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RESEARCH ARTICLE

Eisenbud, Smias, and Psi

Jason Kissner

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Abstract—This paper explores contributions made by Jule Eisenbud pertaining to the substantive interrelationship of psi functioning and probabilistic behavior. It is argued that Eisenbud came very close to articulating a construct that may prove useful to psi theoreticians and researchers. That construct is "smias," which was first articulated by researchers examining the foundations of probability theory in connection with an investigation of the gambler's fallacy. It is shown that the smias construct fits Eisenbud's theorization very well indeed, and that the construct offers psi researchers and theorists an account of the Law of Large Numbers that can accommodate empirically observed convergence while providing a metaphysical alternative to the metaphysics underlying the conventional view of the empirical functioning of the Law. An empirical test is outlined that might provide support for an Eisenbudian conception of the role of psi in connection with probabilistic behavior.

The psychiatrist and parapsychologist Jule Eisenbud was a renegade among renegades. Not only did he research what was considered by many of his colleagues to be highly controversial psi subject matter; he also, in the course of this research, launched incisive attacks on core assumptions underlying probability theory. Exactly what is involved in probabilistic behavior, and how probability theory is best seen as relating to psi theory and research, are issues that Eisenbud meditated upon at length. Readers might agree that his conclusions on these scores are quite intriguing.

One upshot of Eisenbud's theoretical and empirical explorations led him to seize the psi bull by the horns, so to speak, by arguing, rather startlingly, that a psi linkage among events is ultimately the only type of linkage between events that there is. On this issue, here is Eisenbud in 1956 (Eisenbud 1992):

> This author saw in the paradox of the fact that probability had laws at all, and that it worked, what many metaphysicians had already grasped in terms of ontological and cosmological necessity—that nothing in the universe would work, neither atoms nor animals nor astral systems, in the absence of

> what amounts to a communicating dependency of each event upon every other event and upon the whole . . . [i]n his exhaustive study of "what makes probability run," Marbe was led to deny the statistical independence of successive trials in a coin-toss series and came to the same conclusion in regard to every other type of statistical series: i.e. that in a sense nature *does* have a memory (at least he suspected that the answer to the riddle lay somewhere in the psychological sphere, just where he could not finally say). (Eisenbud 1992[1956]:36)

In addition to rendering contributions surrounding the bold claim positing a very intimate and *substantive* interrelationship of psi to probabilistic behavior, Eisenbud made related claims and contributions addressing the important issue as to why psi can, at least sometimes and wholly aside from shortfalls in statistical power, be so difficult to detect and/or replicate in laboratory settings. This paper explores Eisenbud's ideas relating to probability theory's relationship to psi and holds that he came close to articulating a construct that may prove useful to psi theorization and research. That construct is "smias" and was first formulated by Baird and Otte (1982) in the context of an examination of the foundations of probability theory that was prompted in part by their reflections on the "gambler's fallacy" and the Law of Large Numbers. We begin the analysis by comparing the conventional treatment of the Law of Large Numbers with what Eisenbud had to say on the matter. We conclude the analysis with recommendations for empirical testing of whether the Law of Large Numbers operates in an Eisenbudian manner in nature.

The Law of Large Numbers: Framing the Issues

The mathematician David Hand's informal but serviceable definition of the Law of Large Numbers states that the Law "says that the average of a sequence of numbers randomly drawn from a given set of values is likely to get closer and closer to the average of that set" (Hand 2014:64). Hand is highly skeptical as to psi phenomena, but his view on the Law is representative of the nearly universal and conventional approach to the issue—an approach that we are going to criticize.

Grinstead and Snell (2003) meticulously show that the Law of Large Numbers can be logically and mathematically derived with respect to probability mass as well as density functions by employing basic constructs such as variance, expected value, Chebyshev's Inequality, and an independently and identically distributed (hereinafter iid) random variable. Neither Eisenbud nor the current author have any qualms with the formalism of the Law of Large Numbers. The operative question for Eisenbud and the current author is why, *empirically*, the average of a sequence of numbers tends to converge around the average of the population set. As we will see later, there are two different ways convergence can happen empirically. These two ways, left undefined for now, are "dilution" and what might be called "balancing." The former, which supposes that conditions can be achieved wherein iid can establish a guaranteed foothold in nature, is the conventional view. The latter, which, in spite of empirical phenomena such as convergence, supposes that iid may never really hold in nature, is the Eisenbudian perspective. Coinciding with this distinction are two very distinguishable conceptions of the level at which probability operates. The conventional view holds that probability is invariably grounded at the level of individual events. The Eisenbudian view, which we will argue is a "smias" type view, denies that this is so.

We should add that it would seem to be true that an *empirical* failure of the Law of Large Numbers is not a logical impossibility, and it might not be a metaphysical impossibility either (see Coates 1947, 1956, for a rather unsettling characterization of a possible world wherein the Law fails to hold). This is just to say that the formalism of the Law of Large Numbers might be no more self-executing with respect to the empirical world than any other formalism, and that the issue of whether, and if so how, the Law performs empirically is *not* of the order 1+1=2.

We need not deny that events empirically converge in accordance with the Law in order to question whether the Law's formalism really accounts for what transpires in the empirical realm. It may be, for example, that the iid assumption might not really figure into an explanation of empirical convergence. This might occur, for example, if an empirically operative process conceals such violations in a mathematically valid way. In short, we are going to explore the possibility that the empirical convergence implied by the formalism of the Law of Large Numbers might instead be compliance with a substantively very different process. That process is "smias." Intriguingly, we will see that when the smias construct is put together with the balancing conception alluded to a moment ago the prospect of statistical confirmation of the Eisenbudian view emerges even though convergence occurs.

Eisenbud on the Law of Large Numbers

In keeping with the above, Eisenbud (1982) contends that the Law of Large Numbers amounts to a question begging tack-on if it is invoked as a self-executing explanation of the empirical tendency of sample results to increasingly converge around expected values as sample size grows:

> What has puzzled more than one logician is how events that are supposed to originate independently of one another manage to transform themselves into statistical aggregates with the profoundest respect for order.
>
> Unlike other versions of the divine principle which it has replaced in many quarters, probability does not operate with trifling scrupulosity. The fall of a single sparrow may well go unnoticed, but let a thousand fall and the matter is given the strictest attention in order that a certain *espirit de corps* be maintained. Take the familiar coin toss. Since the coin can fall only heads or tails, the probability of either coming up—as true on the thousandth throw as on the first—will be ½. But if every throw is independent of every other, how is it that the ratio of heads to tails will always tend to approximate one on any lengthy series (the longer the series, the closer the approximation)? The coin, say the mathematicians, has neither memory nor consciousness. Then how is the auditing done? And what has induced a hundred or a thousand individual events to waive their rights to fall out as they please and to make common cause in a universe that then takes on the appearance of lawfulness and predictability?
>
> Mathematicians, begging the question, are apt to invoke the law of large numbers—which, of course, is precisely what has to be explained. Or (still begging the question) they insist that the coin does what it does out of logical necessity. (Eisenbud 1982:212)

Here, Eisenbud expresses the view that the independence assumption (as, say, applied to coin flips) is fundamentally at odds with the sort of convergence of sample results around population expected values that the Law is supposed to explain. He seems to think, for example, that the increasing convergence, as a function of sample size, of tosses of a fair coin to a 1:1 ratio of heads and tails no matter what has occurred up to a given point in any particular sequence is suggestive of a kind of "memory" or "auditing" process whereby successive outcomes in the sequence of tosses are, contrary to the independence assumption, in some way sensitive to previous results in the sequence. We are going to argue later that Eisenbud may indeed have been correct in his assertion that independence never really holds in reality, but that he barely missed the correct specification as to why this may be so.

First, however, let us examine more closely the above contentions of Eisenbud in order to see where they fall short. Let us suppose that Eisenbud was merely speaking loosely when he erroneously stated that the probabilities of heads and tails must be .5 and .5 respectively since there are only two possible outcomes to consider; accordingly, let us operate under the supposition that Eisenbud simply meant that we should just suppose a random variable with two possible values, the realization of which is such that each value is held to be equiprobable. With this in mind, the main problem with Eisenbud's perspective as expressed above is that he appears to want to have things both ways; that is he wants to hold the probability of heads and tails constant across trials, but he also wants to claim that, in order to reliably obtain results that are compatible with the Law of Large Numbers, it must be the case that events can "waive their rights to fall out as they please."

Contrary to Eisenbud on this point, however, it is unclear what "waiving their rights to fall out as they please" can mean other than that outcome probabilities on certain trials are altered as a function of what has transpired on previous trials—which would of course militate against what Eisenbud appears to have already granted in the form of constant probabilities across trials. We should add that insofar as Eisenbud is interpreted as holding that statistical independence (formally considered) along with the rest of the formalism of the Law of Large Numbers cannot *possibly* account for the falling out of empirical events in accordance with the Law, he is wrong and the conventional view of the Law is correct. However, an Eisenbudian retort to this point would be that such a world would be one in which our "explanation" for the empirical conformity that we have been discussing would have to run along the rather vacuous lines of "that's just the way things are" or, similarly, "the Law of Large Numbers is writ into the empirical nature of things and that's that."

Where we should take issue with the conventional view of the Law of Large Numbers is with respect to the idea that this is the *only* view of the Law it is rational to hold. In fact, we can adopt a view of the Law of Large Numbers that is fully consistent with empirical convergence but which nonetheless drops the Law's iid assumption. It is true that one could then argue that a Law of Large Numbers without an iid assumption is no Law of Large Numbers at all. The response, which hopefully does not belabor the point, is that if the forthcoming smias argument in this paper is sound, we can shake hands with the conventional theorist as to events empirically converging, but part ways as to whether, or conceivably how often, iid holds—so that each party can subscribe to the Law with respect to convergence but with differing metaphysical commitments. In sum and in regard to an Eisenbudian approach to the Law of Large Numbers, what is needed at this point is a mathematically valid account of sequences that empirically conform with convergence that is also compatible with a metaphysical perspective that can in principle reject the iid assumption. These considerations lead us to a discussion of smias by way of the gambler's fallacy.

Conventional View of the Law of Large Numbers and the Gambler's Fallacy

Turning once more to Hand (2014), we see the argument that granted a variable with equiprobable outcomes and iid trials, results over numerous trials that comport with the Law of Large numbers are exactly what is to be expected, no matter what values have been realized previously in the sequence and regardless of, and indeed contrary to, the operation of some analogue of memory because:

> What actually happens is that the imbalance is diluted, so that over time the proportion of heads gets closer and closer to one-half. One half is just the average of 0 and 1. This is simply the law of large numbers. (Hand 2014:65)

In effect, Hand (2014) asserts that when Eisenbud speaks of events "waiving their rights to fall out as they please," he is in fact committing the gambler's fallacy, which can be stated as:

> [t]he mistaken belief that an initial imbalance in the proportion of coin tosses that come up heads would be counterbalanced by an excess in the other direction as we made more and more throws. (Hand 2014:65)

Clearly, the gambler's fallacy (if such it is) does involve the mistaken imputation of a sort of memory to the system.

However, we must not make the mistake of supposing that the dilution effect Hand speaks of applies empirically simply because that is what the formalism requires. Such a position would be on the order of a brute metaphysical assertion masquerading as an empirical claim. It is worth noting that Hand offers neither an empirical test of dilution nor a citation to a test of dilution even though he does offer, as countless others have, results from an empirical test of convergence. In fact, this author has been unable to locate any empirical tests of the dilution construct. Perhaps this is because conventional theorists believe that no such test is needed. If so, those theorists are wrong. We return to this issue at the conclusion of the analysis, but pause to ask why conventional theorists do not test dilution empirically given that they test convergence empirically and very often at that.

It is easy to see that the balancing that Hand refers to could produce convergence at least as well as dilution; we could suppose that, for example, 5 heads might follow 5 tails as a consequence of the failure of iid. To amplify what was mentioned earlier, no logical contradiction would be involved if a coin with empirically indistinguishable sides and flipped in empirical conditions as close to ideal as possible continually came up heads over a very large number of trials and then tails over a very large number of trials so that a Bayesian might conclude that a failure of iid was in play. Furthermore, it is by no means obvious that this would amount to a metaphysical impossibility. Thus, the fact that the Law of Large Numbers is logically and mathematically valid in and of itself says nothing about its explanatory power with regard to empirical phenomena. To get that power, the conventional theorist must, almost by sleight of hand, smuggle in the metaphysical assumption that the Law is somehow inscribed in empirical nature.

The conventional position (which *is* a metaphysical position) as to the functioning of the Law in empirical nature may in fact be sound. However, what is conceivably very important work by Baird and Otte (1982) raises very serious difficulties for the conventional account. As Baird and Otte (1982) point out, it is never possible to rule out the possibility that probabilities in fact attach only to ensembles of events. This entails, in turn, that empirical events can never be conclusively established as iid. It also entails that we can never be sure that the Law ever in fact achieves "footing" at the level of individual units, and that we can never be sure that the gambler's fallacy is in fact a fallacy. It is easily shown that these truths are quite compatible both with Eisenbud's overarching theoretical perspective on the role of psi in the empirical world as well as his skepticism toward the conventional view of the Law.

One interesting upshot of Baird and Otte's findings as applied to Eisenbud's work is that the occurrence of psi events can be analogized to committing the gambler's fallacy but getting away with it in that the probabilities of events can be altered at any given point in a sequence in such a way that the iid assumption of constant probabilities across trials and the independence of trials appears not to have been disturbed with respect to convergence—even though, in the sense Eisenbud may well have had in mind during his ruminations on psi and probability theory, they have been. In turn, the potential for the sort of statistical masking described by Baker and Otte's smias construct implies that the *absence* of statistical significance in psi studies may be *neither here nor there* with respect to the presence or absence of psi effects.

Smias, Bias, and Preliminary Application to Psi

Baird and Otte's (1982) potentially pivotal concept of "smias" is perhaps best approached initially by contrasting it with the more familiar term "bias." Bias functions the same way in Baird and Otte's framework as it does in many other instances in which statistical testing is involved; that is it simply refers to probabilities assigned to different individual realizations of a random variable or variables together with the standard iid assumption. Thus, to take an example of Baird and Otte's (1982) that is felicitously in line with Eisenbud's coin example, bias can be understood to refer to the hypothesis that each individual coin toss realizes a value of "head" with a probability of .5 and realizes a probability of "tail" with a probability of .5 with each flip iid (any other assignment of probabilities to the two alternatives would involve bias, too, since the key consideration is that bias-type probabilities are probabilities that are assigned to the potential values of individual trial outcomes).

Smias, on the other hand, involves the assignment of well-specified probabilities only to a series, or ensemble, of coin tosses rather than individual coin tosses, and correspondingly adopts a modified independence assumption under which only successive ensembles of a given size are independently distributed. So, for example, and again following Baird and Otte, we can hypothesize that the probability that a coin "will land heads five or more times in 20 flips" (Baird and Otte 1982:173) is .9941, and in addition suppose that sets, or ensembles, of 20 flips are independent of one another while dropping the standard independence assumption that applies between each successive flip.

One of Baird and Otte's key observations is that hypotheses formulated on the basis of bias can appear to be interchangeable with hypotheses formulated on the basis of smias. Thus, if we adopt the bias-type hypothesis of a fair coin along with bias-type iid (p(H) = .5, p(T) = .5 on each flip), we are also necessarily, by way of the binomial distribution, implying the smias-type hypothesis that the probability of 5 or more heads in an ensemble of 20 flips is .9941. An important point about smias, though, is that the converse does not hold—so that the supposition of smias need not imply anything at all about bias. For example, if we suppose only a smias-type ensemble probability in the 20-flip scenario, in all but degenerate cases each of the 20 individual outcomes within the sequence will, at the outset of the flipping process, be compatible with a range of different bias-type probability assignments—assignments that can, as the process unfolds, change in violation of bias-type independence. As Baird and Otte note in linking smias to Hacking's (1980) emergent probabilities construct, the application of smias to the bias-rooted gambler's fallacy issue is clear:

> Hacking says that such emergent probabilities make no difference to statistical inference, but they do. If there is an adequate bias model, then reasoning about the remaining 5 flips of a 20-flip sequence on the basis of the previous 15 flips commits the gambler's fallacy. However, if there is no bias model "underneath" a smias model, then such reasoning is sound. Given

> 15 tails in the first 15 flips of a 20-flip trial, if we assume smias of .999 and no bias underneath, we can legitimately infer that the next five flips will be heads. This is how to commit the gambler's fallacy and get away with it. (Baird & Otte 1982:174)

In theory, then, granted knowledge of the smias system and its parameter or parameters, one could say either as Baird and Otte do that one could commit the gambler's fallacy and get away with it, or, what amounts to the same thing, that under such circumstances no gambler's fallacy exists at all in that the system really does rebalance itself so as to conform with smias constraints.

Similar considerations apply with respect to the Law of Large Numbers. Once more, from Baird and Otte:

> When a statistical uniformity appears in a population, there are two ways to account for it. One way is to ascribe probabilistic properties to individuals and use results such as the law of large numbers to explain stable regularities in ensembles of individuals. Another way is to claim that the uniformity in the population does not arise out of any probabilistic facts about the individual members of the population, but rather that the probabilities are manifest only at the level of an ensemble. (Baird & Otte 1982:171)

Thus, smias is quite capable of yielding, in a mathematically valid way, the *same* empirical convergence the conventional view of the Law of Large Numbers anticipates—but with a very different metaphysical basis. With the preceding in mind, it is interesting to contemplate what happens if we synthesize Eisenbud's contemplations on the Law (quoted above) with Baird and Otte's (1982) smias construct. If we do, we can take Eisenbud to have been suggesting that psi functions at the smias level and that the Law operates in a "smiasing" fashion and therefore, at least in certain instances, due to psi. In addition, for the reasons specified above, the integration of smias with Eisenbud's thinking would absolve him of the charge of committing the gambler's fallacy.

Another intriguing implication of Baird and Otte's (1982) smias construct as it may pertain to psi theory and research revolves around the idea that smias-derived shifting, or perhaps even fixing, of probabilities and violations of independence can be very difficult and perhaps even impossible to detect with full confidence and can therefore function in ways that are practically indistinguishable from bias-type processes:

> We simply are in a position of not knowing for certain whether smias properties are always, ever, or never grounded from below by bias properties. In some cases we may be able to gather some pertinent data. We can test for

> independence of flips. Such tests are not, however, conclusive. Ultimately our belief in grounding probabilities from below rests on a metaphysical assumption. This intuition may be very plausible but it is metaphysical, nonetheless. Consequently, we can never be certain whether the gambler's fallacy is really a fallacy, since it also rests upon this metaphysical assumption. Perhaps there is some consolation in this. After all, how could so many gamblers be so wrong? (Baird & Otte 1982:178)

We will see later that while the proposed smias-based empirical test alluded to at the outset does not offer the prospect of conclusive support for the smias perspective (of course no statistical test could), it does offer the possibility for compelling statistical evidence of an Eisenbud-style, smias-based balancing effect in contravention to conventional, Handian bias–style dilution.

Turning now to a related potentially significant research implication of smias, readers might agree that the properties of smias could conceivably have a role to play in terms of helping to account for the difficulty of securing replicability of experimental psi results for the simple reason that smias-style psi can masquerade as bias—especially if one does not keep a close eye on the dilution versus balancing question. In any event, smias properties certainly do dovetail nicely with Eisenbud's views on the difficulties of detecting, and especially replicating, psi functioning. Braude (1979) offered remarks that are particularly apposite here:

> Jule Eisenbud has suggested that parapsychology's failure to design experiments that are as reliably repeatable as those in other areas of science may be a function of large-scale or cosmic constraints on psi-functioning. Although he regards dramatic laboratory evidence for psi as a kind of chance occurrence, he does not deny the existence of psi functioning. Rather, he argues that psi functioning may be such that it tends to operate in all of us, but unobtrusively, and that occasional dramatic occurrences of psi in the lab are random fluctuations in what, *by its very nature*, is a non-dramatic range of phenomena. (Braude 1979:70)

Braude continues:

> I think this general position deserves to be taken seriously, although Eisenbud and Crumbaugh (especially Eisenbud) state this view in a way that makes it seem as if there were some mysterious feature of psi itself which makes psi difficult to pin down. Some might see this as an effort to explain the mystery of psi by reference to an even greater mystery. (Braude 1979:70–71)

If we grant to Eisenbud smias-type support for his views on pervasive yet typically unobtrusive psi-functioning, there is a way to reconcile at least the thrust of Eisenbud's conjectures with Braude's concerns. We can say, for example, that to the degree that psi is difficult to replicate it is because of the comparatively quotidian reason that psi tends to be associated with the maintenance of smias, or ensemble-level, probabilistic features—features which, since they can function while preserving the illusion of bias-type properties such as independence and constant probabilities across individual trials, are easily disguised.

We might add, though—and this, too, touches on the empirical test to be proposed shortly—that if purportedly random systems, such as random number generators (RNGs), really do function in accordance with smias and balancing, it could be that larger psi effects would be observed if psi efforts were directed at securing a perfect 50/50 balance in system outcomes. The theory here would be that if there is a psi-based tendency for such systems to balance, efforts to disrupt that balance would be confronting an opposing "force." It might be thought that efforts aligned with the force could conceivably produce larger effects. Though the direction of outcomes toward a 50/50 balance might seem counterintuitive, there is nothing to suggest that such a procedure would be a statistically invalid way to proceed since the possibility clearly exists of obtaining 50/50 results at rates greater than chance standards would lead us to expect.

Some Remarks on Smias and Eisenbud's General View of Psi Functioning

It was shown at the outset, by way of the Eisenbud quote referencing Marbe, that Eisenbud's theorization of probability as it pertains to psi functioning posited a sort of psi-type "memory" associated with probabilistic processes in nature whereby without this "memory" function the laws of probability would not function. So far, we have suggested that Eisenbud's "memory" conjecture is compatible with, and might be better construed as, smias functions or laws and an associated balancing mechanism. We have seen that one virtue of the smias approach is that it provides a straightforward explanation as to why psi functioning can be challenging to assess statistically without condemning entirely the statistical investigation of psi and without necessarily falling back on an account of psi that supposes it to function at cross-purposes with itself. Here, with smias in mind, we turn toward a more detailed examination of Eisenbud's provocative claim that psi processes function as the "glue" that ensures that the laws of probability hold, as well as his related claim that psi might be understood as a sort of psychologically mediated and hierarchically organized control system.

It may well have been that Eisenbud was operating with a view of the interrelationships of probability, randomness, and the law of large numbers that was propounded by Bertrand Russell. Baird and Otte (1982), in the course of advancing their smias construct and questioning the metaphysical basis of probability theory, offer the following quote from Russell:

> The theory of probability is in a very unsatisfactory state, both logically and mathematically; and I do not believe that there is any alchemy by which it can produce regularity in large numbers out of pure caprice in each single case. If the penny really chose by caprice whether to fall heads or tails, have we any reason to say that it would choose one about as often as the other? Might not caprice lead just as well always to the same choice? . . . [W]e cannot accept the view that ultimate regularities in the world have to do with large numbers of cases, and we shall have to suppose that the statistical laws of atomic behavior are derivative from hitherto undiscovered laws of individual behavior (Russell 1935:168).

Russell appears to be arguing that if we interpret randomness as caprice there is no satisfactory way to derive the Law of Large Numbers. So a 10,000 consecutive heads sequence and something like a 5,000 tails and 5,000 heads sequence must each be considered equally indicative of chance, or randomness, since in each case, according to Russell, we would have to say "that's just the way things are; it's an arbitrary process." Thus, as the end of the preceding quote shows, Russell adopted determinism at the individual unit level in order to secure the empirical results typically associated with the Law.

It may have been with respect to the metaphysical frailty of the Law on touchy issues such as the correct specification of randomness that Eisenbud was motivated to state, in terms that resonate nicely with smias:

> Thus if we imagine an experiment in which coins, sticks, or needles (or billiard balls in a shuffling machine, molecules in solution, or gas particles in a chamber, to take classical examples) could, through some means, be shielded from all observers and all mental influence, we would find, according to our supposition, that the laws of probability would no longer hold. They would not work by themselves. In the absence of the observer, pure indeterminate chance would reign, chance unfettered by the "laws of chance," chance that would never give rise to any kind of order. Such a thought experiment is at best, of course, a kind of myth. But it makes more sense than than the updated version of the immaculate conception that sees the key to the universe in atomic events in which causality fades out like the Cheshire Cat and only an abstract and impotent probability, suspended in nothingness, is left to do the world's work. It avoids the difficulties, moreover, of leaving to each individual inanimate event (coin, stick, atom) the privilege

> (and the necessity) of making up its own mind about how it will behave. (Eisenbud 1982:214–215)

It would seem reasonable to take Eisenbud to have accepted, along with Russell, a view of randomness as caprice. If we do, we can balance our books by supposing that where Russell plumped for conventional determinism as a way out of caprice/randomness, Eisenbud instead opted for psi-style determinism that can be constructed along smias lines.

Along these lines, Eisenbud stated:

> . . . in the underlying system of psi-mediated probabilistic bookkeeping by which events are kept from getting too much out of whack along one axis or another, a breakthrough in one sector must sooner or later be balanced by a tightening up or oppositely signed trend in another. (Eisenbud 1992[1963]:163)

Furthermore, we have Eisenbud contending:

> With the observer now seen also in his creative aspect, we might envisage control as effected through some sort of a collective unconscious clearing house (not to be confused with Jung's Collective Unconscious), where myriad individual behavioral vectors (arising from dynamic contexts such as those seen in the case studies presented earlier) are sifted, sorted, and graduated hierarchically into the effective determinants of large-scale events on a group level. Particular segments of existence, as I have elsewhere suggested (Eisenbud 1966, 1967) might be the case in the areas of ecology and evolution, would be responsive to individual decisions (not too unlike those of Margenau's electrons) made at grass roots levels; but these, at the same time, would be subject to boundary conditions ("needs") imposed at higher levels. (Eisenbud 1982:215)

In smias terms, Eisenbud is presenting us with the notion that psi is a mental process that governs nature in such a way that events fall out in ways that are sensitive to the falling out of other, supposedly independent, events. We proceed to adumbrate suggestions pertaining to the empirical investigation of this idea in the next section.

Toward the Empirical Investigation of Smias/Psias

Eisenbud can be read as having advanced the idea that psi is written into the fabric of nature in such a way that events invariably fall out in patterns suggesting some type of inter-communication and hence dependency—just as the conventional theorist/skeptic can be read as supporting the idea that probabilistic events can fall out as individual units and independently of one another. A label for the Eisenbud perspective on psi and probability might be handy, and so in light of our synthesis of smias with Eisenbud's work on psi, we might consider him to have advocated a "psias" model. In any event, we have seen that the two views can have very different implications with regard to the manner in which the Law of Large Numbers operates empirically.

Both views are compatible with empirical convergence, but the two views present us with a contest between bias/dilution and smias/balancing. Simple tests involving random number generators should help resolve the dispute. If the conventional/skeptical view is correct, Hand, for example, is right when he asserts that an initial imbalance in heads ". . . is diluted, so that over time, the proportion of heads gets closer and closer to one-half" (Hand 2014:63). This is, after all, exactly what a logical proponent of the bias view who holds that the formalism of the Law of Large Numbers self-executes empirically would say. Notice that Hand says "closer and closer to one half," so that he is acknowledging that in the case of an initial imbalance convergence is expected to be *directional*. Under the Hand view, at any point in the sequence the expected number of heads in the remainder of flips, regardless of how many, will of course be one-half the remaining number of flips. Therefore, Hand is asserting that no matter how many flips subsequent to the initial imbalance we perform, we *must* expect to observe an excess of whatever result was imbalanced at the start of the sequence (heads in Hand's example).

But *is* that in fact what we observe empirically? Might it be the case that what we observe instead is that RNG systems are, we might as well say now, smiased/psiased so that they tend to rebalance excesses, in accordance with a smias, aggregate level parameter of .5, more completely than the bias/dilution model says will happen? Perhaps, for example, the probability of exactly 5,000 heads in 10,000 RNG "flips" after having obtained an initial 15 tails is significantly higher than the bias/dilution model predicts? Similarly, if results really are "pulled" toward a smias system parameter of .5, we might expect to observe a tightening of sampling distributions in that significantly less variance (and therefore smaller standard errors) will be observed than predicted by the bias/dilution model.

To be sure, smias/psias effects may be a function of sample size as well as the nature of the RNG mechanism examined. Also, we should mention that the precise relationship, if any, of the magnitude of initial imbalances to the magnitude of the smiasing of individual outcomes in the direction of hypothesized smias-governed balancing is very much open to question. It should not be difficult to test smias/balancing hypotheses; indeed, scrutiny of extant datasets would serve well as a start (although it is logically possible, but at least to this author quite unlikely a priori, that the absence of the will to test these hypotheses in the past meant that the effects did not show up in the data. The a priori unlikelihood referred to is especially reinforced if Eisenbud is correct in his conjecture that psi is writ in nature).

Discussion and Conclusion

It has been shown that Jule Eisenbud expressed ideas about psi functioning that came very close to capturing that which is expressed by Baird and Otte's (1982) smias construct. Unsurprisingly, then, it was also possible to show that smias and Eisenbud's view on the nature of probability as it pertains to psi have a natural affinity for one another.

Smias is a friend of Eisenbud, and it may be a construct that is worthy of consideration by all psi theorists and researchers. The construct has its virtues in terms of psi research and theorization—it offers a reasonable account, that goes hand-in-hand with psi, of the empirical convergence implied by the Law of Large Numbers; it avoids problems associated with the gambler's fallacy; and it figures prominently in a reasonable argument as to why psi is, at least sometimes, hard to detect and/or replicate statistically. A construct that can do each of these things simultaneously should perhaps not be summarily dismissed.

Furthermore, it has been noted that the conventional view as to why the Law of Large Numbers functions the way it does empirically is in fact a metaphysical view, and not the only metaphysical view on the issue that is rational to hold. Along these lines, a test has been proposed that might empirically substantiate a smias/balancing view of the empirical operation of the Law of Large Numbers as against the conventional bias/dilution view of same. Finally, given the specification of the smias construct, empirical substantiation might be viewed as tantamount to metaphysical support for psi functioning.

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