Trans-Planckian Philosophy of Cosmology

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[DRAFT]

Abstract

I provide some philosophical groundwork for the recently proposed 'trans-Planckian censorship' conjecture in theoretical physics. In particular, I argue that early universe cosmology is, at least as we usually understand it, autonomous with regards to quantum gravity, the high energy physics that governs the Planck regime in our universe. Trans-Planckian censorship is then seen as a means of rendering this autonomy a novel empirical constraint within contemporary quantum gravity research.

1 A new conjecture

In September 2019, two papers were posted to the physics preprint repository ArXiv, which together introduced the trans-Planckian Censorship Conjecture (TCC) to the community engaged at the intersection of theoretical physics and cosmology [Bedroya and Vafa, 2019, Bedroya et al., 2020]. Although the TCC was originally motivated as a particular swampland conjecture in string theory (more on this in section 4), 'trans-Planckian censorship' is nonetheless poised to become a subject of semiclassical study in its own right at the frontiers of quantum gravity research, semi-independent of questions concerning its formulation as any one conjecture (i.e. as would be relevant in any one candidate quantum gravity approach, like string theory). For good reason! As I will argue, trans-Planckian censorship is *worth embracing, given the local epistemic aims of the community*, in such a way as is independent of any of the details required for an explicit formulation of the conjecture.

Here is the TCC, in slogan form:

> Nature precludes the physical relevance of a large class of semiclassical field theories defined on cosmological spacetimes.

The technical terms in this slogan will be discussed in the following section. Roughly speaking, trans-Planckian censorship stipulates that the high energy physics of the Planck regime is sequestered, necessarily, from any standard descriptions we give of structure formation in our remote past. In other words, this practically rules out the viability that certain quantum fluctuations, which would otherwise encode unknown quantum gravitational physics, ultimately seed the evolution of large scale structure observed today [Bedroya and Vafa, 2019].

One might criticize of the slogan that the language "nature precludes" is obscure. But this is intentional: it emphasizes the peculiar status of the subject of trans-Planckian censorship, given current physics. Namely, whereas facts about string theory might (by the original version of the TCC) entail the conclusion just rehearsed (cf. section 4), the epistemic warrant for that conclusion, in such a case, is conditional on string theory coming to provide an empirically adequate theory of quantum gravity. This is so, *even if we grant that the relevant version of the conjecture is true*, i.e. that the relevant swampland conjecture in string theory is essentially correct. Meanwhile, there are, conceivably, other facts, conditional on the successes of other candidate quantum gravity approaches besides string theory, that could likewise entail the same conclusion, just by other means (i.e. by other versions of the TCC, which we may likewise grant are true). In this sense, the most general, disjunctive form of the TCC— as summarized in the slogan above— will clearly remain open for *at least* as long as we lack an empirically adequate theory of quantum gravity. Only having achieved that sought-after theory will we subsequently be able to assign (by conjecture) a truth-value to trans-Planckian censorship: either that theory supplies such facts as would (by means of one particular version of the TCC) be appropriate to secure the conclusion, or else it does not.[1]

As just presented, trans-Planckian censorship concerns a novel understanding of the relationship between lines of evidence in large scale cosmology and theory development in quantum gravity. In fact, my principle contribution in the following is to provide a perspective on the relationship between lines of evidence in large scale cosmology and theory development in *high energy particle physics*, which is intended to be faithful to how *that* relationship is typically understood, in practice. But, crucially, from this perspective, trans-Planckian censorship is rendered productive for furthering frontier physics research, when one turns to consider the prospects of an analogous relationship between large scale cosmology and even higher energy physics, i.e. quantum gravity. I claim that this upshot is what justifies, as a matter of theoretical physics practice, that trans-Planckian censorship is poised to take on a life of its own at the semiclassical frontiers of quantum gravity research. Namely, the TCC does real, substantive work, for *any* quantum gravity theorist who would endeavor to wield it.[2]

[1] This echoes the observation I have made [Schneider, 2020a] about the status of the cosmological constant problem, prior to achieving a future theory of quantum gravity. It also echoes the textbook discussion about cosmic censorship found in [Wald, 1984, p. 304], provided that the physical reasonableness of a relativistic spacetime is understood as ultimately grounded in facts about quantum gravity. These parallels suggest that there is nothing new, methodologically speaking, about something like trans-Planckian censorship coming to acquire a life of its own at the semiclassical frontiers of quantum gravity research, and to thereby shape ongoing research in the discipline. Moreover, at least in the case of the cosmological constant problem, I have argued [Schneider, 2020b] that this state of affairs can be epistemically well founded.

[2] I will not argue here that there is nothing more to the TCC than this function, and so I do

To the extent that the perspective I provide captures the kernel of the community's interest in trans-Planckian censorship, separate from string theory, the perspective ought to provide a suitable foundation for further philosophical engagement with the general subject. That being said, the elephant in the room is that trans-Planckian censorship is still new, and so it may seem premature to discuss its prospects. This is in sharp contrast with other similar cases of philosophical interest at the semiclassical frontiers of quantum gravity research (e.g. cosmic censorship or the cosmological constant problem— cf. footnote 1), where the legacy of the relevant subject so far in history might appear to be the primary justification for studying it.

To put the point slightly differently, in disanalogy to those other cases, one cannot presently appeal to any descriptive claim about the staying power of trans-Planckian censorship, as motivation for a philosophical investigation of the subject. Moreover, if the TCC is abandoned sufficiently quickly, it likely will not have left enough of a mark on the history of the discipline, for such a mark to merit philosophical scrutiny, in retrospect. And while I would like to think of myself as having an ear to the ground (so as to justify the project, provisionally, on the prediction that such doom and gloom will not be the case), it can be quite difficult to spell out the difference between an act of measured foresight and that of reading tealeaves.

More preferable, then, is to locate a reason that motivates broad philosophical engagement with the new subject, which is not reducible to a prediction about the future winds of research in the discipline. In fact, intuitions along these lines might even pull in the opposite direction: perhaps the most responsible philosophical project concerning trans-Planckian censorship, at this nascent stage, is one which steers any such predictions, in the first place. So, for instance, Dawid [2013] offers a framework for generating confirmation in the absence of novel empirical constraints. Along these lines, one might consider whether there is sufficient confirmation to license trust (cf. [Dawid, 2018]) in trans-Planckian censorship, and thereby serve as epistemic warrant for its uptake, right now. And if (as one might privately suspect) there is *insufficient* confirmation to trust trans-Planckian censorship— at least, unconditional on string theory— one might regard the philosopher's greatest possible contribution to be the act of nipping the burgeoning study of trans-Planckian censorship in the bud. Whereas the TCC might happen to live on (or not) as a particular swampland conjecture within string theory, its status therein would thereby ever remain an artifact of the technical details of that particular candidate quantum gravity approach. The further study of trans-Planckian censorship would simply not be pursued, separate from those technical details.

There are, I believe, two responses to this suggestion. The first is procedu-

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not claim that we ought to adopt a functionalist attitude toward trans-Planckian censorship. But that we might nonetheless see fit to do so poses a tantalizing possibility, which is worth considering in detail at a future time: trans-Planckian censorship is a working hypothesis, specifically within the context of quantum gravity research, about a lack of actual trans-Planckian physics within the early history of our universe (at least, as we usually understand that history).

ral: to engage in such a philosophical project as could nip the study of trans-Planckian censorship in the bud, a certain amount of work is already necessary to interpret the subject, thereby disentangling its particulars from the particulars of intimately related others, e.g. string theory. For instance, as will come up in section 4, the general subject requires certain formal infrastructure, in a given candidate quantum gravity approach, which is similar to that provided by swampland conjectures in string theory. But, importantly, that infrastructure only *needs to be* similar in one particular sense. As such, evidential arguments in support of trans-Planckian censorship may not be as constricted as might otherwise be thought.

This may be a sufficient response to motivate philosophical engagement of the kind I have in mind, and for which the perspective I provide in what follows can serve as a foundation. But the second response is more programmatic—and thereby (I suspect) more compelling. Namely, insofar as wielding the TCC is consonant with the local epistemic aims of the community, independent of its formulation within, e.g., string theory, the pragmatic upshots of trans-Planckian censorship serve, *in themselves*, as epistemic warrant for its uptake. (Though, as I will return to in the Conclusion, it may be that its uptake is better thought about in terms other than belief.) Thus, to have put a finger on the pragmatism of trans-Planckian censorship, as pertains to the epistemic situation of the relevant community, is to simultaneously have located a reason for philosophical engagement with the new subject. Namely, it is the subject's *genuine capacity to shape* future research in the discipline that makes it so interesting, and which justifies philosophers diving in, at so early a juncture, to discuss it.

2 Cosmology and high energy physics

The aim of this section is to clarify the relationship between large scale cosmology and high energy particle physics. As will become clear, this relationship is crucial for understanding how it is that large scale cosmology might empirically constrain quantum gravity research, by means of trans-Planckian censorship.

First, some groundwork. The standard model of cosmology, ΛCDM, constitutes our current best theory of the evolution of large scale structure (LSS) in cosmology. As a consequence of our embrace of ΛCDM, we are committed to the descriptive accuracy of a particular narrative of large scale cosmic history. Namely, to zeroth approximation, our universe exhibits (always strictly positive) 'cosmic expansion', wherein space has been uniformly expanding and cooling for all cosmic time. The evolution of any further, non-trivial LSS is then studied perturbatively, given cosmic expansion in the background.[3]

[3]That is, the study of non-trivial LSS is defined on 'cosmological spacetimes': globally hyperbolic spacetimes foliated by spacelike hypersurfaces satisfying the cosmological principle. The leaves of this foliation are threaded by the integral curves of a unit timelike vector field that is everywhere hypersurface orthogonal (the spatial universe is assumed to be non-rotating), defining cosmic time. These integral curves describe the possible worldlines of stationary observers who bear witness to (possibly degenerate, or even negative) cosmic expansion. For more about these spacetimes and their physical interpretation, see [Malament, 2012, ch. 2].

Consequent to this narrative, as one travels backward through cosmic time about any point, LSS approximately uniformly contracts, growing hotter and hotter. Increasing temperatures, we eventually regard a semiclassical interpretation of quantum field theory on curved spacetime (QFTCS), familiar from high energy particle physics, as descriptively relevant to questions about the formation of any non-trivial LSS (still given cosmic expansion in the background). Sometimes, as in the case of inflationary cosmology (discussed below), these QFTCS constructions are further tasked with explaining, by means of a semiclassical treatment of gravity, cosmic expansion itself during that period of suitably high temperatures. In such a case, we may regard the construction as providing a model of structure formation relevant in our remote past [Azhar and Butterfield, 2017]. The embrace of any one particular model of structure formation is thereby understood to modify what we take to be descriptively accurate of the zeroth-order approximation of the spatiotemporal universe during that early period. In this sense, the model of structure formation replaces the account otherwise given by ΛCDM, restricted to our remote past.

What I am discussing presently is the domain of 'early universe cosmology', as the subject is understood in contemporary cosmological practice. (And so, models of structure formation are, equivalently, models of early universe cosmology). In this domain, the semiclassical observables we care about most are various correlations produced along the surface of last scattering (relative to us) in a period of cosmic history following that of the 'early universe', known as 'recombination'. These correlations at recombination are what we may observe today, via measurements of the cosmic microwave background (CMB).

The state of affairs in early universe cosmology may be contrasted with that which follows it. Namely, in 'late-stage cosmology', the same measurements of the CMB are taken to provide a means to infer suitable initial data at recombination for studying the evolution of LSS ever since. In this context, per the dictates of ΛCDM, a classical, relativistic perturbation theory is taken to be descriptively accurate of the dynamics of non-trivial LSS (given cosmic expansion in the background), instead of some QFTCS construction. This classical theory admits a well-posed Cauchy problem specified at recombination, and is assumed to approximate a fully self-consistent, general relativistic description of the evolution of LSS (i.e. including background) at all times following recombination. The upshot is that we may understand the high energy particle physics of early universe cosmology as *seeding* the evolution of LSS in late-stage cosmology: structure formation in the former constrains the Cauchy problem specified at recombination in the latter. (And so, likewise, the Cauchy problem in the latter constrains structure formation in the former.)

There are a number of provisos one might wish to add to this presentation. The one that will be most important below concerns inflationary cosmology. Broadly speaking, 'inflationary cosmology' refers to a class of scalar quantum field theory constructions on cosmological spacetimes, which are proposed as models of early universe cosmology. Given inflationary cosmology, we revise our initial descriptions about the early history of cosmic expansion in ΛCDM. Instead of a consistently slow expansion, we regard our large scale spatial universe as having undergone approximately exponential expansion for a period of time during our remote past, due (semiclassically) to the adiabatic vacuum effects of an 'inflaton' field (or many such fields). This rapid expansion washes out any interesting classical initial data in the early universe that could otherwise be relevant to the Cauchy problem specified at recombination in late-stage cosmology.

Meanwhile, quantum fluctuations about the vacuum state of the inflaton are assumed to evolve semiclassically, comoving with the background cosmic expansion. Were fluctuations to classicalize with wavelengths smaller than the Hubble radius ('sub-Hubble'), they would rapidly become negligible and wash out with cosmic expansion (like any of the interesting classical initial data just mentioned). But were any fluctuations to classicalize with wavelengths larger than the Hubble radius ('super-Hubble'), they would constitute frozen modes. These frozen modes would persist until the end of the period of rapid cosmic expansion. Having done so, they would thereby come to seed background cosmic expansion in late-stage cosmology, or else they would seed non-trivial LSS, as they re-enter the Hubble radius and unfreeze, prior to recombination (for more details, see [Brandenberger, 2004]).[4]

I will not discuss the details or merits of inflationary cosmology here. As has been stressed by Brandenberger [2009], there exist alternatives to inflation that accomplish many of the same empirical feats. In what follows, this is precisely what I will take for granted, as pertains to any such discussion. There occurs, within the domain of early universe cosmology, some high energy physics responsible for producing all of what is observed in the CMB, thereby seeding the evolution of LSS in late-stage cosmology at recombination. This high energy physics admits of a description as a QFTCS construction (suitably interpreted), and is plausibly captured by a model of inflation. (Here, I set aside as too radical to countenance any alternatives to inflation that do not admit of descriptions as QFTCS constructions. Such alternatives are models of entirely different theories of early universe cosmology, whose embrace by the community could dramatically change the role for trans-Planckian censorship in ongoing research, in ways that are difficult to trace in the abstract.)

[4] What does classicalization look like in this proposal? It is a feature of the quantum field theory construction, given any strictly positive cosmic expansion in the background, that we may split super-Hubble modes into a quasi-isotropic term (i.e. a term which is compatible, on average, with the symmetries of the background expansion) and a noise term. The noise term may be identified as sub-Hubble, and so rapidly becomes negligible (provided that cosmic expansion is sufficiently rapid). But discarding the noise term altogether would amount to coupling the quasi-isotropic term to some trivial environment, in which case the quasi-isotropic term could be treated as a quantum system that has undergone decoherence [Polarski and Starobinsky, 1996]. Taking this background-consistent decoherence as a stand-in for classicalization, that quasi-isotropic term propagates as an initially frozen mode according to a classical perturbation theory defined on the rapidly expanding background. For one dissent against such a decoherence view of the quantum-classical transition in inflation, see [Sudarsky, 2011].

2.1 Autonomy (or the necessary lack thereof)

One might be surprised of the interplay, in the above description, between the semiclassical, high energy particle physics of structure formation and the classical, low energy evolution of LSS. In principle, one might even have liked it to be the case that the study of the evolution of LSS is autonomous with regards to any unknown or speculative high energy physics in our universe. I mean this in the sense discussed by [Batterman, 2018], where, for instance "our theories of 'ordinary materials such as water, air, and wood' work remarkably well despite completely ignoring any structure of those materials at scales below centimeters" [note 3]. So, in the present case, autonomy would mean that the study of the evolution of LSS could be pursued adequately by considering only those quantities deemed to exist at the largest, low energy scales, together with some known (and, presumably, classical) laws that we take to govern those quantities— e.g. those of classical field theories.

Despite any such wish, it is clear that early universe cosmology is, as a sub-discipline within contemporary cosmology, predicated on the denial of such autonomy.[5] The point I would like to stress here, however, is that it is a denial of a very curious kind. In particular, the classical theory governing late-stage cosmology is, evidently, autonomous with regards to higher-energy, semiclassical characterizations of the 'late-stage universe'. Likewise, the semiclassical theory governing early universe cosmology is, evidently, autonomous with regards to any lower-energy, classical characterization of the early universe. Where autonomy is denied is within the late-stage *extension* of early universe cosmology, and also the early universe *extension* of late-stage cosmology. And while we see no reason to take these extensions to be, in general, relevant to our narrative of cosmic history, we nonetheless demand that they agree about conditions that obtain along the shared boundary between them: recombination.[6]

To see what I mean by this, consider the manner by which inflationary cosmology rose to prominence as a program within early universe cosmology. Namely, the uniformity at recombination according to late-stage cosmology was regarded as fine-grained evidence of some distinctly high energy, dynamical process within the early universe, which subsequently gave rise to suitable conditions at recombination (and for which there is no relevant lower-energy physical description). In other words, the details observed within the CMB, as inter-

[5]An important exception to this was Misner's program in chaotic cosmology, which was considered in the early days of the sub-discipline (cf. discussion by Beisbart [2009, §5.5.1] and references therein). Misner hoped that the high degree of spatial uniformity exhibited at recombination is an attractor in the classical dynamics of whatever theory ultimately governs the evolution of LSS. If his program had succeeded, the CMB could be considered, in a sense, generic with respect to initial conditions in our remote past, which were otherwise sensitive to high energy physics. In this way, the evolution of LSS could be decoupled from any high energy physics relevant within early universe cosmology, thereby recovering autonomy.

[6]Bursten [2018] argues that, in certain cases, conditions along a shared boundary between multiple models can play a 'model-connecting function' in multiscale explanations. Recombination seems to fit this bill in the multiscale modeling of LSS in cosmology, even though the conditions that obtain there take the form of initial data (or final data, as it were) within each of the two relevant theories.

preted in the classical context of late-stage cosmology, were assumed, in the context of theory development in high energy particle physics, to be the right kind to rest an intricate, semiclassical account of structure formation within our remote past.[7] Conversely, descriptions given at high energy scales in early universe cosmology were here taken to matter to the Cauchy problem specified at recombination in late-stage cosmology.

This perspective remains popular today. To wit: observations concerning the evolution of LSS in the late-stage universe provide an empirical window into high energy particle physics. In particular, this is because high energy particle physics in the early universe, autonomous with respect to any low energy physics there, is understood to seed the conditions that obtain at recombination. Those conditions are then taken to be relevant to the Cauchy problem in the particular low energy, autonomous classical theory that is taken to be descriptive of the evolution of LSS thereafter.

3 Introducing the Planck regime

In the narrative rehearsed at the beginning of the previous section, early universe cosmology is born out of our reluctance to trust the relevance of a classical theory to the study of LSS, when ambient temperatures are sufficiently high. As just discussed, the converse is also true: we are simultaneously reluctant to trust the relevance of the high energy, semiclassical theory to the study of LSS, when ambient temperatures are sufficiently low.

But just as we swap the descriptive relevance of a classical theory for a model of QFTCS when we enter the domain of early universe cosmology (i.e. when we turn away from questions of the evolution of LSS and toward questions of formation), at least in inflationary cosmology, temperatures eventually grow large enough— as we move backward in cosmic time about any point— that we come to abandon trust in the semiclassical theory as well. Instead, we regard a hitherto-unknown theory of quantum gravity as that which reigns supreme. So defines the 'Planck regime'— a regime in which the bare structure of classical spacetime is discarded [Callender and Huggett, 2001]. In this sense, the exit from the Planck regime gains the distinction of constituting the beginning of our large scale, (semi)classical universe.

The Planck regime is sometimes referred to as the 'Planck epoch' (e.g. in [Zinkernagel, 2008]), but it is not properly conceived as situated temporally prior to cosmic expansion. In fact, as will come up below, at least in the context of cosmological spacetimes with always strictly positive cosmic expansion, the exit from the Planck regime is perhaps better pictured as a spatially compact, timelike hypersurface, which is defined relative to any choice of inertial,

[7] As widely popularized by Guth [1998], the discovery of inflation was originally motivated by the prospects of using early universe cosmology specifically to study grand unified theories of particle physics (GUTs) in the framework provided by QFTCS. The 'horizon problem' and 'flatness problem' in the CMB data, referenced obliquely here, provided empirical constraints on the additional quantum fields whose presence was motivated by the viability of GUTs. While GUTs have fallen out of favor, inflation remains.

stationary observer in the spacetime. The Planck regime, defined with respect to that observer, thereby refers to a well-defined region within the spacetime, within which we do not trust the theory to be descriptively accurate. Moreover, since cosmic expansion is always strictly positive, the normalized spatial volume of that region in the spacetime goes to zero over cosmic time (i.e. in coordinates comoving with expansion about the stationary observer's worldline). This asymptotic reasoning suggests one sense in which our ignorance about what goes on in the Planck regime is, with regards to our universe (i.e. according to ΛCDM, perhaps modified by a model of inflation), plausibly of diminishing importance with time.

As already discussed, inflation is immensely popular today as a particular inference concerning high energy particle physics, which came about by leveraging progress at the boundary shared by early universe and late-stage cosmology. One may thereby speculate that quantum gravity within the Planck regime may be constrained, similarly, by the expectation that such physics seeds the relevant conditions within early universe cosmology, at the exit from the Planck regime. In the particular case of inflationary cosmology, this is to inquire: how does the physics operating within the Planck regime give rise to the conditions associated with inflation in our remote past?[8]

3.1 The trans-Planckian problem(s) in cosmology

I have just suggested that quantum gravity in the Planck regime constitutes a target of investigation that we hope to learn about, in virtue of the role it plays seeding conditions at the exit from the Planck regime within early universe cosmology. I have tried to paint this picture in such a way that, given the inference to a model of structure formation in the early universe, the study of the Planck regime is a natural follow-up. In both cases, the essential insight is that autonomy may be denied, in a particular and controlled manner, to fruitful ends. Increasingly high energy physics matters to understanding the evolution of LSS observed today: in the first case, in virtue of structure formation at recombination; in the second case, in virtue of effecting the means of structure formation at the exit from the Planck regime.

But there is at least one major disanalogy between the two cases: the latter is plagued by the 'trans-Planckian problem' in cosmology.[9] In fact, there are really two versions of the trans-Planckian problem in cosmology, which it is

[8]In other words, just as inflation in early universe cosmology is constrained by the classical initial data it must output for late-stage cosmology, one may take the relevant model of inflation itself within our cosmic history as that which the Planck regime must output for early universe cosmology (see, e.g., [Easther et al., 2001, Martin and Brandenberger, 2002, Danielsson, 2006]). Of course, there is a hiccup to overcome in this analogy: as mentioned already, the exit from the Planck regime, considered as a surface in a cosmological spacetime, is not spacelike (as is true of recombination), but is instead timelike, spatially compact, and defined relative to an observer. Holographic approaches might be attractive here, which enforce a correspondence between some bulk region of a spacetime and its boundary.

[9]The trans-Planckian problem originally referred to a structurally similar problem in the context of Hawking radiation. For discussion of contemporary attitudes concerning this older problem, see [Wallace, 2018] and references therein.

helpful to draw apart from one another. As will become clear, reasoning from one of these problems— what I will call the 'weak' version of the problem— makes it difficult to deny autonomy in early universe cosmology, as we usually understand it, with regards to quantum gravity in the Planck regime. But if autonomy cannot plausibly be denied, we lose our primary means of empirically constraining quantum gravity research based on cosmological evidence, which is otherwise secured by the relevance of quantum gravity to the Planck regime. This sets the stage for trans-Planckian censorship in the next section: the TCC stipulates that it is explicitly due to facts about quantum gravity, as become relevant in the Planck regime, that autonomy cannot plausibly be denied within early universe cosmology, at least as we usually understand it. Consequently, given an embrace of a suitable version of the TCC in one's candidate quantum gravity approach, one proceeds to empirically leverage our usual understanding of the early universe in their quantum gravity research, by new means.

Any discussion of the trans-Planckian problem in cosmology must begin with the observation that there can (conceivably) exist classical physical degrees of freedom in descriptions of the late-stage universe, whose origins are quantum processes above any arbitrary high energy cutoff. Of particular interest here are processes that originate at energy scales high enough to be associated with the hitherto undeveloped theory of quantum gravity. These processes are of interest because, given our present understanding of fundamental physics, we currently have very little idea how even to model our own ignorance about them.

Nonetheless, in the context of cosmology, these processes may be readily associated with the Planck regime in our universe. Meanwhile, classical physical degrees of freedom in the late-stage universe whose origins lie in the Planck regime may thereby provide much desired windows into quantum gravity. This is because any such 'trans-Planckian' physics ought to matter to our record of the evolution of LSS, on which we have a comparatively firm grasp. In other words, provided that we find a record of trans-Planckian physics in our universe, we may proceed to leverage our understanding of the evolution of LSS to hopefully learn about quantum gravity within the Planck regime. The trans-Planckian problem in cosmology may therefore be summarized— quite innocuously— as follows: how might we recognize the presence of trans-Planckian physics in our empirical descriptions of the late-stage universe?

Recall that our theory of late-stage cosmology admits of a description in terms of a Cauchy problem specified at recombination. Assuming that our current theory of late-stage cosmology is descriptively accurate, it follows that any record of trans-Planckian physics in the evolution of LSS may be taken as (already) encoded in the initial conditions present at recombination.

Of course, we might imagine ways by which our theory of late-stage cosmology could be descriptively accurate after recombination, despite there being trans-Planckian physics originating well into our late-stage universe. For one project in this tradition, consider the demonstration in [Mersini et al., 2001]. In their case, dark energy is identified as a phenomenological artifact of semi-classical fluctuations produced consistently (by fiat) within the Planck regime, which disperse highly non-linearly (and so, rapidly freeze out as classical modes capable of back-reacting on cosmic expansion). In a similar vein, in the case of entropic gravity presented by [Verlinde, 2017], long-range correlations between spacetime points can give rise to a superfluid-like phenomenology in the dark sector at late stages, which is relevant to descriptions of the evolution of LSS we get from ΛCDM. In both cases, trans-Planckian physics at late-stages is regarded as mimicking consequences of what are otherwise attributed to particular classical states of affairs that obtain at recombination. In other words, these cases imagine that the successes of our theory of late-stage cosmology are misleading us about our late-stage universe. In particular, we imagine being misled in our regarding the evolution of LSS as fully determined by the conditions that obtain at recombination.

But an assumption to the contrary is essential as a premise to infer the conditions at recombination, based on empirical observations of the late-stage universe. Since those inferences are what then, in turn, render the same observations empirical constraints within early universe cosmology, there is some methodological tension between standard practice in cosmology, as discussed in section 2, and what is being suggested by these imaginative exercises. The first version of the trans-Planckian problem— hereafter, the 'strong' problem— concerns this tension. On one hand, we may consider how trans-Planckian physics could originate late in cosmic history— despite appearances— and thereby lose our primary means of constraining our high-energy theorizing about structure formation in the early universe. On the other hand, we may assume that the autonomy of late-stage cosmology, with regards to quantum gravity, is essentially correct, so as to secure the inference needed to constrain theorizing about the early universe. But then, in the latter case, we are assuming that there simply is no trans-Planckian physics originating in our late-stage universe, which would otherwise help constrain quantum gravity research.

Following the latter tack, the most substantive constraint on quantum gravity research that we get from our empirical record of the late-stage universe, at least via late-stage cosmology, is a demand that the evolution of LSS turns out to stand in a suitable limit of the theory of quantum gravity sought. This is to demand that, once we have the theory of quantum gravity sought, there is, built into that theory, a means of articulating an in-principle reduction of late-stage cosmology: i.e. from the classical theory, to a theory of quantum cosmology. But, as Batterman [2018] points out, the multiple realizability of (in this case) the evolution of LSS spoils any sense in which the in-principle reduction explains the success of the descriptions we get from late-stage cosmology. So, inasmuch as the demand on the future theory of quantum gravity can be said to constrain research, it is a fairly weak demand: a multiscale consistency check on the quantum gravity theory applied to a given modeling context, rather than an empirical test of it. In this sense, our standard understanding of structure formation in the early universe— as that which is empirically constrained by its seeding the evolution of LSS at recombination— precludes there being any empirical constraint on quantum gravity research that comes afterward.

The second version of the trans-Planckian problem— the 'weak' problem— begins with the "less imaginative" possibility as to what a record of trans-

Planckian physics looks like in our late-stage universe. Namely, if some trans-Planckian physics were to originate within early universe cosmology, it could leave an imprint on the CMB at recombination, partially seeding the evolution of LSS in late-stage cosmology. If we set aside the strong problem just discussed (e.g. by embracing asymptotic reasoning like that given above, about the diminishing relevance of the Planck regime over cosmic time), we may push the conversation entirely back in time. For the evolution of LSS to offer a trans-Planckian window into quantum gravity, we ask: what does trans-Planckian physics look like in our empirical descriptions of the early universe, as provided by our current best theory of that domain?

As discussed above, our theory of early universe cosmology takes the form of a semiclassical interpretation of QFTCS, where, at least in the case of inflationary cosmology, the relevant QFTCS construction additionally semiclassically sources cosmic expansion in the background. But in order to proceed in the present investigation, some greater detail will be necessary. In particular, these QFTCS constructions are supposed to wear their ultraviolet (UV) frequency cutoffs on their sleeves. In the context of an effective field theory, this can be formalized as a stipulation that all further degrees of freedom above that high energy, UV cutoff have been absorbed into coupling constants within the relevant construction (see, e.g., [Rivat and Grinbaum, 2020], or else the pedagogical presentation in [Polchinski, 1992]). But in the context of QFTCS, understood as an attempt to semiclassically approximate the effects of quantum gravity in the presence of quantum fields, we simply assume that there is some such cutoff scale.[10] The upshot is that we declare, somewhat artificially, a frequency mode above which we throw out any information within the QFTCS construction. This is the Planck regime within the construction: information that we do not trust to be descriptively accurate, on the basis that we lack a field theory of quantum gravity that happens to agree with such descriptions (and according to which we may otherwise explicitly recover the QFTCS construction as a low energy effective field theory).

In some of these constructions though, time-evolving backward certain modes present at future infinity that are below the cutoff reveals that they ought to have begun their lifetimes above the cutoff. Following the prescription just given, these 'trans-Planckian' modes are thereby discarded at the onset, in which case their sub-cutoff effects at late stages are deemed artifacts of the construction that are not descriptive (even though they are below the cutoff scale). That is to say, trans-Planckian modes are artifacts of the UV regime within a QFTCS construction, which we do not trust to be accurate of low energy physics being modeled by that construction. We do not trust them precisely because we do not trust the UV regime of the construction to be accurate of high energy physics.

In [Brandenberger and Martin, 2013], this state of affairs was formalized, at least in the context of cosmological spacetimes, in an elucidating way. First,

[10] We also assume that topological degrees of freedom in the classical theory of gravity only matter above that scale, so that we may freely make use of global spacetime properties in selecting a vacuum state relevant to the physics below the cutoff.

one specifies a spatial origin point, from which the exit from the Planck regime is defined, as described above: as a spatially compact, timelike hypersurface centered on that origin point, with radius on the order of a Planck-length (in coordinates appropriate for labeling frequency modes in a perturbation theory at any point in cosmic time, independent of cosmic expansion).[11] This is something like an initial data surface that encodes all Planckian degrees of freedom in early universe cosmology, defined with respect to an inertial observer comoving with cosmic expansion. It also clearly delimits the Planck regime: that which, with respect to the choice of origin, brings about some initial data on that surface (modulo the hiccup mentioned in footnote 8).

Wherever the conditions along the exit from the Planck regime are well-described in terms of semiclassical perturbations radiating off of it, it must be that those perturbations either do not classicalize (in which case, they would not seed the evolution of LSS), or else they just so happen to agree with the physics dictated by the relevant low energy QFTCS construction, which we have previously inferred about our remote past on the basis of considerations from late-stage cosmology. In other words, either there is no record of trans-Planckian phyiscs at recombination, or else our usual model of structure formation is, by coincidence, predictive of a regime it was explicitly not meant to predict. This is the weak problem: in order to leverage our usual understanding of the early universe so as to empirically constrain quantum gravity research, we must assume that our model of the early universe is predictive of a regime it was not meant to predict.

Taking this assumption to be clearly implausible— an abuse of the usual formalism that we otherwise usually take to describe the early universe—, one concludes that there is no trans-Planckian physics in the early universe, which would otherwise come to seed the evolution of LSS at recombination. Early universe cosmology is, at least as we usually understand it, autonomous with regards to quantum gravity in the Planck regime. So, just as in the discussion above about late-stage cosmology, the only constraint on quantum gravity research provided by early universe cosmology is that of a consistency check. But then, having already set aside the possibility of trans-Planckian physics originating post-recombination, we are left with simply no means of learning about quantum gravity, via a trans-Planckian window in cosmology. Empirical claims from cosmology fail to constrain theory development in quantum gravity, in any substantive way.

As the next section will emphasize, trans-Planckian censorship has the effect of turning this predicament on its head. The TCC amounts to a declaration: the bare fact that denying autonomy in our usual high energy theorizing about structure formation is untenable is itself a substantive empirical constraint on our theorizing about quantum gravity. It is, per the TCC, due to facts about our universe within the Planck regime— i.e. to be understood according to the future theory of quantum gravity— that we may regard structure formation

[11] Note that identifying such a surface in the spacetime is tantamount to imposing a (global) frame in addition to that which defines stationarity (i.e. in addition to the frame that co-moves with cosmic expansion).

in the early universe as, inevitably, autonomous. Our empirical descriptions of LSS from cosmology are thereby rendered, ultimately, *quantum gravitational phenomena* (provided that we have set aside the strong version of the trans-Planckian problem, which preserves the inference to early universe cosmology— as it is standardly understood— in the first place).

4 The TCC

I have, at last, reached the point where I may introduce the TCC, as it was presented in its original context. Here is the authors' own general description of the conjecture, in terms only slightly more technical than those of the slogan provided in the Introduction [Bedroya and Vafa, 2019, p. 4]:

> We conjecture that a field theory consistent with a quantum theory of gravity does not lead to a cosmological expansion where any perturbation with length scale greater than the Hubble radius trace [*sic*] back to trans-Planckian scales at an earlier time.

Putting aside, for the moment, what it means for a field theory to be consistent with a theory of quantum gravity, the spirit of the TCC is clear. Recall that classical perturbations with length scale greater than the Hubble radius are those that are frozen, and so cannot become negligible with rapid expansion. As such, these are the sorts of perturbations, at least in the context of inflationary cosmology, whose consequences are readily observable in the CMB; these are what seed the evolution of LSS in late-stage cosmology. Meanwhile, in the framework of a semiclassical interpretation of QFTCS, as is relevant to early universe cosmology, quantum fluctuations comoving with cosmic expansion can spontaneously classicalize with wavelengths larger than the Hubble radius.

The TCC therefore rules out, by fiat, the possibility that any quantum fluctuations that classicalize in this way encode the effects of quantum gravitational physics deep within our remote past. Constructions that do otherwise— namely, permitting modes emanating from the exit from the Planck regime to grow larger than the Hubble radius in the course of rapid expansion— are simply inconsistent with the future theory of quantum gravity sought. In particular, there is some feature of the quantum gravitational physics present in the Planck regime that spoils how long such a period of rapid expansion could last (at least, in any viable description of the early universe as such a QFTCS construction that sources rapid cosmic expansion).

In this way, the weak version of the trans-Planckian problem is circumvented by fiat: supposing the TCC, trans-Planckian physics fails to become observable content within late-stage cosmology (at least, consequent to facts about structure formation in the early universe— recall that we have set aside the strong problem, from the get-go). As such, the TCC is manifestly a conjecture that our usual understanding of structure formation in the early universe is autonomous with regards to the quantum gravitational physics thought to govern the Planck regime. What seeds late-stage cosmology is (merely) high energy physics below the Planckian cutoff.

But crucially, the TCC states that it is *because of quantum gravity* that this is so, rather than by cosmic accident. In this way, an embrace of the TCC is very much like what Currie [2018, §11.3] has in mind, when he introduces the notion of an 'empirically grounded speculation' in the context of his much more general discussion of the epistemic role for pragmatism in the historical sciences. These speculations are scientific claims that are "justified on their fruits" [p. 288], in the course of ongoing research. In the present case, the speculation is that whatever determines the question of 'consistency' of an arbitrary QFTCS construction in the theory of quantum gravity is also the reason that, due to the relevance of quantum gravity within the Planck regime, there is ultimately no trans-Planckian physics present at recombination (at least, given our usual understanding of the early universe).

This, of course, takes for granted that it makes sense to talk about QFTCS constructions being consistent or inconsistent with a theory of quantum gravity, absent recourse to the latter theory in advance of its own development. Or, to put the matter more strongly: the TCC takes for granted that we know what it means, in quantum gravity research, to conclude that the relevance of some QFTCS constructions at low energies may be discarded, in virtue of facts that will be identified about higher energy physics. Meanwhile, other such constructions persist as descriptive of possible physics, in virtue of those same future facts. The point here is that consistency must mean something other than logical consistency, where (for instance) any description of a quantum gravitational system in the terms of QFTCS fails. But nor can it mean something as weak as approximation: some possibilities in the limit need to be ruled out, by the very means available to articulate the appropriate limit, in the first place.

Consider, for the moment, the more well trodden case of cosmic censorship, whose slogan may be phrased analogously to that of the TCC: "nature precludes the physical relevance of certain singular models of general relativity". Lacking a theory of quantum gravity, physicists have spent forty-odd years studying whether the 'impermissible' models of the classical theory (given cosmic censorship) can be shown to violate certain energy conditions, or else to never arise as a solution to any initial value problem, once the class of possible initial data is constrained in various ways [Earman, 1992]. Where there is progress on one of these fronts, one then considers how those conditions or constraints may themselves come to be explained by features of the quantum gravity theory, in the course of taking the classical limit. But were it the case that, when all is said and done, the necessary satisfaction of those conditions or constraints does not follow in the course of recovering the classical theory in the limit, cosmic censorship would be false. Oppositely, insofar as one would like cosmic censorship to be *rendered true*— that is, insofar as one assumes that it will be true of that future theory, once we have it— the present discussion gives rise to a new condition of adequacy for that future theory, in the course of its development.

Elsewhere [Schneider, 2020a], I have argued that making assumptions about the future theory of quantum gravity sought is an ordinary state of affairs in the course of developing it. It can be instructive to consider how much of the contour of the future theory is already decided, when one proceeds to develop the future theory on the basis of such assumptions. In the present case, the original authors of the TCC favor a string theory approach to quantum gravity. But their own general framing of the conjecture should make us inclined to think that not all of the toolkit of string theory is being used. As I will now claim, one needs only something in one's candidate quantum gravity approach that is analogous to the tools, known in string theory, as 'swampland conjectures'. If there are such analogues, one may make use of the TCC in developing a theory of quantum gravity accordingly.

In string theory, it is expected that for many QFTCS constructions with a stipulated UV cutoff, there exist string theoretic models that agree about all physics below that cutoff, and meanwhile are UV-complete. In other words, the latter are descriptive at all energies, but their descriptions at low energies are identical to those provided by the former. The QFTCS constructions that are arranged in this correspondence form the low energy 'landscape' of string theory. This landscape circumscribes all that is string theoretically possible, provided that our attention is restricted to the effects of that string theoretic physics that are below some or other high energy cutoff.

As was first discussed in 2005, it need not be the case that all such QFTCS constructions reside in the landscape. Some reside in the 'swampland', failing to admit any such UV-completion [Vafa, 2005]. Which reside in the swampland is a matter investigated via 'swampland conjectures': formal conjectures about families of such constructions that plausibly fail to admit UV-completions in the framework of string theory, because of obstructions that tend to arise in the attempt to explicitly construct the completions. As such, any particular swampland conjecture states that there is something distinctive of the formal apparatus of string theory that prohibits the descriptive relevance of any construction in the relevant family (provided that our universe is, ultimately, stringy).

One such swampland conjecture provides the intellectual origins of the TCC: the family of constructions relevant to inflationary cosmology, which in particular raise the specter of the weak version of the trans-Planckian problem in cosmology, plausibly fails to admit stringy UV-completions. In [Saito et al., 2020], it is challenged whether there is such a swampland conjecture, properly construed, that realizes the TCC (and which is consistent with current empirical commitments). Nonetheless, for present purposes, it is merely relevant that there exists such a tool as that provided by swampland conjectures in the relevant candidate quantum gravity approach, in order to formulate a version of the TCC. This is, after all, just what is needed to stipulate *that* the TCC be rendered true of the future theory, as a condition of adequacy in the course of that theory's development in string theory. Similarly, in order for the TCC to constrain the development of the future theory in any candidate quantum gravity approach, an analogue to such a tool is needed in that approach. That is, we need a way of adjudicating which QFTCS constructions *cannot be descriptive* of the low energy physics of the quantum gravity theory sought, via that approach.

5 Conclusion

We have seen a little of how trans-Planckian censorship can help empirically constrain quantum gravity research, and provided that one ensures that the relevant conditions of theorizing are met within their chosen candidate quantum gravity approach. Namely, the TCC amounts to an empirically grounded speculation about quantum gravity within the Planck regime, as is relevant to effecting the means of structure formation— as we usually understand the subject— in early universe cosmology. As I flagged in the Introduction, insofar as it pertains to the local epistemic goals of the community, the pragmatic upshot could constitute epistemic warrant for the speculation's uptake. On the other hand, such a pragmatism pairs poorly with a view of uptake in terms of belief: different assumptions about the future theory of quantum gravity make for different versions of the conjecture.

It could be, of course, that what we believe is the disjunction of all such versions, as summarized in the slogan given in the Introduction. But it is difficult to conceive of an argument in favor of such a belief. For instance, one might hope to reason abductively to it, but it is far from clear that the successes of our theories so far are better explained by such generic facts about quantum gravity, rather than by facts of their own. After all, it may simply be that there is no trans-Planckian physics in our cosmological record. Alternatively, one might hope to reason inductively to the belief, based on the first conjectural case in string theory. But the strength of this induction is questionable, even putting aside the conjectural status of the base case. In addition to being a bold inductive leap from just one candidate quantum gravity approach, the reference class for the induction is highly ambiguous: it is never obvious how such candidates like string theory should be understood, qua broad programs of ongoing research, to partition theory space.

Instead, I think the appropriate lesson here is that we may not wish to think of the uptake of trans-Planckian censorship as, ultimately, a matter of belief or disbelief. Fleisher [2018] (see also [Fleisher, 2019]) has recently developed the notion of 'rational endorsement', which he argues is relevant to scientific inquiry. As he stresses, endorsement is a doxastic attitude distinct from belief or disbelief, but which is nonetheless still in the province of epistemology. So, in particular, epistemic warrant in some cases may secure endorsement, rather than belief. This is the sort of flexibility I have in mind: trans-Planckian censorship might better be thought about in terms other than a *belief-exclusive* epistemology. I do not mean to suggest that rational endorsement is, in particular, the relevant alternative here— I do not know. Personally, I prefer to think of trans-Planckian censorship as a working hypothesis in quantum gravity research— cf. footnote 2. Whether rational endorsement is the appropriate attitude to extend, in general, toward working hypotheses in theoretical research, I am unsure. My point here is merely that the role of trans-Planckian censorship in quantum gravity research seems complex and valuable to the epistemic aims of the community, in a way that appears surprisingly far removed from any discussion about whether we ought to believe it.

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