Eternal Worlds and the Best System Account of Laws

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Abstract

In this paper we apply the popular Best System Account of laws to typical eternal worlds – both classical eternal worlds and eternal worlds of the kind posited by popular contemporary cosmological theories. We show that, according to the Best System Account, such worlds will have no laws that meaningfully constrain boundary conditions. It's generally thought that lawful constraints on boundary conditions are required to avoid skeptical arguments. Thus the lack of such laws given the Best System Account may seem like a severe problem for the view. We show, however, that at eternal worlds, lawful constraints on boundary conditions do little to help fend off skeptical worries. So with respect to handling these skeptical worries, the proponent of the Best System Account is no worse off than their competitors.

1 Introduction

One of the most popular accounts of laws is the Best System Account (BSA).[1] On this account, the laws are, roughly, simple and informative descriptions of what the world is like. This account is popular for a number of reasons: its deflationary nature avoids uncomfortable metaphysical commitments, it lines up well with the methodological virtues scientists often espouse, and, most importantly, it seems to yield the kinds of laws that scientists have suggested would hold. E.g., given a typical classical world, the BSA seems to yield something like the laws of Classical Statistical Mechanics; given the kind of cosmology physicists envision, the BSA seems to yield something like the inflationary theories physicists have offered, and so on.[2]

In this paper we'll argue that in a wide range of cases this last claim is mistaken: the BSA won't yield the kinds of laws that physicists suggest. In particular, some

[1]Prominent proponents include Lewis (1994), Loewer (2001), Hoefer (2005), and Albert (2012).

[2]We use the term "typical" here in its colloquial sense, that (in some good sense) the vast majority the things we're talking about (viz. a certain kind of world) are as we describe. That said, we take our use of the term "typical" to line up closely with the formal notions of typicality used in the typicality literature; see e.g. Maudlin (2007b), and Frigg & Werndl (2012). One example: our descriptions are true of (near) measure 1 of one-way eternal classical worlds, using the Lebesgue measure on phase space.

prominent physical theories have been proposed that arguably require lawful constraints on boundary conditions.[3] And we'll argue, for at least some of these theories, that if the world is eternal, the BSA won't yield laws that constrain boundary conditions in the ways these theories suggest.

It's generally assumed that these lawful constraints on boundary conditions are required in order to avoid skeptical consequences—viz. that we should be near certain that our evidence about the past is highly misleading.[4] So the failure to yield such constraints might seem like a devastating blow to the BSA. But we'll argue that, surprisingly, this turns out not to be the case. When one works through the details of how these skeptical arguments are supposed to go at eternal worlds, one finds that the theories which don't posit lawful constraints on boundary conditions are in fact no more susceptible to skeptical worries than the theories which do. Thus, at the end of the day, it's not clear that the BSA's failure to yield these lawful constraints on boundary conditions at eternal worlds is something that should bother proponents of the BSA.

This paper will proceed as follows. In section 2, we'll lay out some background. In section 3, we'll argue that at typical classical eternal worlds, the BSA won't yield meaningful constraints on boundary conditions. In section 4, we'll argue that at typical eternal inflation worlds, the BSA won't yield meaningful constraints on boundary conditions. In section 5, we'll assess the skeptical consequences of these results. In section 6, we'll briefly summarize our results.

2 Background

2.1 Classical Statistical Mechanics

Statistical mechanics (SM) aims to predict thermodynamic phenomena, like milk diffusing into tea, ice cubes melting in warm water, and a raw egg's absorbing a hot pan's heat. In part, then, SM aims to predict entropy's tendency to increase over time in (approximately) isolated systems that aren't already at their maximum entropy.[5] For now we restrict our focus to Newtonian worlds fundamentally describable in terms of the motion of point particles, and so to *classical* statistical mechanics (CSM). We begin by briefly summarizing Albert's (2000) formulation of CSM and the justification for its inclusion of a nondynamical law, the past hypothesis.

CSM's predictions are probabilistic. For example, the CSM probability that (approximately) isolated, non-maximum entropy systems evolve in entropy-increasing

[3] Interestingly, those working on these issues have taken different attitudes towards these constraints in different contexts. For example, in classical statistical mechanics, many have taken the requirement for such constraints to be an unproblematic feature of the theory (e.g., see Feynman (1965) and Albert (2000)). By contrast, in eternal inflation theories, most of those who have thought that such requirements might be needed have taken this to be a demerit of the theory (e.g., see Steinhardt (2011)).

[4] For example, see Albert (2000) and Carroll (forthcomingb).

[5] SM doesn't always predict entropy increase. Two examples: first, SM predicts that a system in its maximum entropy (equilibrium) state will tend to remain in that state. Second, systems are sometimes observed to *decrease* in entropy. SM predicts these infrequent entropic decreases, too.

ways is overwhelmingly high.[6] Hence, we reasonably expect such evolutions. The CSM apparatus for generating these probabilities has several components.

First, it assumes the laws of classical mechanics (CM).

Second, it employs a *phase space*. The classical phase space for a system of n particles is a $6n$-dimensional space representing both the position and momentum of each particle. Every point of phase space thus represents a complete specification of an instantaneous state the classical system could be in. Call these the *microstates* of such a system, and say that a system is *located at* the point in phase space which represents its current microstate. Given the determinism of classical mechanics,[7] a system's being in a microstate at a time determines what microstates it was and will be in. Likewise, a system's current location in phase space determines where it has been and where it will be located in that space.

Third, distinguish microstates from *macrostates*. Macrostates are macroscopic ways things can be, sequences of which comprise the thermodynamic evolutions that we directly observe. Many different microstates are macroscopically indistiguishable from each other, so that any of these microstates would yield (underlie, realize, etc.) the very same macrostate. In other words, such microstates are *compatible with* the same macrostate. Just as microstates are represented by points of phase space, macrostates are represented by *regions* of that space – viz. the region of points representing microstates that are compatible with that macrostate. When there is little danger of confusion, we conflate macrostates with the regions of phase space representing them in what follows.

Fourth, it employs a *probability distribution* over phase space. In particular, CSM uses a flat distribution defined over the standard Liouville measure of phase space regions. Higher-entropy macrostates are larger on this measure than are lower-entropy ones, and time-evolving (the microstates comprising) a macrostate preserves its measure. Call this probability distribution the *statistical postulate* (SP). This yields a general definition of the CSM probability that macrostate A obtains, given that macrostate B does – i.e., relative to background propositions K, like CM and all lawful boundary conditions, if any:

$$Prob_K(A|B) = \frac{m(A \cap B \cap K)}{m(B \cap K)}$$

In other words, the probability of A, given B (and given background conditions K), is a matter of *how much* of B (plus K) is taken up by A. Any same-measure part of B is equally likely, so the probability that the actual microstate is in A, given its being located in B, is a matter of the size of their intersection.

To demonstrate: suppose B is the macrostate that some ice cubes are floating in a cup of hot water at t, and A is the macrostate of there being at t-plus-five-minutes only tepid water in that cup. Then it turns out that the vast majority of B is taken up

[6] This is essentially the probabilistic version of the second law of thermodynamics. That said, it should be noted that this claim is not entirely uncontentious; see Shenker & Hemmo (2012).

[7] Strictly speaking, classical mechanics is not deterministic (for various kinds of counterexamples, see Earman (1986), Xia (1992), and Norton (2008)). But these counterexamples are widely believed to be of measure of zero, and so for our purposes can be safely ignored.

by $A \cap B$: nearly all the microstates in B (on the standard measure) are such that time evolving them forward by five minutes yields microstates wherein the cup contains only tepid water. That is, the probability of this evolution is extremely high. It will be useful to have a name for the apparatus described thus far, i.e. the combination of CM and SP. Call this CSM^-.

More is needed. For just as B is overwhelmingly comprised (on the standard measure) of microstates that evolve forward to be compatible with A, something analogous is true for microstates evolved *backward*: the vast majority of microstates in B had their ice melted in the past, too. That is, CSM^- wrongly has it that ice spontaneously materialized from warm water.

Moreover, if we compare the chance CSM^- assigns to the world having been like we think it was a minute ago, and the chance that our memories are false and we spontaneously fluctuated out of a higher entropy state, we'll find that CSM^- assigns a vastly higher chance to the latter. And thus, if our beliefs should line up with the chances (cf. section 2.4), then it seems we're rationally required (conditional on CSM^- being true) to disbelieve our memories. As Albert (2000) has argued, this seems to make CSM^- self-undermining. For if we disbelieve our memories, then we lose our reasons for believing something like CSM^- in the first place.

The canonical solution is to add to the background propositions K a *past hypothesis* (PH), to the effect that the world was in a very simple, low-entropy, globally initial macrostate M of the sort that cosmology presumably aims to discover. Nearly all microstates in B that increase in entropy toward the past, rather than decrease, are incompatible with the PH. So by adding the PH, the vast majority of remaining microstates evolved in ways compatible with what we remember, and so the probability that the world evolved in this way is high. CSM is the result of adding the PH to CSM^-.

To summarize, Albert's formulation of CSM is the conjuction of the following three theses:

(CM) The laws of classical mechanics.

(SP) The statistical postulate, i.e. a flat probability distribution over the Liouville measure of phase space regions.

(PH) The past hypothesis, i.e. a statement to the effect that the universe was in a very simple, low-entropy, globally initial macrostate M of the sort we might expect cosmology to discover.

2.2 Eternal Inflation

Eternal inflation is a cosmological model of considerable popularity and interest. It is best introduced by way of the more familiar Big Bang model, for inflationary cosmology aims to explain facts the Big Bang model cannot. Eternal inflation is then thought to be a consequence of the mechanism that drives the hallmark of inflationary cosmology: rapid spatial expansion in the early universe.[8]

[8]For an accessible discussion of eternal inflation, see Guth (2001), Guth (2007), and Steinhardt (2011).

According to Big Bang cosmology, the early universe was comprised of an extremely hot, dense plasma which expanded and cooled. This cooling plasma then synthesized into lighter chemical elements, which under the attraction of gravity coalesced to form stars and galaxies. This, in the barest of outlines, is the standard cosmological model. But there are facts this model cannot explain, and inflationary cosmology promises to fill the gaps.

One putative gap is the flatness of space. The curvature of the early universe must have been extremely flat; otherwise, the universe now would be far more curved than it in fact is. But why was space so flat to begin with? Another gap: soon after the Big Bang, the universe was remarkably homogeneous. We know this by observing the cosmic microwave background (CMB), the remnant radiation left by the cooling plasma, which has spent the last dozen or so billion years zooming across space to reach us. Everywhere across the sky, this radiation is very evenly distributed, to about one part in 100,000, indicating that the plasma that produced the CMB was also highly uniform.[9] But why was this plasma so homogeneous? A third gap: the early universe wasn't perfectly thermally uniform, and the way in which it deviated from uniformity is also interesting. Specifically, the inhomogeneities found in the CMB are very nearly *scale-invariant*, i.e. their magnitudes are largely the same whether you look at smaller or larger regions of the CMB. But why should the early universe have been inhomogeneous in just this way?[10]

Roughly, inflationary cosmology explains these puzzling facts as follows. Prior to the formation of this hot, dense plasma, the universe quickly underwent enormous spatial expansion, growing within a mere 10^{-30} seconds by a factor of at least 10^{25} – i.e. from about one quadrillionth of the size of an atom to around that of a dime.[11] Such expansion stretched any prior spatial curvature to a tiny fraction of what it had been, leaving a virtually flat arena for the hot plasma that we indirectly observe. Inflation fills the other two gaps by way of its hypothesized mechanism. This is a scalar field, called the *inflaton*, whose energy density varies across space. The field's density in a region determines its potential energy, which is repulsive rather than attractive. Call this *inflationary energy*. So where there is high inflationary energy, space undergoes rapid, exponential expansion; and when and where that energy fizzles out, inflation ends.

Like other scalar fields, the inflaton classically moves from higher potential energies to its minimum, like a ball rolling down a hill. And when the inflaton's potential energy drops, inflationary energy converts into ordinary energy, thereby "thermalizing" the newly expanded space and creating the sort of plasma that gave rise to the CMB. Moreover, rapid expansion results in a highly uniform distribution of this energy in the expanded region, the decay of which yields a largely uniform thermal-

[9] Guth (2001), p75.

[10] To explain the apparently remarkable flatness of the early universe is to solve the so-called *flatness problem*. Standardly, solving this problem helps motivate inflationary cosmology (cf. Guth (2007), p6813). But see section 4 of Carroll (forthcominga) for an argument that the flatness of the early universe is typical, given the canonical measure on possible spacetime trajectories the universe might take. While this may undercut the flatness problem, Carroll argues that the homogeneity of the early universe is extremely atypical on this canonical measure, lending value to explanations of that homogeneity.

[11] Steinhardt (2011), p40.

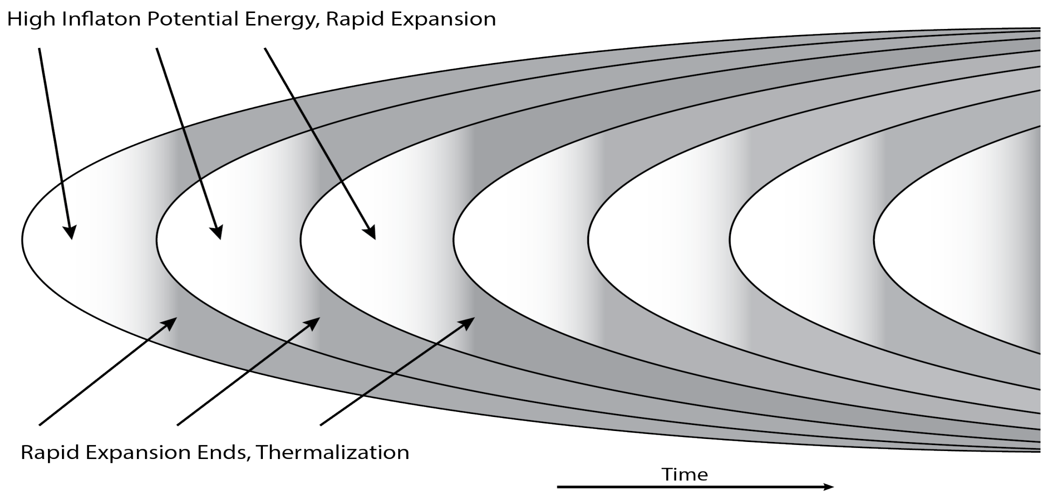

Figure 1: Simplified Eternal Inflation

ization. And quantum fluctuations in the inflaton field give rise to scale invariant inhomogeneities in the resulting thermalization: earlier fluctuations are magnified by expansion to cosmic scales, later fluctuations undergo less magnification, and so fluctuations over the course of the inflationary period yield similar thermal nonuniformities on scales from small to cosmic.

Eternal inflation – the thesis that there is always some region that has a high inflationary energy and so undergoes rapid exponential expansion – seems to be a consequence of the picture just described. Like the half-life of radioactive decay, inflationary energy decreases only with some probability.[12] So there is a non-zero chance that this energy remains high in a region. And because this potential energy doesn't dilute with expansion, even small regions where the inflaton hasn't decayed quickly grow to dominate space. If the chance that inflationary energy remains high is not too small – and the exponential expansion caused means even a small chance is sufficient – then this pattern of growth and decay and subsequent growth elsewhere continues *ad infinitum*.

Eternal inflation leads to a fractal spacetime structure: as an initial region of high inflationary energy evolves, some thermalizes and some does not; the space that does not thermalize likewise expands, some of which thermalizes and some of which does not; and so on. A helpful way to picture this is in terms of a simplified nested structure, illustrated in Figure 1.

Each elipse in Figure 1 represents the beginning of a new region of rapid expansion, and the white-to-gray gradient indicates the transition from high inflaton po-

[12]These probabilities result from the combination of the inflaton's classical movement (toward its minimum potential energy) with chancy, quantum fluctuations that "bump" the inflaton either lower or higher on its potential. These fluctuations were mentioned earlier in explaining the scale invariant inhomogeneity in the CMB.

tential energy to thermalization. So the outermost elipse thermalizes around a single region of rapid expansion, which expands and thermalizes around a second region of rapid expansion, and so on. The illustration is simplified in several ways. First, we shouldn't expect a single region of continuing rapid expansion; rather, expanding regions are likely scattered in a sea of thermalizing space; they then grow to dominate the spatial volume, surrounding and cutting off pockets of thermalization from one another. Second, the diagram is not to scale. It does roughly represent, however, the way that expanding space comes to dominate the spatial volume: as one progresses farther to the right, less and less of the diagram's height is comprised of thermalized space from earlier phases of expansion. And third, thermalized space can beget its own "child" regions of high inflation energy and subsequent rapid expansion, viz. by quantum tunneling to a high inflationary energy (though this is extremely unlikely). These child regions are not represented.

We assume that this eternal inflation world began globally with a largely homogeneous distribution of high inflationary energy, which then sets off the infinite cascade of thermalization and further inflation. This assumption is plausible: as Brandenberger (2017) summarizes, recent modeling suggests that large inhomogeneities in the initial conditions would stop inflation from getting started. Since the world we're considering is such that there was initial inflation, we assume that it was not initially inhomogeneous in that way.

Observe then that essentially the same macrocondition, call it M, arises non-initially: those regions that do not thermalize expand to have largely homogeneous distributions of high inflationary energy, of effectively the same size and shape as the initial conditions. This occurs infinitely many times, and so in addition to the initial occurrence of M, there are infinitely many occurrences of M at particular local non-initial regions, as shown in figure 2.[13]

The laws that eternal inflation theories appeal to are not as clear cut as those of CSM, and there's some variation between different version of eternal inflation regarding what the dynamical laws are, and whether it needs to include a constraint on initial conditions. That said, it will be convenient in what follows to have a name for the laws that eternal inflation theories propose. In what follows, we'll take COS to refer to these laws when understood as including a lawful constraint on initial conditions, and COS^- to refer to the laws one gets by removing this constraint on initial conditions.

2.3 The Best System Account

The Best System Account (BSA) of laws is a version of the regularity account of laws. Like other regularity accounts, it offers a deflationary account of what the laws are:

[13]Might the initial and later macrostates of high inflationary energy be different? One clear difference is that the initial macrostate occurs globally, whereas later macrostates occur only locally. Another difference is that while exponential expansion leaves non-initial macroconditions of high inflationary energy quite uniform, the initial condition may be less so. We return to these points in section 4, but suffice it to say neither consideration plausibly effects our argument.

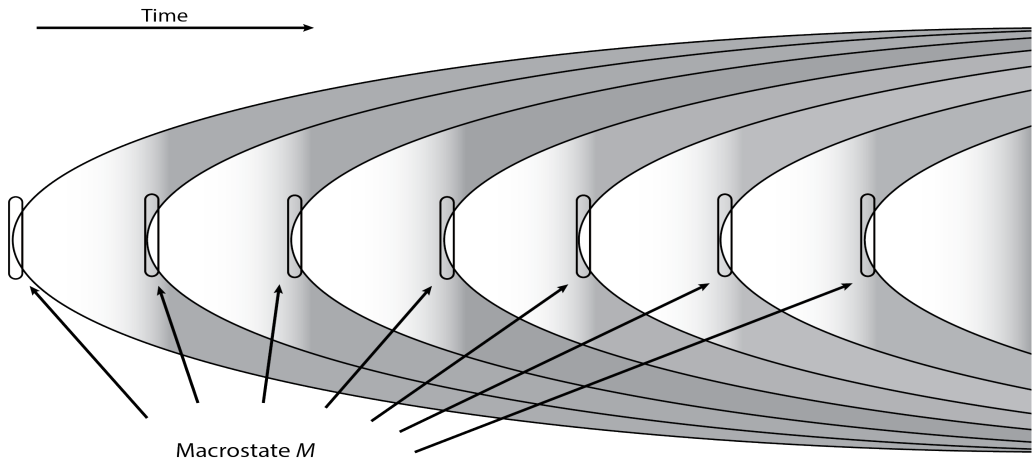

Figure 2: Infinite Recurrence of Macrostate M

according to the BSA, the laws are nothing more than a simple and informative description of what the world is like.

The BSA is popular for several reasons. One reason is that the BSA seems to closely adhere to the kind of methodology employed by the sciences, and to yield the kinds of laws our scientific community would endorse. A second reason is that by taking the laws to merely be a certain kind of description of what the world is like, it avoids the spookiness of primitive laws, or laws grounded in non-occurrent facts, like counterfactuals or necessitation relations between universals.[14] A third reason is that the BSA is compatible with the popular thesis of Humean Supervenience – the claim that all of the qualitative features of the actual world supervene on the qualities instantiated at points, and the spatiotemporal relations between them.

The classic formulation of the Best Systems Account (BSA) of laws comes from Lewis (1994). On this account, we can determine the laws of a world w in the following way. First, consider a language whose only predicates are (a) predicates corresponding to perfectly natural properties and relations, and (b) a chance predicate. Second, consider every set of sentences in this language, and remove any set containing sentences that are false at w, or chance assignments that aren't probabilistic. Third, evaluate the remaining sets of sentences according to three desiderata: simplicity, informativeness, and "fit", where fit is a measure of how high a chance these sentences assign to the history of w. If one of these sets of sentences is robustly best with respect to these desiderata – i.e., it does substantially better than any other candidate – then

[14] For proponents of primitive laws, see Carroll (1994) and Maudlin (2007a); for an account that analyzes the laws in terms of counterfactuals, see Lange (2009); for an account that analyzes the laws in terms of necessitation relations between universals, see Armstrong (1983). That said, not all proponents of the BSA are moved by this kind of consideration; see Demarest (2017) for a proponent of the BSA that also accepts these kinds of intangible non-occurrent facts.

it's the *best system* at w. And any regularity entailed by the best system is a law.

What if there are several sets of sentences that are (roughly) tied, and so no set of sentences that's robustly better than any of the others? Lewis vacillated about how to best handle these cases. One approach, suggested in Lewis (1986), is to take the laws in this case to be only those regularities that are entailed by all of the systems that are in contention. Another approach, advocated by Lewis (1994), is to take there to be no laws in such cases. We'll take both of these approaches to be live possibilities in what follows; as we'll see, our arguments will go through either way.[15]

Although this classical formulation of the BSA is attractive in many respects, a number of issues arise regarding this formulation, especially when viewed through the lens of statistical mechanics.[16]

One issue is that the classical BSA restricts laws to *regularities*. As a result, it rules out lawful constraints on boundary conditions by fiat. And this makes it unable to recover popular formulations of statistical mechanics that appeal to lawful constraints on initial conditions, like Albert's (2000) Past Hypothesis. In light of this, we'll join most modern proponents of the BSA in discarding the constraint to regularities, allowing any proposition entailed by the best system to be a law.[17]

Another issue is that the classical BSA requires the language we use to evaluate candidate systems for simplicity to be one in which all of the predicates (excepting the chance predicate) refer to perfectly natural properties and relations. But if this is how we're evaluating candidate systems, it's unclear how something like the Past Hypothesis could be one of the laws. For describing this macrostate in the language of perfectly natural properties and relations (e.g., mass, charge, spatiotemporal relations) would yield something extraordinarily complicated. And thus any system entailing such a proposition would presumably itself be very complicated, and thus not a plausible candidate for best system.[18]

In light of this, we'll join many modern proponents of the BSA in modifying the account in order to allow for laws like the Past Hypothesis. There are a number of different variants of the BSA in the literature which do this; the particular variant we'll

[15] Glynn et al. (2014) suggest that, with respect to ties between systems that differ only in their chance assignments, we should adopt a third option: taking the chances to not be sharp. In such cases, we should take the chances to be given by the set of probability functions the roughly tied candidates assign. It's worth noting that this is not, in fact, a third way to treat ties. For this treatment of chances is entailed by Lewis's (1986) suggestion to take the propositions entailed by all of the viable candidates to be laws. In particular, suppose that there are two candidate systems, S_1 and S_2 that disagree only about whether the chances should be given by ch_1 or ch_2. Both of these systems will entail disjunctions of the form "The chance of A is x $[= ch_1(A)]$ or y $[= ch_2(A)]$". Thus, on Lewis's (1986) proposal, these disjunctive claims about chances would be lawful, even though each of its disjuncts would not (since neither disjunct is entailed by both systems). Thus the proposal of Glynn et al. (2014) in fact follows from Lewis's first suggestion regarding how to handle ties.

[16] In addition to the three issues discussed below, a number of other worries have been raised about using the BSA to recover the laws of statistical mechanics; for further discussion see Meacham (2010) and the references therein.

[17] For example, see Loewer (2001), Hoefer (2005), Ismael (2009), and Albert (2012). It's worth noting that at times Lewis himself seemed amenable to this suggestion; see Lewis (1986), p123.

[18] For versions of this worry, see Schaffer (2007) and Winsberg (2008).

adopt draws from Hoefer (2005) and Loewer (2007).[19] On this approach, we allow candidate systems to be formulated in *any* language. And then we evaluate candidate systems for simplicity, informativeness and fit, where these criteria are now understood in terms of simplicity and informativeness for subjects like us, with scientific communities like ours.[20] Given this way of understanding the BSA desiderata, it's no longer implausible that something like the Past Hypothesis could be a law.

A third issue is that the notion of fit Lewis employs seems incapable of recovering the chances of statistical mechanics. On Lewis'sconception, the fit of a system at w is determined by how high a chance the system assigns to w's history. But statistical mechanics will assign a chance of 0 to any particular history coming about. And this would still be true if we changed the chances statistical mechanics assigned to individual events – e.g., made the chance of heads 2/3 instead of 1/2. Thus Lewis'snotion of fit won't discriminate between statistical mechanics and alternatives to statistical mechanics that assign different chances to individual events, because all of these theories assign the same chance to the w's history – 0. And so, given this notion of fit, it's unclear how the BSA could recover statistical mechanics, since statistical mechanics will do no better with respect to fit than alternative theories which assign different chances to individual events.

In response to this problem, we'll follow Elga (2004b) in modifying the notion of fit the BSA employs. Elga's proposal is to assess the fit of systems by looking at the chances they assign to a restricted set of test propositions. Then we compare the fit of different systems by comparing the chances they assign to the true test propositions, with higher chances indicating better fit.

This proposal requires a way of picking out a restricted set of test propositions. Elga's suggestion is to take the test propositions be those that are simply expressible in a privileged language; presumably the privileged language Lewis proposed to use when formulating candidate systems. Since, following Loewer, we don't appeal to Lewis's privileged language, we'll adopt a slightly different approach: we'll take the test propositions to be those that are simply expressible for subjects like us, and scientific communities like ours.[21]

[19]For a survey of these variants of the BSA, and a discussion of their relative merits, see Eddon & Meacham (2015) and the references therein.

[20]One might worry about the objectivity of appealing to what is simple and informative for such communities. For discussion of this point, see Loewer (2007, 325 and 327) and Eddon and Meacham (2015, 2.3 and 3.5).

[21] Although Elga's proposal is perhaps the most sophisticated proposal for how to assess fit on offer, one might have worries regarding how to spell out the details in a satisfactory way. For example, one might worry that given the kind of privileged language Lewis proposes, there will be infinitely many true simple sentences expressible in that language. (E.g., if we have names for spacetime points, and a fundamental exact occupation relation, then there will be uncountably many simple true sentences asserting that something does/does not occupy a given spacetime point.) And one might worry that the project of spelling out how to weigh and agglomerate the chances assigned to infinitely many propositions in a satisfactory way will raise many of the same worries that arose for Lewis's proposal regarding how to assess fit. Due to space constraints, we won't try to resolve these issues here.

2.4 Credence and Chance

In this section, we lay out the standard framework for the relationship between *de se* credences and chances, viz. in terms of centered worlds and Lewis's Principle Principle. For such a framework makes clear the self-undermining nature of theories like CSM^-. Given that we are rationally required to match our credences to the chances, then conditional on CSM^- we should be virtually certain that our memories are false. And yet if so, we are left with little reason to believe CSM^- in the first place.

Let a *centered world* be an ordered triple consisting of a world w, a time t, and an individual i. Let a *de dicto proposition* be a proposition that is true at a centered world $\langle w, t, i \rangle$ *iff* it's true at every other centered world at w. Let an *irreducibly de se proposition* be a proposition that doesn't satisfy this clause – a proposition such that, for some w, it's true at some centered worlds at w, and false at others. Intuitively, *de dicto* propositions correspond to claims that are entirely about what the world is like, while irreducibly *de se* propositions correspond to claims that are also about one's location within a world.[22]

For any irreducibly *de se* proposition A, there will be a corresponding *de dicto* proposition that is true at all of the centered worlds A is true at, and also true at any centered world located at the same world as one of those worlds. This is the proposition that there exists some individual at some time for whom that *de se* proposition is true. For convenience, we'll use the following notation to flag this relationship: if A is an irreducibly *de se* proposition, then $\hat{A}$ is the *de dicto* proposition that one gets by "filling in" A in the manner just described.

Let a subject's *credence function cr* be a function which assigns values to propositions between 0 and 1 representing the subjects confidence in those propositions, with 0 representing maximal confidence that the proposition is false, and 1 representing maximal confidence that the proposition is true. It's generally assumed that rational subjects will have probabilistic credence functions. In what follows, we'll restrict our attention to credence functions that are probabilistic.

For concreteness, we'll assume Lewis's (1996) conception of evidence. On this picture, a subject's total evidence E correspond to the set of centered worlds $\langle w, t, i \rangle$ such that at world w, at time t, individual i has the same perceptual experiences and memories as you do. (Following Lewis, we're understanding "memories" here in a non-veridical sense; e.g., having the memory that it rained on your tenth birthday does not entail that it actually did.)

If a subject with total evidence E satisfies the Bayesian updating rule (Conditionalization), then we can express their current credences as a function of their initial credences (or "priors") *ic* and their total evidence E as follows:[23]

[22] This framework borrows heavily from Lewis (1979), though he uses slightly different terminology (e.g., Lewis calls *de se* propositions "properties").

[23] Two caveats. First, while IC-Conditionalization is largely uncontroversial if A and E are *de dicto* propositions, a number of tricky questions arise if A and E are *de se* propositions. (See Titelbaum (2012) and the references therein.) To skirt these issues, in what follows we'll only rely on arguments that appeal to IC-Conditionalization (and this equation) when A and E are *de dicto* propositions. Second, this equality only holds if a subject doesn't lose evidence. If a subject loses evidence, and so E is strictly weaker than the conjunction of their evidence, then this equation won't express what Conditionalization prescribes. In what

IC-Conditionalization: $cr_E(A) = ic(A \mid E)$, if defined.

We take there to be some constraints on rational priors (such as the Principal Principle, described below), That said, we'll adopt a broadly permissive approach which takes a number of different prior functions to be rationally permissible.[24]

Let a *chance function* $ch_{T,K}(A)$ be the chance assigned to A by chance theory T, given background K. As usually understood, A, T and K must all be *de dicto* propositions.[25] It's widely held that one's beliefs about the chances place constraints on what it's rational for one to believe. A typical formulation of this constraint is Lewis's (1980), Principal Principle, which holds that a rational subject's initial credence function should be such that:[26]

Principal Principle: $ic(A \mid T \wedge K) = ch_{T,K}(A)$, if defined.

It follows from IC-Conditionalization and the Principal Principle that for rational subjects, $cr_{T \wedge K}(A) = ch_{T,K}(A)$, when defined. But subjects like us will virtually never have total evidence of the form $T \wedge K$ needed to yield well-defined chance assignments. After all, our evidence is compatible with many different chance theories, and many different backgrounds given those chance theories. Nonetheless, the Principal Principle will impose strong constraints on us. In particular, it will require our credence in A to be equal to the average of the chances assigned to A by the different $T \wedge K$s compatible with our evidence, weighted by our credence that those $T \wedge K$s obtain:

$$cr_E(A) = \sum_i cr_E(T \wedge K_i) \cdot ch_{T,K_i}(A), \text{ if defined,}$$

where i ranges over the elements of some partition of E into $T \wedge K$s compatible with E.

3 The Classical Case

In this section we argue that, by the BSA's lights, typical examples of certain classical worlds – worlds that globally satisfy the Poincaré Recurrence Theorem – have no

follows, we'll assume that the subjects we're considering don't lose (*de dicto*) evidence. (For discussion of this issue, see Meacham (2016) and the references therein.)

[24] We make this assumption largely for concreteness; most of our points can be tweaked to go through given a picture on which there's only one rational prior, given that this rational prior lines up with priors like ours (cf. section 5). The question of whether there are many rational priors or only one is a contentious issue; for a classic attack on permissivist stances on priors, see White (2005), for a defense see Urbach & Howson (2005) and Meacham (2013).

[25] See Lewis (1980).

[26] Lewis required K to be a complete history up to a time; like many other authors, we drop this constraint in order to allow for statistical mechanical chances (see Loewer (2001), Meacham (2005), Hoefer (2005), Winsberg (2008), and Ismael (2009)). Lewis (1994) himself later followed Hall (1994) in endorsing a more complicated principle – the "New Principle" – in order to address certain worries regarding the compatibility of the Principal Principle and the BSA. However, in the cases we're concerned with the two principles will yield the same prescriptions. So we'll employ the simpler Principal Principle in what follows.

lawful boundary conditions. Or, at the very least, our argument shows that what lawful boundary conditions those worlds do have are so weak as to underwrite SM probabilities which hardly constrain our rational credences.

The argument, in outline, is this. Consider again PH, the thesis that the world was initially in a very simple, low-entropy macrostate M. Call the PH a *boundary proposition* (BP), as it claims that a certain boundary condition obtains. Given the version of the BSA we've assumed, it is plausible that the PH earns its keep in a best system, and so other things being equal it is a lawful boundary condition. After all, the PH is relatively simple to state, in a language that is salient to us. And together with SP and CM, the PH plausibly underwrites a vast increase in the fit of systems including it, by way of assigning high SM probabilities to test propositions, like those concerning the evolution of everyday thermodynamic systems.[27]

Say then that the PH is *highly eligible* to be a member of a best system, given its balance of complexity and fit. Observe then that in the worlds we're considering – where there is one-way eternal recurrence of the initial low-entropy macrostate M – there is not just one highly eligible BP, but plausibly an infinite number of them. That is: in the same way that PH seems highly eligible in such worlds, so do later *middling hypotheses* (MHs), BPs which state that M occurs at specific, non-initial times—viz. those at which M does in fact occur. In such worlds, PH and infinitely many MHs are more-or-less on a par with respect to the BSA's desiderata of simplicity, informativeness, and fit. And we argue that, given the BSA, this result – plus the plausible claim that there are no other, more eligible BPs – vitiates against any one of these BPs being a lawful boundary condition.

3.1 Parity between boundary propositions

Different collections of PH and/or MHs are axioms of different systems; on the BSA, the best of these is such that its theorems (axioms included) are the laws. Of course, there are other BPs, not all of which are made equal: plausibly, only PH and MHs could prima facie earn their keep in a best system. Setting aside those that are less eligible, we'll use 'BPs' below to refer only to the PH and MHs, and 'recurrences' to refer only to occurrences of M. In this section, we observe that in a typical, classical one-way eternal recurrence world, w, infinitely many BPs are roughly on a par with respect to the BSA's desiderata of simplicity, informativeness, and fit.

It is easy to see that this claim is true for simplicity and informativeness. There are infinitely many MHs in addition to the PH, and all of these BPs state that the simple, low-entropy macrocondition M occurs. One way a BP p could be more informative than another p' is if p entails, but is not entailed by, p'. So note then that no BP entails any other: each is such that some microstates compatible with it are not compatible with the other BPs.[28] Moreover, the only difference between these BPs is at which time condition M is said to occur. Stating that M occurs at t or at t', however, makes

[27] See discussions of how the BSA could yield the laws of CSM, see Loewer (2001), Hoefer (2005), Winsberg (2008), and Albert (2012).

[28] Thanks to an anonymous referee for making clear the need to highlight this point.

little difference to either the relative complexity or the informativeness of the BPs in question. Hence,

(1) All BPs in w are roughly on a par with respect to informativeness and simplicity.

To see that infinitely many BPs are on a par for fit, our focus on a typical recurrence world becomes important. At typical worlds of this kind we should expect the world to come to have macrostate M infinitely many times. (It is nomologically possible, but extraordinarily unlikely, for such worlds to have macrostate M only finitely many times). Thus at typical worlds there will be infinitely many BPs that are roughly equal in the fit they confer on systems containing them.

We make two assumptions about what a typical recurrence world w is like, concerning the typicality of recurrences that take place within it. The first assumption:

(2) In w, a typical recurrence r and its corresponding BP p are such that r's surrounding evolution is what is highly likely to occur, conditional on p.

Consider the recurrence of macrostate M at t. Leading up to and following M-at-t are various events and thermodynamic phenomena, comprising the broad reduction of entropy resulting in M-at-t and the more-or-less steady entropic increase thereafter. These events comprise the *surrounding evolution* for that recurrence, and so we also say that these events *surround* that recurrence. We can sharpen (2) somewhat. Focus on a certain kind of event, viz. the obtaining of macroconditions which are simple and salient: like milk's being diffused in coffee and an ice cube's having not yet melted. Then (2) entails the following:

(3) In w, a typical recurrence r and its corresponding BP p are such that a large majority of the simple, salient events surrounding r each have a very high SM probability of occurring, conditional on p.

The restriction to simple, salient events here is important, as is allowing typical recurrences to have only a "large majority" of highly likely events surrounding it. For arbitrary compounds of simple events have arbitrarily low SM probabilities, conditional on p; and the SM probability (conditional on p) that some unlikely, simple and salient events will infrequently occur is high.[29] A further point: there must be a limit to how temporally distant events can be from M-at-t to be counted as surrounding it. Go too far and events occur which are very unlikely conditional on M-at-t, e.g. earlier and later recurrences of M. For definiteness, say that M-at-t's surrounding evolution occurs during the following interval: conditional on M-at-t, its beginning marks the most likely last point of entropic maximum (equilibrium) earlier than t, and its end marks the most likely first point of entropic maximum later than t. In other words:

[29] The focus on simplicity and salience here connects with the BSA framework we've assumed, viz. one using Elga's "test propositions" to measure fit and the notion of what is salient for creatures like us in measuring simplicity and informativeness. See 2.3.

a recurrence's surrounding evolution takes place during the interval around t of plus or minus its *relaxation time*.[30]

We turn to our second assumption about typical BPs:

> (4) For each typical recurrence r in w, there are infinitely many others such that they arise and evolve in largely the same way as r.

No doubt there are many highly likely ways for a typical recurrence's surrounding evolution to go. But quite plausibly in typical classical recurrence worlds, for each of these ways there are infinitely many recurrences that evolve quite similarly. Even unlikely things happen infinitely many times in eternal recurrence worlds, so surely *likely* things happen infinitely many times, too. And similarly-evolving typical recurrences are among the highly likely ways that a surrounding evolution could go.

(4) entails a relevant lemma about test propositions. Recall that in the context of SM, test propositions must include statements that salient macroscopic events occur. Given this, (4) entails that:

> (5) For each typical recurrence r in w, there are infinitely many others such that they are largely similar to r concerning which of their surrounding events correspond to test propositions.

If infinitely many recurrences are largely similar in their surrounding events, then whichever events are test propositions in one surrounding evolution will largely correspond to test propositions in the others. To extend our usage, call the test propositions corresponding to events surrounding a recurrence r the *surrounding test propositions* for both r and the BP corresponding to it.

Combining (5) with (3) and our definition of fit yields:

> (6) In w, for each BP p corresponding to a typical recurrence, there are infinitely many other BPs such that each of these, p', purchases roughly equal additional fit as p, concerning the surrounding test propositions of p and p', respectively.

That is, if infinitely many typical recurrences have largely similar sets of test propositions surrounding them, and the BPs corresponding to each assign roughly the same (high) SM probabilities to their respective surrounding events, then those BPs are roughly the same with respect to the fit that each purchases from their respective test propositions.

Of course, (6) alone is not sufficient to show that BPs corresponding to typical recurrences are roughly on a par for fit. For the fit they purchase overall concerns more than the probabilities they assign to their surrounding test propositions. What of their non-surrounding test propositions? These concern events that occur beyond a recurrence r's surrounding evolution, i.e. the events occurring beyond the interval of plus or minus r's relaxation time. In fact, it is a consequence of SM that no two BPs differ in the probabilities they assign to non-surrounding events. The reason: beyond the relaxation time – either forward or backward in time – the SM probabilities

[30] Recall that we focus on classical worlds that have a bounded phase space, with the result that they have an equilibrium state.

conditional on the recurrence invariably assign overwhelmingly high probabilities to the equilibrium state. (This is related to SM's prediction that a system at equilibrium stays at equilibrium.) So any two BPs make essentially the same statistical predictions for events falling outside their respective surrounding evolutions. That is:

(7) In w, any two BPs p and p' corresponding to typical recurrences are such that they purchase equal fit with respect to test propositions that surround neither p nor p'.

Moreover, consider any two BPs p and p' that are roughly on a par for the fit they earn from their respective surrounding test propositions: each will do as poorly as the other for the probabilities they assign to the other's surrounding test propositions. That is, they will each predict that equilibrium occurs during the other's surrounding evolution, rather than the interesting low-entropy events that in fact take place. And so:

(8) In w, any two typical BPs p and p', which earn the roughly the same fit for their respective surrounding events, are such that they earn equal fit for surrounding events of the other.

Together, (6), (7), and (8) concern the fit that may be earned from all test propositions. Hence, the roughly equal fit earned between p and p' for all of these entails:

(9) In w, for each BP p corresponding to a typical recurrence, there are infinitely many other BPs such that each, p', purchases roughly equal fit for all particular macroscopic events as p, and so p and p' are roughly equal in fit overall.

Finally, we combine the rough parity of simplicity, informativeness, and fit claimed by (1) and (9) to get:

(10) For each typical BP p, there are infinitely many others that are roughly equal to p with respect to all three of the BSA's desiderata.

For a world like w – i.e. a typical, classical world that (i) begins in a simple, low-entropy macrostate M, and (ii) has a bounded phase space, so that it undergoes infinite recurrence – any BP is such that it is either bested by or is roughly on a par with infinitely many other BPs.

3.2 Three salient possibilities

Which of these infinitely many BPs is part of a best system for such worlds? We consider three salient answers to this question:

(I) Zero BPs are part of a best system for such worlds.

(II) A finite, positive number of BPs are part of a best system for this world.

(III) Infinitely many BPs are part of a best system for this world.

We argue that none plausibly leads to there being (meaningful) lawful BPs, given the BSA. Clearly (I) leads straightforwardly to our conclusion. And so our arguments in the subsections below focus on (II) and (III). In outline, we argue that (II) results in infinitely many effectively tied-for-best systems, where different ways the BSA could treat such ties each yield our conclusion. Our response to (III), on the other hand, is different: we argue that this putative possibility is untenable for the BSA.

3.2.1 From (II) to effective ties

Suppose that a best system S includes n BPs, where $n > 0$, and consider an abitrary BP p in S. Given the conclusion of the previous section, there are infinitely many BPs such that each one, p', is roughly on a par with p overall. So consider a system S' which differs from S only in that it contains p' instead of p. Since p' is roughly on a par with p overall, then the same is true of S' and S.[31] And since on the BSA, systems that are roughly tied are effectively tied – i.e. a uniquely best system must be *robustly* best – it follows that S is effectively tied with S'. The reasoning generalizes to any BP in S, and thus to any putative best system containing n many BPs, for any $n > 0$.

What should the BSA say the laws are, when there are effectively tied-for-best systems? Recall the two main approaches discussed in 2.3. According to the first: if there is no robustly best system, then no theorems of any system deserve to be called 'laws', so there are none. If there are no laws, *a fortiori* there are no lawful boundary conditions, and so our conclusion follows. Accoding to the second approach: when there are effectively tied-for-best systems, the laws are the theorems shared between all such systems. For the latter, there *are* BSA-lawful boundary conditions at w, of a sort. For the disjunction of all the BPs found among the tied-for-best systems will be among the theorems for each tied system, and so this disjunction will be a law. Given the conclusion of the previous section, there are infinitely many such BPs, and so the lawful boundary condition in question here is an infinite disjunction – that the world is in state M at t_1 *or* state M at t_2 *or* ... for infinitely many ts. This is an extraordinarily weak constraint – adding such a constraint would yield chances virtually indistinguishable from those of CSM^-, and would be far too weak to help with the kinds of reversibility objections raised against CSM^-. Such lawful boundary conditions are hardly meaningful. So on either standard way of dealing with ties, our conclusion follows.

3.2.2 Against (III)

If a best system for w contained any finite number of BPs, our conclusion follows. But what of systems with infinitely many BPs – might one of those be best? In this section, we argue that this claim is untenable. In support of that conclusion, we offer three considerations, from specific to general. First: in a classical world, on one natural way of understanding the probabilities in question, the claim that a best system has

[31] This assumes that the contribution a BP makes to a system containing it is invariant from system to system, and so viz. that BP's contribution of fit is invariant. This assumption seems right, for it is plausible that BPs are statistically independent of one another.

infinitely many BPs is incoherent. Second: a best system with infinitely many BPs would undermine one of the main motivations for the BSA. And third: such a system's being best violates the very spirit of the BSA.

3.2.3 Incoherence

If one adopts a standard understanding of the probabilities involved, one can argue that it's incoherent to suppose that a system S with infinitely many BPs is best, in a classical world w. Why? Because infinitely many BPs cannot all earn their keep in such a system, and yet they must do so if that system is best.

In more detail, recall that BPs earn their keep in a best system by way of significantly increasing that system's fit, such that the fit they purchase outweighs their cost in simplicity. Otherwise, a system S^- lacking those BPs that do not significantly increase S's fit would be better than S: S^- would forgo the cost in simplicity of adding those BPs, with little sacrifice in fit. This yields the following necessary condition:

(N1) A system S containing some BPs is best only if each BP in it individually and significantly increases the fit of S.

Moreover, in a classical world a BP increases fit by increasing the SM probabilities of test propositions. In part, a BP increases SM probabilities by being added to the background propositions K in the SP:

$$Pr_K(A|B) = \frac{m(A \cap B \cap K)}{m(B \cap K)}$$

Adding a BP p to K increases fit when it increases this ratio and A is a test proposition. In general, adding p to K reduces both the numerator and denominator in the above ratio – call these the *SP numerator* and *denominator*. To significantly increase the SM probability of $(A|B)$, adding p to background propositions K must reduce the SP denominator by significantly less than the SP numerator. That can only be true if adding p to background propositions K reduces the SP denominator significantly full stop. We have, then, a necessary condition on a BP's significantly increasing the SM probability of test propositions, and so the fit of a system containing it:

(N2) A BP p increases the fit of a system S only if adding p to K significantly decreases the SP denominator.

Finally the argument. Suppose for reductio that S – which contains infinitely many BPs – is best in a classical recurrence world w. By (N1), it follows that each such BP significantly increases the fit of S. And by (N2), adding each of these BPs to the background propositions K must significantly decrease the SP denominator. Take any sequence of all the BPs in S, and add them one by one to K. Each addition reduces the SP denominator by a significant amount, and in the limit of adding them all the SP denominator goes to zero. (Because the reductions in the denominator must be significant, they must be non-vanishing.) Standardly, however, this would mean that the SM probabilities underwritten by S are all undefined, including those assigned to test propositions. Such a system earns no fit; hence its BPs do not earn their keep; and thus that system cannot be best.

Now there are various ways in which one might resist this argument (by adopting non-standard probabilities, for example). But we won't explore this issue further. For there are more general reasons to think that, given the BSA, an infinitary system like S cannot be best. We turn to these next.

3.2.4 Undermining the motivation and spirit of the BSA

Distinguish two kinds of infinite systems. We'll call an infinite system *robustly infinite* if there's no salient, intelligible and straightforward way to express it in finitary terms. By contrast, we'll call an infinite system *non-robustly infinite* if there is a salient, intelligible and straightforward way to express it in finitary terms.

For an example, consider the laws of CM, whose axioms plausibly include something like Newton's Second Law, standardly formulated as $F = ma$. Read literally, this formulation seems to suggest that the determinable property *force* is identical to the product of two other determinables, *mass* and *acceleration*. But that can't be right: a product of two determinables seems incoherent. Rather, the law expresses systematic relations between the determinate properties falling under those determinables. If axioms are to be formulated in terms of determinate properties – particular quantities of mass, acceleration, and force – then an infinitely large family of laws results, describing the relationship between triples of these determinates: $F_1 = m_1a_1$, $F_2 = m_2a_2$, $F_3 = m_3a_3$, etc. We take this to be a non-robustly infinite system. For we can intelligibly and straightforwardly formulate these laws in a finite way, e.g. by quantifying over the determinate properties and the systematic relationships that hold between them.[32]

By contrast, a system S that includes infinitely many BPs is robustly infinite. A finite axiomatization of such a system would require finitely expressing each of the infinitely many times that M recurs, as specified by S's constituent BPs. And there is no intelligible and straightforward way of doing this. And so S must include an infinitely long list of times at which M recurs.

As we're understanding the BSA, if a system is non-robustly infinite, this need not be a significant mark against its simplicity. For there's a salient way of formulating it in a finite manner that we have no trouble understanding. Robustly infinite systems, on the other hand, are deeply problematic – they do extraordinarily poorly with respect to the notion of simplicity relevant to the BSA. Since the system with infinitely many BPs we're considering is robustly infinite, it does poorly with respect to the BSA's criteria.

Moreover, the robustly infinite nature of such a system undermines a main motivation for the BSA, viz. that it mirrors actual scientific practice. Indeed, many have noted that the BSA is a kind of idealization of theory choice in science, using as an abductive base not our empirical evidence, but all the particular matters of fact over the course of the world's history. Woodward's (2013) characterization of this motivation is a good example:

> "A substantial part of the appeal of the BSA is that it is supposed to correspond (in a very idealized form) to how abductive inference and theory

[32] Cf. Hawthorne (2006) p236-7, and Eddon (2013) p96-7.

> choice in science work – the HSB [the Humean Supervenience Base] represents the most extensive body of inductive evidence we could possibly possess, and (it is contended) simplicity and strength are the criteria scientists actually employ in choosing theories and laws on the basis of this evidence."[33]

Take seriously the idea that the standards of simplicity and informativeness used in the BSA are those actually employed in science.[34] Then observe our straightforward rejection of proposals of laws that are infinitely complex. For instance, a revision of the standard model that included an infinite variety of fundamental particles (and an infinite variety that couldn't be described in a salient, intelligible and straightforward way) wouldn't be seriously considered. If the world had an infinite variety of particles, some finite systematization of them would be called for. And if no such systematization were possible, then such a world might well be deemed too chaotic to be lawful.

Our rejection of robustly infinite accounts of laws suggests that our standards of simplicity and informativeness do not allow such systems to be best, i.e. their cost in simplicity is insurmountable. If that's right, then any version of the BSA which allowed a robustly infinite system to be best would not accord with the standards of simplicity and informativeness employed by our scientific community, and would fail to line up with actual scientific practice.

The claim that a robustly infinite system like S is best clashes with the BSA in another way, for it violates the spirit of that view. In an early description of it, Lewis likens the BSA to "a *Concise Encyclopedia of Unified Science*" provided to humankind by God.[35] Concision is required, of course; God's List of All Truths would be useless for finite creatures like us. But for the same reason that God's List would be too complex for us to grasp, the same is true for a robustly infinite system like S. Supposing S to be best, then, would be to forsake this core idea of the BSA: that it provides the best systematization of truths about the world that is intelligible for creatures like us.

4 The Cosmological Case

In this section, we consider typical eternal inflation worlds, w', of the sort described in section 2.2: w' begins in a largely homogeneous macrostate M of high inflationary energy, and it goes on to evolve in a way that M infinitely recurs.[36]Analogous to the previous section, we argue that this recurrence vitiates any (meaningful) lawful boundary conditions at w', given the BSA. Like before, we'll restrict ourselves to what are plausibly the most eligible BPs: the PH and infinitely many MHs. And by 'recurrences' we'll narrowly mean recurrences of M.

[33] Woodward (2013), p49.

[34] See Eddon & Meacham (2015), sections 3.5 and 3.6.

[35] Lewis (1973), p74.

[36] A natural question is whether M in fact intitially occurs in w'. For M is highly uniform, but why think w' started out this way? Importantly, the initial conditions of w' cannot be too inhomogeneous, for otherwise inflation wouldn't begin. (See section 2.2 for discussion of this point.) So the initial conditions can only

It is again plausible that each BP incurs roughly the same cost in simplicity. One notable difference concerns the fact that M's initial occurrence is global, whereas its later occurrences are merely local. This means the PH and MHs do differ in complexity: the former is surely simpler, since it is easier to state (where does M occur? Everywhere!). But this difference is small. The main simplicity cost comes with having to specify a local region at all. But specifying different local regions requires little change in complexity. Different choices of coordinate systems make it arbitrarily easier (harder) to specify different local regions, and simplicity ought to be invariant between such choices. But the mere difference between having to specify a local region and not having to do so seems to involve no great jump in complexity. If so, then BPs stating that M occurs locally are close enough to be roughly equal in simplicity to the PH. So it is plausible that:

(1′) In w', each BP is roughly on a par with every other with respect to simplicity.

Turning to informativeness, it is important to observe a certain ambivalence in our judgments about which BPs are more or less informative. On the one hand, all BPs involve the same phenomenon occurring in regions of the same size, suggesting that they are equally informative. On the other hand, cosmic expansion means later recurrences are situated in larger spatial "arenas", so to speak, suggesting that later BPs tell us less about what's going on therein. Consider an analogy. You have a list of the books in your library's science fiction collection. The library then expands, adding new books but no new Sci-Fi titles. Post expansion, your list is just as accurate as it was, suggesting it is equally informative. But your list also now tells you less about the library's collection overall, suggesting it is less informative. One might call these *positive* and *negative* conceptions of informativeness: the former gauges a statement's informativeness by what it tells you; the latter gauges informativeness by how much it hasn't told you.

Both conceptions are intuitively compelling. Yet if informativeness is univocal – an implicit assumption of the BSA – they are incompatible. In what follows, we'll assume the former "positive" conception. This conception of informativeness yields the following:

be so inhomogeneous. Does this mean that M doesn't initially occur? In fact, it doesn't much matter. The effects of not-too-large initial inhomogeneities will be swamped by the ensuing spatial expansion and subsequent thermalization. So specifying those initial inhomogeneities – and paying the corresponding cost in simplicity – purchases only small increases in fit. At best then, those costs and benefits balance out, meaning a less homogeneous initial macrocondition in w' is roughly on a par with with M, had w' instead begin in that macrocondition. The possibility thus doesn't affect our argument, and so to simplify we assume in the main text that w' begins in M. Similar remarks apply to inflationary cosmological models that aim to explain later, local instances of M without positing an initial, global instance of high inflationary energy. An example is the model proposed by Carroll and Chen Carroll & Chen (2004). On that model, instances of M eventually arise via extremely unlikely quantum fluctuations from an otherwise largely static vacuum state. But a proposition p stating that this vacuum state globally holds at a time surely earns no more in fit compared to BPs stating when and where M occurs, nor does it seem simpler to express. Hence, it seems at best no more eligible to be part of the best system than do the BPs. Thanks to an anonymous referee for raising the question of models like Carroll and Chen's.

($2'$) In w', each BP is roughly on a par for informativeness.

Like before, we unpack the typicality of w' by way of specific assumptions about what typical recurrences are like in such a world. The first:

($3'$) In w', each typical recurrence r is such that its subsequent evolution is highly likely, conditional on the BP corresponding to r.

In the context of SM, we focussed on a recurrence's surrounding evolution, i.e. the macroscopic events occurring before and after it, within a certain interval. For eternal inflation, we focus instead on a recurrence's *subsequent evolution*, i.e. the later events over which the recurrence has causal influence. In other words, these are the events in the recurrence's future light cone.[37]Whatever chances the correct theory of eternal inflation trades in, typical recurrences are such that their forward light cones contain events that are highly likely (for chances of that theory), conditional on the relevant BP.

The second assumption:

($4'$) In w', each typical recurrence r is such that there are infinitely many other recurrences whose subsequent evolutions develop in largely similar ways to r.

Like before, ($4'$) plausibly entails a relevant lemma concerning test propositions:

($5'$) In w', each typical recurrence r is such that infinitely many other recurrences have subsequent evolutions which include roughly similar constellations of test propositions to r.

The following is a consequence of ($5'$) and ($3'$), plus our conception of fit:

($6'$) In w' and for each typical BP p, there are infinitely many other BPs p' such that p and p' earn roughly as much fit for the events in their respective subsequent evolutions.

What does ($6'$) tell us overall about the fit between such BPs? To better appreciate the fit a BP earns, it is helpful to return to our simplified diagram. Figure 3 illustrates the fit earned by the PH.

Recall that the chance of high inflationary energy in a region not decaying is low. Hence, conditional on the PH, a low chance is assigned to there being high inflationary energy in the region R where M next recurs. Likewise, the events comprising the subsequent evolution of M-in-R are also assigned lower chances conditional on PH. And the same is true for each next expanding region in the infinite sequence of such expansions. The result: the PH earns less and less fit for events in subsequent regions of expansion.

The subsequent evolution of the PH comprises all the events of w' and so all opportunities for the PH to earn fit. Now recall what ($6'$) says – that typical BPs earn roughly the same fit for events in their respective subsequent evolutions. Thus the

[37]Note that, given exponential expansion ensuing from that recurrence, the size of its light cone also grows exponentially.

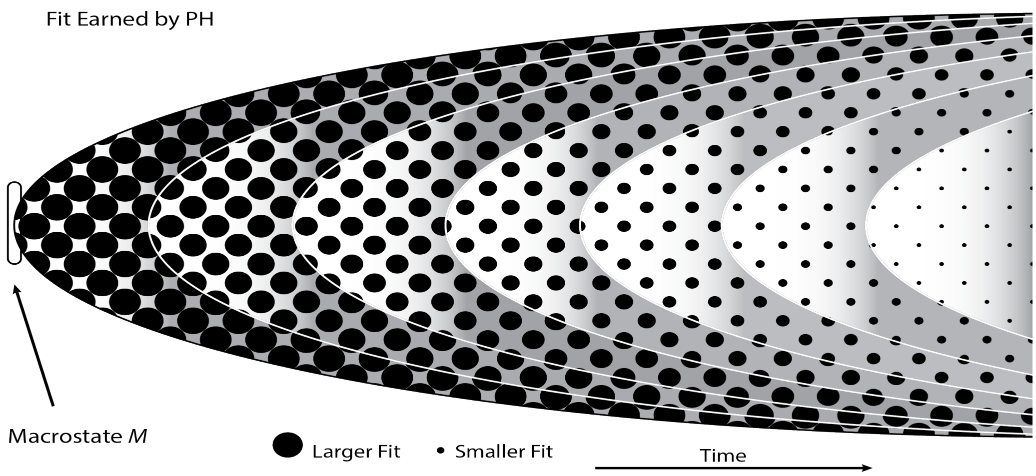

Figure 3: Fit Earned by PH

question of how the MHs stand fit-wise to the PH (and to each other) comes down to what fit they earn from events that aren't in its subsequent evolution, i.e., events outside their future light cones. This of course depends on what probabilities a MH underwrites for those events. It is plausible that these events have some low but non-zero probability, conditional on the relevant MH.[38]Figure 4 is a possible example.

The smaller the probabilities a MH underwrites for events outside its future light cone, the less additional fit it earns from those events, and the less difference this extra fit makes to a MH's fit overall. Assuming that these probabilities are small enough, and given (6′), we have the following:[39]

[38]Some versions of quantum mechanics rule this out. In particular, GRW theory has the consequence that later BPs underwrite no probabilities to events lying outside their future light cones. See see Albert (2000), chapter 7. In that case, (6′) alone secures the result that the relevant BPs are roughly on a par for fit.

[39]We assume this for simplicity (and because we take this to be plausible), but our conclusion follows regardless of the magnitudes of these probabilities. There are a few salient possibilities.

First, the probabilities a MH underwrites for events outside its future light cone might decrease the further they are (spatiotemporally) from the recurrence corresponding to the MH, at a rate fast enough to yield a limit on how much extra fit a MH earns beyond the fit it derives from events in its subsequent evolution. (Figure 4 represents this.) (1) If this limit is low, then although subsequent MHs have higher and higher fit, the differences are relatively small, and all of the MHs (and the PH) will be roughly on a par with respect to fit. (This is the possibility assumed in the text.) (2) Alternatively, if this limit is high, then earlier MHs (and the PH) may fall far enough behind with respect to fit to be eliminated as viable candidates for a best system. But there will still be infinitely many later MHs that are roughly on a par for fit, which is all we need to derive our conclusion.

(3) A different possibility is that the probabilities a MH underwrites for events outside its future light cone do not decrease, or don't decrease fast enough to yield a limit on how much extra fit a MH earns beyond the fit it derives from its subsequent evolution. If so, then for any BP, there is another BP that is arbitrarily better with respect to fit. This will yield an infinite sequence of better and better systems – since one can always trade worse BPs for better ones – so there will be no system that is best, and thus no BSA

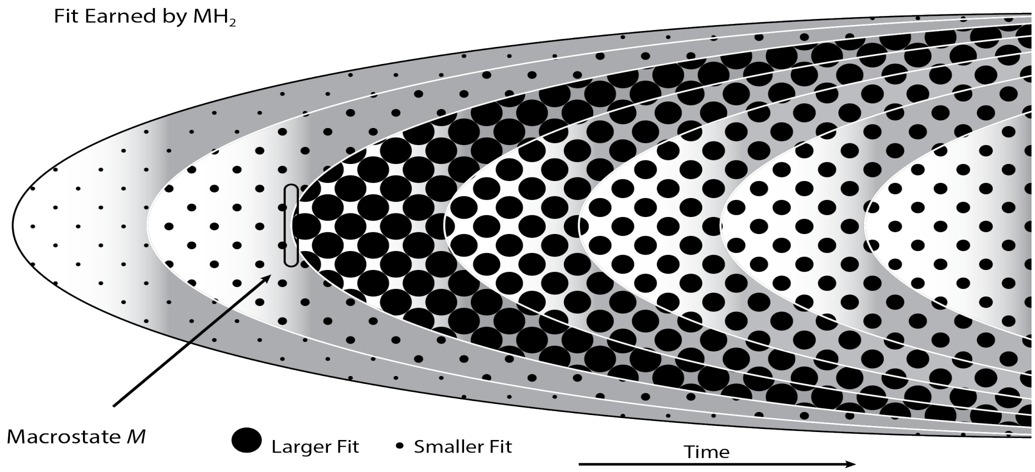

Figure 4: Fit Earned by MH_2

(7′) In w' and for each typical BP p, there are infinitely many other BPs that are roughly equal in fit to p, overall.

Finally, combining (7′) with (1′) and (2′) results in:

(8′) In w' and for each typical BP p, there are infinitely many other BPs that are roughly equal to p with respect to simplicity, informativeness and fit.

Given (8′), there are infinitely many systems that are effectively tied for best in w: for any system S with finitely many BPs, there are infinitely many other BPs that could be traded out to produce a system S' that is roughly just as good. (Recall from section 3.2.2 that it is untenable that a best system have infinitely many BPs.) And as argued in section 3.2.1, on either way the BSA might approach ties, the result is that no (meaningful) boundary conditions are lawful.

The foregoing argument is somewhat speculative. There's no established view on how one should conceive of informativeness on the BSA, nor is there a consensus with respect to how to conceive of fit. And, of course, the cosmological theory itself is somewhat speculative. But the assumptions made above are plausible, and we argue that the conclusion inherits this plausibility. If ours is a typical world of eternal inflation, and if the BSA is true, we have good reason to think that there are no (meaningful) lawful boundary conditions.

laws. *A fortiriori*, there will be no lawful constraints on boundary conditions, and our conclusion follows.

5 Skeptical Arguments

In section 2.1, we noted that it's standardly held that CSM^- (the standard classical statistical mechanical laws minus the Past Hypothesis) is self-undermining – the chances it assigns seem to rationally require subjects like us, with evidence like ours, to believe that CSM^- doesn't hold. This in turn suggests that subjects like us with evidence like ours should believe that CSM^- is false. Thus only theories like CSM, which posit lawful boundary conditions, should be serious contenders for belief.

In a similar vein, it's been suggested that the corresponding versions of cosmological theories which don't posit any lawful constraints on boundary conditions (COS^-) also seem to be self-undermining.[40] This again suggests that subjects like us with evidence like ours should believe COS^- to be false, and that only cosmological theories like COS, which posit lawful boundary conditions, should be serious contenders for belief.

Combined with the results from sections 3 and 4, this suggests trouble for the BSA. For the BSA entails that at typical eternal worlds there won't be any (or only very weak) lawful constraints on boundary conditions. And if subjects like us shouldn't believe such theories, then the BSA leads to a skeptical problem: that subjects like us at such worlds shouldn't believe that the laws are what they actually are.

In this section we'll show that this conclusion is mistaken. Starting with the classical case (in section 5.1) we'll spell out in detail the standard argument for the claim that subjects like us shouldn't believe CSM^-. Doing so will allow us to see (in section 5.2) that the standard argument is too quick – some of its premises become implausible when we consider eternal worlds. Focusing on eternal worlds, we'll then show (in section 5.3) that given certain assumptions, one can repair the argument against believing CSM^-. But we'll show that these assumptions allow one to construct a similar argument against believing CSM. Thus at typical eternal classical worlds – the worlds where the BSA yields CSM^- – CSM^- laws are no more subject to skeptical worries than CSM laws.

We'll then turn to theories of eternal inflation, i.e. COS and COS^-. Here we'll show (in section 5.4) that a similar dialectic obtains. In particular, we'll show how given certain assumptions, eternal inflation theories both with and without lawful boundary conditions are subject to skeptical worries, to the effect that we shouldn't believe such theories are true. And we'll show how given a different set of assumptions, eternal inflation theories both with and without lawful boundary conditions aren't subject to skeptical worries. Thus, again, we find that at typical eternal inflation worlds – the worlds where the BSA yields COS^- – COS^- laws are no more subject to skeptical worries than COS laws.

5.1 The Argument Against CSM^-

In this section we'll spell out the standard argument for why subjects like us shouldn't believe CSM^-. To simplify things, we'll start by introducing some notation.

[40] See Carroll (forthcomingb).

Let E be our total evidence. It corresponds to the set of centered worlds that are centered on possible individuals who have the same perceptual experiences and memories as we do. Then $\hat{E}$ corresponds to the set of centered worlds located at world at which there exists an individual who has the same perceptual experiences and memories as we do.

Let V be the proposition that we have total evidence E, and that our evidence about the past is largely veridical, i.e., not extraordinarily misleading. Then $\hat{V}$ corresponds to the set of centered worlds located at a world at which there exists an individual who has our total evidence E whose evidence about the past is largely veridical.

Consider the CSM^- worlds compatible with $\hat{E}$. Let the *canonical partition*$_{\hat{E}}$ of these $\hat{E} \wedge CSM^-$ worlds be the partition of these worlds into the coarsest background propositions $\hat{E}_i$ such that $\hat{E}_i$ fixes the non-dynamical properties of the world (e.g., the number of particles, the spatiotemporal extension, etc). So the canonical partition$_{\hat{E}}$ is the coarsest way of carving up the $\hat{E} \wedge CSM^-$ worlds into propositions that yield well-defined chance assignments to the dynamical properties of the system (the particle positions and velocities).

The standard argument for the conclusion that subjects with priors and evidence like ours should believe CSM^- is false requires three premises.[41]

The first premise of the argument is that for evidence E like ours, every element $\hat{E}_i$ of the canonical partition$_{\hat{E}}$ will be such that $ch_{CSM^-,\hat{E}_i}(\hat{V}) \approx 0$. That is, given any way things might be such that there exists a subject with our evidence, the chance according to CSM^- of there existing a subject with our evidence whose evidence about the past is largely veridical is approximately 0. This seems plausible because according to CSM^-, it seems much more likely that an individual with our evidence is the recent result of a spontaneous fluctuation from a higher entropy state – and so their evidence about the past is extraordinarily misleading – than it is that they came from the kind of low entropy past which which would make their evidence about the past largely veridical.[42]

The second premise of the argument, usually left implicit, is that for subjects with priors and evidence like ours, $cr_{\hat{E}}(\hat{V} \mid CSM^-) \approx cr_E(V \mid CSM^-)$. Roughly, the idea is that for subjects with priors like ours, changing our evidence from E to $\hat{E}$ and the object of our credence from V to $\hat{V}$ shouldn't really change what our credences are. In other words, whether we work with irreducibly *de se* propositions or their *de dicto* counterparts shouldn't really bear on our credences regarding subjects having veridical evidence like ours.

The third premise of the argument is that for subjects with priors and evidence like ours, $cr_E(CSM^- \mid \neg V) \approx 0$. That is, for subjects with priors and evidence like ours, credence in CSM^-, conditional on evidence about the past being extraordinarily misleading, is very low. For our only reasons for believing that, say, something like classical mechanics holds is that our evidence suggests strong agreement between what classical mechanics tells us and how the world has behaved so far. And if our

[41] See Meacham (forthcoming) for a (differently formatted) version of this argument.

[42] Of course there will be some special backgrounds K for which the CSM^- chance of $\hat{V}$ is reasonably high – e.g., Ks that specify that everything evolved from a (non-lawfully required) very low-entropy initial condition. But given evidence E like ours, such Ks won't be members of the canonical partition$_{\hat{E}}$.

evidence about the past turned out to be extraordinarily misleading, then we'd lose our reason for thinking there is such agreement, and thus lose our reason for believing classical mechanics holds.

Given these premises, the standard argument against believing CSM^- goes as follows:[43]

The Argument Against CSM^-:

P1. For evidence E like ours, every member E_i of canonical partition$_{\hat{E}}$ will be such that $ch_{CSM^-,E_i}(\hat{V}) \approx 0$.

P2. For subjects with priors and evidence like ours, $cr_{\hat{E}}(\hat{V} \mid CSM^-) \approx cr_E(V \mid CSM^-)$.

P3. For subjects with priors and evidence like ours, $cr_E(CSM^- \mid \neg V) \approx 0$.

L1. For rational subjects with priors and evidence E like ours, $cr_{\hat{E}}(\hat{V} \mid CSM^-) \approx 0$. [From P1, the Principal Principle and IC-Conditionalization.]

L2. For rational subjects with priors and evidence like ours, $cr_E(V \mid CSM^-) \approx 0$. [From P2, L1.]

L3. For rational subjects with priors and evidence like ours, $cr_E(V \wedge CSM^-) \approx 0$. [From L2.]

L4. For subjects with priors and evidence like ours, $cr_E(\neg V \wedge CSM^-) \approx 0$. [From P3.]

C. For rational subjects with priors and evidence like ours, $cr_E(CSM^-) = cr_E(V \wedge CSM^-) + cr_E(\neg V \wedge CSM^-) \approx 0$. [From L3, L4.]

Note that a similar argument doesn't seem to work against CSM because the analog of P1 is implausible. For unlike CSM^-, the CSM chance according of there being an E-having subject with veridical evidence, given that there exists an E-having subject, is reasonably high.

[43] To get L1, note that for evidence E like ours, P1 and the Principal Principle entail that rational subjects will be such that, for every $\hat{E}_i$ in the canonical partition$_{\hat{E}}$, $ic(\hat{V} \mid \hat{E}_i \wedge CSM^-) \approx 0$. Since these $\hat{E}_i$s form a partition of $\hat{E} \wedge CSM^-$, the probability axioms entail that $ic(\hat{V} \mid \hat{E} \wedge CSM^-) \approx 0$. That and IC-Conditionalization then entail that $cr_{\hat{E}}(\hat{V} \mid CSM^-) \approx 0$.

To get L2, note that L1 and P2 entail that for rational subjects with priors and evidence like ours, $cr_E(V \mid CSM^-) \approx 0$.

To get L3, note that L2 entails that $cr_E(V \wedge CSM^-) \approx 0$, since it can only be the case that $cr_E(V \mid CSM^-) \approx 0$ if the numerator of the conditional probability is much smaller than the denominator, which entails that the numerator of the conditional probability is very small full stop.

To get L4, note that P3 entails that $cr_E(CSM^- \wedge \neg V) \approx 0$, since it can only be the case that $cr_E(CSM^- \mid \neg V) \approx 0$ if the numerator of the conditional probability is much smaller than the denominator, which entails that the numerator of the conditional probability is very small full stop.

To get C, note that the probability axioms entail that $cr_E(CSM^-) = cr_E(V \wedge CSM^-) + cr_E(\neg V \wedge CSM^-)$. L3 entails that one of these terms is approximately zero, and L4 entails that the other term is also approximately zero. Thus for rational subjects with priors and evidence like ours, $cr_E(CSM^-) \approx 0$, which gives us our conclusion.

5.2 The Argument Against CSM^- and Eternal Worlds

The argument presented in section 5.1 is valid. But once we take the possibility of eternal worlds into account, worries arise regarding whether it's sound.

Consider P1, the claim that for evidence E like ours, every element $\hat{E}_i$ of the canonical partition$_{\hat{E}}$ is such that $ch_{CSM^-,\hat{E}_i}(\hat{V}) \approx 0$. Once we notice that there will be elements of the canonical partition$_{\hat{E}}$ that pick out eternal worlds, this claim seems false, because there will be some $\hat{E}_i$ such that $ch_{CSM^-,\hat{E}_i}(\hat{V}) \not\approx 0$. To see this, consider an $\hat{E}_i$ which specifies that the temporal extension of the world is infinite, and consider the chance of such a world coalescing into the kind of very low entropy macrostate that the Past Hypothesis refers to. This will be extraordinarily unlikely to happen in any time scale we're familiar with, but given enough time, the chance goes to 1. Likewise, the chance of the world coalescing into that low-entropy state and then evolving (over the next several billion years) to give rise to a subject with evidence E that's largely veridical about the past, is extraordinarily low over any time scale we're familiar with. But again, given enough time, the chance goes to 1. Since $\hat{V}$ is the proposition that there exists an E-having subject whose evidence about the past is largely veridical, this entails that $ch_{CSM^-,\hat{E}_i}(\hat{V}) \approx 1$, not 0. Thus we have a counterexample to P1.

Likewise, consider P2, the claim that for subjects with priors and evidence like ours, $cr_{\hat{E}}(\hat{V} \mid CSM^-) \approx cr_E(V \mid CSM^-)$. But once we take eternal worlds into consideration, this claim is implausible. To see this, consider a subject whose priors assign most of their credence in CSM^- to an element $\hat{E}_i$ of the canonical partition$_{\hat{E}}$ which entails that the world is eternal.[44] As we've just seen, for such $\hat{E}_i$, $ch_{CSM^-,\hat{E}_i}(\hat{V}) \approx 1$. It follows from the Principal Principle that $ic(\hat{V} \mid CSM^- \wedge \hat{E}_i) \approx 1$, and thus (given our stipulation) that $ic(\hat{V} \mid CSM^- \wedge \hat{E}) \approx 1$. Given IC-Conditionalization, it then follows that $cr_{\hat{E}}(\hat{V} \mid CSM^-) \approx 1$.[45] But while holding such beliefs seems plausible for such a subject (since at an eternal CSM^- world the chance of *some* subject with evidence E having veridical evidence about the past is $\approx$1), holding that $cr_E(V \mid CSM^-) \approx 1$ (as P2 would require) does not. After all, that would entail that, conditional on CSM^- obtaining, they're virtually certain that their evidence is largely veridical, despite being confident that there are infinitely many subjects with the same evidence for whom this evidence is extraordinarily misleading! Thus at least sometimes subjects with priors and evidence like ours are such that $cr_{\hat{E}}(\hat{V} \mid CSM^-) \not\approx cr_E(V \mid CSM^-)$.

Here's another way to see why P2 is implausible. Suppose you know you're in a CSM^- world, that you have evidence E, and that there are 10^{100} E-having subjects. P2 requires your credence that *someone* has veridical evidence E ($\hat{V}$) to be the same as your credence that *you* have veridical evidence E (V). And your credence that someone has veridical evidence E will be the same (≈ 1) whether you know that only one of the 10^{100} E-having subjects has veridical evidence or whether you know that all of the 10^{100} E-having subjects have veridical evidence. Thus P2 requires your credence that your evidence E is veridical to be the same whether you know that only one of

[44] This assumption simplifies the argument, but is stronger than required. All that's needed is the assumption that a subject with priors like ours might assign a non-trivial amount of their credence in CSM^- to element(s) $\hat{E}_i$ that entail the world is eternal.

[45] Assuming, of course, that the subject is approximately rational, at least in these respects.

the 10^{100} E-having subjects has veridical evidence or whether you know that all of the 10^{100} E-having subjects have veridical evidence. But many subjects like us would not be virtually certain that their evidence E is veridical if they knew that only one of the 10^{100} subjects with evidence E is situated such that their evidence is veridical.

5.3 Repairing the Argument Against CSM^-

Once we take eternal worlds into consideration, the argument for disbelieving CSM^- no longer goes through. But it's natural to think that one could repair the argument against CSM^- so that it does work at eternal worlds. Let's look at how one might do this.

Since our primary concern is eternal worlds, we can simplify the dialectic by restricting our attention to typical eternal worlds.[46] Let "CSM^-_∞" stand for the conjunction of CSM^- and the claim that the world is eternal. Similarly, let "CSM_∞" stand for the conjunction of CSM and the claim that the world is eternal. In what follows, we'll focus our attention on whether one can construct skeptical arguments against CSM^-_∞ and CSM_∞.

In the Argument Against CSM^-, P1 and P2 serve the role of allowing us to derive L2: that $cr_E(V \mid CSM^-) \approx 0$. To avoid the worries facing P1 and P2, we need to provide a rationale for the eternal version of L2 that doesn't rely on these suspect premises. It follows from the probability axioms that:

$$cr_E(V \mid CSM^-_\infty) = \sum_{w \in CSM^-_\infty} \frac{cr_E(w) cr_E(V \mid w)}{cr_E(CSM^-_\infty)}$$

One natural thought is that for subjects like us, $cr_E(V \mid w)$ will be generally equal to the proportion of E-having subjects at w whose evidence is veridical. After all, if there are two E-having subjects at a world, and that evidence is veridical for one of them, then adopting a credence that your evidence is veridical (given w) that's greater than $1/2$ might seem overly optimistic, and a credence lower than $1/2$ unduly pessimistic. Call this assumption about the priors of subjects like us the Indifference Assumption.[47] At typical CSM^-_∞ worlds the vast majority of E-having subjects are situated such that their evidence about the past is extraordinarily misleading, since it's much more likely (according to CSM^-_∞) for a subject with E to have fluctuated into existence from a higher entropy state than it is for them to have correctly remembered that they came from an even lower entropy state. Thus given the Indifference Assumption, and the assumption that for subjects like us most of the credence in CSM^-_∞ is assigned to CSM^-_∞ worlds that are typical, it follows that one's credence that one's evidence about the past is largely veridical given CSM^-_∞ should be very low; i.e., $cr_E(V \mid CSM^-_\infty) \approx 0$.

But this argument runs into difficulties when one realizes the infinite numbers involved. For this argument relies on the intuitively plausible claim that the vast

[46] That is, eternal worlds which satisfy the conditions for recurrence.

[47] This constraint is an instance of the restricted Indifference Principle proposed by Elga (2004a). Elga's Indifference Principle is contentious (see Weatherson (2005)). But the argument under consideration doesn't need to endorse Elga's Indifference Principle, it merely needs to assume that subjects like us generally have priors that line up with the prescriptions Elga's Indifference Principle makes.

majority of E-having subjects don't have veridical evidence about the past. And this claim is hard to make sense of at typical CSM^-_∞ worlds, since such worlds will contain both infinitely many E-having subjects with veridical evidence and infinitely many E-having subjects with non-veridical evidence. (This is an instance of what cosmologists call "the measure problem".[48])

In order to circumvent these difficulties, we need a more sophisticated way of measuring ratios between E-having subjects with and without veridical evidence about the past. Here is a schema for how to provide such an account.[49] Specify a way of picking out a spatiotemporal region at a world containing a finite number of centered worlds that are subjectively indistinguishable from your own. Assess the proportion of these centered worlds at which A is true. Then specify a way of sequentially expanding this region, and assess the proportion of centered worlds at which A is true for these larger and larger regions. If the proportion of centered worlds at which A is true converges to x in the limit, then (according to the account) x is the correct proportion of these centered worlds at which A is true.[50]

Of course, this is just a schema, and there are a number of ways to fill in the details. But for a wide range of plausible ways of filling in these details, the proportion of E-having subjects with veridical evidence will converge to ≈ 0. And if subjects like us generally have priors corresponding to such a proposal, and most of our credence in CSM^-_∞ worlds is assigned to typical CSM^-_∞ worlds, then it follows that subjects like us should generally be such that $cr_E(V \mid CSM^-_\infty) \approx 0$.[51]

Thus given the Indifference Assumption and a plausible way of applying it to the infinite case, we can modify the Argument Against CSM^- so that it works given eternal worlds, as follows:

The Modified Argument Against CSM^-_∞:

P1. For rational subjects with priors and evidence like ours, $cr_E(V \mid CSM^-_\infty) \approx 0$.

P2. For rational subjects with priors and evidence like ours, $cr_E(CSM^-_\infty \mid \neg V) \approx 0$.

L1. For rational subjects with priors and evidence like ours, $cr_E(V \wedge CSM^-_\infty) \approx 0$. [From P1.]

L2. For rational subjects with priors and evidence like ours, $cr_E(\neg V \wedge CSM^-_\infty) \approx 0$. [From P2.]

C. For rational subjects with priors and evidence like ours, $cr_E(CSM^-_\infty) = cr_E(V \wedge CSM^-_\infty) + cr_E(\neg V \wedge CSM^-_\infty) \approx 0$. [From L1, L2.]

[48] See Carroll (forthcomingb), and the references therein.

[49] See Arntzenius & Dorr (2017) and Carroll (forthcomingb).

[50] The relevant notion of "correct" here is correct with respect to the Indifference Assumption; i.e., a correct description of the priors of subjects like us.

[51] It's perhaps worth flagging that, as we note in section 2.4, we're assuming a somewhat permissivist account of rational priors here. Thus the justification for holding that subjects like us will have priors that yield $cr_E(V \mid CSM^-_\infty) \approx 0$, is simply that it's plausible that subjects like us have priors which roughly satisfy the condition we've described. And if someone had rational priors that didn't satisfy these conditions, then this skeptical argument regarding CSM^-_∞ wouldn't apply to them. (We thank a referee for encouraging us to address this question.)

The conclusion of this argument is that rational subjects with priors and evidence like ours should be virtually certain that CSM_∞^- is false. This leaves open the question of whether the same is true for CSM_∞. Can we construct a similar argument against believing CSM_∞?

Let's consider how the argument looks if we replace CSM_∞^- with CSM_∞ throughout. The argument will remain valid, so the only question is whether it's sound.

The CSM_∞ version of P2 (roughly, that for subjects like us $cr_E(CSM_\infty \mid \neg V) \approx 0$) is just as plausible as the CSM_∞^- version. For in both cases, it's *prima facie* plausible that if our evidence about the past is largely misleading, then our credence in the kinds of theories suggested by this evidence – e.g., theories to the effect that the world behaves according to the laws of classical mechanics – would drop.

The CSM_∞ version of P1 (roughly, that for subjects like us $cr_E(V \mid CSM_\infty) \approx 0$) is also plausible. Let Δ_r be the interval of time that starts at the initial state, and ends when the world first reaches equilibrium. It's true that at CSM_∞ worlds the ratio of E-having subjects with veridical evidence will generally be greater than at CSM_∞^- worlds during Δ_r. But in intervals of time following Δ_r, the expected proportion of E-having subjects with veridical evidence will be exactly the same at CSM_∞ and CSM_∞^- worlds – namely, very small. And as we take more and more of the time following Δ_r into account, the impact of the post-Δ_r period will continue to increase, eventually swamping the contributions of the Δ_r period. Thus in the limit, the expected proportion of E-having subjects with veridical evidence will be the same at CSM_∞ worlds and CSM_∞^- worlds. And so the claim that $cr_E(V \mid CSM_\infty) \approx 0$ is just as plausible as the claim that $cr_E(V \mid CSM_\infty^-) \approx 0$.[52]

So, once we restrict our attention to eternal worlds, we find that this kind of skeptical argument can be raised against both CSM_∞^- and CSM_∞. In either case, given certain assumptions (i.e., the Indifference Assumption, a plausible way of applying it to the infinite case, and the assumption that most of our credence in CSM_∞^- is assigned to typical CSM_∞^- worlds), one can argue that subjects like us should be virtually certain these theories are false. Thus at typical eternal classical worlds – the worlds where the BSA yields CSM^- laws – CSM^- laws are no more subject to skeptical worries than CSM laws. And thus the fact that the BSA yields CSM^- laws at such worlds is not clearly a mark against it.

5.4 The Argument Against COS_∞^-

Let "COS_∞^-" stand for the conjunction of COS^- and the claim that the world is eternal, and let "COS_∞" stand for the conjunction of COS and the claim that the world is eternal. In the previous section we looked at arguments for why subjects like us shouldn't believe CSM_∞^-. And we assessed whether such arguments give us a reason to favor CSM_∞ over CSM_∞^-. In this section we'll consider arguments for why subjects like us shouldn't believe COS_∞^-, and whether such arguments give us a reason to favor COS_∞ over COS_∞^-.

[52] Again, we're assuming here that it's plausible that subjects like us have priors which roughly satisfy the condition we've described (cf. footnote 51). For subjects with priors that don't satisfy these conditions, this argument isn't rationally compelling. (We thank a referee for encouraging us to address this question.)

In section 5.3 we presented a valid argument for the conclusion that subjects like us shouldn't believe CSM^-_∞. By replacing CSM^-_∞ with COS^-_∞, we can provide a similar valid argument for the conclusion that subjects like us shouldn't believe COS^-_∞:

The Argument Against COS^-_∞:

P1. For rational subjects with priors and evidence like ours, $cr_E(V \mid COS^-_\infty) \approx 0$.

P2. For rational subjects with priors and evidence like ours, $cr_E(COS^-_\infty \mid \neg V) \approx 0$.

L1. For rational subjects with priors and evidence like ours, $cr_E(V \wedge COS^-_\infty) \approx 0$. [From P1.]

L2. For rational subjects with priors and evidence like ours, $cr_E(\neg V \wedge COS^-_\infty) \approx 0$. [From P2.]

C. For rational subjects with priors and evidence like ours, $cr_E(COS^-_\infty) = cr_E(V \wedge COS^-_\infty) + cr_E(\neg V \wedge COS^-_\infty) \approx 0$. [From L1, L2.]

The second premise of this argument is plausible for the same reasons as before. If our evidence about the past is extraordinarily misleading, then we'd have little reason to be confident that something like COS^-_∞ – an account largely motivated by past empirical evidence – is true. So the soundness of this argument hangs on the first premise. Is it plausible that rational subjects like us will be virtually certain that our evidence isn't veridical given COS^-_∞?

The typical COS^-_∞ worlds will be eternal inflation worlds with both infinitely many E-having subjects whose evidence is veridical and infinitely many E-having subjects whose evidence is not veridical. So if one assumes that our credence that our evidence is veridical generally mirrors the expected proportion of E-having subjects whose evidence is veridical – that is, if one adopts the Indifference Assumption – then one will run into the same kinds of comparing infinities worries that came up in section 5.3.

We can address this problem in the same way as before: by adopting an account of how to correctly measure proportions in these infinite cases, and using that to apply the Indifference Assumption. For eternal CSM^- worlds it seemed like most reasonable ways of measuring proportions yield the result that vanishingly few E-having subjects are positioned so that their evidence is veridical. The same is not true at eternal inflation worlds. For the fractal structure of typical eternal inflation worlds allows for different reasonable measures to yield wildly diverging results.

For example, some reasonable ways of measuring proportions will yield the result that the vast majority of E-having subjects were spontaneously created near the beginning of the local big bang of some emerging bubble universe, and so they have evidence about the past that's extraordinarily misleading. Other reasonable ways of measuring proportions will yield the result that the vast majority of E-having subjects were spontaneously created in empty space at times long after the local big bang, and have evidence about the past that's extraordinarily misleading. And yet a third class of reasonable ways of measuring proportions will yield the result that some non-trivial number of E-having subjects – perhaps the majority of them – came about in a manner similar to how we did, and have evidence about the past that's largely veridical.[53]

[53] See De Simone et al. (2010) for examples of all three kinds of measures.

So the plausibility of the first premise of the Argument Against COS^-_∞ hangs on what we take the correct way to measure these proportions to be. Given some reasonable measures, P1 will be plausible; for others, it will not.

Now let's assess the merits of a similar argument against COS_∞. This argument will be the same as the Argument Against COS^-_∞, but with COS^-_∞ replaced by COS_∞.

As before, the argument will be valid, and P2 will be plausible. So the plausibility of the argument against COS_∞ hangs on the plausibility of P1 – the claim that rational subjects like us will be virtually certain our evidence isn't veridical given COS_∞.

As before, if one takes the Indifference Assumption to be plausible, then it's natural to address this question by adopting an account of how to correctly measure proportions in infinite cases, and using that to apply the Indifference Assumption. And, again, different ways of measuring proportions will yield different results. Some ways of measuring proportions will yield the result that despite the lawful constraints on initial conditions imposed by COS_∞, the vast majority of E-having subjects will have evidence about the past which is extraordinarily misleading. While other ways of measuring proportions will yield the result that a significant proportion – perhaps even the majority of – E-having subjects will have evidence about the past which is largely veridical.

So, as in the classical case, at eternal worlds this kind of skeptical argument can be raised against both COS^-_∞ and COS_∞. Given some ways of measuring proportions, both theories will succumb to such arguments; given some other ways of measuring proportions, both theories will escape such arguments. Either way, these considerations give us little reason to think that COS^- is any more subject to skeptical worries than COS. Thus the fact that the BSA yields COS^- laws at typical eternal inflation worlds is not clearly a problem for the BSA.

6 Conclusion

In this paper we've looked at two prominent theories that take there to be lawful constraints on boundary conditions. And we've argued that at typical eternal worlds of the kind these laws describe, the BSA won't take these constraints to be laws.

It's generally thought that, for certain theories, the lawful constraints on boundary conditions allow us to avoid skeptical results. Without them, it seems, we should believe that it's extremely likely that we're the result of spontaneous fluctuations out of the void with highly misleading evidence about the past. So, at first glance, the conclusion that the BSA won't yield such lawful BPs at eternal worlds seems like a threat to the tenability of the BSA.

But we've argued that, surprisingly, at eternal worlds the versions of these theories without lawful BPs are no worse off than the versions which include lawful BPs. In broad strokes, the reason is that the kinds of lawful constraints on initial conditions some versions of these theories impose only have a finite effect on the ratio of E-having subjects with veridical versus non-veridical evidence. And as the duration of the universe increases, the impact of such lawful initial conditions becomes less and less meaningful, and in the limit has no effect on the proportion of E-having subjects with veridical versus non-veridical evidence. Thus at eternal worlds, we end up in the same epistemic situation regardless of whether we believe theories which posit such lawful constraints on initial conditions or not.

So, at the end of the day, we suggest that this is not really a problem for the BSA. Although the BSA's deviations from these prominent theories is initially surprising, it's not clear that it gives us any reason to be concerned about the BSA.[54]

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[54]We'd like to thank Valia Allori and an anonymous referee for helpful comments and discussion.

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