non-vacuum case, I find this misleading.

2. In the definition of future and past indeterminism on p. 195, one needs to specify that $M$ and $M'$ are already maximal in order for the definition not to be trivial.

3. The definition of a relativistic spacetime at the start of §11.1.2 should include the property “connected”; once one adds that, then being second countable is otiose, since it is equivalent to paracompactness under the assumption of connectedness and being Hausdorff. (Indeed, the two are equivalent under the weaker assumption of having at most countably many connected components.) Also, that definition uses the technical term “edge”, which should be defined for consistency, given that much less recherché concepts (such as an extension of a spacetime) are defined.

4. In the definition of a $\phi$-maximal spacetime (p. 196), it is not stated that the spacetime being extended itself has the property $\phi$, but the subsequent discussion relies on that.

3 “Epistemic holes and determinism in classical general relativity”

The recent proposal by Manchak of the definition of an “epistemic hole” marks, in my opinion, an important advance in the attempt to understand the possibilities for different kinds of pathologies that may arise in relativistic spacetimes, for several reasons, among them that it is the first such condition that does not require a modal reference class of other spacetimes in order to characterize a spacetime as satisfying the condition or not. This paper by Doboszewski on Manchak’s condition itself marks a major advance in our understanding of that condition, and thus of the general idea of “holes” in spacetimes and how they bear on the problem of determinism.
The paper starts with a superb review of different characterizations of holes in relativistic spacetimes, and what it may mean for a spacetime to be “hole-free”, with excellent coverage of the problems with the standard proposals. It then describes Manchak’s particular proposal, with great physical and philosophical insight, relating it in an interesting way to the idea of the “physical reasonability” of a relativistic spacetime. Doboszewski then proposes an extension to Manchak’s definition, making it (in a precise sense) symmetric under time-reversal, by appending the temporal contrariety of Manchak’s condition. Given the manifest importance of temporal notions to the question of determinism, this is an insightful and important development, and his discussion of the philosophical relevance of the extension is illuminating. In particular, I think the examples Doboszewski gives of truncated spacetimes manifesting “Doomsday” indeterminism, and the accompanying discussion, is insightful and compelling with regard to the viability of epistemic hole-freeness as a condition that may guarantee determinism of some form.

These are the questions I have about it. (All page numbers refer to the page numbering at the bottom of the unpublished manuscript included in the PhD thesis.)

1. I do not understand the first part of footnote 10, “Note that despite the fact that anti-de Sitter spacetime is not globally hyperbolic, achronal subsets $S$ do have non-empty domains of dependence.” That a spacetime is not globally hyperbolic does not have a fixed logical relation to whether there is an achronal subset with non-empty domain of dependence, so I’m not sure what Doboszewski is getting at here.

2. On p. 9, Doboszewski says,

Unfortunately, epistemic hole freeness does not distinguish between any non-globally hyperbolic extension of Misner spacetime and the same spacetime with a point removed from a non-globally hyperbolic region (or with a point removed from the globally hyperbolic region, for that matter): all these spacetimes have epistemic holes in the sense of EHF(g).

I think that Doboszewski may miss the point here. EHF was not intended, nor should it be thought of as a way, to distinguish all cases of “removing points from a manifold”. I take it that the relevant issue is the following: if a spacetime already has an epistemic hole, then removing a point from it should not change that. And that seems to be the case.

3. On pp. 10–11, Doboszewski says,

A form of general relativistic indeterminism is demonstrated by the existence of spacetimes which are maximal globally hyperbolic, but are extendible in non-globally hyperbolic and non-isometric ways. This can be naturally interpreted as indeterminism, in the sense that there are situations when the initial value problem has non-unique solutions.

It is not correct to think of this situation as a case where the initial-value problem has non-unique solutions. The Cauchy evolution starting from the given initial data breaks down at the Cauchy horizon. One then extends the spacetime (if one can at all) “by hand”, so to speak, and not by continuing to solve the initial-value problem. So the part of the extended
spacetime beyond the Cauchy horizon in no way counts as part of a solution to the initial-value problem. Doboszewski does say something like this in footnote 13, but I think it is a mistake to view what I said as a “possible interpretation” of the situation. It is mathematical fact that extensions beyond Cauchy horizons are not produced by solving partial-differential equations starting from the given initial data that eventuated in the Cauchy horizon.

In that same footnote, Doboszewski raises another possibility for giving a philosophical interpretation of this situation, based on the idea of causal dependence. I find this suggestion potentially rich, and would like to see it developed in greater detail and to greater depth.

4. Doboszewski notes on p. 11 that “all known examples of extensions of extendible maximal globally hyperbolic spacetimes violate generalized epistemic hole freeness.” I conjecture that this is always the case, given the way that null geodesics necessarily must either “pile up” or become inextendible at a Cauchy horizon. I think that this would be an excellent problem to work on. If true, I suspect the proof would not be too difficult.

5. The point he makes on p. 13, that there seems to be an important difference between naked singularities that are produced dynamically and those that are constructed “artificially” by manipulation of the topology and the metric, seems to me to be of great importance and relevance for the problem of determinism. I would like to see it developed more.

The following are places where I feel that Doboszewski is sloppy in the exposition.

1. The caption to Figure 1 (p. 8) says that it is a “conformal diagram”, but it cannot be in any standard sense of that term, as the null lines are not Euclidean straight lines at 45° angles to the horizontal.

2. At the top of p. 13, Doboszewski states that maximal extensions violate chronology as though it were established fact; it is only a conjecture.

3. In footnote 22, Doboszewski says, “epistemic hole freeness should be deemed unacceptable on the grounds of classifying a proper subset of Minkowski spacetime as hole free.” As stated, the claim is not viable, and trivializes the notion. It should rather read (something like): “epistemic hole freeness should be deemed unacceptable on the grounds of classifying a connected, simply connected proper subset of Minkowski spacetime as hole free.”

4  “Relativistic spacetimes and definitions of determinism”

This paper marks a real watershed in Doboszewski’s thought, as this is where he first fully articulates and advocates an attitude of pragmatic pluralism—which I am deeply sympathetic to—with regard to definitions of determinism. He had gestured at and implicitly suggested this view in the earlier papers, but he clearly states the idea and vigorously and convincingly defends it here. I think that alone makes this paper one of the most important in recent years on conceptual problems in the foundations of classical general relativity. He clearly states the virtues of the advocated pragmatic approach in point 3 on p. 2 (as well as spelling it all out in greater detail later). I want
to suggest a friendly amendment: another reason why this pragmatic attitude is potentially so fruitiful for philosophical investigation of these kinds of questions is that it affords us a deeper conceptual understanding of the very idea of “determinism”, both in its own right (*per se*) and as it can and cannot be meaningfully applied to individual physical theories (*per accidens*).

Another important aspect of this paper is that it articulates and discusses to much greater depth and in much finer detail than in previous papers, with a correlative gain in richness of philosophical conception, Butterfield’s schema for definitions of determinism. This also makes the paper particularly valuable. The four features of Butterfield’s schema that he spells out on pp. 10–11 should be attended to by all philosophers who discuss these issues in the future. In particular, Doboszewski’s extended and modified version of Butterfield’s schema (p. 11) marks, I think, significant philosophical progress in the task of providing fruitful explications for the intuitive idea of “determinism”. I particularly admire the way that Doboszewski uses this new schema to argue for his version of pluralism.

These are the questions I have about the paper. (All page numbers refer to the page numbering at the bottom of the unpublished manuscript included in the PhD thesis.)

1. I think the definition of a concept’s “being compatible with a theory” (§1.2, p. 3) is not useful as stated. It makes, e.g., Maxwell fields incompatible with general relativity, since not every spacetime model has a Maxwell field.

2. In §1.2, Doboszewski defends the idea that decisions about whether a theory is deterministic (in some relevant sense) or not should be made with the minimum possible input from metaphysics. I endorse that idea, but his discussion has, I think an important lacuna: that it is important to take into consideration the empirical content of the theory in making such determinations, where by “empirical content” I mean at a minimum understanding exactly how the theoretical formalism makes substantive contact with empirical knowledge gained through experimentation. One should indeed not have to take a position on the metaphysical character of laws of nature in order to decide whether a given equation has a well posed initial-value formulation, as Doboszewski says. But one *must* understand what the initial-value formulation of the theory’s equations *means physically* in order to make substantive connections with possible characterizations of determinism. I would like to see more discussion of how the empirical content of a theory in general bears on such interpretive issues as determinism.

On a related note, I do not think that Doboszewski’s invocation of the substantival/relational debate about the nature of spacetime (p. 3) can motivate the claim that one cannot completely decouple metaphysical questions from questions about determinism, at least not unless one shows that the substantival/relational debate is both well posed and important (which I deny). I suggest that a more fruitful avenue to discussing the relation among metaphysics, empirical content, and determinism can be found in the tripartite classification of kinds of interpretations of a physical theory given in Curiel (2009). I would like to see Doboszewski discuss these questions *vis-à-vis* that classification and the philosophical consequences of adopting it.

3. On p. 8, Doboszewski claims that spacetimes that violate “even very weak causality condi-
tions” do not admit an initial-value formulation. On a natural construal of “very weak”, this is not true: spacetimes with closed timelike curves can have a well posed initial-value problem (Friedman 2004). I would like to see the philosophical consequences of this fact discussed, with regard to the problem of determinism in a Laplacian sense.

4. The fourth “pleasant feature” (“Actual Use”) of Butterfield’s schema for definitions of determinism, which Doboszewski states and discusses on p. 11, is of great interest, and I think the discussion is insightful and helpful, but I have a big, broad and somewhat vague question about it that I would like to see Doboszewski address: when tying a conception of determinism to predictability, how much will depend on how one hooks the formal apparatus of the theory up to experimentation, the source and foundation for all our knowledge of what can and cannot be “predicted” by a theory? How much will our actual, current epistemic limitations (experimental prowess, calculational procedures and tools, computational tractability) bear on the relevant notion of “predictability”? It must to some degree; otherwise it is difficult to see how predictability comes apart from determinism at all.

5 “Interpreting cosmic no hair theorems: Is fatalism about the far future of expanding cosmological models unavoidable?”

This paper, though still touching on and involved with problems of determinism, extends the scope of Doboszewski’s philosophical work to completely new regimes and questions in the context of the foundations of classical general relativity, and so indubitably shows that he is not limited to a narrow set of problems in his capacity for producing compelling and important philosophical analyses and arguments. Doboszewski addresses a set of problems that had not, before this, received any philosophical attention at all (to the best of my knowledge). In introducing these important conceptual problems, grounded in interesting and deep physics, he has done the field of philosophy of physics a great service; he has done an even greater service with the sophistication and depth of the treatment he gives the problems.

I have only a few minor questions to pose and points to make about this paper.

1. On pp. 4–5, in explaining the origin of the moniker “no-hair theorems” for the class of results in cosmology he discusses, Doboszewski to my mind leaves out the most important analogy with the no-hair theorems for black holes: the macrostate of the system is insensitive to the fine details of any underlying micro-structure.

2. I think the argument at the bottom of p. 6, that symmetry assumptions in cosmic no-hair theorems are innocuous because of presumed stability under small perturbations, is suspect. Without compelling technical arguments, all claims about stability of some feature or property of spacetimes under “small perturbations” must be merely speculative, because of the extreme non-linear character of the Einstein field equation, and the great promiscuity in possible spacetime models (all Lorentzian 4-manifolds). Consider the example of the singularity-free cosmological models proposed by Bekenstein (1975); although Bekenstein argued for their stability on heuristic grounds, Tipler (1978) subsequently showed that the lack
of singularities was intimately tied up with the perfect symmetries of the model: introducing inhomogeneities necessarily produced singularities.

3. I think the following, in footnote 8, is a typo: “Presumably once energy density of material sources becomes of Planckian order, quantum fluctuations will become the dominant contribution...” The limit of energy densities at issue here is one where the density decreases; Planckian energy density is extremely large.

4. I do not understand this claim, in footnote 9: “it seems that insofar as classical general relativity is concerned, we could be living in a universe with a negative value of the cosmological constant.” All observational evidence overwhelmingly favors a strictly positive value for the cosmological constant, so I’m not sure what Doboszewski is getting at here.

5. In footnote 12, Doboszewski offers the tantalizingly brief suggestion that the property established by cosmic no-hair theorems may be best thought of as neither local nor global (on standard construals of those terms), but rather as quasi-local in some sense. I would very much like to see this idea followed up on, and its philosophical consequences discussed. In particular, I think it would be extremely useful for philosophical purposes to have a good explication of “quasi-local” in the context of general relativity, similar to Manchak’s for “local”.

6. I don’t get what Doboszewski seems to be suggesting is the full force of the problem that black holes introduce, as he discusses it in §5.2. Consider the following: the cosmic no-hair theorems show (or strongly suggest) that any appropriately chosen region to the future is either locally de Sitter or else is contained in an event horizon. Why doesn’t that satisfactorily address the putative problem?

7. P. 19: “If \( \Lambda \) is fundamentally some non-classical field or a quantum effect, then the classical physics of \( \Lambda \) should be sensitive to the high energy (UV) physics in the late stages of expansion.” What “high-energy” physics is being referred to here?

The following are places where I feel that Doboszewski is sloppy in the exposition.

1. On p. 14, he says, “in gravitational collapse of a spinning axisymmetric body, incomplete geodesics are timelike (although in spherically symmetric solutions singularities are spacelike...).” He conflates here the conformal character (timelike, spacelike or null) of an incomplete, inextendible curve that indicates singular structure from that of the entirety of the singular structure itself. Every incomplete, inextendible geodesic in Schwarzschild spacetime is either timelike or null, but the “singularity itself” is spacelike.

2. A related sloppiness occurs in footnote 21, where he says, “In spherically symmetric black hole spacetimes the singularity is spacelike; hence there is no region \( R \) (of fixed coordinate size) evolving in a way which terminates in a singularity.” The claim makes sense, and is true, only for certain choices of spatial slicings.

3. On p.16, Doboszewski says that the closed timelike curves of maximally extended analytic Kerr occur in regions “beyond the ring singularity”. In fact, they occur generically behind
the Cauchy horizon threading the ring singularities, and so, under no reasonable construal of the phrase, only "behind the ring singularity".

6 Summary

This is a sophisticated and compelling piece of work in the foundations of physics. It marks a real advance in our understanding of determinism in general relativity, a problem of deep significance and, as these papers admirably show, great subtlety and difficulty. These papers will surely form the starting place for all future foundational work on these problems. This thesis indubitably deserves the awarding of the doctoral degree, but, even more, I think it unquestionably merits an award with distinction.

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