Unsharp Humean Chances in Statistical Physics: A Reply to Beisbart

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Abstract

In an illuminating article, Beisbart (this volume) argues that the recently-popular thesis that the probabilities of statistical mechanics (SM) are Best System chances runs into a serious obstacle: there is no one axiomatization of SM that is *robustly best*, as judged by the theoretical virtues of simplicity, strength, and fit. Beisbart takes this 'no clear winner' result to imply that the probabilities yielded by the competing axiomatizations simply fail to count as Best System chances. In this reply, we express sympathy for the 'no clear winner' thesis. However, we argue that an importantly different moral should be drawn from this. We contend that the implication for Humean chances is not that *there are no SM chances*, but rather that *SM chances fail to be sharp*.

1. Introduction

In an illuminating article, Beisbart (this volume; all further references to Beisbart are to this article) argues that the recently-popular thesis that the probabilities of statistical mechanics (SM) are Best System chances runs into a serious obstacle: there is no one axiomatization of SM that is *robustly best*, as judged by the theoretical virtues of simplicity, strength, and fit. Beisbart takes this 'no clear winner' result to imply that the probabilities yielded by the competing axiomatizations simply fail to count as Best System chances. In this reply, we express sympathy for the 'no clear winner' thesis. However, we argue that an importantly different moral should be drawn from this. We contend that the implication for Humean chances is not that *there are no SM chances*, but rather that *SM chances fail to be sharp*.

In Section 2 we outline the Humean Best System Analysis (BSA) of chance. In Section 3, we explain why it has been thought that the BSA justifies an interpretation of the SM probabilities as genuine chances. In Section 4, we describe Beisbart's arguments for the no clear winner result. As noted above, after establishing this thesis, Beisbart goes on conclude there in fact are no SM chances. It is this second step that we wish to question, and we explain why by appeal to the notion of imprecise chances in Section 5.

2. The Humean Best System Analysis (BSA) of Chance

According to Humean theories of objective chance, the chances supervene upon the *Humean mosaic*: that is, the distribution of categorical (i.e. non-modal) properties throughout all of space-time. The most promising attempt to capture the manner in which the chances supervene on the mosaic is the Best Systems Analysis (BSA), which has received its most significant development by Lewis (1983, 1994).

According to the BSA, the objective chances are those probabilities that are entailed by that set of axioms which best systematizes the Humean mosaic, where goodness of systematization is judged against the theoretical virtues of simplicity, strength, and fit. The BSA is offered as an analysis of laws as well as of chances: the laws are the (axioms and)

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theorems of the Best System.[1]

A system is *strong* to the extent that it says "what will happen or what the chances will be when situations of a certain kind arise" (Lewis 1994, 480). A system is simple to the extent that it comprises fewer axioms, or those axioms have simpler forms (e.g. linear equations are simpler than polynomials of degree greater than 1). Often greater strength can be achieved at a cost in terms of simplicity (e.g. by adding axioms), and vice versa.

A candidate system may sometimes achieve a good deal of strength with little cost in simplicity if it is endowed with a probability function (cp. Loewer 2004, 1119): that is, a function $Ch_t(p)$ that maps propositions and time pairs $\langle p, t\rangle$ onto real values in the [0, 1] interval, and that obeys the axioms of probability.[2] This is where Lewis's third theoretical desideratum comes it: a system *fits* the actual course of history well to the extent that the associated probability function assigns a high probability to the actual course of history: the higher the probability, the better the fit (Lewis 1994, 480).[3]

The Best System is that which strikes the best balance between the theoretical virtues of simplicity, strength, and fit. According to the BSA, the probability function associated with the Best System is the *chance function* for the world. The laws are the (axioms and) theorems of the Best System.

The idea, then, is that the Humean mosaic, together with the theoretical virtues, serves to fix a Best System. As Lewis (ibid.) puts it: "The arrangement of qualities provides the candidate … systems, and considerations of simplicity and strength [and fit] and balance do the rest". Or, more concisely, chances are 'Humean Best System-supervenient on the Humean mosaic' (Frigg and Hoefer (unpublished)).

Lewis himself acknowledges that the BSA is not completely unproblematic: "[t]he worst problem about the best-system analysis" (Lewis 1994, 479) is that notions such as simplicity and balance are to some extent *imprecise*. There is, for example, no unique and maximally determinate simplicity metric that is obviously the correct one to apply to candidate systems, nor is there a unique and maximally determinate correct exchange rate between the competing virtues of simplicity, strength, and fit. The worry is that, within acceptable ranges, different precisifications of the simplicity metric and of the exchange rate between the virtues will yield different verdicts about which system counts as *best*. Lewis has little more to offer than the hope that this will not turn out to be so:

> If nature is kind, the best system will be *robustly* best–so far ahead of its rivals that it will come out

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1 Strictly speaking, the BSA and Humean supervenience about laws and chances are logically independent theses. The BSA becomes a Humean account of laws and chances only when it is stipulated that (as we are assuming here) what gets systematized is the Humean mosaic. This stipulation excludes from the supervenience base any putative irreducibly modally-involved properties.

2 Or alternatively a Renyi-Popper measure $Ch(p|q)$ that maps proposition pairs $\langle p, q\rangle$ onto the reals in the [0, 1] interval. (Plausibly, if one takes conditional chance as basic in this way, then it is redundant to include a 'time' index to the chance function; see Hoefer 2007, 562-565; Glynn 2010, 78-79.)

3 This notion of fit applies only if there are only finitely many chance events. See Elga (2004) for an extension to infinite cases. In addition, if one wants to allow the possibility of statistical mechanical probabilities counting as chances, then one needs a notion of *fit* according to which one way a system may fit better is if its probability function assigns a relatively high probability to the macro-history of the world conditional upon a coarse-graining of its initial conditions (as well as assigning a relatively high probability to the micro-history conditional upon a fine graining of the initial conditions).

> first under any standards of simplicity and strength and balance. We have no guarantee that nature is kind in this way, but no evidence that it isn't. It's a reasonable hope. Perhaps we presuppose it in our thinking about law. I can admit that *if* nature were unkind, and *if* disagreeing rival systems were running neck-and-neck, then … the theorems of the barely-best system would not very well deserve the name of laws. But I'd blame the trouble on unkind nature, not on the analysis; and I suggest we not cross these bridges unless we come to them. (Lewis *op cit.*, 479; italics original)

One *might* think that the same thing that Lewis says about laws should be said of chances: if there is no clear winner of the best system competition, then there would be nothing deserving of the name *chance*. (Lewis himself, however, does not explicitly say this.) As we shall see in Section 5, Beisbart's central thesis is that we *do* in fact have good reason to think that there is a set of rival systems – each of which is associated with a different probability function – that are running neck-and-neck in the best system competition for our world. Beisbart draws the conclusion that, for the Best System analyst, there simply is nothing (at least nothing in the domain of statistical physics) deserving of the name of chance.

3. The BSA and Statistical Mechanics

Lewis himself appears to have thought that the probability function associated with the best system for our world would simply be the quantum mechanical probability function: that is, the function that yields all and only the probabilities entailed by quantum mechanics, or whatever fundamental physical theory replaces it (see Lewis 1986, 118; 1994).

Yet Loewer (2001, 2007, 2008, 2012a, 2012b) has influentially argued that the probabilities of statistical mechanics (SM) can also be understood as probabilities of the Best System, and therefore as genuine objective chances on the BSA. Loewer appeals to the axiomatization of SM described by Albert (2000, Chs. 3-4). Albert suggests that SM can be derived from the following:

(FD) the fundamental dynamical laws;

(PH) a proposition characterizing the initial conditions of the universe as constituting a special low-entropy state; and

(SP) a uniform probability distribution (on the standard Lebesgue measure) over the regions of microphysical phase space associated with that low-entropy state.[4]

Albert (2012) and Loewer (2012a, 2012b) dub the conjunction FD & PH & SP 'the Mentaculus'.

The argument that the SM probabilities are derivable from the Mentaculus goes roughly as follows. Consider the region of microphysical phase space associated with the low-entropy initial state of the universe implied by PH. Relative to the total volume of that region, the volume taken up by microstates that lead (by FD) to fairly sustained entropy increase until thermodynamic equilibrium is reached (and to the universe staying at or close to equilibrium thereafter) is extremely high. Consequently, the uniform probability distribution (given by SP) over the entire region yields an extremely high probability of the universe following such a path. When it comes to (approximately) isolated subsystems of the

4 In the quantum case, the uniform probability distribution is not over classical phase space, but over the set of quantum states compatible with the PH.

universe the idea is that, since a system's becoming approximately isolated is not itself correlated with its initial microstate being entropy-decreasing, it is extremely likely that any such subsystem that is in initial disequilibrium will increase in entropy over time (see Loewer 2007, 302; 2012a, 124-5; 2012b, 17; and Albert 2000, 81-5).[5]

Albert (2000, 2012) and Loewer (2007, 2008, 2012a, 2012b) have argued that the Mentaculus entails many of the probabilities of the special sciences. In virtue of this, Loewer claims that the Mentaculus is much stronger than a system consisting of the fundamental dynamical laws, FD, alone. And since it is not much more complicated (it only requires the addition of the axioms PH and SP), Loewer claims that it is a plausible *best* system for our world.[6]

4. Beisbart's Response

Let us assume that Loewer is correct that the Mentaculus constitutes a better system than one comprising the fundamental dynamics alone, and that it entails the SM probabilities. If the Mentaculus comes out *best*, then the SM probabilities will count as objective chances on the BSA.

But an axiom system consisting of *only* the fundamental dynamic laws is not the only rival to the Mentaculus. Schaffer (2007, 130-2), Hoefer (2007, 560), and Beisbart consider another candidate, which consists of the fundamental dynamic laws plus an axiom giving the *precise initial conditions* of the universe. This is a very strong system. Schaffer (2007, 131-2) suggests that it is maximally strong, while Hoefer (2007, 560) questions this. Hoefer (ibid.) points out that it's not obvious how to quantify the complexity of the two candidate systems in such a way as to allow comparison. It is also not obvious how to decide whether any increase in complexity is worth it because of the strength thereby bought (see Frisch 2011).

Beisbart suggests that there are still further competitors to the Mentaculus. As Beisbart observes:

> We can improve fit when we … assume a flat probability distribution over a certain sub-region of the past low-entropy macro-state [as opposed to over the whole of the past low-entropy macro-state, as per (SP) of the Mentaculus]. That sub-region may be defined by the demand that a certain elementary particle has a kinetic energy larger than a particular value e_0, for instance.
>
> If we do so, we have to pay in simplicity though because [in addition to the assumed low-entropy initial state, we have to further specify that the initial] kinetic energy of a particular particle be e_0.
>
> So overall, would we improve the system … ? This is a difficult question, and the answer is far from clear. The only thing we can say is that fit is considerably improved, but that there is a considerable cost in simplicity too. So it's not a case in which the right sort of balance favors one system rather than the other in a clear way.

Beisbart's worry is that, depending upon which sub-region of the phase space associated with PH the flat distribution is applied to, we get a range of candidate best systems (cp. also Schaffer 2007, 131n). At the one extreme, we have the Mentaculus (where a uniform distribution is applied to the whole region of phase space compatible with PH);[7] at the other

5 Though see Winsberg (2004), Earman (2006), and Callender (2011) for criticisms of this line of argument.

6 This proposal requires that initial conditions, such as PH, are potential axioms of the best system. The BSA has not always been construed as allowing for this. However, Lewis (1983, p. 367) himself seems sympathetic to the view that they may be.

7 Indeed, as Beisbart points out, it is not clear precisely *how low* initial entropy is specified to be by the PH.

extreme, we have a system comprising the fundamental dynamic laws together with the *precise* initial conditions. The latter is equivalently to what we get in the limit as we apply a uniform distribution to smaller and smaller sub-regions of the phase space associated with PH, each of which contains the point-sized region of phase space that the universe actually initially occupied. The former is very simple, but gives an inferior fit; the latter gives a better fit, but is less simple. In between we have a continuum of systems involving the application of a uniform distribution to progressively smaller sub-regions of the phase-space compatible with PH (where each sub-region contains the actual point in phase space at which our universe was initially located). Such systems are increasingly better fitting, since they assign an increasingly high probability to the actual macroscopic course of events, but also increasingly complex, since picking out progressively smaller sub-regions requires building into the axioms an increasing amount of information about the actual initial state of the universe.[8]

If we had a precise simplicity measure, and a precise exchange-rate between simplicity and fit, then perhaps the measure and the exchange rate could produce an exact tie between systems located on this continuum. The idea would be that, according to the exchange rate, the change in fit as we move along the continuum is precisely counterbalanced by the change in simplicity. More plausibly, a precise simplicity measure and a precise exchange rate is something we cannot reasonably hope to have. If so, it is quite plausible that none of the systems on this continuum is robustly better than all – or indeed *any* – of the others. That is, none is superior to all – or *any* – others given the imprecision of simplicity and of the exchange rate. It is on these grounds that Beisbart argues that:

> The conclusion looming large here is that Humean considerations do not provide us with any clearly optimal solution to the problem of how to define a dynamics of chances. And good just isn't good enough. We need a best system that is clearly best, and not just a good one. If there isn't a best system, then there are no chances ….

In other words, Beisbart's idea is that the Humean mosaic, together with the theoretical virtues, fails to single out a unique best system, and therefore a corresponding probability function. Consequently he claims that the BSA implies that there is nothing that counts as the *objective chance function*. This is precisely analogous to Lewis's claim that there would be nothing deserving of the name of *law* if several systems were roughly tied for *best*.

5. Humean Chances Aren't Sharp

One might wonder whether it is really the case that the choice of initial phase space region (and of initial probability distribution) significantly affects the probabilities that systems exhibit thermodynamic-like behavior. While there may not be a unique best system, the

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Different precisifications of the PH yield different sized regions of phase space to which the uniform distribution is to be applied. So it seems that the number of competing systems may be larger still.

8 Callender (2011) calls attempts to derive the SM probabilities from a probability distribution over the initial conditions of *the universe as a whole* 'Globalist' approaches to axiomatizing SM. The Mentaculus is one example of a Globalist approach. Beisbart points out that there are rivals. In contrast to Globalist approaches, 'Localist' approaches (see Callender *op cit.*) attempt to derive the SM probabilities from probability distributions over the initial states of the various *approximately isolated subsystems of the universe*. Beisbart observes that there is a range of competing Localist approaches to axiomatizing SM. While, for reasons of space, we will here focus upon the competing Globalist approaches, many of our points will carry across to the competition between Localist axiomatizations, if one thinks that the Localist approaches are more promising.

range of best systems might nevertheless all agree on the probabilities that they assign to macro-events.[9] This is a possibility that Beisbart himself notes (pp. 16 & 18 of draft). Yet even if different systems roughly tied for first place assign different probabilities to the macroscopic course of events, we do not agree that this implies that there can be nothing deserving of the name *chance*.[10] The fact that the Humean mosaic, together with the (imprecise) relation of Humean Best System supervenience does not uniquely fix a single axiom system should not lead the Humean to deny that there are chances in the world. Rather, it should lead her to deny that the relevant chances must be sharp. If there is no clear winner in the best system competition, it seems to us that the natural thing for the Humean to say is that the *set* of probability functions corresponding to the tied systems constitute the set of chance functions for the world. Where the probability functions for the tied systems agree on the probability for a particular event then the objective chance for that event is sharp. This seems quite possible when, for example, we are considering microphysical events like the decay of a tritium atom within the next 12.32 years. On the other hand, when the probability functions of the tied-for-best systems yield a range of values, as – if the reasoning of Beisbart and others is correct – may well be the case for macro-events, then the range constitutes an unsharp chance for the event in question.

The basic intuition behind the introduction of unsharp Humean chances in this context is that it seems unreasonable to claim, in the case of a tie between systems, that there simply is *nothing* playing the chance role in guiding rational credence, and thus nothing serving as a 'guide to life' for dealing with the relevant situations. Hoefer (2007, 580-587) and Frigg & Hoefer (2010) argue that Humean chances are constraints on rational credence because of the tight connection between Humean chances and actual frequencies. Specifically they argue that this allows for a 'consequentialist' justification of the chance-credence connection: given the tight connection between Humean chances and actual frequencies, agents betting according to the Humean chances will do well in the long run. The assumption that there is a unique probability function that serves as the Humean chance function does not seem essential to this argument.[11] In the case of a tie between systems, it seems that one can analogously argue that the set of probability functions entailed by the tied systems would also constrain reasonable credence. Specifically, when confronted with a tie between systems, an agent who knew the *set* of probability functions corresponding to the tied-for-best systems, and who knew that a certain chance setup is instantiated, and who had no inadmissible information (no relevant information beyond knowledge of the initial chance setup and the set of probability distributions entailed by the tied-for-best systems), rationally ought *not* to adopt a credence value that lies outside the range of probability values

9 At least this might be so if, with Frigg and Hoefer (unpublished), we exclude information about the precise micro-state of the world as inadmissable, since "*chance rules operate at a specific level and evidence pertaining to more fundamental levels is inadmissible.*" See Maudlin (2007) for a discussion of how one can derive typical thermodynamic behavior without committing to precise assumptions either about the initial probability distribution or the size of the initial phase space region.

10 As we saw, Lewis claims that, if there were not a unique best system, there would be nothing deserving of the name *law*. However, he earlier (Lewis 1983, 367) said theorems entailed by *all* of the tied systems would count as laws. We're sympathetic to his earlier position. If, for instance, the fundamental dynamics are the same in all the tied systems (this is not something that is disputed by Beisbart and others who have examined rivals to the Mentaculus), then they will come out as laws.

11 Frigg and Hoefer (unpublished) themselves find it plausible that there may not be a unique best system for our world.

entailed by the systems that are tied-for-best.

For example, any tied-for-best system will entail a very low probability for entropy decrease in (most) isolated systems. A system that does not would be straightforwardly excluded from the set of tied-for-best systems: it will inevitably be highly ill-fitting, since fit implies a close correspondence to the actual frequencies. We can thus offer a 'consequentialist' justification for not adopting a credence outside of the set of probabilities yielded by the tied systems: betting as though entropy-decrease is not very improbable would lead one to do very badly in the long run.

Indeed, rather plausibly, in such a case, a reasonable agent would have a credence assignment which was unsharp: specifically, it would be represented by a set of values corresponding to those entailed by the probability functions of the tied systems. In such situations an agent would have no rational basis to choose between the probability functions entailed by the tied systems but would be rationality compelled to base their behaviour upon the relevant unsharp chance. Thus, in such a situation the set of values entailed by the tied systems is playing the key chance-role of guiding reasonable credence, and thereby constitutes an unsharp Humean chance.[12]

There is one worry here. Given a set of systems that are tied for best - because some have greater fit, while others have greater simplicity - it would appear to be (instrumentally) rational to set one's credences according to the probabilities entailed by that system which has the greatest fit (i.e. accords best with the frequencies), since betting according to those probabilities would yield the greatest payoff in the long run. We agree that this is a worry, but it is a general problem for Best System analyses of chance. Even in the case of a unique best system, the best system chances are liable to depart somewhat from the actual overall world frequencies. This is because considerations of *simplicity* go into determining a best system, and not just considerations of fit. But it seems difficult to argue that the best system probabilities, rather than the actual overall world frequencies, are the best players of the chance role in guiding rational credence: if one knew both, one would do better in the long run if one bet according to the actual frequencies. The Best System analysis of chance requires some general explanation of why the best-fitting probabilities (i.e. the actual overall world frequencies) aren't automatically the chances (perhaps, for example, one could appeal to other aspects of the chance role). Whatever answer is deployed in this context will also be available to us in explaining why the best fitting of the tied systems doesn't automatically deliver the chances. Consequently, while we admit this issue to be problematic for our approach, we do not take it to be unduly or uniquely so.

Let us now go back and reconsider the tied-for-best systems discussed in the previous section in the context of unsharp probabilities. Consider again the Mentaculus, i.e. FD & PH & SP. Given FD, as defined above, there are two things that need to be fixed: the initial macrostate that the universe is in, and the probability distribution over the associated region of microphysical phase space. The Mentaculus requires the macrostate to be a special low-entropy macrostate, M_0, which is associated with a phase space region, Γ_0. It requires the probability distribution over Γ_0 to be the uniform distribution ρ_U. The discussed tied-for-best systems may vary *either* the size of the initial low-entropy region *or* the probability distribution over that region. In many cases, systems that vary the region will be equivalent

12 Elga (2010) argues that unsharp credences are incompatible with perfect rationality. If this were correct, then perhaps any player of the chance role in guiding rational credence must itself be sharp. However, we find Joyce's (2010) defense of unsharp credences against Elga's argument compelling.

to systems that vary the probability distribution. So, for now, we consider only changes to the size of the initial low-entropy region. Beisbart, as discussed, considers a sub-region Γ_B of the phase space region Γ_0 associated with the low entropy macrostate M_0 specified by the Mentaculus. This sub-region Γ_B is defined by adding to the requirement that the region be associated with a low-entropy macrostate, the specification that one of the elementary particles comprising the initial universe has kinetic energy above a certain value. As discussed in the last section, one can consider a continuum of sub-regions of Γ_0, given by specifying more and more of the microphysical details of the universe's initial state. As we move along this continuum, we get better and better fit at the cost of less and less simplicity up to the extreme case where we specify the precise initial condition, given by the region Γ_δ, which contains only one point: namely the exact microphysical state of the universe.

We can now formulate a family of Mentaculus-like axiomatizations of SM, which we call $Ment(\Gamma)$. Members of the family $Ment(\Gamma)$ comprise FD plus the following:

(PH) a proposition characterizing the initial condition of the universe as some particular element of the set $m = \{M_0, \dots, M_B, \dots, M_\delta\}$; and

(SP) a probability distribution $\rho = \rho_U$ over the associated region $\gamma = \{\Gamma_0, \dots, \Gamma_B, \dots\}$ of microphysical phase space.

(SP is redundant for the case where the precise initial condition is taken.) The suggestion is that SM probabilities are entailed by $Ment(\Gamma)$ for each choice of $\Gamma \in \gamma$, and that, by considering all $\Gamma \in \gamma$, we obtain SM probabilities that are set-valued, i.e. are unsharp.

There are several issues that need to be addressed. First, one can obviously generalize the above to include a variety of probability distributions over the initial phase space region, i.e. to consider $Ment(\Gamma, \rho)$, if axiom systems containing different ρs are among those that are tied-for-best. Whether such a generalization is necessary, given the existence of the family $Ment(\Gamma)$ of tied-for-best systems, is questionable. Consider, for example, Frigg & Hoefer's (unpublished) suggestion of "a peaked distribution, nearly Dirac-delta style" whose peak is at the precise initial condition represented by ρ_δ. In effect, $Ment(\Gamma_0, \rho_\delta) = Ment(\Gamma_\delta)$. Second, we have only considered subregions of the low-entropy macro-state that are in γ. One could also consider regions larger than Γ_0 and may find systems that are tied for best. But is entropy then still low enough to provide at least a minimally good fit with the actual course of history? How low is low enough is a non-trivial question.[13] Third, the exact

[13] Also it's not clear to us that an axiom system specifying that the universe was initially in a larger region of phase space than Γ_0 is necessarily simpler than one specifying that the universe was in Γ_0. As is well known (e.g. Lewis 1983, 367), simplicity is vocabulary-relative and, in order to avoid trivializing the desideratum of simplicity, we must take simplicity-when-formulated-with-unnatural-predicates to be less desirable than simplicity-when-formulated-with-reasonably-natural-predicates (and perhaps simplicity-when-formulated-with-perfectly-natural-predicates to be more desirable than either). In our not-too-unnatural macro-vocabulary, we may be able to formulate simple axioms that pick out moderately large regions of phase space like Γ_0. Picking out smaller regions will often require employing complex microphysical predicates, thus increasing fit (and perhaps naturalness of predicates) at the expense of simplicity. But picking out larger regions may (unless they can simply be picked out by specifying their entropy level) require disjunctions of reasonably natural macrophysical predicates. If so, the resulting axiom systems will be both worse fitting and less simple (or worse fitting and formulated in less natural language). They will then be clearly inferior to an axiom system specifying that the universe was initially in Γ_0.

relation between $Ment(\Gamma)$ and the SM probabilities is a difficult question. Whether, for instance, a continuum of subregions translates into a continuum of probabilities for SM is far from trivial and is a question for detailed calculations.

While each of these three issues undoubtedly warrant further detailed attention, we feel that they are indicative of the potential for fruitful refinement of the idea of unsharp Humean chances. We look forward to attending to such developments in future work.

6. Conclusion

Beisbart's paper is a useful addition to the literature on Statistical Mechanics and the BSA. In particular, it bears emphasis that the notions of simplicity, strength, and balance are imprecise and that plausibly, in conjunction with the Humean mosaic, do not single out a single best system, but rather a set of tied-for-best systems. But we don't think that, from the existence of such tied-for-best systems, one is justified in concluding that statistical physics does not yield Humean chances. Rather, we take such a tie to imply the existence of *non-sharp Humean chances*. In this brief reply, we have offered an outline of such a proposal. Detailed development of this view must await another occasion.

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