Randomness and Reality

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1. Benioff's Ideas on Physical Implications for Mathematics

In previous technical work ([1] and [2]) on which his present paper [3] draws, Benioff has presented results conforming with the following argument-scheme:

> First, *if* we construe Quantum Mechanics as making claims to the effect that infinite outcome sequences (generated by repeated measurements on a system for a given observable in a given state) be random; *and* second, *if* a strong definition of "random" is adopted in this construal, *then* certain models of Zermelo-Fraenkel set theory (ZF) *cannot* be "carriers for the mathematics of physics".

How interesting is this? One can approach the matter on two levels, the level of the specific technical results, and the level of more general implications of this pattern of argument for understanding connections between mathematics and physics. The second more general level should be our primary focus, but it will pay at the outset to look briefly at the technical level.

As he indicated, Benioff takes a random outcome sequence, for the purposes of the first premiss, to be one that avoids every Borel set (of sequences or reals) of measure zero (on a fixed probability measure) that is definable in the language of ZF (with one fixed parameter, interpreted as the probability measure). (Equivalently, a Benioff-random sequence must belong to every such Borel set of measure 1.) Below I will have more to say concerning this definition. For now, let us note the motivation: a measure 0 Borel set is very special, its members are highly improbable. Intuitively, we would like to say that a random sequence avoids any such special set, or, equivalently, that it possesses every measure 1 property, often called a "property of randomness". The trouble with this, however, is that no sequence could

PSA 1978, Volume 2, pp. 79-97

be random on this definition, since the singleton of any sequence is itself a measure 0 set (for standard probability measures)- including various sequences fitting his definition schema for "D-random sequences." Here, "D" is to be filled by a requirement restricting the measure 0 Borel sets that a random sequence must avoid. Benioff's choice, for his technical results, takes D to be "definable in ZF'. The next step is to focus on certain models of ZF about which a good deal is known. In particular he considers M_0, the minimal model of ZF. This is the smallest model in which all sets are constructible (in the sense of Gödel); it is countable. Moreover, it can readily be proved that every set in this model is the extension of a formula of ZF with one free variable, i.e., is ZF-definable. Thus, so is every measure 0 Borel set of reals. But a random sequence must avoid every such set. But it can't avoid its own singleton. Therefore, the minimal model cannot contain any random sequences.

Such a result is, of course, not surprising. The combination of a strong definition of randomness, together with choice of a very small model in which everything is specifiable, straight-forwardly yields the conclusion. Furthermore, no one who believes in ZF set theory will be surprised to learn that the mathematical universe must embrace more than the minimal model. A set-theoretic realist believes there are uncountable sets and unconstructible sets. From that perspective, appeal to what physics may say about random sequences surely must appear esoteric, since physics already quantifies over uncountable sets, such as the reals.[1]

Now I don't take Benioff to be disputing any of this. Rather, I understand him to be using this result, however unsurprising in its own right, as an illustration of a way of making a new kind of connection between mathematics and physics. Let us consider this.

What is striking and potentially interesting about the argument scheme in question is the suggestion that physics could help decide the nature of the mathematical universe. From the standpoint of time-honored philosophical positions, this is heresy, since mathematical truth has traditionally been supposed _a priori_ and not subject to the claims of empirical science. Note that _it is_ not that some statements of _applied_ mathematics are being said to depend on physics, but rather that statements of _pure_ mathematics are: very pure indeed, perhaps the purest of the pure: _what_ sets exist!

Such a position has been defended in a general way --by pragmatism, for example. Rational belief, at least, in set existence statements has been argued to depend on what we find we must postulate in order to do physics or natural science generally. (I allude here to views of W.V.Quine, and related views of Hilary Putnam. See, e.g., [16]).

Benioff's proposal could be assimilated to this mould: quantum mechanics requires us to postulate the existence of certain infinite sequences of natural numbers (certain reals, that is). This, at any rate, according to the first premiss. The question of just which

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192463 Published online by Cambridge University Press

sequences are thus required is the question, which definition of randomness should we fix upon. Having decided on one in particular, we can then argue that the mathematical universe must be rich enough to contain such random sequences.

Now from the mathematical point of view, this may seem somewhat strange. After all, these definitions of randomness come from mathematics. Weren't the mathematicians who thought them up already committed to the existence of sequences fulfilling their definitions? Surely they didn't suppose they were writing down a formula coextensive with '$x \neq x$'! Of course, the careful mathematician provides a proof of this. But in what system? Well, let's say in ZF. Then the next question is, can one prove in ZF that there exist Benioff-random sequences? First we have to check that we can state the definition (of "Benioff-random") in the language of ZF. But this is not possible! The definition employs the notion of 'arbitrary set definable in ZF' (a semantic notion, relative to some fixed standard transitive model of ZF), and it is well-known that this notion is not translatable into ZF, on pain of contradiction. (In terms of 'definability in ZF', one can define satisfaction and truth in ZF, contra a theorem of Tarski.)

So Benioff's definition of randomness takes us beyond the resources of ZF set theory. In fact, the technical results mentioned above establish the independence of the existence of Benioff-random sequences. by pointing out that the minimal model contains none. Therefore, no theorem of ZF, properly interpreted, can say that there exist such sequences. ZF is a very incomplete theory, and we cannot appeal to it on this score.

In this respect, Benioff-randomness differs from most mathematical definitions of randomness. Suppose, in fact, we modify the definition only slightly: instead of speaking of ZF-definable Borel sets of measure 1, let us instead speak of such sets definable by a formula of ZF with quantifiers restricted to sets of rank of some countable ordinal level, in fact ranks of an ordinal level nameable in ZF. (One can readily define a hierarchy of such languages with restricted quantifiers and prove that there is a "minimal fixed point" in this hierarchy, beyond which no new sets get defined. Further remarks on "restricted-Benioff-randomness" can be understood in terms of this minimal fixed-point level.) Now this modified definition of randomness, "restricted-Benioff-randomness", <u>can</u> be given in the language of ZF;[2] further, it is straightforward to prove that random sequences in this sense exist.[3]

Thus, by the slight change of introducing restricted quantifiers, the whole question of the existence of random sequences is resolvable in the affirmative in ZF. <u>If we believe in ZF, there is nothing left for physics to say.</u>

At this point, it is worth mentioning a connection with the early parts of Benioff's paper. There he posed the question of finding

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"the correct definition of randomness", and further suggested that this should be the "weakest allowable" in a certain sense. Now there are a number of serious problems raised here, not least of which is the fact that, as Benioff concedes, his proposal on "correctness"--which is fundamentally nothing but a consistency requirement--requires that randomness be relative to a physical theory plus interpretive rules (connecting theory and observation). The mathematical definitions relevant to the main issue we've been considering thus far are, of course, free of any such relativity. Perhaps there are particular physical theories of interest such that the sets of random sequences on definitions "correct" for those theories coincide with those determined by some of the mathematical definitions. Far greater precision in formulating the theory-relative notion would be needed before one could begin to investigate this.[4] In any case, notice that my suggested "restricted-Benioff-random" is weaker than "Benioff random", though it is still quite strong, in allowing talk of sets of very large transfinite rank. Still, it is weak enough so that ZF can settle the question of existence. If this modified definition or any weaker one is conceded to be "allowable", then, on Benioff's view too, there should be nothing left for physics to say.

The crucial point is this: if we believe that quantum mechanics does have something new to say about set existence, via a notion of randomness, then we must be prepared to believe that QM distinguishes between Benioff-random sequences on the one hand and restricted-Benioff-random sequences on the other. Is this credible? It seems to me not.

At this point let me raise a related but more elementary objection, one whose solution would seem to be a precondition for meeting the main objection just raised. Note that QM as usually formulated doesn't directly speak of random sequences at all. Rather it makes probabilistic assertions about measurements on individual systems. Of course, there are deep questions concerning the interpretation of such probabilistic statements. For example, the most common limit-of-relative-frequency interpretation confronts the problem of ordering the outcomes of hypothetical infinite sequences, since limits depend crucially on order. Furthermore, there is the thorny problem of how we can confirm probabilistic statements so interpreted on any finite data basis. The point to be made here, however, is that if one builds into QM claims about the <u>randomness</u> of infinite outcome sequences, in accordance with Benioff's first premiss, <u>one is incorporating claims that raise serious problems of confirmation over and above those already present in ordinary quantum mechanics.</u> The reason is this: in the case of an ordinary probabilistic statement, one can observe a quasi-asymptotic approach to a limit by observing actual relative frequencies in longer and longer finite outcome sequences. This at least provides a rational basis for evaluating counterfactuals to the effect that, were the sequence extended to infinity, the relative frequency would have such and such a limiting value. No comparable situation exists with respect to claims of randomness of infinite sequences. All finite

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sequences, on the definitions under consideration, are non-random. There is no asymptotic approach to randomness. One could meet this objection, perhaps, by switching to a definition of randomness which applies to finite sequences or is such that asymptotic approach to randomness makes sense. (For instance, definitions based on statistical tests or a measure of complexity, cf.,[14]) But such a switch would not be in the interest of Benioff's approach, since such definitions raise no problems at all concerning set existence. One may be apprehensive that by building claims of Benioff-randomness into quantum mechanics, one is making the theory untestable in principle.

Even if this objection can be met, Benioff's approach is far from home. For as already indicated, in order for QM to have something new to say about set existence by the route proposed, it would have to provide a basis for making distinctions such as that between Benioff-randomness and what we have called restricted-Benioff-randomness. Clearly, no finite body of data could make such a distinction; and I see no theoretical consideration in physics -- at least, in the laws of QM of the sort that I am acquainted with -- that would do so either.

But there is a still more fundamental point that must be raised. Many interpreters of quantum mechanics have wanted to see in the theory and its success confirmation of some claim about the absence of causality at the micro-level. This is the real source of talk of physical randomness. Is such talk--if we want to take it seriously--properly represented by construing physical randomness as mathematic randomness, on any of the known definitions?

2. Mathematical and "Ultimate" Physical Randomness

My view is that there is at best an epistemic connection between mathematical notions of randomness and the physical notion, which I take to be roughly interchangeable with "indeterministic". I do not believe that mathematical definitions can be taken as explicating physical randomness. In brief, this is because mathematical definitions attempt to capture a notion of orderlessness, something that must be relativized to a language or some other framework for classifying and detecting or predicting outcomes. Like simplicity and like similarity, what is orderly varies radically as we shift from one framework to another. This need for relativization is one factor that has generated so many different definitions of mathematical randomness; this was especially evident in Benioff's nice codification scheme for 'D - randomness'. However, whether a physical process is "fundamentally random" in the sense of its outcome being not causally determined should not depend on the predilections of one peculiar species of hairless ape for certain kinds of order. Indeterminism, if a relative matter at all, is not relative to the same degree or in the same ways as mathematical randomness.

These points can be brought out by looking at notions of mathematical randomness that do apply to finite sequences, such as those of

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Kolmogorov, Chaitin, and Martin-Löf based on a measure of complexity. (See [6], [11], [12] and [14].)

The basic idea behind these definitions is simply this: non-random sequences (say, of 0's and 1's) may be compactly described; instructions to a machine to print out, say, a hundred repetitions of '01' can be compressed into a short formula compared with the length of the sequence whereas instructions to print out a disorderly, "random" sequence will require nearly as many bits as the sequence itself. More precisely, following Kolmogorov ([11] and [12]), the notion of "the complexity of a sequence relative to an algorithm" is defined first: letting x, y, etc. range over finite or infinite sequences, φ over algorithms going from the programming instructions, p, to the sequences, we set

$$K_\varphi(x) = \begin{cases} \text{min length } (p) \text{ such that } \varphi(p) = x, \text{ or} \\ \infty, \text{ if no p exists such that } \varphi(p) = x, \end{cases}$$

that is, the complexity of a sequence on algorithm φ is the length of the minimal binary program for the sequence, if such exists, and diverges otherwise. At this point, appeal is made to Church's Thesis, according to which the functions properly called "computable by algorithm" are just the partial recursive functions, and the variable φ in the above is taken as ranging over the latter. (Recall that the recursive functions are those a Turing machine can compute, and the partial ones are those that need not be defined for every integer as argument.) Now relative to a partial recursive function, a (finite) sequence may be classed as <u>random on significance level m</u> (an integer) if its complexity is within m of its length.

Inspection of this definition clearly reveals dependence on the algorithm relating program to sequences. However, proponents of this approach have cited a "fundamental theorem" ([11], pp. 5-6; [8], pp. 135-36) which is supposed to reduce severely this aspect of relativity. The idea behind this theorem is this: one appeals to a <u>universal</u> Turing machine (or partial recursive function) in evaluating complexity of sequences and observes that the greatest amount by which the universal machine's measure can exceed that of another machine's is given by an index for the latter plus a systematic translation of its programming language into that of the universal machine. Thus, the theorem states, there exists a partial recursive function, A, such that for any other partial recursive function φ, the following inequality holds:

$$K_A(x) \leq K_\varphi(x) + c_\varphi,$$

where the constant c_φ does not depend on the sequence x to be computed but only on the given function (machine), φ. The order of quantifiers here is this: $\exists A \forall \varphi \exists c_\varphi \forall x$, from which the elements of strength and weakness in the theorem may be inferred. Functions A satisfying this theorem are called "asymptotically optimal"; it is to these that one

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then appeals in delineating the class of "random sequences": their complexity as measured by an asymptotically optimal function is within (chosen) significance level of their length.

While there certainly is some power to this theorem, it should be clear that relativity to programming language has by no means been eliminated. For one thing, the optimal functions satisfying the fundamental theorem are not unique, i.e., there are many. What can be established is the following:

$$\left|K_{A_1}(x) - K_{A_2}(x)\right| \leq {}_{A_1, A_2}$$

i.e., the difference in complexity values assigned relative to any two such functions to sequences is uniformly bounded by a constant, that is, it is never greater than some finite amount depending only on the functions (and not on the sequences whose complexity is in question). This means, intuitively, that for any two asymptotically optimal programming methods, there will be indefinitely many sequences of sufficient complexity such that any disagreement as to their complexity on the part of the two programming methods will be insignificantly small. However, it does not mean that such disagreement will be small for all sequences (or problems). On the contrary, there is no bound that can be set on the values of the constants appearing either in the expression above (for the difference between optimal functions) or in the fundamental theorem's inequality which defines optimality. On the contrary, since programming languages of arbitrary complexity (intuitively) are being considered (i. e., we're talking about arbitrary Turing machines), it is clear that as the index φ (in the fundamental theorem) varies over the whole class of machines or partial recursive functions, the constant c_φ assumes arbitrarily large finite values. (For instance, translations from arbitrarily "cumbersome" programming languages must be taken into account.) Similarly for the constants bounding the differences between pairs of asymptotically optimal machines. What this means is that for any optimal method, there will be sequences of arbitrary length such that some non-optimal method can be found which generates the sequence more compactly or efficiently than the given "optimal" method. And this "more" may be itself of arbitrary size (measured in bits) as the length of such sequences grows. Further, it means that there exist sets of sequences of arbitrary length (finite) such that the rank ordering of ascending complexity induced by one asymptotically optimal method is totally different from the rank ordering given by another asymptotically optimal method. This is important for it means that different optimal methods may disagree widely on how they explicate qualitative judgments as to which sequences are more complex than which.

What does any of this have to do with claims that certain physical processes are ultimately, irreducibly random? Let us consider finite outcome sequences obtained by subjecting a quantum mechanical system to a given state preparation and measurement of a "yes-no" observable such that the state is not an eigenstate of the observable. We may, for simplicity, further suppose that the quantum mechanical probability for a "yes" answer (designated '1') is 1/2, and likewise for the probability of a "no" ('0'). Now if probability is understood in terms of relative frequency in an ensemble of identically prepared systems, we face the problem that ensembles don't come ordered, whereas order is at the heart of Kolmogorov randomness. We can, however, with Benioff, stipulate that time provide the ordering. Just what this means if measurement destroys the system and a new one must be prepared is not clear, but let us not pause over this. Under any method not involving prevision, we would expect the generated binary sequences to exhibit a random character. In fact, we can even prove that a composite system made up of the reprepared segments will yield sequences whose probability of being Kolmogorov random (on a reasonable significance level) approaches 1 as the length of the sequence grows without bound, provided that it is assumed that the components of the system are uncorrelated.

Here then is some connection between physical and mathematical randomness. But just what is the connection? First, note that we must not make the mistake of supposing that a high probability of obtaining Kolmogorov random sequences even tends to show that the process generating the sequences is "ultimately acausal" or "indeterministic". That this is a mistake is immediately obvious when one considers sequences generated by coin flips or other "classical" systems. Second, however, the assumption that the components of the system are uncorrelated does have physical implications, and without that assumption the argument does not go through. To see the significance of this, suppose we were to observe the improbable: a long outcome sequence of a highly non-random character, such as 10101010... Observation of such sequences in a laboratory under controlled conditions might well lead some physicist to contrive an "On-off extension of quantum mechanics"! At least it would be reasonable to question the uncorrelatedness of the components. In short, such observations of non-random sequences would cause us with cause to seek causes.

The link, then, between mathematical and physical randomness is epistemic and only that. Observations of mathematically non-random sequences can be used to decide when further explanation in terms of as yet undiscovered causal factors is wanting. But, in no sense is any notion of mathematical randomness serving as an explication for "ultimate physical randomness", whatever that might be.

Even this much connection must not be exaggerated. In particular, one must not commit the fallacy of supposing that the a priori likelihood of the specific alternating sequence of our example is any less than that of any other sequence. To the best of our knowledge, equiprobability for all sequences is the proper assumption, and it would be surprising to find any sequence whatever generated over and over again. But the non-random sequence of our example has the special property of being repetitive, so that its subsequences are non-random in just the same way. Thus, observation of a single instance of this sequence (very long) may be taken as a repetition of a number of (still long) sequences of the same type. It is this feature that provides justification for treating a single case of the sequence of our example as cause for greater concern than a single occurrence of a random sequence, or of many other non-random sequences. Thus it would seem to be primarily the phenomenon of obtaining the same result under repetition rather than the specifically, non-random character of the initial observed sequence that provides the rationale for seeking further causal factors. As it turns out, this only complicates, but does not upset, the sort of connection I am urging, since m repetitions of a relatively random sequence of length n will yield a sequence of length m x n which is relatively non-random, the more so (on a given significance level) as m increases.

3. Indeterminism

Talk of "ultimate randomness" of physical events is not to be explicated in terms of mathematical randomness. As already indicated, it is closely linked with talk of causal indeterminism. Is there any hope of making sense of this kind of talk? One often finds appeals to "ultimate indeterminism in nature" made in connection with claims that a statistical theory such as quantum mechanics is "complete", or "the best that we can do, *in principle*." (Cf.,e.g., [5], p. 48.). When one presses for clarification of what is really being asserted by such phrases, one is quickly disappointed. Answers generally exhibit one of the following three defects: (1) the explication makes the notion in question relative to humans or parochial in a way that undermines the "objective" thrust of the phrases as physicists and philosophers have used them; (2) the explication avoids the first pitfall but has the immediate consequence that no matter how the world may happen to be, it is deterministic, thereby trivializing the notion; or (3) the account avoids both (1) and (2) but is *irreducibly metaphysical* in character, taking as given a full set of "possible worlds" with or without a temporal branching structure on which the truth or falsity, respectively, of indeterminism directly depends.

The major open question in this area seems to be to be this: are these three categories exhaustive, or is there a fourth, a non-parochial, non-trivial, not irreducibly metaphysical explication of any of the phrases in this interrelated family? I do not have an answer, but the question seems to me important. But to highlight this, a few comments on why each of the three types is defective are in order. Nothing really needs to be said on (2), since nobody intends to be making trivial statements in the contexts at issue. As to parochiality ((1)), this is almost as clearly unintended, and so, with non-triviality, may be taken as a condition on the problem of explication. However, let us note in passing two brief points. First, just what counts as non-parochial is susceptible of degrees and is quite vague. What is intended, I think, is that whether a physical system is indeterministic (in given magnitudes) ought to be as much a fact about the system itself as any other theoretical physical fact (such as being in a certain state of motion relative to a coordinate system). Second, however, at least parochial interpretations, such as "predictable by human physicists", can be comprehended as yielding intelligible assertions (subject to elimination of some residual vagueness), and even interesting ones. This cannot, I believe, be said for "explications" of the third kind. I do not suppose that all talk of possible worlds is on a par, and that we must accept or reject all of it. But in this context I regard it as completely unhelpful and, by and large, unintelligible. Let me briefly explain.

What does it mean, on a possible worlds approach, to say that the actual world is (futuristically) indeterministic in magnitudes m_i? It means that there is some possible world indistinguishable from the actual up till (or at) some point in time and thereafter different in respect of the m_i (construed as functions from physical systems at times to the real numbers). Now "possible" here cannot mean "logically possible" in any sense based on analyticity or linguistic meaning, since if it did, the world would always be indeterministic in whatever magnitudes you like. No contradiction is involved in saying that future branching occurred. So on such an interpretation, we get trivialization. If "possible" means "physically possible", then there are two cases to consider. (i) This is <u>explained</u> as "satisfying all (true) physical laws", in which case either the notion of "true physical law" is being taken as primitive and the account is not really of type (3) anyway, or "physical law" is itself explained as "true in all physically possible worlds" (or something equally obscure), in which case we have what amounts to vicious circularity. (ii) Physical possibility isn't explained at all. In this case, we are being asked to make sense of statements of the form, 'the world might (physically) have been the same till time t and different (in specified ways) afterwards', or of the form, 'if any world had been physically the same as the actual till time t, it would have continued to be the same thereafter'. But, I claim, we have no way whatsoever of evaluating such statements without appealing to some body of theory taken to hold in the actual world.[5] But what body of theory could this be? One might respond, "quantum mechanics together with strong negative mathematical results on hidden variable extensions of quantum mechanics (e.g., [10])." Space does not permit an adequate treatment of this, but an important problem with the proposal seems to me to be that it does nothing to help clarify what the counterfactuals mean, what it is, that is, that we're appealing to our "best scientific theory" to confirm. Moreover, it is hard to see how the appeal is even relevant to confirmation when we realize that claims of indeterminism involve quantification over <u>all</u> "laws of nature", in contrast (I would argue) to more homely counterfactuals evaluated by invoking specific theories. What, it may be asked, can quantum mechanics possibly tell us about "all laws of nature"? <u>In short, as long as we construe indeterminism claims in such</u>

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fashion, we are quantifying over too ill-defined a totality to know what we're talking about.

It must be stressed that our problem concerns indeterminism as predicated of physical systems or the physical universe as a whole. This must not be confused with the problem of characterizing theories as deterministic or indeterministic. Here it seems, we have a much firmer grasp. In addition to clear examples, such as Newtonian mechanics, we have the work of Montague [15] which presents general definitions in terms of the models of the theory in question. Roughly, a theory T is (futuristically) deterministic in magnitudes $m_1 \ldots m_n$ (represented by magnitude-signs $\bar{m}_i$) just in case any pair of models of T which agree at a given time on their interpretations of the vocabulary of T also agree on the m_i at any other (future) time.[6]

One might suppose that, given such a characterization of 'deterministic theory', it is but a short step to saying what counts as a deterministic world: the world is deterministic in magnitudes m_i iff it is truly described by a theory deterministic in the m_i, indeterministic (in m_i) otherwise. But unless restrictions are placed on what is to count as a theory, trivialization follows trivially, since, given a modicum of set theory, there is always a theory with a predicate which picks out the actual "m_i-trajectory" of the physical universe (just a function from the real line R to R^n (assuming $1 \leq i \leq n$), itself a set theoretic object), and if the theory simply uses this function to predict future states, it is by definition deterministic. (Earman has made a similar observation in [7].)

Is there any hope for this approach? So far little has been done beyond pointing out that genuine deterministic theories must consist of lawlike sentences as opposed to accidental ones. But until we have some grounding for this distinction, we don't really have an approach. And it is just here that we face the danger of lapsing back into either the first or the third category of explication mentioned above. A Humean approach to lawlikeness will give us parochialism, and a possible worlds approach will give us nothing worth having. We seem to have regenerated our problem.

A word here is in order on a related problem confronting applications of Montague's work. This concerns the question of what it is to represent a magnitude in a theory. (This was also raised by Earman in [7].) Normally we take for granted that certain terms in our own language designate magnitudes of theoretical interest. However, a deterministic theory need not be in our own language, and so the question how this semantic relation gets established has at some point to be faced. If magnitudes are taken as intensional abstracta in the manner of possible worlds semantics, the problem is essentially that of making sense of "rigid designation" of magnitudes, in the terminology of Kripke (see [13]). In particular, any possible worlds approach to the problem of lawlikeness must deal with this. Why is this crucial for a

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metaphysical approach to indeterminism? For the simple reason that, without a strong, intensional relation of magnitude designation, in a very large class of conceivable cases we get once again trivialization of the determinism issue. For in the absense of such an intensional relation, our criterion of magnitude designation must be extensional. In addition, if parochialism is to be avoided, the "possible worlds" or model-theoretic structures representing them must contain enough structure to interpret a large variety of possible languages. But these conditions will allow us, in a great many cases, to pass from a given indeterministic theory to a deterministic theory in Montague's sense, preserving both truth in the actual world and lawlikeness (where this is understood as truth in all the relevant "worlds"). (For a proof-sketch of this claim, see Appendix.)

Note that this is just one more problem for the metaphysical approach. It is not necessarily a problem that arises in any context where model-theoretic structures are taken as representing a possible world. So long as we are working with structures taken as our own "constructions", we simply appeal to our own stipulations as to what is designated by a term. The difficulty arises when we speak of "possible worlds" as somehow given in advance. On this picture, how do we know what we are designating?

If any progress is to be made on this approach, some principled restrictions must be found on the languages in which physical theories may be framed. Such restrictions would carry over to the model-theoretic structures used to represent possible worlds, so that such structures could not be used to interpret arbitrary theories in arbitrary languages. Perhaps it would be illuminating to consider restricting languages to those learnable by any viable form of intelligence. The main problem here is to avoid a circular appeal to "physical possibility"--one is in effect seeking an independent characterization of "physically possible physicist". This would look toward a synthesis, not between physics and mathematics, but between physics and biology. That, however, brings us to the limit of this discussion.

Appendix

On the Montague-Earman approach to determinism, the world is deterministic in magnitudes v_i provided there is a theory T consisting of (true) laws which represent the v_i such that the class of (relevant) models of T exhibits no temporal branching. It is assumed that the models have a temporal structure isomorphic to the real line. Branching (futuristic) occurs if two structures agree on the interpretations of the magnitude terms up until a given time but differ on some such term at a later time. An ordered triple, $\langle m,m',t\rangle$, where m and m' are relevant models which agree on the v_i terms (we designate these v_i) prior to (or =) t but disagree at some t' later than (or =) t, will be

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called a branch-point. Earman (in [7]) called the approach under consideration "linguistic" since the notion of lawlike sentence was taken as primitive. Apart from its ultimate status, if we take "law" to coincide with "sentence true in a sufficient class of structures representing natural possibility", then trivialization threatens unless the conditions for a theory's representing a magnitude are quite strong. This can be made more precise as follows.

Let α be a class of model-theoretic structures (taken as representing some notion of natural possibility). We assume that each m in α is suitably rich in the sense of containing a large set of (n-ary) extensions from its domain, so that (reducts of) these structures can serve as models of theories in a wide variety of languages. Furthermore, we assume each m contains as a part a mathematically rich structure including at least the real numbers and functions from concrete objects in the domain to reals. A magnitude v may be taken as a function from α to such functions (from concreta in the domain in question to reals with appropriate units). Thus, intuitively, the magnitude position (classical) may be taken as a function which assigns to each $m \in \alpha$ the function in m which assigns each particle in m at t the real number representing the position of the particle at t.

Suppose now that T is a theory in the variables v_i consisting of laws over α, i.e., the reducts $m|T$ of each m in α to the vocabulary of T is a model of T. (It is assumed that T is fully interpreted over α.) Suppose further that one of the structures in α represents the actual world. Call it G. Our main claim is this: If T is indeterministic in (some) v_i in α, T can be transformed into a T* by a reinterpretation of the terms v_i (and possibly other extralogical vocabulary) with the following properties:

(i) T* is indistinguishable from T in the real world G;
(ii) T* consists of laws over α;
(iii) T* is deterministic in the v_i in α,

provided that extensionally equivalent interpretations of the v_i (in just the structure G) suffice for theories to represent the same magnitudes.

The strategy in constructing T* is to modify the interpretations of the v_i so as to eliminate all branch points in α. For simplicity, suppose we have a single branch point, $\langle G,m,t\rangle_v$, where the subscript 'v' indicates that it is just with respect to this magnitude that G and m diverge at time t. Let us further suppose that G and m contain the same enduring objects (this way we avoid having to introduce isomorphisms between the models--there is no essential loss of generality). Suppose v is position, and suppose that at t, a single particle p in m diverges θ^o to the left (in some plane in some fixed coordinate system) of the straight path p takes in G. Now, change the interpretation v of the position variable v of T to v* as follows: for all t, $v^{*G}(p) = v^G(p)$;
for $t' < t$, $v^{*m}(p) = v^m(p)$;

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for t' ≥ t, $v^{*m}(p)$ = θ° to the right of position in m, i.e., $v^{*m}(p) = v^G(p)$.

That is, v* is just like position except that at times ≥ t in m it coincides with <u>position in the real world G rather than position in m</u>. ('θ° to the right ...' is just an informal abbreviation of a predicate picking out the G trajectory of the particle p.) If no other objects or magnitudes are affected, this is the only change needed to eliminate the branch-point. By our hypothesis on the mathematical richness of m, v* is well-defined, since the function describing the real-world trajectory of p exists in m. By fiat, condition (i) of the claim is satisfied. Similarly for condition (ii): m|v under the new interpretation, v*, satisfies the sentences of T*, since $v^{*m} = v^G$ for all t, and G|v, is by hypothesis a model of T. On condition (iii), obviously $\langle G,m,t\rangle_{v^*}$ is not a branch-point. But we must be sure that the reinterpretation does not introduce any new branch points with other structures, m'. To deal with this, we must compare all structures in α with G as we did m, eliminating all branch-points $\langle G,m',t\rangle_v$ by re-interpretation as above. Now we observe that, since the reinterpretations force all structures to agree with G, any branch points of the form $\langle m,m',t'\rangle_{v^*}$ (t' > t) introduced by reinterpreting v over m can be rewritten as $\langle G,m',t'\rangle_v$, since by stipulation $v^{*m'} = v^{m'}$ for all t, and $v^{*m} = v^G$, also for all t. Thus, eliminating all branch points involving G will automatically take care of any introduced by reinterpretation. Proceeding in the same way for other structures fixed in the role of G, taking care not to alter previous reinterpretations, we eliminate all branch points in α.

We have been assuming that a single magnitude behaved indeterministically in an isolated way. This is of course highly unrealistic, since theories typically relate a set of magnitudes. In general, the above procedure would have to be carried out on each of the magnitudes in question, preserving interpretations on G while eliminating branch points by forcing agreement with G (or whatever structure is held fixed). Again the assumption of mathematical richness of the structures insures that the required functions exist in all structures. And by forcing agreement with G (or other fixed structures) on all the reinterpreted variables of the theory, it is guaranteed that the models and the sentences of T* are laws (in virtue of being satisfied by all m in α).

It may be remarked that this trivialization of determinism is itself a trivial consequence of the extensional criterion of magnitude representation together with the assumption of richness of the structures. It is. What follows is that this metaphysical approach to determinism rests critically upon some intensionalist account of magnitude representation. That is what one would expect; our claim and the details supporting it are merely designed to bolster that expectation in a somewhat precise way.

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Notes

[1]It is true that, by the well-known Löwenheim-Skolem theorem, there is a countable model of any consistent physical theory even if the theory includes all of mathematical analysis. The minimal model of Zermelo-Fraenkel set theory could serve as such a model. But this only shows that it is possible to interpret mathematical physics over a countable domain so that all its theorems come out true on the interpretation. It by no means shows that such an interpretation is intended. From the mathematical realist's standpoint, any such interpretation can be regarded as deviant, in that one is simply quantifying over too small a class of functions. In other words, on the realist's interpretation of mathematical-physics, it is not in the deviant sense in which the theory quantifies over uncountable sets.
Still, there is a difference in the case of random reals: new axioms asserting their existence do rule out the minimal model (and any others not containing any), whereas no new axioms (in a countable language) can rule out all countable models. Benioff's point might then be put in this way: if physics is construed as incorporating certain claims about random sequences, even certain otherwise consistent though deviant interpretations must be categorically rejected.

[2]The proof is analogous to that of ZF-definability of "partial satisfaction", i.e., of satisfaction in a transitive structure for the language of ZF with domain a set. See [18], pp. 66-68, Theorems 7.5, 7.10.

[3]The proof is as follows: by definition (in ZF) we have x is restricted-Benioff random iff x lies in every measure 1 Borel set B such that B is definable by a formula of ZF with restricted quantifiers, as indicated above. Let X denote the class of all such Borel sets. It suffices to show that $\bigcap X$ is non-empty. But this follows trivially from the fact that X is countable (which it is, since defining wffs form a countable set (the bounds on the quantifiers coming from a countable set of countable ordinals)), because a countable intersection of measure 1 sets is measure 1 (by the - additivity of the measure). Q.e.d. Since this proof can be carried out in ZF, it follows that every model of ZF (including M_o) must contain restricted-Benioff random reals.

[4]In essence, Benioff's "weakest allowable" criterion comes to this: require of random sequences (relative to theory T) membership in only those Borel sets of measure 1 which the theory in question (plus "interpretive rules") force you to require. As he formulated it, given a theory T plus rules R, require that a random sequence belong only to those measure 1 Borel sets B in a class D_p (P a fixed probability measure) such that for some (experimentally generable?) sequence $\psi \notin B$ which lies in all the other (measure 1) sets in D_p,"ψ can be used to derive a contradiction" in T plus R. ([3], §3) The only way I can make sense of this is to read it as saying that B is to be a required property of randomness only if a contradiction can be derived from T, R,

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and the sentence, "$\psi \notin B$", which fits with my interpretation.

Frankly, I am at a loss to see any real motivation for such a criterion, except perhaps that of relying on one's favorite theory. But even if this were regarded as sufficient, there are still serious problems with the approach. These emerge in his example ([3], §3) involving quantum mechanics. This example purportedly shows that the property of invariance of limit mean under observer-realizable subsequence selection is a necessary property of randomness for QM because, allegedly, a contradiction is derivable in QM from the assumption that a sequence generated by a QM process lacks this property. But how is such a contradiction derivable? Only if some additional strong assumption is made, such as, "Every sequence generated by a QM process has this invariance property." Nothing that I know of in current physical theory supports such an assumption. As far as we know, repeated measurements for a question observable on a system in a state exhibiting dispersion for the question can yield any sequence whatever. The most we can say is that almost every sequence (in the measure theoretic sense, which allows for uncountably many exceptions!) satisfies the condition. In his own argument, Benioff derived a contradiction; but he tacitly assumed that the theory-experiment connection is such that observation of a single sequence not satisfying the predicted limit mean allows the inference that either a different state was prepared or a different question-observable was tested. But, if I am right, we have no grounds for any such assumption.

In sum, for Benioff's criterion to work, we must be able to prove in our theory T plus interpretive rules that every sequence generated by a T-explained process have the property of randomness in question. But in the case of quantum mechanics, perhaps our most highly developed physical theory and the one that seems to involve statistical notions essentially, we have no basis for proving these kinds of theorems.

One might reply that the criterion should be thought of not in connection with QM but rather some more comprehensive extension of QM. This leads, however, to a curious predicament: for the very kind of extension that would enable the criterion to work--namely, one which predicted that all generated sequences have certain measure 1 properties--would, in all likelihood, no longer be an essentially statistical theory. The very need to talk about random sequences or outcomes (within the theory itself, as opposed to a general inductive logic) would have been overcome.

[5]A similar line of reasoning is suggested by L. Sklar, in [17], with regard to counterfactuals on limits of relative frequencies faced by proponents of a "propensity" interpretation of probability.

[6]There is an interesting technical problem here concerning whether model theoretic conditions for deterministic theories must be met by every pair of models of such theories, or whether it is sufficient to require that the conditions be satisfied by a relevant subset of the models. The problem arises because typically many of the models of a rich theory are non-standard even in ways concerning "structure"

(independent of the particular objects in the domain of the model), and may therefore be irrelevant to the intended interpretation of "determinism". That such a situation obtains with respect to notions of "determination" between levels of scientific theories has been explained in [9] where it is also shown how the irrelevance of non-standard interpretations leads to independence of determination principles from reductionist principles.

In the present case, restriction to "standard" models of deterministic theories results in further independence of determinism from theoretical predictability (of the behavior of the magnitudes in question), beyond what has already been noticed. (Cf.[4].) The main idea here is that, if all models of a theory are relevant, then Montague's conditions on models amount to implicit definability of future predicates in terms of past; by Beth's definability theorem this yields explicit definability, which is only a stone's throw from "predictability-in-principle". If some models are irrelevant, this argument breaks down.

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192463 Published online by Cambridge University Press

References

[1] Benioff, Paul A. "Models of Zermelo Frankel Set Theory as Carriers for the Mathematics of Physics, I." Journal of Mathematical Physics 17(1976): 618-628.

[2] ---------------. "Models of Zermelo Frankel Set Theory as Carriers for the Mathematics of Physics, II." Journal of Mathematical Physics 17(1976): 629-640.

[3] ---------------. "On the Correct Definition of Randomness." In PSA 1978, Volume 2. Edited by P.D. Asquith and I. Hacking. East Lansing, Michigan: Philosophy of Science Association, 1981. Pages 63-78.

[4] Boyd, Richard. "Determinism, Laws, and Predictability in Principle." Philosophy of Science 39(1972): 431-450.

[5] Bub, Jeffrey. "On the Completeness of Quantum Mechanics." In Contemporay Research in the Foundations and Philosophy of Quantum Theory. (University of Western Ontario Series in Philosophy of Science, Volume 2). Edited by C.A. Hooker. Dordrecht: Reidel, 1973. Pages 1-65.

[6] Chaitin, Gregory J. "Randomness and Mathematical Proof." Scientific American 232 #5(1975): 47-52.

[7] Earman, John. "Laplacian Determinism, or Is This Any Way to Run a Universe." Journal of Philosophy 68(1971): 729-744.

[8] Fine, Terrence L. Theories of Probability. New York: Academic Press, 1973.

[9] Hellman, Geoffrey, and Thompson, Frank W. "Physicalism: Ontology, Determination, and Reduction." Journal of Philosophy 72(1973): 551-564.

[10] Kochen, Simon, and Specker, E.P. "The Problem of Hidden Variables in Quantum Mechanics." Journal of Mathematics and Mechanics 17 (1967): 59-87.

[11] Kolmogrov, A.N. "Three Approaches to the Quantitative Definition of Information." Problemy Peredachi Informatsii 1(1965): 3-11.

[12] --------------. "On the Logical Foundations of Information Theory and Probability Theory." Problemy Peredachi Informatsii 5 (1969): 3-7.

[13] Kripke, Saul. "Naming and Necessity." In Semantics of Natural Language. Edited by G. Harman and D. Davidson. Dordrecht: Reidel, 1972. Pages 253-355.

[14] Martin-Löf, Per. "The Definition of Random Sequences."

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192463 Published online by Cambridge University Press

Information and Control 9(1966): 602-619.

[15] Montague, Richard. "Deterministic Theories." In Decisions, Values, and Groups, Volume 2. Edited by D. Willner. New York: Pergamon, 1962. Pages 325-370. (As reprinted in Formal Philosophy, (ed.) R. Thomason. New Haven: Yale University Press, 1974. Pages 303-359.)

[16] Putnam, Hilary. Philosophy of Logic. New York: Harper, 1971.

[17] Sklar, Lawrence. "Is Probability a Dispositional Property?" Journal of Philosophy 67(1970): 355-366.

[18] Takeuti, Gaisi and Zaring, Wilson M. Axiomatic Set Theory. New York: Springer, 1973.