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Statistical Mechanical Imperialism

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12.1 Introduction

Let us suppose that the basic fundamental laws of nature are given by the axioms of the best system of the fundamental properties of the world (Lewis, 1994; Loewer, 1996; Hall, 2009). And let us suppose that it is a harmless idealization to treat these as the deterministic, time-reversal invariant laws of Newtonian mechanics. The non-basic fundamental laws are (subsets of) the theorems of that system. Let us further suppose that the basic non-fundamental laws of nature are given by the axioms of the extension of this best system to the non-fundamental properties of the world. The non-basic non-fundamental laws are (subsets of) the theorems of that system.¹

David Albert (2000, 2012, Ch. 8) and Barry Loewer (1996, 2001, 2004, 2007, 2008, 2009, 2012a, 2012b; forthcoming) have defended the following remarkable hypothesis:

STATISTICAL MECHANICAL IMPERIALISM (IMP) The only basic non-fundamental laws are those required to ground the second law of thermodynamics.

More precisely, according to (IMP) the only basic non-fundamental laws are the following:

PAST HYPOTHESIS (PH) The initial macroscopic state of the universe is one of extremely low entropy.

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¹ For the sake of argument, I take for granted a preordained distinction between fundamental and non-fundamental properties. By fundamental and non-fundamental laws, I simply mean laws that are grounded in the manner stipulated. Following Winsberg (2008: 884), I have characterized this view of laws in terms of a two-stage process where we first fix the best system for the fundamental laws and then extend the system to fix the non-fundamental laws. An alternative characterization can be made in terms of a one-stage process where we simultaneously fix the fundamental and non-fundamental laws (see Frisch, Ch. 11). The differences do not matter for present purposes.

STATISTICAL POSTULATE (PROB) The probability that a given macroscopic state is realized by a given microscopic state is provided by the canonical statistical mechanical probability distribution for that macroscopic state, conditional on PH.

Inspired by the film *A Serious Man*, Albert and Loewer have recently taken to referring to the fundamental laws, PH and PROB collectively as ‘the Mentaculus’. Accordingly, in what follows I will collectively refer to them as MENTACULUS.

Winsberg (2008: 884) wonders why it is that PH and PROB make it into the set of basic non-fundamental laws at the expense of the second law itself. There are two worries here. The first worry is that there is no ‘single description of the world’ for which the fundamental laws, PH and PROB provide the best system. The idea is that if we best systematize the fundamental properties we will get the fundamental laws, while if we best systematize the thermodynamic properties we will get the second law. In neither case do PH and PROB make an appearance. Loewer (2007: 305 n. 23) is explicit however that we are considering the best system for the *conjunction* of the fundamental properties and the thermodynamic properties (among others). And the claim is that, relative to this set of properties, PH and PROB will be laws.²

The second worry is why this should be so. Why isn’t the conjunction of the fundamental laws with the second law at least as simple and strong as the conjunction of the fundamental laws with PH and PROB? The answer to this turns on the fact that PH and PROB promise to *explain* why the second law holds by connecting the fundamental properties with the non-fundamental properties. They thereby add strength without countervailing loss of simplicity.³ This is revealed in two ways. First, PH and PROB explain why the second law has exceptions, and explain the frequencies of the exceptions (Albert, 2012). Second, if Albert and Loewer are right that *all* non-fundamental laws can be explained in the same way, then PH and PROB will clearly be much stronger than the second law alone.

Why should we believe that all of the laws of the non-fundamental sciences are logical consequences of MENTACULUS? Here is an argument that can be discerned in Loewer (2008) and Albert (2012):

1. MENTACULUS makes ‘good empirical predictions about the values of the thermodynamic parameters of macroscopic systems’ (Albert, 2012: 20).
2. The empirical success of MENTACULUS warrants belief that it is true qua theory of thermodynamics. (To be true qua theory of *X* is to truly specify the propositions that determine the objective chances of the *X* properties obtaining.)
3. Therefore we should believe that MENTACULUS is true qua theory of thermodynamics (1, 2).

² Of course, it is natural to wonder what the motivation for this starting point is. For discussion, see Frisch, Ch. 11.

³ For the role that these notions play in the best system view of laws see Frisch, Ch. 11.

4. MENTACULUS has the logical consequence that probabilities are assigned to all nomologically possible propositions and so in principle provides a ‘complete scientific theory of the universe’ (Albert, 2012: 21).
5. Therefore we should believe that MENTACULUS is true qua theory of everything (3, 4). (To be true qua theory of everything is to truly specify the propositions that determine the objective chances of all nomologically possible propositions.)
6. The laws of the non-fundamental sciences are the propositions concerning non-fundamental properties that are assigned a high probability of obtaining by MENTACULUS.⁴
7. When propositions concerning non-fundamental properties are well confirmed as belonging to the best system of those properties, we have reason to believe that they have a high probability of obtaining.
8. Therefore we have reason to believe that the well-confirmed laws of the non-fundamental sciences are logical consequences of MENTACULUS (5, 6, 7).
9. Therefore the explanations provided by the non-fundamental sciences are in principle reducible to the explanations provided by MENTACULUS (8) (‘those chances are going to bring with them—in principle—the complete explanatory apparatus of the special sciences’, Albert, 2012).

To support this reading of the argument for IMP, consider the discussion by Albert and Loewer of an example due to Kitcher (2001: p. 71). Kitcher notes John Arbuthnot’s discovery that male births outnumbered female births in London for the eighty-two years following 1623, and compares two explanations:

DERIVATION Specify the complete microscopic state of the world at 1623 and use the fundamental laws (in conjunction with appropriate bridge laws) to derive the exact number of male births and the exact number of female births.

EQUILIBRIUM Show that when certain constraints are satisfied, the equilibrium sex ratio at reproductive age in biological populations will be 1:1, that the human population in London in 1623 satisfies those constraints, and that the mortality rates of male and female children in London in 1623 differ by the ratio required to produce the equilibrium ratio.⁵

Albert and Loewer focus on Kitcher’s diagnosis of the defect of DERIVATION, which is that it ‘would not show that Arbuthnot’s regularity was anything more than a gigantic coincidence’ (Kitcher, 2001: 71). In terms of the argument as I have outlined it, they both reply by presupposing (5) and arguing from (6) and (7) to (8). That is, they argue that since Arbuthnot’s regularity is well confirmed, it must be given a

⁴ Thus stated, it is vague what the laws of the non-fundamental sciences are. An alternative would be to render lawhood a matter of degree. I set this issue aside, as it will not be important for what follows.

⁵ For more on the structure of this variety of explanation, see Sober (1983). For a standard biological reference work see Charnov (1982).

high probability by MENTACULUS.⁶ They then interpret Kitcher as affirming (7) while denying (8) which—given (5) and (6)—is simply inconsistent:

it gets hard to see what Philip can possibly have had in mind in supposing that something can amount to a ‘gigantic coincidence’ from the standpoint of the true and complete and universal fundamental physical theory of the world and yet (somehow or other) *not* be (Albert, 2012).

if a regularity is lawful then it must also be likely and MENTACULUS is the arbiter of what is likely (Loewer, 2008: 161).

For now, I merely want to use this example to support my reading of the argument for IMP (later, I will argue that the explanatory advantage of EQUILIBRIUM is not located where Kitcher says it is). In the remainder of this chapter I explore the merits of this argument. In §12.2 I consider the argument from (1) and (2) to (3). In §12.3 I consider the argument from (3) and (4) to (5). In §12.4 I consider the argument from (5), (6), and (7) to (8). And in §12.5 I consider the argument from (8) to (9).

12.2 What does the Mentaculus Explain?

The bulk of Albert (2000) is dedicated to arguing for (1). This argument has received considerable attention in the literature, which I will not add to here.⁷ Instead, in this section I focus on an argument Albert (2000) gives for (2).

On the face of it, a simpler hypothesis for making thermodynamic predictions is simply PROB and the fundamental laws. These two together suffice to predict all thermodynamic phenomena (including exceptions to the second law). So PH looks redundant. If so, then since there exists a simpler, empirically equivalent theory, we should not infer the truth of MENTACULUS from its empirical success. Now a natural reply here is to say that, while this is true for predictions regarding future thermodynamic behaviour, it is false for predictions of past thermodynamic behaviour, for which PH is required. Indeed, this is why PH was introduced in the first place. Albert, however, argues in addition that one of the grounds for believing MENTACULUS is that it underwrites predictions in the future that PROB and the fundamental laws alone do not. Here is how the argument goes.

Let us call the probabilities conferred by PROB and the fundamental laws alone the SM-probabilities, the probabilities conferred by MENTACULUS the PH-probabilities, and the probabilities in the special sciences the SS-probabilities (throughout, I take the special sciences to be all sciences involving non-fundamental properties).

Albert (2000: 65) notes that there are many SS-probabilities that the SM-probabilities by themselves do not entail. Albert’s example is the probability of the location of a spatula among apartments that contain spatulas (a generalization he calls ‘very *general* and *robust* and *lawlike*’, p. 95). The SM-probabilities assign equal probability to all locations with the same spatial volume, but we know that it is more

⁶ Callender and Cohen (2010: 436–7) also read Albert and Loewer as reasoning in this way.

⁷ Callender (2011) is an excellent survey. See also Leeds, 2003; Winsberg, 2004; Parker, 2005; and Earman, 2006.

likely that the spatula will be in the kitchen than in the bathroom. Albert considers a response to this where we limit the domain of the SM-probabilities, so that they are silent on the locations of spatulas, and more generally only tell us the probabilities of microstates conditional on a limited class of macrostates. That is, he considers the response that we should believe that the SM-probabilities are true qua theory of thermodynamics but not true qua theory of spatulas. His reply to this is worth quoting (pp. 66–7):

The trouble with the *original* postulate (remember) was that it seemed to be making false claims about (say) the locations of spatulas in apartments. And what we've done by way of *solving* that problem is simply to *rewrite* the postulate in such a way as to preclude it from making *any* claims about things like the locations of spatulas in apartments *at all*. And that would seem—or it *might* seem—to go a bit *too far*. There *do* appear to be such things in the world, after all, as robust statistical regularities about the locations of spatulas in apartments. And whatever regularities there *are* will be rendered altogether *uncapturable* by our fundamental statistical postulate if we fix that postulate up as I am here proposing.

The assumption of this passage is that we ought to expect statistical mechanics to provide us not merely with probabilities for thermodynamic phenomena, but for all phenomena whatsoever.

Albert, crediting Feynman (1965), goes on to argue that while MENTACULUS does not make any predictions that differ from the SM-probabilities concerning thermodynamic properties, it does make different predictions concerning non-thermodynamic properties such as the locations of spatulas. Here is what he says (2000: 94–5):

if the distribution I use is the one that's uniform over those regions of the phase space of the universe which are compatible *both* with everything I have yet been able to observe of its present physical situation *and* with its having initially started out with a *big bang*, *then* (and *only then*) there is going to be good reason to believe that (for example) spatulas typically *get to be where they are* in apartments only by means of the intentional behaviours of *human agents*, and that what human agents typically *intend* vis-à-vis spatulas is that they should be in *kitchen drawers*.

It is far from clear, however, that this is correct. Why should we believe that the low entropy condition for the initial state of the universe specified by \mathcal{P}_H is one that would underwrite our ordinary inferences, rather than one that would undermine them? We can grant that the reliability of predictions that presuppose the former rather than the latter gives us reason to believe in it; the question is why we should believe that we have any independent justification, via statistical mechanics, that this is the case. That is, the question is why we should think that \mathcal{P}_H is not only necessary but also sufficient for grounding the inferences Albert claims it does, as when he concludes that the \mathcal{P}_H -probabilities 'appears to get the story about spatulas *just right*' (2000: 96).

Consider, for instance, the hypothesis that vastly intelligent and powerful aliens play jokes on humans by frequently moving spatulas from kitchens to bathrooms while leaving no macroscopic traces of having done so. This is a hypothesis perfectly

consistent both with a low-entropy initial condition and with the current macroscopic state of the world, so far as we can survey it. And of course we justifiably think it is improbable, and that it will lead to unreliable predictions. But what reason do we have to believe that it is rendered improbable by the lights of MENTACULUS conditional on the current macroscopic state? Albert is perfectly right to appeal to our knowledge of human intentions, and to note that these inferences depend for their reliability on the truth of MENTACULUS. But this at best establishes the necessity rather than the sufficiency of MENTACULUS for these inferences.⁸

In sum, Albert argues that both: (a) it is a problem for statistical mechanics without PH, qua theory of thermodynamics, that it cannot inter alia predict spatula locations; and (b) it is an advantage of MENTACULUS, qua theory of thermodynamics, that it can inter alia predict spatula locations. In my view both of these claims require further argument. I will discuss both in more detail in the following section. In this section, I have raised doubts, against (b), whether we have reason to think that MENTACULUS does any better, qua arbitrary macroscopic properties, than statistical mechanics without PH.⁹ Rather, the truth of MENTACULUS seems at best a necessary condition for the reliability of our ordinary inferences.

Of course, none of this implies that we should not believe PH, or that we should not believe that MENTACULUS best systematizes the conjunction of the fundamental and thermodynamic properties. Rather, I have here criticized one argument given by Albert for premise (2), an argument which promised a shortcut to premise (5). But there is no such shortcut—if we are to believe that MENTACULUS can ground the ss-probabilities, we need an argument to that end.¹⁰

12.3 Is the Mentaculus the Theory of Everything?

Let us now suppose that (3) is true. There is no doubt that (4) is true. So our next question concerns the inference from (3) and (4) to (5). As I see it, this is the weakest step in the argument—and yet it is widely granted, both by those who accept and who reject (5).

For example, Leeds (2003: 129–30) worries that if we say that the PH-probabilities are correct for thermodynamics but not for spatulas, the claim that we genuinely

⁸ Earman (2006: §10) argues that MENTACULUS is not even necessary, on grounds that these inferences can be underwritten by presuppositions that do not include PH.

⁹ Leeds (2003: 131) and Callender (2011: 99) suggest that the SM-probabilities will agree with the PH-probabilities for future predictions.

¹⁰ An interesting alternative strategy for defending premise (2) is outlined by Leeds (2003: 129), who suggests that the probabilities invoked in an explanation must be physically real in order to be explanatory, and that only mentaculus invokes physically real probabilities (the idea is that the sm-probabilities are not physically real because they generate mistakes when used to predict the past). One way to reject premise (2) is to reject the inference from success to truth. For example, Winsberg (2008) can be interpreted as arguing that the difficulty of making sense of PROB, given the assumption of determinism, requires us to resist the urge to move from empirical success to truth. Instead, we should take empirical success to confirm PROB as the inference rule to be used by creatures in our limited epistemic situation. Similar worries are expressed by Leeds (2003: 129 n. 2) and Torretti (2007). For a different problem concerning the realistic interpretation of these probabilities, probabilities, see Lyon, Ch. 6.

have an explanation for thermodynamics is undermined, and we are forced towards instrumentalism about those probabilities. The assumption is that MENTACULUS as a theory of thermodynamics stands or falls with MENTACULUS as a theory of everything. Likewise, Loewer (2012a: 18; my emphasis) writes:

The Mentaculus is imperialistic since it specifies a probability distribution over all physically possible histories and hence a conditional probability over all pairs of (reasonable) macro propositions. These are understood as objective probabilities. *It follows that any objective probabilities would either be derivable from them or conflict with them and thus threaten Mentaculus's explanation of thermodynamics.*

Again, Callender (2011: 103) says that, since MENTACULUS as a theory of everything is unattractive, 'readers may wish to retreat' from MENTACULUS as a theory of thermodynamics.

This consensus is, I think, remarkable. Consider the epistemic structure of the situation. We formulate a theory designed to generate the correct probabilities for the thermodynamic properties, which form a tiny subset of the macroscopic properties. The theory is successful, and so has been tested with respect to its predictions for those properties. We then notice that it is a logical consequence of the theory that it assigns probabilities to all macroscopic properties whatsoever. The question arises whether we should conclude that the probabilities it assigns to the non-thermodynamic properties are correct. The obvious answer, it seems to me, is that we should not. After all, the theory has not been tested with respect to its predictions for those properties.¹¹ The question then arises whether this should undermine our confidence that the theory is correct for the thermodynamic properties. After all, either the theory is true or it is false. It is this thought, I think, that is behind the comments of Leeds, Loewer, and Callender in the preceding paragraph. But surely the theory is correct for the properties for which it has been so well tested. Rather than capitulate to the thought that (3) and (5) stand or fall together, we should seek a way to believe (3) without believing (5).

At this point it is important to see the distinction between a theory being true qua thermodynamics and being true qua theory of everything. As I introduced the notion in §12.1, to be true qua theory of X is to truly specify the propositions that determine the objective chances of the X properties obtaining. The position that is available here is one on which MENTACULUS is true qua thermodynamics but not true qua non-thermodynamic properties. This is the position considered and rejected by Albert in the passage quoted in §12.2. Now there is a question about how to coherently formulate this position. On the face of it, PROB simply specifies the probability that a given macroscopic state is realized by a given microscopic state. Either that is the

¹¹ 'Outside of thermodynamics there is simply not a shred of evidence that [PH-probability] is underlying non-thermodynamic regularities' (Callender, 2011: 103). By thermodynamic properties, I mean simply the properties quantified over by the thermodynamic laws. One might employ a more liberal definition instead, on which any property coextensive with a region of phase space counts as thermodynamic. The more restrictive use I adopt simplifies the discussion.

correct probability or it is not. What could it mean to say that it is the correct probability *with respect to* determining the probability of that state evolving towards a particular subclass of macroscopic properties, but *not* with respect to determining the probability of that state evolving towards some other subclass of macroscopic properties? It is a concern over the coherence of this sort of claim that lies, I think, behind Leeds's suggestion that such a position is only available for an instrumentalist about the relevant probabilities. For there is of course no tension between saying that one set of probabilities is to be used for one purpose, and another for a different purpose.

Now I am confident that there are ways of making such a view coherent.¹² But a more straightforward way to bring the issue into focus at this point is to consider an alternative to MENTACULUS suggested by Albert.¹³ The idea is to replace PROB with an alternative statistical postulate involving the set of (uncountably many) probability distributions that agree with the canonical statistical mechanical probability distribution merely with respect to the probability of the thermodynamic laws. Let us call this alternative postulate PROB*, and the corresponding alternative for the best system of thermodynamics MENTACULUS*.¹⁴ MENTACULUS* agrees with MENTACULUS on the probabilities of all thermodynamic properties, but is massively indeterminate with respect to the probabilities of all non-thermodynamic macroscopic properties.

For our purposes the question is whether we have any reason to believe MENTACULUS rather than MENTACULUS*. They have been equally well tested for the propositions on which they agree, and neither has been tested on any propositions on which they disagree—propositions, that is, on which only MENTACULUS confers a determinate probability. From this perspective, it seems an advantage of MENTACULUS* that it offers us indeterminacy where our evidence runs out. From the standpoint of the argument for IMP, the important point is that we should not believe (5). We can think of this in two ways. Either we reject (2), on grounds that MENTACULUS* offers a better theory of thermodynamics than MENTACULUS, or we resist the inference from (3) and (4) to (5), on grounds that the mere fact that MENTACULUS offers verdicts on all macroscopic propositions does not license belief that those verdicts are all correct (it is on this option that we must provide a coherent formulation of how MENTACULUS could be correct only qua a particular class of macroscopic properties).

So far I have argued that we should be neutral on whether (5) is true. Callender and Cohen (2010) go further, and suggest a number of reasons to reject (5). They do so in the context of defending an alternative view of laws according to which they are given

¹² My preferred strategy combines a view of chance developed by Loewer (2001, 2004) with the view of non-fundamental laws developed by Schrenk (2008) and Callender and Cohen (2009, 2010), to be discussed further later. A similar strategy is pursued by Hoefer (2007). Another family of strategies involves giving up on the idea that theories are truth-evaluable absent their contexts of application, as has been urged by Nancy Cartwright (see Bailer-Jones, 2008, for an overview). I am grateful to Lina Jansson for suggesting these latter possibilities.

¹³ In personal communication to Callender (2011: 107).

¹⁴ Callender (2011: 107) calls this position on statistical mechanics *Liberal Globalism*.

by the axioms of the best systems for arbitrary sets of properties.¹⁵ Callender and Cohen are motivated in part by concerns about the role that natural properties play in orthodox formulations of the best system view of laws, and for present purposes discussion of that issue would take us too far afield. So to frame the discussion in the remainder of this section I will employ a more conservative alternative conception of special-science laws, with respect to which the relevant issues raised by Callender and Cohen still arise.

On the alternative conception I will discuss, we take the special-science laws to be specified by the axioms and (subsets of the) theorems of the best system of the non-fundamental properties of the world. Call this the *SPECIAL-SYSTEM*.¹⁶ The question I will address in the remainder of this section is what relationship these laws have to *MENTACULUS*. We have seen that according to Albert and Loewer, the basic non-fundamental laws are simply *PH* and *PROB*. This is the position I have called *STATISTICAL MECHANICAL IMPERIALISM*. According to Callender and Cohen (and, perhaps, Winsberg 2008), on the other hand, the basic non-fundamental laws are the *SPECIAL-SYSTEM* laws. I'll call this position *STATISTICAL MECHANICAL ANARCHISM*.¹⁷

To bring our questions into focus, suppose that *SPECIAL-SYSTEM* contains a generalization $S_1 \rightarrow S_2$ that assigns a certain probability to A , an event which involves the instantiation of a non-fundamental non-thermodynamic property: $P_s(A)$. Now consider the probability of A provided by *MENTACULUS*: $P_m(A)$. Our questions:

- Q1 What reason do we have to suppose that in general $P_m(S_1 \rightarrow S_2) \approx 1$?
- Q2 If $P_m(S_1 \rightarrow S_2) \not\approx 1$, is $S_1 \rightarrow S_2$ nevertheless a law?
- Q3 What reason do we have to suppose that in general $P_s(A) = P_m(A)$?
- Q4 If $P_s(A) \neq P_m(A)$, which probability should be used for making inferences involving A ?

Callender and Cohen offer the following reasons for thinking that the answer to Q1 and Q3 is 'very little':

STATE SPACE The state spaces over which the probabilities in the non-fundamental sciences are defined are typically parameterized with respect to different variables.

¹⁵ As they note, a similar view was earlier defended by Schrenk (2008), who also provides an elegant semantics for *ceteris paribus* clauses. See also Callender and Cohen (2009).

¹⁶ It may be better to postulate a set of systems, each defined with respect to the subsets of non-fundamental properties proprietary to some particular special science. This is the approach taken by Schrenk (2008) and Callender and Cohen (2009, 2010). The difference is irrelevant for present purposes.

¹⁷ Here and in what follows I will talk about this position as if it is Callender and Cohen's view. It should be kept in mind that their view is in fact different, since, unlike Schrenk (2008), they would eschew the idea of a preordained set of properties against which the laws of the special sciences are to be defined. The difference is irrelevant for present purposes.

OPEN ENDED The class of fundamental properties realizing a given non-fundamental property may be open-ended and so incapable of being captured by a Lebesgue measure.¹⁸

Moreover, they go on to suggest that the answer to Q2 is ‘yes’. Their basic point here is, I think, best captured by reflecting on one of the central motivations for endorsing the best system view of laws in the first place. This motivation is that the view identifies the laws with the propositions that would be identified as laws by an ideal theorist using the norms that (perhaps implicitly) govern scientific theorizing, and thereby makes sense of the methods that scientists actually employ in their search for laws.¹⁹ But to the extent this is plausible for the fundamental laws, it is also plausible for the non-fundamental laws. Since SPECIAL-SYSTEM specifies just those laws that an ideal special scientist would formulate had they but world enough, and time, we should accept that these would be the special-science laws, even if they involved generalizations that were improbable by the lights of a more fundamental theory. Even Albert and Loewer then, according to this line of thought, should accept that the SPECIAL-SYSTEM laws really are laws, regardless of their status with respect to MENTACULUS. Put these answers to our questions together, and we have a case for STATISTICAL MECHANICAL ANARCHISM.

What about Q4? The question is related to, but distinct from, a puzzle that Ned Hall (2009) has raised for Humean reductionism concerning laws of nature more generally.²⁰ Consider a world conforming to Newtonian mechanics except that not all particle collisions follow those laws—instead, sometimes there are perfectly inelastic collisions in which particles become fused into one particle with the sum of the masses and charges of the colliding particles. Suppose that the frequency of these collisions cannot be formulated as a simple function of anything else that happens in the world. An ideal scientist would formulate a range of hypotheses concerning the laws of the world centred around one in which the objective probability of the two kinds of collision is equal to the actual relative frequencies of the collisions. The reductionist, however, does not want to say that there are a corresponding range of metaphysical possibilities concerning the laws, but rather that we should in Hall’s term *round off* and set the laws to those specified by the hypothesis with the highest likelihood. So far so good. But now consider a world in which there exist two simple functions from properties of the particles involved in the collisions to the probabilities, each equally simple, informative, and fit, but which assign different single-case chances to many or perhaps all collisions. Here we want to say, Hall thinks,

¹⁸ The basic point here dates to Fodor (1974, 1997).

¹⁹ For a sophisticated argument along these lines for a family of views, of which the best system view is the most famous representative, see Earman and Roberts (2005a, 2005b). The motivation is also emphasized by Hall (2009).

²⁰ Humean reductionism as Hall defines it is a more general thesis than the specific best system analysis. Roughly, the Humean reductionist claims that ‘the implicit standards for judging lawhood are in fact constitutive of lawhood’, while Lewis adds to this the hypothesis that the implicit standards are those balancing simplicity, informativeness, and fit.

that it is genuinely metaphysically indeterminate what the laws are. The puzzle is to specify what the difference is between these two cases, and an obvious answer suggests itself—we have indeterminacy whenever we have more than one hypothesis conferring maximal likelihood on the actual distribution of properties.

Hall's puzzle is primarily about lawhood and derivatively about probability. The solution involves specifying a function from the actual distribution of properties to the laws. It is a consequence of the solution that some probabilities are indeterminate. Our question is primarily about probability and derivatively about lawhood. In fact, it can be seen as an instance of the notorious reference class problem (Hájek, 2007).²¹ For we have two ways of assigning a probability to a given event, and the question concerns which probability is correct. Here the solution involves specifying a function from the actual distribution of properties to a probability. It may be a consequence of the solution that certain propositions are to be regarded as laws, but that would involve additional argument.

Here is an argument that Callender and Cohen are correct to think that $P_s(A)$ should be used in preference to $P_m(A)$. MENTACULUS can be interpreted as providing partial information concerning the precise initial condition of the universe. If we knew the precise initial condition and the fundamental laws, and had the time and computational capacity, we could dispense with PH and PROB (we could deduce that PH is true, while PROB would tell us nothing we needed to know for purposes of prediction). Not knowing the precise initial condition, we have to infer it from the macroscopic regularities we can detect. The thermodynamic regularities provide evidence that the initial condition is one that assigns high probability to their obtaining, which is to say that having observed thermodynamic behaviour in many instances we infer that the initial condition is such to make likely that we will continue to observe thermodynamic behaviour. In this way we come to justified belief in MENTACULUS*. I suggest that the non-thermodynamic non-fundamental regularities should be treated in exactly the same way. They provide evidence that the initial condition is one that assigns high probability to their obtaining, which is to say that, having observed those regularities, we infer that the initial condition is such to make likely that we will continue to observe them. If $P_s(A) \neq P_m(A)$, we should use $P_s(A)$ since we have reason to believe that the initial condition is such to ground $S_1 \rightarrow S_2$. MENTACULUS* is only relevant insofar as it provides us with reason to believe that A will be thermodynamically typical. To put it differently, we have no reason to believe that only thermodynamic behaviour provides relevant information concerning the initial condition. Indeed, the only way in which this seems capable of being confirmed is by discovering that $P_s(A) \approx P_m(A)$.²²

In this section I have argued that we should not believe (5). We should not believe that MENTACULUS grounds the probabilities in all sciences simply because it can be

²¹ It is very similar to the problem of the reference environment for defining fitness raised by Shimony (1989: 261–3). To the best of my knowledge Abrams (2009) constitutes the only serious attempt to address the problem in that context.

²² Frisch in Ch. 11 endorses a similar argument.

used to assign probabilities to all empirical propositions. Moreover, in the event that special-science regularities are not made probable by MENTACULUS, we should still believe that they are laws, and still employ their probabilities in our inferences.

12.4 Are All and Only the Laws Likely?

I turn now to the argument from (6) and (7) to (8). As I described in §12.1, both Albert and Loewer employ this line of reasoning to respond to Kitcher's claim regarding the explanatory advantage of EQUILIBRIUM OVER DERIVATION. I will consider Kitcher's claim about explanation in the following section. For now, let us focus on (6) and (7): are all and only the laws likely? Not all, and not only, or so I will argue.

As many authors have noted, the laws of the special sciences are highly contingent from the standpoint of the fundamental laws (Beatty, 1995; Waters, 1998; Strevens, 2008).²³ In the lovely phrase introduced by Crick (1968), they are *frozen accidents* (Schaffner, 1993: 121; Strevens, 2008: 19). Now the contingencies on which these laws depend are also accidental from the standpoint of MENTACULUS. To adapt a metaphor offered by Gould (1989), if we replayed the tape of the universe from the beginning—as characterized by PH, leaving PROB to determine the exact initial condition—we would be unlikely to obtain the same special-science laws. Not all the laws are likely, conditional on MENTACULUS.

A flat-footed response to this problem is to seek some later macroscopic state with respect to which the laws are all likely. A more natural response is suggested by Loewer: 'Let's say that the special science laws that hold at t are the macro-regularities that are associated with high conditional probabilities given the macro-state at t' ' (Loewer (2008: 160). By 'the macro-state at t' ', Loewer means the macrostate of the entire universe. On this proposal, special-science laws are relative to times. The idea is that, for every law, there is some time t such that the law is probable given MENTACULUS conditional on the macroscopic state at t .

Let us grant that this is the case. Still, STATISTICAL MECHANICAL IMPERIALISM requires that the laws are the *only* generalizations made probable by conditionalizing on macroscopic states of the world at times. The problem with Loewer's suggestion is that it fails to discriminate between laws and accidental generalizations. There are all sorts of regularities that are assigned high conditional probabilities by MENTACULUS in conjunction with the macroscopic state of the universe at a time, including many that are paradigmatic accidental generalizations. For instance, it is plausible that the generalization that all gold spheres are less than a mile in diameter (van Fraassen, 1989: 27) has been assigned a high probability at *all* times earlier than the present. But this is a paradigmatic non-law. Not only the laws are likely, conditional on MENTACULUS.²⁴

²³ This point is also noted in this context by Frisch in Ch. 11.

²⁴ Note that the argument of this section doesn't depend on the particular details of MENTACULUS. It works equally well against any proposal on which the laws are identified with the generalizations that are

Now a natural fallback position here is to claim that *MENTACULUS*, conditional on the macroscopic state of the world at times, entails that the laws are (probably) *true* but not that they are (probably) *laws*.²⁵ On this view, *MENTACULUS* entails that the distribution of non-fundamental properties is likely, but does not itself entail the non-fundamental laws or probabilities—that task is left to *SPECIAL-SYSTEM*. Nothing I have said provides an argument against this position.²⁶ But to take this option is to give up on *STATISTICAL MECHANICAL IMPERIALISM* and endorse *STATISTICAL MECHANICAL ANARCHISM*.

12.5 Does Reducing the Laws Reduce the Explanations?

I turn now to the inference from (8) to (9). Suppose that the non-fundamental laws are logical consequences of *MENTACULUS* after all. Would it follow that the explanations provided by the non-fundamental sciences are reducible to the explanations provided by *MENTACULUS*? I understand this question in the following way: the explanations provided by the non-fundamental sciences are reducible to the explanations provided by *MENTACULUS* *iff* they do not possess any additional explanatory value. In other words, they are reducible just in case they are explanatorily dispensable.²⁷

Let us consider the question by comparing the explanatory value of *DERIVATION* and *EQUILIBRIUM*. As I described in §12.1, one suggestion made by Kitcher (2001) is that *EQUILIBRIUM* is not reducible to *DERIVATION* since only the former entails ‘that Arbuthnot’s regularity was anything more than a gigantic coincidence’ (p. 71). This claim is difficult to make sense of, for reasons pointed out by Albert (2012), Loewer (2008: 160–2) and Frisch (Ch. 11, §6). But in the passage preceding this claim Kitcher gives a different argument, in the context of a discussion of Mendel’s Second Law. Here Kitcher writes: ‘What’s crucial is the form of these processes, not the material out of which the things are made. The regularity about genes would hold so long as they could sustain processes of this form, and, if that condition were met, it wouldn’t matter if genes were segments of nucleic acids, proteins, or chunks of Swiss cheese’ (2001: 70–1). This remark could be cashed out in a number of different ways, but in my view the best way to develop the idea is as follows.²⁸

Many accounts of explanatory value have emphasized a variety of generality possessed by the generalizations employed in explanations (Hempel, 1959; Woodward, 2003; Woodward and Hitchcock, 2003). Since the fundamental laws are

probable. To be fair to Loewer, he recognizes that his proposal ‘needs a lot of tinkering with if it is to capture those regularities that are deemed to be laws in the special sciences’ (2008: 160). My claim is that this tinkering must involve abandoning *IMP*.

²⁵ This possibility was suggested to me by Wolfgang Schwarz.

²⁶ Note, however, that it requires giving up on the claim that all logical consequences of the laws are laws (some think we should give up this claim for other reasons, e.g. Fodor, 1974: 109–10).

²⁷ I am less certain that Albert and Loewer endorse this claim than I am that they endorse the earlier premises in the argument, but the question is of independent interest.

²⁸ The remainder of this section describes a position defended in more detail in Weslake (2010).

maximally general in the relevant senses, it is a consequence of these accounts that explanations employing fundamental laws are always more valuable than explanations employing non-fundamental laws. Elsewhere I have argued for an account of explanatory value that focusses not on the generality of explanatory generalizations, but rather on the generality of whole explanations (Weslake, 2010). Call *abstraction* the degree to which an explanation applies to a range of possible situations. Non-fundamental explanations will be more abstract than corresponding fundamental explanations, in this sense, if the following conditions are met:

- SUPERVENIENCE** Every possible situation in which the fundamental explanation applies is one in which the non-fundamental explanation applies.
- MULTIPLE REALIZATION** There are possible situations in which the non-fundamental explanation applies in which the fundamental explanation does not apply.

These conditions are met for **EQUILIBRIUM** with respect to **DERIVATION**, so if abstraction makes for explanatory value, the former is more valuable in this one respect. Notice that this claim does not depend on any more controversial claims concerning explanatory relevance, difference-making, unification, provision of explanatory information, or claims about what would have been the case had the fundamental laws been different. Notice too that it is possible both that special-science laws are highly contingent with respect to **MENTACULUS**, and that they figure in more abstract explanations than those provided by **MENTACULUS**. That is, it is possible both that it was highly unlikely for a particular special-science law to have obtained, and that explanations involving that law supervene on and are multiply realizable by the explanations provided by **MENTACULUS**.²⁹

While I will not defend the claim here, I believe that abstraction is a genuine dimension of explanatory value, and therefore that the inference from (8) to (9) is not warranted. Even if all of the laws and probabilities of the non-fundamental sciences could be derived from **MENTACULUS**, there is a dimension of explanatory value on which the explanations provided by those sciences are not reducible to the explanations provided by **MENTACULUS**.

I conclude that we should not endorse the argument for **STATISTICAL MECHANICAL IMPERIALISM**. We should be anarchists.

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²⁹ I am grateful to Karola Stotz for raising this question.

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