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CHAPTER 22

CHANCE AND DETERMINISM

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22.1 Introduction

Determinism and chance seem to be irreconcilable opposites: either something is chancy or it is deterministic, but not both. Yet there are processes which appear to square the circle by being chancy and deterministic at once, and the appearance is backed by well-confirmed scientific theories, such as statistical mechanics, which also seem to provide us with chances for deterministic processes. Is this possible, and if so how? In this chapter I discuss this question for probabilities as they occur in the natural sciences, setting aside metaphysical questions in connection with free will, divine intervention and determinism in history.

The first step is to come to a clear formulation of the problem. To this end we introduce the basic notions in play in some detail, beginning with determinism. Let W be the class of all physically possible worlds. The world $w \in W$ is deterministic iff for *any* world $w' \in W$ it is the case that: if w and w' are in the same state at some time t_0 then they are in the same state at *all* times t (Earman 1986: p. 13). The world w is indeterministic if it is not deterministic. This definition can be restricted to a subsystem s of w. Consider the subset $W_s \subseteq W$ of all possible worlds which contain a counterpart of s, and let s' be the counterpart of s in w'. Then s is deterministic iff for *any* world $w' \in W_s$ it is the case that if s and s' are in the same state at some time t_0 then they are in the same state at *all* times t. This makes room for partial determinism because it is in principle possible that s is deterministic while (parts of) the rest of the world are indeterministic. The systems formulation of determinism will facilitate the discussion because standard examples of deterministic processes occur in relatively small systems rather than the world as a whole.

To introduce chance we first have to define probabilities. Consider a non-empty set Ω. An algebra on Ω is a set Σ of subsets of Ω so that $\Omega \in \Sigma$, $\sigma_i \backslash \sigma_j \in \Sigma$ for all $\sigma_i, \sigma_j \in \Sigma$, and $\cup_{i=1}^{n} \sigma_i \in \Sigma$ if all $\sigma_i \in \Sigma$.[1] A probability function p is a function $\Sigma \to [0, 1]$ which assigns every member of Σ a number in the unit interval $[0, 1]$ so that $p(\Omega) = 1$ and $p(\sigma_i \cup \sigma_j) = p(\sigma_i) + p(\sigma_j)$ for all $\sigma_i, \sigma_j \in \Omega$ for which $\sigma_i \cap \sigma_j = \emptyset$. The requirements that p be in the

[1] If n is finite, then Σ is just an algebra. If it is closed under countable unions, it is a *sigma algebra*.

unit interval, assign probability 1 to Ω, and satisfy the addition rule for non-overlapping sets are known as the *axioms of probability*.[2] Provided that $p(\sigma_j) > 0$, $p(\sigma_i | \sigma_j) = p(\sigma_i \cap \sigma_j) / p(\sigma_j)$ is the conditional probability of σ_i on σ_j. If we throw a normal die once Ω is the set $\{1, 2, 3, 4, 5, 6\}$, and Σ contains sets such as $\{3\}$ ('getting number 3'), $\{2, 4, 6\}$ ('getting an even number') and $\{5, 6\}$ ('getting a number larger than 4'). The usual probability function is $p(\{i\}) = 1/6$ for $i = 1, ..., 6$. The addition rule yields $p(\{2, 4, 6\}) = 1/2$ and $p(\{5, 6\}) = 1/3$. In what follows we will refer to the elements of Σ as *events*. This is a choice of convenience motivated by the fact that the sort of things to which we will attribute probabilities below are most naturally spoken of as 'events'.

An alternative yet equivalent approach formulates the axioms of probability in terms of propositions (or sentences). To every element of $\sigma \in \Sigma$ there corresponds a proposition $\pi[\sigma]$ which says that σ is the case. The second and third axioms of probability then say that $p(\pi[\Omega]) = 1$ where $\pi[\Omega]$ is a tautology and $p(\pi[\sigma_i] \vee \pi[\sigma_j]) = p(\pi[\sigma_i]) + p(\pi[\sigma_j])$ for all logically incompatible propositions $\pi[\sigma_i]$ and $\pi[\sigma_j]$, where '$\vee$' stands for 'or'.

An interpretation of probability specifies the meaning of probability statements. Interpretations fall into two groups: objective and subjective. Objective probabilities are rooted in objective features of the world. If, say, the objective probability of obtaining heads when flipping a coin is 0.5, then this is so because of facts in the world and not because of what certain agents believe about it or because of the evidence supporting such a claim. Objective probabilities contrast with subjective probabilities or *credences*. A credence is a degree of belief an agent has (or ought to have) in the occurrence of a certain event. We write *cr* to indicate that a certain probability is a credence.

The two most common kinds of objective probability are relative frequencies and chances. *Frequencies* are calculated in a sequence (finite or infinite) of events of the same kind and hence provide a statistical summary of the distribution of certain features in that sequence.[3] *Chances* are different from frequencies in that they apply to single cases in virtue of intrinsic properties of these cases. There is a 0.5 chance that the particular coin that I am going to flip now will land heads. But the fact that 10% of students at LSE get first-class marks does not warrant the claim that James has a 0.1 chance of getting a first-class mark for his next essay. Frequncies can be both the manifestation of, and evidence for, chances, but they are not themselves chances. To indicate that a certain objective probability *p* is a chance we write *ch*.

Let us introduce a few notational conventions. In what follows we often speak about events such as getting heads when flipping a coin. If we speak about an event informally I use 'e' rather than 'σ' to keep notation intuitive, and I take 'E' to be the outcome-specifying proposition saying that e obtains. It has become customary to attribute chances to propositions. I will follow this convention and write $ch(E)$ for the chance that e occurs. Likewise $cr(E)$ refers to the degree of belief of an agent that e occurs.

A chance function is nontrivial if there are events for which it assumes non-extremal values, i.e. values different from zero or one. There is widespread consensus that nontrivial chances are incompatible with determinism. In an often-quoted passage Popper (1982: p. 105) states that

[2] As is well known, there are different axiomatizations of probability. Nothing in what follows depends on which axioms we choose (in particular, nothing depends on whether probabilities are assumed to be finitely additive or countably additive). For a discussion of alternative axiomatizations see Lyon's chapter in this volume (Lyon 2016).

[3] For a discussion of frequentism see Hájek (1997) and La Caze's chapter in this volume (La Caze 2016).

> objective physical probabilities are incompatible with determinism; and if classical physics is deterministic, it must be incompatible with an objective interpretation of classical statistical mechanics,

and Lewis (1986: p. 118, original emphasis) exclaims that

> [t]o the question how chance can be reconciled with determinism […] my answer is: *it can't be done*.

Let us refer to this view as incompatibilism; conversely, compatibilism holds that there can be nontrivial chances in deterministic systems.

Incompatibilism is often asserted and seems to enjoy the status of an obvious truism, which is why one usually finds little argument for the position (we turn to exceptions below). Since incompatibilism undoubtedly has intuitive appeal, there is a temptation to simply leave it at that. Unfortunately things are more involved. In fact, there is a tension between incompatibilism and the fact that common sense as well as scientific theories assign probabilities to deterministic events. We assign nontrivial probabilities to the outcomes of gambling devices such as coins, roulette wheels, and dice even though we know that these devices are governed by the deterministic laws of Newtonian mechanics, and statistical mechanics attributes nontrivial probabilities to certain physical processes to occur even though the underlying mechanics is deterministic.[4]

A conflict with incompatibilism can be avoided if the probabilities occurring in deterministic theories are interpreted as credences rather than chances. On that view, the probabilities we attach to the outcomes of roulette wheels and the like codify our ignorance about the situation and do not describe the physical properties of the system itself. Outcomes are determined and there is nothing chancy about them; we are just in an epistemic situation that does not give us access to the relevant information.

This is unsatisfactory. There are fixed probabilities for certain events to occur, which are subjected to experimental test. The correctness of the probabilistic predictions of statistical mechanics has been assessed in countless laboratory experiments, and the owners of casinos make sure that their roulette wheels are unbiased to avoid loss when faced with attentive punters. These observations are difficult to square with a view that interprets these probabilities as credences. The chance of a roulette wheel stopping at slot No. 23 seems to have nothing to do with what we know about it, let alone with the existence of belief-forming creatures. The values of these probabilities seem to be determined by how things are and not by what anybody believes about them.[5]

[4] Let me add two caveats. First, in general, Newtonian mechanics need not be deterministic (Norton 2008). However, in the applications we are concerned with in this essay (gambling devices and statistical mechanics) the resulting laws are deterministic. Secondly, we now believe that Newtonian mechanics is just an approximation and the true underlying theory of the world is quantum mechanics, which is indeterministic (according to the standard interpretation). We can set aside the question of whether or not the true fundamental theory of the world is deterministic. What matters for the current discussion is the conceptual observation that probabilities are assigned to events in a deterministic context, and the issue is how to understand such probabilities – whether probabilities of real roulette wheels are of that kind is a different matter.

[5] This point is often made in the context of statistical mechanics; for instance see Redhead (1995: pp. 27–8). Lyon (2011) extends arguments of this kind to the probabilities we find in biology.

This leaves us with a dilemma: either we deny, the above points about empirical testing notwithstanding, that the probabilities in deterministic theories such as statistical mechanics are chances, or we reconsider incompatibilism. This chapter is about the second horn of the dilemma. In doing so I restrict attention to broadly Humean approaches to chance and set aside propensity interpretations.[6] These include various versions of what has become known as Humean Objective Chance (Sections 22.2 and 22.3) and the so-called method of arbitrary functions (Section 22.4). Throughout I will use the example of a coin, which can land either heads (H) or tails (T). This is for two reasons. First, the example is intuitively easy to grasp and yet sufficiently complex to make all the essential points. Secondly, the arguments developed with this example can be carried over without difficulties to more complicated cases, in particular in statistical mechanics.

22.2 Humean Objective Chance

The interpretation of probability now known as Humean Objective Chance (HOC) originates in the work of Lewis (1980, 1986, 1994),[7] and the entire approach in which HOCs occur is known as the Humean Best Systems (HBS) approach. Consider an outcome-specifying proposition E. The following definition then encapsulates the core idea of HOC:

The HOC of event e occurring at a particular time t in world w, $ch_{tw}(E)$, is a number in the interval [0, 1] such that

(1) $ch_{tw}(E)$ satisfy the axioms of probability;
(2) $ch_{tw}(E)$ supervene on the Humean Mosaic in the right way;
(3) $ch_{tw}(E)$ be the correct plug-in for X in the Principal Principle.

The first clause is a necessary condition for HOCs to be an interpretation of probability.[8] The second clause is more involved. The *Humean Mosaic* (HM) is the collection of everything that actually happens; that is, all occurrent facts everywhere and at all times. There is a question about how exactly occurrent facts ought to be characterized; the important point for now is that irreducible modalities, powers, propensities, necessary connections, and so forth are not part of HM. That is the 'Humean' in HOC.

Supervenience requires that chances be entailed by the overall pattern of events and processes in HM. HOCs supervene on HM, but unlike actual frequencies they don't supervene *simply*. The problem with simple supervenience is that it makes no room for

[6] For discussion of the compatibility of propensities with determinism see Clark (2001) and Berkovitz (2015). Alternative routes are explored in Eagle (2011), Ismael (2009), and Clark and Butterfield (1987).

[7] 'Humean Objective Chance' could be deemed bad terminology because chances by definition are objective. I use the term because it has become customary to refer to the kinds of chances introduced in this section as HOCs (witness the title of Lewis' 1980 paper!). For a general introduction to Best Systems approaches see Schwarz's chapter in this volume (Schwartz 2016).

[8] The presentation of HOC in this section follows see Frigg and Hoefer (2010). I set aside problems that are tangential to the issue of compatibilism. Among these are the temporality of chance, the justification of the Principal Principle, the alleged commitment of HOC to classical physics, and undermining. For a discussion of these see Hoefer (2007), Pettigrew (2012), Darby (2012), and Roberts (2001), respectively.

frequency tolerance. Imagine that in the history of the universe only one roulette wheel with 83 slots has ever been built and that it has been destroyed after having been spun only three times. The outcomes of these three spins were 25, 39, and 25. Actual frequentism commits us to saying that the chance for 25 is 2/3, the chance for 39 is 1/3, and all other chances are 0. This conclusion can be avoided if we see chances as supervening on facts not simply but instead in a Human Best System (HBS) way: chances are the numbers assigned to events by probability functions that are part of a *best system* (BS), where 'best' means that the system offers the best balance of *simplicity, strength* and *fit* that the events in HM allow.

Simplicity and strength are notoriously difficult to explicate.[9] It is sufficient for now to go with intuitive notions of simplicity and strength; we will return to the issue below in the context of the discussion of compatibilism. Fit is more straightforward. Every system assigns probabilities to possible courses of history. The fit of the system is measured by the probability that it assigns to the *actual* course of history. The more likely a system sees the actual course of history as, the better its fit. As an illustration, consider an HM that consists of just ten outcomes of a coin flip: $HHTHTTHHTT$. A system positing $ch(H) = ch(T) = 0.5$ has better fit than one that says $ch(H) = 0.1$ and $ch(T) = 0.9$ because $0.1^5 0.9^5 < 0.5^{10}$. This example also shows that a system has better fit when it stays close to actual frequencies, as we would intuitively expect.[10]

The motivation behind the Principal Principle (PP) is that chances are guides to action, and the PP establishes a connection between chances and the credences a rational agent should assign to certain events. In a nutshell the PP says that a rational agent who knows the chance of E should have credence in E that is equal to the chance of E as long as the agent has no inadmissible knowledge relating to E's truth. Let 'cr' be an initial credence function.[11] In formal terms, PP is the rule that

$$cr(E|X\&K) = x \tag{1}$$

where X is the proposition that the chance of E takes the value x at time t in world w (i.e. X says '$ch_{tw}(E) = x$') and K is the agent's total evidence pertaining to E, which must not contain inadmissible elements.

The power of the PP depends on what qualifies as admissible evidence.[12] Intuitively, a proposition is inadmissible if it 'bypasses' X and provides information about E other than the information already contained in X. The most obvious case of such a proposition is E itself. If, for some reason (maybe because you have a reliable crystal ball), you know that E is true, then knowing the truth of E trumps any chance law about E and a rational agent's credence in E should be 1 no matter what the chance of E. Lewis (1986: p. 92) does not provide a definition of admissibility, but he offers a characterization: 'Admissible

[9] See Lewis (1994: pp. 478–80) for a forceful reply to this problem.

[10] This explication of fit has certain limitations. It is readily applicable only to HM's with a finite number of discrete chance events. Since events we consider in what follows are of this kind this limitation need not concern us. A generalization of the above definition to infinite sequences has been suggested by Elga (2004).

[11] See Lewis (1986: pp. 87–9). The credence function is 'initial' in the sense that it is the credence function of a hypothetical actor who is able to evaluate conditional probabilities but otherwise knows absolutely nothing – in Alan Hájek's words, it is the credence function of 'super-baby'.

[12] The correct formulation of the PP and the characterization of admissibility are fraught with controversy. I follow Hoefer (2007) in preferring Lewis' original formulation (given here). For a compact account of the various moves in the debate see Vranas (2004).

propositions are the sort of information whose impact on credence about outcomes comes entirely by way of credence about the chances of those outcomes.' Within the class of admissible propositions two kinds of propositions are of particular significance. The first is historical information. If a proposition is entirely about past matters of fact, then it is admissible. Boolean combinations of such statements are admissible too, and so it follows that at any given time t, H_{tw}, the entire history of world w up to time t, is admissible. The second kind is statements of laws of nature. As with historical propositions, Boolean combinations of laws are admissible too. L_w, the conjunction of all laws of nature in w, is therefore admissible.

22.3 Incompatibilism Scrutinized

We will now see that one's stand on compatibilism depends on how the details of the above are fleshed out. Recall from the introduction, Lewis (1986: p. 120) was an advocate of incompatibilism, but failed to provide an argument for his position and instead merely asserted the point: 'There is no chance without chance. If our world is deterministic there are no chances in it, save chances of zero and one.' Several authors have tried to fill this gap. Hoefer (2007: pp. 558–9) and Schaffer (2007: p. 128) provide the following reconstruction of the incompatibilist's argument. Let $ch_{tw}(E) = x$ be the nontrivial chance of E in world w at time t. As we have seen in the last section, Lewis regards historical facts and laws as admissible. The PP then tells us that

$$cr(E|\, ch_{tw}(E) = x \,\&\, H_{tw} \,\&\, L_w) = x$$

Assume that E is true.[13] If w is deterministic, then H_{tw} and L_{tw} logically imply E. Hence the axioms of probability dictate that

$$cr(E|\, ch_{tw}(E) = x \,\&\, H_{tw} \,\&\, L_w) = 1$$

So under determinism credences should be 0 and 1, but this is in contradiction with the PP.

If we stick with Lewis' view that H_{tw} and L_{tw} are admissible, then the only way to avoid the contradiction is to deny that there is a nontrivial chance for E. Recently Schaffer (2007) has provided further reasons for thinking that this is the right response. He formulates six platitudes about chance and then argues that these platitudes cannot hold true in a deterministic setting. His platitudes are the Principal Principle, the Basic Chance Principle, the Lawful Magnitude Principle, the Intrinsicness Requirement, the Future Principle, and the Causal Transition Constraint. We here focus on the first four.[14]

The first of Schaffer's platitudes is the PP itself, which, as we have just seen, leads to a contradiction when applied in deterministic setting. Since he grants the PP the status of a platitude, the PP itself cannot be given up, which leads us to the conclusion that only trivial chances are compatible with determinism.

[13] This can be done without loss of generality. The argument is *mutatis mutandis* the same if 'not E' rather than E is true.

[14] We set aside the last two because, contra Schaffer, the Future Principle and the Causal Transition Constraint are not platitudes about chance. See Hoefer (2007: pp. 554–5) for a discussion of the former; Glynn (2010: pp. 75–6) argues against the latter.

The *Basic Chance Principle*, originally due to Bigelow, Collins, and Pargetter (1993), asserts that if at time t there is a nontrivial objective chance for E in world w, then there is a possible world with the same history as w up to t and in which E is true. Schaffer proposes a stronger version of this principle, his *Realization Principle*, which adds the requirement that the possible world in which E is true must have the same laws as w. If $ch_{tw}(E)$ is nontrivial, then both $ch_{tw}(E)$ and $ch_{tw}(\neg E)$ are strictly between 0 and 1 ('$\neg$' stands for 'not'), which implies that there is at least one possible world with the same history and laws as our world in which E is true, and likewise there is at least one such possible world in which $\neg E$ is true. This, however, is precisely what determinism denies. Hence, nontrivial chances are incompatible with determinism.

The *Lawful Magnitude Principle* codifies the view that chance values should fit with the values given by the laws of nature. If there is a chance for the coin to land heads, then this chance has to follow from the laws of nature. But if the laws are deterministic, they cannot imply nontrivial probabilities ('no probability in, no probability out').

The *Intrinsicness Requirement* says that if you have physically identical setup conditions at two different times, then the chances of their corresponding possible outcomes must be the same. This platitude is violated in a deterministic world because we only have the same setup conditions if the system's initial state is the same, but by determinism same initial conditions lead to same outcomes, which rules out nontrivial chances.

This, thinks Schaffer, seals the case against compatibilism. To see how one could counter this argument it is instructive to notice in what way exactly determinism and chance are at loggerheads. The conflict is not one of *simple* inconsistency: there are no chance laws covering exactly the same events as deterministic laws. The conflict arises if we accept reductive relations. There is a chance law for coins, and there is a deterministic mechanical theory for elementary particles. These two are inconsistent once we assume that coins are made up of atoms and that the behaviour of the coin is therefore determined by the behaviour of the atoms.

The above arguments for incompatibilism are based on giving primacy to fundamental laws wherever they are in conflict with (purported) non-fundamental laws (such as chance laws for gambling devices). This reaction is closely tied to Lewis' metaphysics, which sees the world as consisting of a manifold of spacetime points which instantiate perfectly natural monadic properties. And this is all there is: '"how things are" is fully given by the fundamental, perfectly natural, properties and relations those things instantiate' (Lewis 1994: p. 474). What counts as a perfectly natural property is determined by physics, which 'is a comprehensive theory of the world, complete as well as correct. The world is as physics says it is, and there's no more to say' (Lewis 1999: pp. 33–4). Even though Lewis rarely says so explicitly, the emphasis is clearly on *fundamental* physics: the world is how fundamental physics says it is. Let us call this position *Lewisian physicalism*.

On the basis of this metaphysics, denying that there are chances if the fundamental laws of physics are deterministic is a stringent move. If E is about a perfectly natural property, then by assumption there cannot be a chance law for it. If, by contrast, E is about a property that is not perfectly natural (such as *coin* and *roulette wheel*), then a best system will not contain laws for E-type events (and a fortiori no chance laws) because a best system does not say anything about E-type events at all.

This austere elegance comes at a price: there are no laws about any non-fundamental kinds. Where we *seem* to have such laws, this is an illusion. Generalizations that look like laws are in fact mere rules of thumb for feeble beings incapable of applying fundamental laws to complex situations.

A number of authors felt that this was too high a price to pay. A view of chance that denies the status of chance not only to probabilities attached to gambling devices, but also to the probabilities we find in macrophysics, genetics, engineering, meteorology, and many other non-fundamental sciences, has thrown out the baby with the bathwater. These probabilities codify information about the world and are subject to empirical test. This, so the thought continues, indicates that they are chances.

Those intending to pursue this line of argument have to provide a reformulation of the HBS approach, that departs from Lewis' formulation in at least two respects. First, they have to argue that non-fundamental laws are part of the best system. Secondly, they have to reformulate PP so that the above contradiction no longer arises. Different approaches differ in how they achieve these goals.

The first to present such a view was Loewer (2001, 2004). His account focuses on Bolzmannian statistical mechanics (BSM) as presented by Albert (2000): the package of Newtonian mechanics, the Past Hypothesis, and the Statistical Postulate.[15] In this approach the relevant system is considered to be the entire universe. Very roughly, the Past Hypothesis says that the universe started in a low-entropy state, which is associated with a certain small part Γ_p of the world's entire phase space. Newtonian Mechanics provides the time evolution φ_t, specifying how a point x of the universe's phase space evolves over time. The Statistical Postulate says that we should assume a uniform probability distribution over Γ_p at time t_0, the time of the Big Bang, and generate probabilities for the system's state being in any other part of the universe's phase space at any later time $t > t_0$ by conditionalizing on the past hypothesis and the system's dynamics.[16] Loewer submits that this package is a best system (2001: p. 618; 2004: p. 1124). His reasons for thinking so are that adding the Past Hypothesis and the Statistical Postulate to Newtonian Mechanics results in a powerful system:

> It is simple and it is enormously informative. In particular, it entails probabilistic versions of all the principles of thermodynamics. That it qualifies as a best system for a world like ours is very plausible.
>
> (Loewer 2001: p. 618)

The probabilities generated by the theory's Statistical Postulate therefore are chances. He calls them 'macro chances', indicating that a revision of the PP is needed (Loewer 2001: p. 618). The modified principle, 'PP(macro)', differs from the PP in that it posits that '[m]ore detailed information about the micro-condition at *t* than that given by the macro-condition at *t* is macroscopically inadmissible' (Loewer 2001: pp. 618–19). Regimenting admissible information successfully blocks the above contradiction, and hence removes the incentive to deny the existence of chances in a deterministic world (we will return to Schaffer's platitudes below).

There are a number of challenges for Loewer's approach, which are all rooted in the fact that the work in Loewer's approach is done entirely by an appeal to simplicity. The first

[15] Loewer also includes Bohmean quantum mechanics in his discussion. For want of space I discuss only statistical mechanics here, but nothing is lost since (as Loewer points out) the arguments are entirely parallel for the two cases.

[16] For an extensive discussion of statistical mechanics see Frigg (2008), Uffink (2006), and Myrvold's chapter in this volume (Myrvold 2016).

challenge is to justify why one would introduce macro chance laws at all. In BSM macrostates supervene on microstates and so a Lewisian physicalist may insist that BSM macrostates are as dispensable in the best system of the world as coins, roulette wheels, genes, steel fatigue, cloud albedo, or any other not perfectly natural property. Loewer's motivation for introducing macrostates is that describing a tremendously complex swarm of molecules as a gas having a certain macrostate simplifies things enormously and hence adding that bit of conceptual machinery to fundamental mechanics greatly simplifies the entire system. But this argument implicitly invokes a notion of simplicity that has a computational component built into it. In principle the behaviour of the gas is completely determined by the behaviour of its constituent molecules. But it would be tremendously complicated to make predictions about the gas in terms of molecules, and having the conceptual tool of macrostates at hand allows us to say much of what we actually want to say about swarms of molecules at relatively low cost.

This argument only gets off the ground if the computational cost incurred in deriving a desired result is at least part of what we mean by simplicity; Frigg and Hoefer (2015) call this 'simplicity in derivation'. This is not a notion of simplicity a Lewisian physicalist finds natuarally appealing. One would either have to show that a Lewisian physicalist should accept such a notion or else argue that the position ought to be rejected altogether. Whichever of these options one choses, the further question then is: why stop short at BSM? The probabilistic laws of genetics seem to bring equal computational simplifications with them as the laws of BSM, so why not include these in the best system too? And so on for many laws in the special sciences.

A related worry is that postulating a uniform distribution over Γ_p for an event that happens only once in the entire history of the universe, namely the Big Bang, seems conceptually problematic even if one takes frequency tolerance seriously. It is difficult to see how such a distribution could be seen as supervening on HM, and the only reason to accept it at all is its simplifying power. This is an uncomfortable move as long as we operate with an unexplained notion of simplicity.

Even if we were prepared to set these worries aside, there is another problem lurking. One can show that the fit of a system can be improved by choosing a distribution that is peaked over the actual initial condition (Frigg 2008, 2010). A peaked distribution is not less simple than a uniform one (or at any rate only marginally less simple) while it greatly increases the fit of the system, and so the best system would contain a peaked rather than a uniform distribution. Countering this objection would involve arguing that peaked distributions come at simplicity costs that outweigh all gains in fit, which tells against their inclusion in a best system. But it is hard to see how such an argument could be made as long as no tight notion of simplicity is in place.

Alternative compatibilist accounts have recently been proposed by Glynn (2010) and Frigg and Hoefer (2010, 2015). These accounts have in common that they regard chances as situated at particular levels (for instance the level of genes), and they endorse a thoroughgoing pluralism which allows for chances to occur (at least in principle) at every level (and not only at the level of BSM). They differ in how they justify and develop their position. Glynn (2010: p. 57) posits that 'there exist probabilistic high-level or special scientific laws even in such worlds [i.e. fundamentally deterministic worlds]' and points out that '[t]he probabilities projected by these laws should be regarded as genuine, objective chances because the laws in question are genuine, objective laws'. His reason for regarding special science laws as genuine laws are similar to Fodor's (1974) reasons to support the autonomy of the special sciences: higher-level properties are typically multiply realizable at the macro level, and therefore laws about higher-level kinds cannot follow from micro properties alone. This is because the micro theory by itself does not tell us which micro kinds fall into the class of realizers for a certain macro property, and hence is unable to ground generalizations about macro kinds. Therefore, adding special science laws to a system of laws makes that system more informative and stronger, and the probabilities in these laws are chances in Lewis' sense (Glynn 2010: pp. 58–9). The above contradiction (between determinism and nontrivial chance) is avoided by stipulating that a complete conjunction of all laws together with information about facts at more fundamental levels is inadmissible (Glynn 2010: pp. 68–70).

Frigg and Hoefer (2015) extend this line of argument in two ways. First they provide a more extensive argument for the conclusion that non-fundamental laws are part of a best system. The first step of their argument draws a parallel between the issue of compatibilism and the philosophy of mind. They then argue that Lewisian physicalism is untenable for reasons similar to those put forward against eliminativism. Rejecting eliminativism makes room, at least in principle, for there to be laws formulated in non-fundamental terms. To show that at least some laws of that kind are also part of the best system, they distinguish between numerical simplicity (measured in terms of the number of different laws a system contains), simplicity in derivation (roughly, the computational costs incurred in deriving a desired result), and simplicity of formulation (roughly, the ease with which a law can be formulated). They argue that the gain in simplicity in derivation due to the introduction of non-fundamental laws far outweighs the costs in numerical simplicity, while the scope and the fit of the system remain constant. For this reason such laws should be included in a best system.

The second amendment is a requirement of coherence: whenever two parts of the HBS have the same (or sufficiently overlapping) domains of application, then there must be a Humean account of how their prescriptions relate to one other. In the case where a chance law covers events that are also covered by deterministic laws, the less fundamental law must supervene in a Humean best systems way on the facts in the domain of the more fundamental law. The requirement is best illustrated with the above example of SM. Frigg and Hoefer take laboratory systems as the subject matter (rather than the entire universe). Throughout HM there are many copies of every system and so one can look at the distribution of initial conditions over Γ_p of these systems. The postulate that the best system ought to contain a uniform distribution over Γ_p then has to be justified by arguing that such a distribution is in fact the best summary of the distribution of actual initial conditions. If the conditions are spread out more or less evenly, this arguably is the case. But if it is the case that all points are concentrated in one corner of Γ_p, then a uniform distribution is not the right one to choose.

This addresses the worries that arose in connection with Loewer's account. It remains to be shown that Schaffer's platitudes can be dealt with successfully. The first one has been dealt with by altering our understanding of admissibility (which, compatibilists insist, does not require changing Lewis' characterization of admissibility; rather it requires making proper use of that characterization). Consistency with the realization principle is restored by reformulating the principle so that the scope of the principle is restricted to histories *at the relevant level*: the 'same history up to *t*' refers to the *admissible* history. Intrinsicness is dealt with along the same lines: restrict sameness of the setup to sameness with regard to admissible properties. The Lawful Magnitude Principle turns out not to be a platitude at all; in fact it is a statement of physicalism in disguise. It is the whole point of the approaches of Loewer, Glynn, and Frigg and Hoefer that not all chances are deductive consequences of fundamental laws.

Incompatibilists remain unconvinced. But rather than quibbling about the particulars of any of the above moves, they argue that the probabilities thus introduced simply aren't chances after all. In this vein Lyon (2011) argues that the chances we find in BSM (and, needless to say, theories of gambling devices) are not chances. His reasons are that he chooses to 'stick with the usage of "chance"' that Lewis, Schaffer, and others prefer. This is because once we lay out all the platitudes we seem to have about chance, it appears that indeterministic conceptions of chance satisfy these platitudes better than deterministic ones can' (Lyon 2011: p. 420). Lyon (2011: p. 429) is quick to point out that this does not turn BSM probabilities into credences and argues that probabilities like these are of a third kind which is neither chance nor credence and which he calls *counterfactual probability*.

We have reached an impasse. If one thinks that 'chance' *means* something like propensity, primitive fundamentally chancy laws of nature, or other kinds of fundamental modalities, then one will follow Lewis and dismiss the compatibilist's probabilities as a 'kind of counterfeit chance', which is 'quite unlike genuine chance' (Lewis 1986: p. 120). If, on the other hand, one is not committed to such a view, then compatibilism is a live option and the above proposals deserve consideration. There is no ultimate right and wrong in the use of a word, and depending on one's other philosophical commitments one *can* go either way. If nothing else, the above discussion has at least shown where compatibilists and incompatibilists part ways.

22.4 The Method of Arbitrary Functions

Let us now briefly turn to an approach that is broadly Humean (in that it does not appeal to propensities, powers, and the like) but does not stand in the Lewisian best systems tradition: the method of arbitrary functions (MAF). The method was introduced by Poincaré in 1896 and has subsequently been developed by a number of eminent mathematicians, among them Borel, Fréchet, Hopf, and Khinchin.[17] Recently Strevens (2011) and Myrvold (2011) have, in different ways, appealed to the method to make sense of objective probabilities in physics.

MAF is a mathematical technique to determine a unique probability distribution for the outcomes of mechanical games of chance, or the evolution of deterministic mechanical systems more generally. A discussion of the entire theory is beyond the scope of this chapter; we restrict ourselves to illustrating the main ideas with the example of the coin. The mechanical state of a coin can be described by two sets of variables: the angle at which the coin stands with respect to the ground and the angular velocity ω (how fast the coin rotates), and its height above ground and the vertical velocity v (how fast it is thrown upwards when tossed). Assuming the coin is tossed vertically and gravity is the only force acting on it, one can classify the initial conditions according to the outcomes they will produce. If the coin is always tossed at the same height and with the same initial angle, variations in ω and v alone determine the outcome completely (because the movement of a coin is deterministic). Keller (1986) has done the calculations and the result is the graph shown in Figure 22.1, where black initial conditions result in tails and white ones in heads.[18]

[17] Von Plato (1983) provides a historical introduction to the method.
[18] The graph is a reproduction from Diaconis (1998: p. 803).

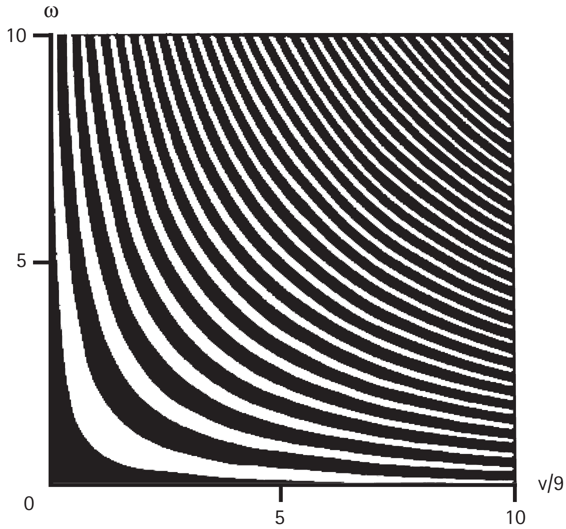

FIGURE 22.1 Black initial conditions result in tails; white ones in heads.

Assuming a probability distribution $\rho(v, \omega)$ over initial conditions, the probability for heads is just the probability of the initial conditions resulting in heads, which can be calculated by integrating $\rho(v, \omega)$ over the white regions (and *mutatis mutandis* for tails). MAF comes into play when we ask how the result of these calculations depends on our choice of $\rho(v, \omega)$. The basic result MAF seeks to establish is that for 'reasonable' $\rho(v, \omega)$ the result does not depend on the precise shape of $\rho(v, \omega)$; that is, the end result is the same for all $\rho(v, \omega)$ unless we start with a completely unreasonable $\rho(v, \omega)$ (we expect to retrieve the usual rule saying that the chance for either heads or tails is 0.5). The crucial question is of course what counts as reasonable, and the answer depends (as one would expect) on the particulars of the situation. Poincaré's original example was a roulette wheel and he argued that we obtain the usual probabilities as long as the initial probability distribution does not fluctuate wildly (in technical terms: the modulo of the derivative of the distribution has to be bounded). This approach will work here too. As we see in Figure 22.1, the black and white lines are relatively fine, and they become finer as we move towards higher ω and v; unless $\rho(v, \omega)$ fluctuates strongly on a scale of the width of the stripes, the result of the integration will not depend much on the concrete shape of $\rho(v, \omega)$. An arbitrary $\rho(v, \omega)$ will do to determine the probabilities for heads or tails – this insight gave the method its name.

In sum, MAF shows that an (almost) arbitrary distribution yields the same outcome probabilities under a deterministic physical dynamics, which seems to provide a reconciliation of determinism and chance. This enthusiasm is premature. MAF does not create probabilities *ex nihilo*; MAF only shows that outcome probabilities are invariant under the variation of *given* input probabilities. For this reason MAF per se does not ground a particular interpretation of probability, let alone provide us with a notion of deterministic chance. Whether or not MAF assists a reconciliation of chance and determinism depends on where probability distributions over initial conditions come from.

Savage (1973) interprets both input and output probabilities as credences and hence denies that MAF plays any role in reconciling chance and determinism. Myrvold (2011: p. 76) follows Savage in interpreting input probabilities as credences, but sees outcome probabilities as *epistemic chances*. This choice of terminology indicates that outcome probabilities are determined both by epistemic and physical considerations, namely the initial credence function and the dynamics of the system. Since both are essential and irreducible, the resulting conception of probability is a combination of epistemic and physical factors. The chance aspect of these probabilities is highlighted by the fact that they are taken to satisfy a principle that is structurally similar to the PP.

There is a question, though, whether the chance aspect of Myrvold's epistemic chances is strong enough to ground a reconciliation of chance and determinism. A sceptic could argue that what MAF shows is that all reasonable initial credence functions converge towards the same credence function under the system's dynamics. This shows that all reasonable agents must have the same outcome probabilities, but it does not show that the outcome probabilities are anything other than credences: credences in, credences out. Outcome probabilities are physically constrained credences, but credences nevertheless.

Strevens takes a different line and aims to interpret MAF probabilities as physical probabilities without an epistemic component. He interprets the initial distribution $\rho(v, \omega)$ as expressing properties of the frequencies of initial conditions. Such frequencies are facts about physical world, but Strevens (2011: p. 350) emphasizes that these need not be *probabilistic* facts. Nothing forces us to interpret frequencies as probabilities; and given all the well-known difficulties of frequentism, interpreting frequencies as probabilities is best avoided. But this does not prevent us from regarding the outcome distribution as a probability distribution. So the crucial move in Strevens' account is to regard only the outcome of a process covered by MAF as probabilitiy. Strevens (2011: p. 351) calls these probabilities 'microconstant probabilities'. They are physical probabilities in that they are determined solely by facts about the frequency of initial conditions and properties of the system's dynamics.

Even though Strevens' account seems to come close to a reconciliation of chance and determinism, it is not clear whether we have passed the finishing line. Strevens never refers to microconstant probabilities as chances; nor does he ever discuss the relation between microconstant probabilities and chances. It is therefore unclear whether his account advances the compatibilist's cause. However, not much seems to be needed to turn microconstant probabilities into Humean chances (as defined by compatibilists). Borrowing from the best systems account the idea of coherence one could say that $\rho(v, \omega)$ should be chosen such that it provides the best summary of actual initial conditions, and MAF then shows that outcome probabilities do not depend sensitively on our standards of simplicity (which will determine which function we fit to the actual points). Once $\rho(v, \omega)$ is understood in this way, MAF probabilities can at least in principle be understood as Humean chances in the compatibilists' sense, which would justify their status as chances.

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