Deterministic Chance

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ABSTRACT

I argue that there are non-trivial objective chances (that is, objective chances other than 0 and 1) even in deterministic worlds. The argument is straightforward. I observe that there are probabilistic special scientific laws even in deterministic worlds. These laws project non-trivial probabilities for the events that they concern. And these probabilities play the chance role and so should be regarded as chances as opposed, for example, to epistemic probabilities or credences.

The supposition of non-trivial deterministic chances might seem to land us in contradiction. The fundamental laws of deterministic worlds project trivial probabilities for the very same events that are assigned non-trivial probabilities by the special scientific laws. I argue that any appearance of tension is dissolved by recognition of the level-relativity of chances. There is therefore no obstacle to accepting non-trivial chance-role-playing deterministic probabilities as genuine chances.

1 Introduction

It has often been assumed without argument that determinism is incompatible with non-trivial objective chance. Popper ([1982], p. 105), for example, simply says:

> [O]bjective physical probabilities are incompatible with determinism; [...]

In a similar vein Lewis ([1986c], p. 118) states that:

> If the chance [of a fair coin landing heads] is zero or one, [...] then it cannot also be 50%. To the question how chance can be reconciled with determinism, or to the question how disparate chances can be reconciled with one another, my answer is: *it can't be done*.

Some argument for such claims is needed. Deterministic physical theories such as Classical Statistical Mechanics (CSM) yield non-trivial probabilities that are used by physicists in prediction and explanation. Both the manner of their genesis and the nature of their employment make these probabilities good candidates to be considered objective chances.[1]

But Schaffer ([2007]) has recently argued that they are *not* chances. His argument is the most sophisticated and well-developed such argument currently to be found in the literature. He argues that although deterministic probabilities such as those generated by CSM may be formally eligible to count as chances–they are generated by a function from propositions, times and worlds onto the closed unit interval in accordance with the axioms of the probability calculus–they do not qualify as chances because they fail to play the *chance role*. Generating values that play the chance role is what distinguishes the chance function from the many other formally eligible functions that no-one would regard as the chance function (Schaffer, *op. cit.*, p. 123).

Schaffer claims that the role of chance is characterised by its connections with '*credence, possibility, futurity, intrinsicness, lawhood,* and *causation*' (ibid.). He argues that, in a deterministic world, a chance function outputting non-trivial values would violate three of these connections: namely, those from chance to *credence*, *possibility* and *lawhood* (ibid., p. 132).[2] By contrast, a function that outputs just trivial values in deterministic worlds plays the chance role perfectly, and so *this* should be regarded as the chance function (ibid. p. 127). Schaffer's conclusion is that there are therefore no non-trivial deterministic chances. His argument shall be outlined in §2.

In §§3-4 I shall seek to show that Schaffer is wrong: there *are* non-trivial deterministic probability functions that underwrite the connections from chance to credence, possibility and lawhood, and that therefore play the chance role. Important to the demonstration that this is so is the demonstration that chance is *level*-relative (this notion of level-relativity is explicated in §4). Recognition of the level-relativity of chances allows us to see the flaws in Schaffer's arguments.[3]

Not only are there non-trivial deterministic probability functions that play the chance role, some such functions play it better than any *trivial* deterministic probability function. In §4.1 and §4.2 I argue that, for at least some deterministic worlds, *only* a function that outputs non-trivial values can underwrite the connections from chance to lawhood and credence.

I seek to reinforce the case for non-trivial objective chances in §5 by considering one of the remaining three connections discussed by Schaffer: that from chance to *causation*. Schaffer says (*op cit*. p. 132) he sees 'no problem' regarding the consistency of a non-trivial deterministic chance function with this connection, nor with the other two–from chance to *futurity* and *intrinsicness*. I argue that, not only is Schaffer correct in this, but that there are good reasons to think that a *trivial* deterministic chance function is *inconsistent* with the chance-causation connection. The conclusion (§6) is that there are non-trivial deterministic chances.

2 Schaffer's Incompatibilist Argument

I will now review Schaffer's argument that (non-trivial) deterministic chances would violate the connections from chance to credence, possibility and lawhood and that chance is therefore incompatible with determinism.

2.1 Chance and Credence

Schaffer takes the chance-credence connection to be adequately captured by Lewis's ([1986b]) *Principal Principle* (*PP*), which can be stated as follows (ibid., p. 87). Let *C* be any reasonable initial credence function, let *t* be any time and let *w* be any world. Let Ch_{tw} be the chance function (which receives time and world indices because of the time- and world-relativity of chance[4]), let *x* be any real number in the closed unit interval and let *p* be any proposition in the domain of the chance function. Finally, let *X* be the proposition that $Ch_{tw}(p) = x$, and let *E* be any proposition compatible with *X* that is admissible at *t* and *w*. Then:

$$C(p|X.E) = x \qquad (PP)$$

The idea is that if one started with a reasonable initial credence function (and updated by conditioning) and one learned that $Ch_{tw}(p) = x$ and if the rest of one's evidence *E* were admissible at time *t* (and consistent with the proposition that $Ch_{tw}(p) = x$), then one would have credence in *p* equal to *x*.

To get a handle on the content of the *PP*, we need to know what sort of evidence counts as 'admissible'. Lewis's characterization of admissible information is as follows:

> Admissible propositions are the sort of information whose impact on credence about outcomes comes entirely by way of credence about the chances of those outcomes. (ibid., p. 92)

Lewis suggests (ibid., pp. 92-6) that two different sorts of information are generally admissible at a time *t* and world *w*: *historical information* (or information about matters of particular fact at times no later than *t*) and *information about the laws* of *w*. This allows him to reformulate the *PP* in a manner that 'will prove easier to use' (ibid., p. 98), as follows.

Let H_{tw} be a proposition giving the complete history of *w* up to *t* and let L_w be a proposition giving the laws of nature that obtain at *w*. These propositions are both admissible at *t* and *w* and so, Lewis claims (ibid., p. 96), is their conjunction. Suppose that *X* is true: that is, $Ch_{tw}(p) = x$. Then, since both $H_{tw}.L_w$ and *X* hold at *w*, they are compatible. The conjunction $H_{tw}.L_w$ can therefore be substituted for the proposition *E* to yield (ibid. p. 97):

$$C(p|X.H_{tw}.L_w) = x \qquad (PP')$$

Indeed, according to Lewis (ibid., p. 97, [1994], pp. 477-8), the laws of *w* together with the initial history of *w* through *t* entail the chances that obtain at *t* and *w*. Hence, $L_w.H_{tw}$ entails *X*. So $X.L_w.H_{tw}$ can be simplified to $L_w.H_{tw}$. Since it is also true that $Ch_{tw}(p) = x$, *PP* can be reformulated as follows (Lewis [1986b], pp. 96-7, [1994], p. 487):

$$C(p|L_w.H_{tw}) = Ch_{tw}(p) \qquad (RPP)$$

The idea is that the laws of *w* together with the initial history of *w* through *t* entail the chances obtaining at *w* and *t* and that reasonable credence for someone whose evidence includes just the laws and initial history (and who updates by conditioning on her evidence) is equal the chance of *p* that they entail.

We are now in a position to consider Schaffer's argument for the incompatibility of non-trivial deterministic chances with the connection from chance to rational credence. His argument is as follows (*op. cit*., p. 128; cf. Hoefer [2007], pp. 558-9).

Suppose that p_e is the proposition that some event e occurs,[5] and that w is a deterministic world. Suppose, moreover, that $Ch_{tw}(p_e)$ is a non-trivial chance: that is $1 > Ch_{tw}(p_e) > 0$.

Now either e occurs in w or it does not. Suppose it does. Then, since w is deterministic, the laws of w together with the history of w up to (and including) any time t entail p_e. Accordingly, for any reasonable credence function, $C(p_e|L_w.H_{tw}) = 1$. It follows immediately by the *RPP* that $Ch_{tw}(p_e) = 1$.

Suppose, on the other hand, that e does *not* occur in w. Then the laws of w together with its history through t entail $\sim p_e$. Accordingly, for any reasonable credence function, $C(p_e|L_w.H_{tw}) = 0$. It follows immediately by the *RPP* that $Ch_{tw}(p_e) = 0$.

So, by the assumption that $Ch_{tw}(p_e)$ is a non-trivial deterministic chance, we have that $1 > Ch_{tw}(p_e) > 0$. But, by the *RPP*, we have that $Ch_{tw}(p_e)$ equals either 1 or 0 (depending on whether or not e occurs in w). Contradiction! Schaffer's conclusion: non-trivial deterministic chance assignments are incompatible with the connection from chance to rational credence captured by the *PP* (of which the *RPP* is a reformulation).

2.2 Chance and Possibility

Schaffer argues that non-trivial deterministic chances would also violate the connection from chance to possibility. He claims (*op cit*., p. 124) that this connection is captured by what he calls the *Realization Principle* (*RP*):[6]

> (*RP*) If $Ch_{tw}(p) > 0$, then there exists a world w' such that (i) w' matches w in laws; (ii) w' matches w in occurrent history up until time t; (iii) p is true at w'.

Schaffer (ibid., p. 130) argues that non-trivial deterministic chances are inconsistent with the *RP* as follows. Suppose that w is a deterministic world and $Ch_{tw}(p_e)$ is a non-trivial chance: that is $1 > Ch_{tw}(p_e) > 0$.

Now either e occurs in w or it does not. If it does then, since w is deterministic, the laws and initial history of w entail p_e, so there cannot be a world that has the same laws and initial history as w at which $\sim p_e$ is true. Therefore, by the *RP*, $Ch_{tw}(\sim p_e) = 0$ from which it follows by complementation that $Ch_{tw}(p_e) = 1$.

If, on the other hand, e does *not* occur at w, then the laws and initial history of w entail $\sim p_e$, so there cannot be a world with the same laws and initial history as w at which p_e is true. It follows directly by the *RP* that $Ch_{tw}(p_e) = 0$.

So, by the assumption that $Ch_{tw}(p_e)$ is a non-trivial deterministic chance, we have that $1 > Ch_{tw}(p_e) > 0$. But, by the *RP*, we have that $Ch_{tw}(p_e)$ equals either 1 or 0 (depending on whether e occurs in w). Contradiction! Schaffer's conclusion: non-trivial deterministic chance assignments are incompatible with the connection between chance and possibility captured by the *RP*.

2.3 Chance and Laws

Finally, Schaffer (ibid.) argues that non-trivial deterministic chances are incompatible with the connection from chance to lawhood. He claims (ibid., p. 126) that this connection is captured by the *Lawful Magnitude Principle* (*LMP*):

(LMP) If $Ch_{tw}(p) = x$, then the conjunction of H_{tw} (a proposition giving the history of w up until time t) with the laws of w entails that $Ch_{tw}(p) = x$.

The *LMP* says, in effect, that chances are lawfully-projected magnitudes. Schaffer argues (ibid., p. 130) that non-trivial deterministic chances would be inconsistent with the *LMP*, since 'laws at deterministic worlds do not project chances.'

The idea is as follows. Suppose that $Ch_{tw}(p_e)$ is a non-trivial deterministic chance: that is 1 > $Ch_{tw}(p_e)$ > 0. Since w is deterministic, $L_w . H_{tw}$ entails either $Ch_{tw}(p_e)$ = 1 or $Ch_{tw}(p_e)$ = 0 depending on whether e occurs at w. It follows that the only chance assignments compatible with the *LMP* are $Ch_{tw}(p_e)$ = 1 or $Ch_{tw}(p_e)$ = 0. But now we have that 1 > $Ch_{tw}(p_e)$ > 0 and that either $Ch_{tw}(p_e)$ = 1 or $Ch_{tw}(p_e)$ = 0. Contradiction! Schaffer's conclusion: non-trivial deterministic chance assignments are incompatible with the connection between chance and laws captured by the *LMP*.

Since a (non-trivial) deterministic chance function would 'sever the connections from chance to credence, possibility, and lawhood' (ibid. p. 132), Schaffer concludes that '[t]his is hardly a viable conception of chance' (ibid.).

In §3-4 I argue that Schaffer is wrong: there are non-trivial deterministic chance functions that are consistent with all three of these connections. Recognition of the level-relativity of chance allows us to see that this is so and undermines Schaffer's apparent demonstration that these connections, when taken together with the supposition of non-trivial deterministic chances, land us in contradiction.

Indeed I will argue that, once the level-relativity of chance is recognised, it can be seen that *only* a chance function that outputs non-trivial values in deterministic worlds can underwrite the connections from chance to credence and lawhood.

3 Special Scientific Laws

The main reason to think that there are non-trivial objective chances even in fundamentally deterministic worlds is that there exist probabilistic high-level or special scientific laws even in such worlds (§3.1).[7] The probabilities projected by these laws should be regarded as genuine, objective chances because the laws in question are genuine, objective laws. Not only are they accommodated as such by Lewis's Humean analysis of lawhood (§3.2) but, because they play the *law role*, they must be similarly accommodated by *any* adequate account of lawhood (§3.3).

3.1 Probabilistic Special Scientific Laws in Deterministic Worlds

Quantum mechanics–at least on standard, 'collapse' interpretations (e.g. Copenhagen and GRW)–indicates that the fundamental dynamics of *our* world are probabilistic. But it is not just fundamental physics that is probabilistic. Many of the high-level or special sciences also give probabilistic laws for events falling under their purview. As already noted, statistical mechanics gives such laws. So does Mendelian genetics. And probabilistic functional laws are encoded in the models of economists and meteorologists. Together with the initial history of the world, these special scientific laws entail non-trivial chances for the events that they concern.

The Mendelian genetic laws of Segregation and of Independent Assortment, for example, give a chance 0.25 for a dihybrid cross between two parents heterozygous for each (binary) trait yielding a child that is homozygous for each trait. So suppose that Jim and Jill are common garden pea plants (*pisum sativum*) heterozygous for both pea shape and colour (both binary variables, with *round* and *yellow* the dominant alleles, and *wrinkled* and *green* the recessive alleles), and Tom is a plant produced by crossing Jim and Jill. Let *t* be the time of crossing, and *p* the proposition that Tom is homozygous for both shape and colour. Then the Mendelian laws together with the history of the world through *t* (which includes the fact that Jim and Jill are heterozygous for both shape and colour and the fact that they are crossed), entail a chance 0.25 for Tom's being homozygous for both pea shape and colour.

In most cases, the special sciences make no presupposition about whether the fundamental dynamics of the world are deterministic or indeterministic. Where they do, as in CSM, the assumption is often one of microphysical determinism. It follows from the compatibility of the probabilistic special sciences with fundamental determinism that there exist fundamentally deterministic worlds (Newtonian or Bohmian worlds, perhaps) with probabilistic special scientific laws.

A lot of work remains to be done to show that the non-trivial probabilities projected by the probabilistic special scientific laws of these deterministic worlds are genuine objective chances. The opponent of deterministic chance will presumably seek to dismiss this claim, arguing that such probabilities are merely epistemic (indeed Schaffer makes precisely this argument, *op cit*. pp. 136-9).

The key to showing that these probabilities *are* objective chances is, of course, to show that they play the objective chance role. This will at least involve defusing Schaffer's arguments, reviewed in the previous section, that *no* non-trivial deterministic probability function can play the chance role. I shall argue that there are some such functions that can and that, moreover, *trivial* deterministic functions cannot.

It seems that the best place to start in showing that non-trivial deterministic probability functions can play the chance role will be with Schaffer's claim (considered in §2.3 above) that such functions violate the chance-law connection because the laws of deterministic worlds don’t project chances. After all, it has just been argued that Schaffer is simply wrong in this claim. True, the fundamental laws of deterministic worlds don’t project chances. But not all laws are fundamental. And it has just been argued that there are probabilistic high-level, special scientific laws even in fundamentally deterministic worlds.

Schaffer is aware of the vulnerability of his position to this kind of objection. His reaction (ibid., pp. 130-2) is to argue that, on Lewis’s Humean view of laws, there are good reasons for regarding these probabilistic special scientific ‘laws’ as failing to be genuine laws. Before considering his argument, it is worth getting a bit clearer on Lewis’s analysis.

3.2 Lewis’s Humean Analysis of Laws

Lewis’s analysis of laws ([1994], pp. 478, 480) is as follows. Consider all deductive systems whose theorems pertain to what happens in history, in the sense that they give either the outcomes or chances of outcomes in various situations. Exclude those systems whose theorems aren’t true in what they say about outcomes. (Lewis is attempting to deliver a simultaneous analysis of laws and chances, so it is not yet required that the theorems must be true in what they say about the chances.) Also, exclude any that say what an outcome will be without also saying that the outcome never had a chance of not coming about.

The remaining systems may differ in simplicity, strength (or informativeness), and fit (or the chance that they assign to the actual course of history). And there will be trade-offs between these virtues: simpler systems may be less strong or fit less well, and so on. According to Lewis:

> The best system is the system that gets the best balance of all three. [...]
>
> [T]he laws are those regularities that are theorems of the best system [...]

> [and] the chances are what the probabilistic laws of the best system say they are. (ibid., p. 480)

If it can be shown that (at least some of) the special scientific laws are theorems of the Best System, then it will follow (*pace* Schaffer) that Lewis's Humean analysis accommodates these special scientific laws as genuine, objective laws.

Indeed, there are good reasons to think that (at least some) special scientific laws *are* theorems of the Best System. In particular, it seems that a system that yields the special scientific laws as theorems will be much stronger or more informative than one that yields merely the fundamental, microphysical laws. This is because the microphysical laws tend to fall silent about the higher-level properties that the special scientific laws relate.

The high-level properties in question are typically *multiply realisable* at the microphysical level. That is, different distributions of microphysical properties may realise the same high-level property. So, for example, two individuals *a* and *b* sharing some biological property *F* could be in rather different microphysical states, consisting of differing distributions of particles with differing values of mass, spin, charge, etc. If the property *F* were to be characterised in microphysical vocabulary, it would be in terms of a very long disjunction with each disjunct–itself a highly complex *conjunction* of propositions attributing microphysically simple properties to particles–corresponding to one possible microphysical realisation of *F* (cf. Hoefer [2007], p. 593).

This relationship of realisation grounds the distinction between higher- and lower-levels. Two or more sciences can be regarded as characterising distinct levels when various distributions of the properties of concern to one *realise* those of concern to the other. Where this is not the case, the sciences in question should be regarded as each providing partial characterisations of the *same* level.

Each level, consisting of the distribution of a certain set of properties together with the nomic relations between those properties, constitutes a relatively closed system in the sense that adequate (non-reductive) explanations for the instantiation of any property within that set can be given in terms of the instantiation of other properties in that set together with the nomic connections between them.

Where two or more sciences partially characterise the same level, each taken individually will lack explanatory closedness. Climate change, for instance, cannot be adequately explained without appeal to human industrial activity as well as geological and cosmological factors. This reflects the fact that climatology, economic sociology, geology and cosmology do not each characterise distinct levels, but rather each provide partial characterisations of a certain relatively 'high' level.

The multiple realisability of higher-level properties at the microphysical level is the key to understanding how special scientific laws enhance the informativeness of a system that entails them. Suppose that the biological property F can be realised by any one of the complex microphysical states, α, β, γ, etc. And suppose that there is some other biological property, G, that can be realised by any one of the complex microphysical states α', β', γ', etc. For each particular microphysical realisation of F, the microphysical laws will perhaps give some well-defined chance of the system's going on to exhibit one of the microphysical states that realises G. However, it cannot be expected that the microphysical laws alone will tell us anything about the relationship between F and G in general. Specifically, it cannot be expected that they will tell us the chance that G is instantiated given that F is instantiated, since they themselves do not give us a probability distribution over the states α, β, γ, etc., conditional upon F's being instantiated.

Consequently, a system that entails only the microphysical laws will lack a certain amount of strength or informativeness in Lewis's sense: it will fail to say 'either what will happen or what the chances will be when situations of a certain kind arise' ([1994], p. 480). The kinds of situation include, of course, situations of kind *F*. This would be a heavy cost indeed where *F* and *G* correspond to important biological kinds as is the case, for example, if *F* is the property of being a crossing of two parents heterozygous for a given trait, and *G* is the property of the offspring's being homozygous for that trait.

The fact that the microphysical laws fall silent here means that there is a gap to be filled by the special scientific laws. In this case the Mendelian laws fill the gap. The addition of axioms that entail these laws will augment a system's strength because the resulting system will tell us what the chance of *G* is in situations of kind *F*: namely (on the current interpretation of *F* and *G*) 0.5.

Nor need the addition, to a system, of axioms required to entail the special scientific laws cost much in terms of simplicity. What we need to add to the axioms, in order to get the special scientific laws to fall out as theorems, is a function that takes macrostates as inputs and yields probability distributions over regions of microphysical phase space as outputs (this would yield the requisite probability distribution over the microphysical states α, β, γ, etc., conditional upon *F*'s being instantiated, from which an overall chance of *G* given *F* could be derived).

A function that yields a *uniform* probability distribution over those regions compossible with the macrostate might result in the best fit with the frequencies observed at a world like ours. If, for example, the various possible microphysical realisations of *F* lead to microphysical realisations of *G* with an average frequency of 0.5 then a uniform probability distribution over microphysical realisations of *F* will yield a chance of *G* given *F* close to the actual frequency of *G*s amongst *F*s. In any case, the function could be tweaked to ensure the best fit with the observed frequencies in our world.

The simple addition of such a function to the axioms would thus increase the strength of the system greatly by ensuring that the system tells us what the chances of *G* will be when situations of kind *F* arise and, in general, ensuring that it tells us what the chances will be when situations of kind *X* arise, where *X* is *any* microphysically disjunctive high-level kind. It consequently seems likely that such a system will come out best.

The illustration just given of how the Lewisian Best System Analysis can accommodate special scientific laws is similar to Loewer's attempt to show that the Best System for a Newtonian world will entail CSM. Loewer ([2001]) considers a formulation of CSM given by Albert ([2001]). Albert's formulation consists of three postulates: (1) the Newtonian dynamical laws, (2) a uniform probability distribution over the possible points in microphysical phase space at the beginning of the universe, and (3) a statement characterising the beginning of the universe as a low-entropy state. I shall borrow the terminology of Schaffer (*op cit*., p. 122) in calling postulates (2) and (3) respectively the 'Statistical Postulate' and the 'Past Hypothesis'.

Albert shows that from these three postulates the whole of statistical mechanics follows, as well as probabilistic versions of the principles of thermodynamics. Thus Loewer (*op cit*., p. 618) says:

> [T]his package is a putative Best System [...]. The contingent generalisations it entails are laws and the chance statements it entails give the chances. It is simple and it is enormously informative. [...] By being part of the Best System the probability distribution earns its status as a law and is thus able to confer lawfulness on those generalisations that it (together with the dynamical laws) entails.

The Albert package is similar to the system sketched above in its combination of the fundamental dynamical laws with a statistical postulate specifying a uniform probability distribution over points in microphysical phase-space. The main differences are, first, that whilst the Albert package just gives a single probability distribution over phase-space points at the beginning of the universe, the postulate I described is an atemporal *function* from macrostates to probability distributions. Second, the Albert package includes the low-entropy condition.[8]

Schaffer ([2007], pp. 130-1) advances an argument against the view that the Best System will entail the special scientific laws. He argues that, even though the addition of some statistical postulate to a system may result in a gain in informativeness vis-a-vis one that entails the microphysical laws alone, these two systems aren't the only competitors. He suggests that we consider an alternative package of the fundamental dynamical laws together with the Precise Initial Conditions. Where the microphysical laws are deterministic, this package will have great strength, since it 'entails every single detail of the entire history of the world' (ibid., p. 131).

A crucial question here concerns the simplicity of this latter system vis-a-vis one having just the fundamental dynamic laws and a statistical postulate as axioms. Hoefer ([2007], p. 560) puts the question well when he asks, regarding the Precise Initial Conditions,

> [D]o they increase the complexity of the system infinitely, or by just one 'proposition', or some amount in between? Lewis's explication does not answer these questions, and intuition does not seem to supply a ready answer either.

It is clear that different explications of the notion of simplicity may have strong implications for which system comes out as best.

But, in any case, Schaffer is wrong to claim that the package of the microphysical laws together with the Precise Initial Conditions is ‘maximally strong’ (*op cit*. p. 132). The package is *not* maximally strong on Lewis’s explication of the relevant notion of strength. For, as has already been seen, it fails to say ‘either what will happen or what the chances will be when situations of a certain kind arise’. For example, it fails to tell us what will happen or what the chances are when situations of the microphysically disjunctive biological kind F arise, as opposed to situations of the realising microphysical kinds α, β, γ, etc.

3.3 Special Scientific Laws and the *Law Role*

Following Schaffer and Loewer, I have focused on the Lewisian approach to laws, but there seems no good reason why other Humean approaches, as well as non-Humean approaches to laws, should not also be able to accommodate special scientific laws as genuine. Indeed there is good reason to think that, in order to be fully adequate, any analysis of laws *must* be able to accommodate special scientific laws. The reason is that these laws seem to play the *law role* just as well as the microphysical laws. In particular, the special scientific laws fulfil all the usual criteria for genuine lawhood by supporting counterfactuals, being confirmed by their instances and underwriting explanations and predictions.

For example, it is true in virtue of the Mendelian laws that *if* I had conducted a dihybrid cross of two pea plants heterozygous for two binary traits, *then there would* have been a chance 0.25 of the offspring being homozygous for each trait. Moreover, if I know the Mendelian laws, then I should have confidence of degree 0.25 in the *prediction* that a given pea plant resulting from such a cross will be homozygous for each trait. Furthermore, if a resulting pea plant does in fact turn out to be homozygous for each trait, then a good *explanation* (if there is such a thing as a good covering law explanation) would be that I conducted the cross and that the Mendelian laws indicate that there was a 0.25 chance that this outcome would result.

Finally, the Mendelian laws derive inductive *confirmation* from experimental results in just the same manner as do microphysical laws. Mendel ([1866]) himself conducted experimental crossings of 29,000 pea plants. His experiments with mixing one trait with another consistently yielded a 3:1 ratio between dominant and recessive phenotypes, whilst his experiments with mixing two traits resulted in 9:3:3:1 ratios. These experiments are rightly seen as having conferred rather good inductive confirmation upon the laws named after him.[9]

Someone might object to the foregoing along the following lines: 'If the microphysical laws project different chances from the Mendelian laws for the outcomes of dihybrid crosses, then the counterfactual "if I had conducted a cross, there would have been a chance 0.25" is false and, moreover, one should not (if one knew the microphysical laws) have confidence 0.25 in the outcome, nor does the 0.25 chance really *explain* the outcome.' The objector might instead maintain that it is the microphysical laws that determine which counterfactuals are true, that underwrite genuine explanations and that ground reasonable, well-informed, predictions.

But such objections are misguided. The special scientific laws do not really compete in this way with the microphysical laws. Suppose, for example, that there is a fact of the matter about what the microphysics would have been had I crossed the pea plants and that the microphysical laws consequently generate a chance 1 for the cross resulting in a child homozygous for each trait. Then it is true *both* that 'if I had conducted a cross, then there would have been a chance 1' *and* that 'if I had conducted a cross, then there would have been a chance 0.25’. Of course, these divergent chances cannot be the same chance. As I shall argue in §4, they are chances of different *levels*: the former is a micro-level chance, the latter a higher-level chance.

In some cases only the latter counterfactual will be true. Suppose, for example, that there is no fact of the matter about what the microphysics would have been had I crossed. Then it is nevertheless true in virtue of the Mendelian laws that ‘if I had conducted a cross, there would have been *a* chance 0.25’.

Likewise, suppose that I am going to conduct a cross but it’s still not settled what the microphysical details of the cross will be, or I just don’t know the microphysical details. Then I should have confidence of degree 0.25 in the *prediction* that a given resultant pea plant will be homozygous for each trait. Indeed, as I shall argue in §4.2 below, I should have this degree of credence even if the microphysical details *are* settled and I know *all* the admissible information about the case.

As regards *explanation*, there may be more than one good explanation of an event. If a resulting pea plant turns out to be homozygous, then a good explanation would be that I conducted the cross and that the Mendelian laws indicate that there was a 0.25 chance that this outcome would result. This is true even though a still more satisfying explanation might be that I conducted the cross and the microphysical laws and circumstances entailed a chance 1 for this outcome.

The special scientific laws play the law role, and ought therefore to be accommodated as genuine laws by any adequate account of lawhood. And, in the previous subsection, I argued that there is good reason to think that they *are* accommodated by the Lewisian account.

4 Deterministic Chance

4.1 Chance and Laws Again

In the previous section it was argued that there exist probabilistic special scientific laws even in fundamentally deterministic worlds. When taken together with an initial history of the deterministic world in question, these laws entail non-trivial probabilities for the events that they concern. Since these laws are genuine objective laws, the probabilities that they project are genuine objective chances. There therefore exist fundamentally deterministic worlds with chance-projecting special scientific laws.

In light of this, it is clear that Schaffer's assertion (noted in §2.3 above) that the laws of deterministic worlds don't project chances is just false. So Schaffer is wrong that non-trivial deterministic chances are incompatible with the connection from chance to lawhood, captured by his *LMP*:

> (*LMP*) If $Ch_{tw}(p) = x$, then the conjunction of H_{tw} (a proposition giving the history of *w* up until time *t*) with the laws of *w* entails that $Ch_{tw}(p) = x$.

The compatibility of non-trivial deterministic chances with the *LMP* can be seen by reconsidering the earlier example of the crossing of pea plants.

Suppose that the world *w* at which the crossing occurs is a Newtonian one (the Mendelian laws are perfectly compatible with this). The history of *w* through *t* (the time of crossing)–which includes the fact that the crossed plants (Jim and Jill) are heterozygous for both shape and colour–together with the laws–which include the Mendelian laws–entail a chance 0.1875 for the proposition p_{rg}, that Tom (the resulting pea plant) has round, green peas.[10] Thus, we have $1 > Ch_{tw}(p_{rg}) = 0.1875 > 0$.

But there is a difficulty here. Since *w* is a deterministic Newtonian world, the history of *w* through *t*–which includes the complete microphysical state of the world at *t*–together with the laws of *w*–which include the fundamental dynamical laws (alongside the Mendelian laws)–entails $Ch_{tw}(p_{rg}) = 1$ or $Ch_{tw}(p_{rg}) = 0$, depending on whether or not p_{rg} is actually true at *w*. Thus, we have that $1 > Ch_{tw}(p_{rg}) > 0$ and that $Ch_{tw}(p_{rg}) = 1$ or 0. Contradiction!

Or so it seems: so long as it is maintained that chance is a function of just three arguments, there is a tension in assigning the same proposition two divergent chances at the same world and time. But one can escape the contradiction by allowing that chance isn't after all just a function of propositions, worlds and times.

Consider an analogy: for a given world and a given proposition, we may get more than one different chance. No one would regard this as a contradiction because chance is a function not just of worlds and propositions, but also of times. There is no tension between its being the case, for instance, that $Ch_{tw}(p) = x$ and $Ch_{t'w}(p) = y$ (where $x \neq y$, $t \neq t'$). Similarly, there is no tension between $Ch_{tw}(p) = x$ and $Ch_{tw'}(p) = y$ (where $x \neq y$, $w \neq w'$).

Likewise, if chance is a function of some fourth argument, then there would be no difficulty in admitting that there may be two or more divergent chances for the same proposition, in the same world *even at the same time*.

What might this fourth argument be? The consideration that puts pressure on us to accept these divergent chances is the existence of laws of different levels which entail them. It seems, then, that there is an important *level*-relativity of chance, as well as its proposition-, world-, and time-relativity.

One can capture this additional dimension of relativity by introducing an additional subscript to the chance-function: '$Ch_{twl}(p)$' can be read 'the *l*-level chance of *p* at time *t* and world *w*'. The *l*-level chance of *p* at *t* and *w* is just the chance entailed for *p* by the *l*-level laws of *w* when taken together with the history of *w* through *t*.

In the above example, the Mendelian laws of *w* together with the history of *w* through *t* entailed that $Ch_{tw}(p_{rg}) = 0.1875$. The microphysical laws of *w* together with the history of *w* through *t*, on the other hand, entail that $Ch_{tw}(p_{rg}) = 1$ or 0. But contrary to first appearances this is no contradiction, for the chances entailed by the Mendelian laws are not the same as those entailed by the microphysical laws: they are chances of different levels. Any appearance of contradiction evaporates when the appropriate indices are added to the chance functions. Let l_h (*h* for *higher*) be the level with which the Mendelian laws are concerned, and let l_f (*f* for *fundamental*) be the microphysical level. Then, making the level-relativity of the chances explicit, we have $Ch_{tw\,l_h}(p_{rg}) = 0.1875$ and $Ch_{tw\,l_f}(p_{rg}) = 1$ or 0. No contradiction there!

In general, it might be that $Ch_{tw\,l_i}(p) = x$, but that $Ch_{tw\,l_j}(p) = y$ (where $x \neq y$, $l_i \neq l_j$). Divergent chances may exist for the same proposition at the same world and time because of the level-relativity of chance. Recognition of this fact is the key to the reconciliation of determinism with non-trivial chance.[11]

Whilst the *LMP* sanctions apparently inconsistent chances, relativizing chances to levels results in a statement of the chance-law connection that avoids this:

> (LMP^*) If $Ch_{twl}(p) = x$, then the conjunction of H_{tw} (a proposition giving the history of *w* up until time *t*) with the *l*-level laws of *w* entails that $Ch_{twl}(p) = x$.

In a deterministic world that has probabilistic laws of non-fundamental levels, the original *LMP* sanctions non-trivial chances (as we saw earlier). The revised *LMP** just makes it clear that these non-trivial chances are not the same as the trivial chances that are also sanctioned. Thus the appearance of inconsistency is avoided. Either way, given the existence of probabilistic special scientific laws in deterministic worlds, it is clear that non-trivial deterministic chances are compatible with the chance-law connection.

Indeed, it seems that the conceptual connection between chances and laws might be tighter than that captured by either the *LMP* or the *LMP**. These principles state roughly that all chances are lawfully entailed. But one might think, in addition, that all lawfully entailed probabilities for the occurrence of events are chances.

If there is this closer connection between chances and laws, then taking *only* the values projected by *fundamental* laws as chances is incompatible with the chance-law connection. That connection now not only *sanctions* but *entails* the existence of non-trivial chances in deterministic worlds with probabilistic special scientific laws and would be violated by a function that outputted only trivial chances in such a world.

4.2 Chance and Credence Again

Recall that Schaffer apparently showed that a non-trivial chance assignment to a proposition *p* in a deterministic world contradicts the deliverances of the reformulated *Principal Principle*:

$$C(p|L_w.H_{tw}) = Ch_{tw}(p) \qquad (RPP)$$

The problem was that, if w is deterministic, then the laws and initial history will entail either p or $\sim p$. Therefore, reasonable credence conditional upon the laws and initial history is equal to 1 or 0, from which it follows directly by the RPP that $Ch_{tw}(p)$ is correspondingly equal to 1 or 0.

Note that it is the original PP rather than RPP that, according to Lewis, enjoys the 'direct intuitive support' ([1986b], p. 98). The PP was reformulated only because the reformulation 'prove[s] easier to use' (ibid.). Importantly, the demonstration of the incompatibility of non-trivial deterministic chances with the PP depends upon the validity of RPP as a reformulation of the PP. This is important because the reformulation is invalid.

The original PP, it will be recalled, states that reasonable credence in p conditional upon the proposition X that $Ch_{tw}(p) = x$ and any other proposition E compatible with X that is admissible at time t and world w, is equal to x:

$$C(p|X.E) = x \qquad (PP)$$

The reformulated PP was arrived at by assuming that the big conjunction of the complete set of laws of w with the history of w through t is an admissible proposition at t and w. But this big conjunction is not, in general, fully admissible.[12]

Recall that Lewis characterises admissible propositions as propositions whose impact on credence about outcomes comes entirely by way of credence about the chances of those outcomes. On the assumption (which Lewis makes) that there can only be *one* chance for a proposition in a world at a time, a chance that is entailed by the big conjunction of the initial history and the laws, it might seem that this big conjunction is indeed admissible. But this assumption is incorrect: there exist laws of different levels which, taken together with initial history, entail divergent chances for the same propositions even in the same worlds at the same times. And, as shall now be seen, the existence of these divergent chances makes it false that the big conjunction is in general admissible.

Consider the high-level chance, entailed by the Mendelian laws, for the proposition p_{rg} that Tom has round, green peas: $Ch_{tw\,l_h}(p_{rg}) = 0.1875$. Relative to *this* chance, the conjunction of the history of *w* through *t* with the Mendelian laws (and the high level laws in general) is admissible. It is admissible because its impact on reasonable credence in the proposition p_{rg} comes entirely by way of its impact on credence about the value of $Ch_{tw\,l_h}(p_{rg})$. The proposition $H_{tw}.L_{w\,l_h}$ entails $Ch_{tw\,l_h}(p_{rg}) = 0.1875$ and it contains no other information that is relevant to whether or not p_{rg} is true.

But it cannot be inferred from this that $H_{tw}.L_w$, the proposition giving the history of *w* through *t* together with the *complete* set of laws of *w*, is admissible. The problem is that this proposition may well carry information relevant to whether p_{rg} is true over and above that expressed by the proposition that $Ch_{tw\,l_h}(p_{rg}) = 0.1875$. In particular, L_w includes the laws of levels other than l_h and these may project divergent chances for p_{rg}. These chances constitute additional information relevant to whether p_{rg} is true.[13] Indeed where *w* is fundamentally deterministic $H_{tw}.L_w$, which includes the fundamental laws, *entails* p_{rg} and so clearly carries such additional information.

Hence, relative to $Ch_{tw\,l_h}(p_{rg})$, $H_{tw}.L_w$ is simply inadmissible. And since the validity of the reformulation of the *PP* rested upon the assumption that $H_{tw}.L_w$ is in general admissible, it can be concluded that the reformulation is invalid and that Schaffer's demonstration that non-trivial deterministic chances are incompatible with the *PP*, a demonstration that depended upon the validity of the *RPP*, is unsuccessful.

This argument that *RPP* is invalid as a reformulation of *PP* and that therefore non-trivial deterministic chances can't be shown to be inconsistent with the *PP* is one that has been made by Hoefer ([2007]). Regarding the alleged derivation of a contradiction from *PP* plus the supposition of a non-trivial deterministic chance for some proposition *A*, Hoefer says (*op cit*., p. 559):

> [T]his derivation is spurious; there is a violation of the correct understanding of admissibility going on here. For if $H_{tw}.L_w$ entails *A*, then it has a *big* (maximal) amount of information pertinent as to whether *A*, and not by containing information about *A*'s objective chance! So $H_{tw}.L_w$, so understood, must be held inadmissible, and the derivation of a contradiction fails.

Whilst recognition of the level-relativity of chance makes it clear that the reformulation of the *PP* is invalid, it does not create any problem for the *PP* itself. The only necessary adjustment is that, once it is allowed that there may be more than one chance attaching to the same proposition at the same world and time, Lewis's supposition of uniqueness must be dropped. Thus, *X* must not be read as the proposition that *the* chance at *w* and *t* of *p* is equal to *x*, but rather as the proposition that the *l* chance at *w* and *t* of *p* is equal to *x*, where *l* is a variable ranging over levels. What information is admissible will depend upon which level is in question.

Lewis's reformulation of the *PP* was intended to facilitate ease-of-use. Unfortunately, as has been seen, it involved the fallacious substitution of the not-generally-admissible proposition $H_{tw}.L_w$ for the admissible proposition *E*. Can one give a user-friendly reformulation of the *PP* without committing such a fallacy? The key to doing so is (as with the *LMP*) to restrict the laws so as to be of the same level as the chances, as in (*RPP**):

$$C(p|L_{wl}.H_{tw}) = Ch_{twl}(p) \qquad (RPP^*)$$

Intuitively: the initial history and l-level laws of the world entail the l-level chances and reasonable credence for someone whose evidence included all and only the initial history and l-level laws (and who learns by conditioning) is equal the chance for p that they entail.[14]

Where $l = l_f$, the microphysical level, and w is a deterministic world, the chance entailed by $L_{wl}.H_{tw}$ will be trivial and reasonable credence conditional upon just $L_{wl}.H_{tw}$ will be correspondingly trivial. But where l is some non-fundamental level, l_h, the chance entailed by $L_{wl}.H_{tw}$ need not be trivial and, if it is not, reasonable credence conditional upon just $L_{wl}.H_{tw}$ will be correspondingly non-trivial.

It would, of course, be interesting to know what reasonable credence would be for someone who knew the complete laws and initial history, $L_w.H_{tw}$, and consequently the chances projected by the laws of each of the different levels. The *RPP**, which is the correct reformulation of the *PP*, does not tell us–it does not indicate which of the divergent chances for p would guide rational credence in p.

It does seem plausible that where w is a fundamentally deterministic world the answer is *the trivial, fundamental* (*level* l_f) *chance of* p, $Ch_{tw\,l_f}(p)$.[15] The reason for thinking this is that, in this case, $L_w.H_{tw}$ actually *entails* p or ~p. Still *RPP**, which is the correct reformulation of *PP*, does not yield this (or any other) answer. This is just a limitation of *RPP**.

In any case, *PP* is not after all incompatible with non-trivial deterministic chances. It is only the incorrect reformulation *RPP* that is inconsistent with such chances. Once the reformulation is corrected, it becomes clear that the existence of non-trivial chances in deterministic worlds with probabilistic special scientific laws is perfectly compatible with the connection from chance to rational credence.

Indeed, it is possible to go on the offensive against the incompatibilist regarding chance and determinism. Once it is acknowledged that chances are level-relative, it can be seen that a chance function that outputted only trivial values in a deterministic world with probabilistic special scientific laws would not be one that outputted values that play the role of chance in guiding rational credence captured by the *PP* (and its valid reformulation, RPP^*).

For example, where l_h is the level of concern to the geneticist and *w* is a deterministic world with Mendelian genetic laws, a chance function that assigned a level l_h chance 1 or 0 at the time at which Jim and Jill are crossed to the proposition that Tom will have round, green peas would be one that assigned values that fail to guide rational credence given all of the relevant admissible information, information which includes just the initial history plus the l_h laws (among which are the Mendelian laws but not the microphysical laws). Reasonable credence given just this information is 0.1875.

Consequently, not only is it not the case that non-trivial deterministic chance functions are incompatible with the chance-credence connection, it is in fact the case that *only* such a function can underwrite this connection in deterministic worlds with probabilistic special scientific laws. In the previous section, we saw that the same was true with respect to the chance-law connection.

4.3 Chance and Possibility Again

It will be recalled that Schaffer argues that non-trivial deterministic chance functions are incompatible with the connection from chance to possibility captured by the *RP*:

(RP) If $Ch_{tw}(p) > 0$, then there exists a world w' such that (i) w' matches w in laws; (ii) w' matches w in occurrent history up until time t; (iii) p is true at w'.

As was seen in §2.2 above, if w is a deterministic world and p_e is the proposition that some event e occurs, then the initial history and laws of w will entail either p_e or $\sim p_e$ (depending on whether e occurs in w). Accordingly, a non-trivial chance assignment to p_e is incompatible with the RP.

Note that the formulation of the RP presupposes that the chance function yields a unique chance assignment for a given p, t and w. But I have been arguing that this supposition is erroneous: chances are level-relative and, consequently, there may be two or more divergent chances attaching to a proposition at a world and a time (these chances being chances of different levels). And note that the RP will be compatible with deterministic chance provided that the laws in question are restricted so as to be of the same level as the chance. Thus consider RP^*:

(RP^*) If $Ch_{twl}(p) > 0$, then there exists a world w' such that (i) w' matches w in l-level laws; (ii) w' matches w in occurrent history up until time t; (iii) p is true at w'.

Where l_f is the microphysical level, and w is a deterministic world in which event e fails to occur, then $L_{wl_f}.H_{tw}$ will entail $\sim p_e$. There is therefore no world w' at which $L_{wl_f}.H_{tw}.p_e$ is true. So, by RP^*, $Ch_{twl_f}(p_e) = 0$. If, on the other hand, e *does* occur at w then analogous considerations show that RP^* implies $Ch_{twl_f}(p_e) = 1$.

But, where l_h is some non-fundamental level, $L_{w\,l_h}.H_{tw}$ needn't entail either p_e or $\sim p_e$, but may instead entail some non-trivial chance for p_e. If so, there are some worlds at which $L_{w\,l_h}.H_{tw}.p_e$ is true and some worlds at which $L_{w\,l_h}.H_{tw}.\sim p_e$ is true. So it is consistent with *RP** that $1 > Ch_{tw\,l_h}(p_e) > 0$. Nor is there a contradiction between its being the case that $1 > Ch_{tw\,l_h}(p_e) > 0$ and its being the case that $Ch_{tw\,l_f}(p_e)$ is equal either to 1 or 0. This is because $Ch_{tw\,l_h}(p_e)$ and $Ch_{tw\,l_f}(p_e)$ are not the same chance, but are chances of different levels.

One might insist that high-level deterministic 'chances' aren't really chances since they merely fulfil *RP** and not *RP*, which (it might be insisted) is the correct chance-possibility principle. In order to evaluate this objection, it is necessary to consider the nature of the platitude concerning chance and possibility, of which *RP* and *RP** are both candidate precisifications.

Let us suppose that Schaffer (ibid., p. 124) is correct when he says that the platitude is roughly that 'if there is a nonzero chance of *p*, this should entail that *p* is possible, and indeed that *p* is compossible with the circumstances.'

Note, first of all, that non-trivial deterministic chances aren't supposed to be chances for events that are impossible in the sense that they are ruled out by the laws alone (as would be the case if, in our world, a non-trivial chance were assigned to the transmission of a super-luminal signal). But it might be said that they are chances for events that, given the laws, are not *compossible* with the circumstances, including the microphysical initial history of the world.

But, we might ask: *which laws*? In a fundamentally deterministic world, the events in question may be ruled out by the *fundamental* laws, given the circumstances. But, as has been seen, they need not be ruled out by the non-fundamental laws. *RP** allows that the *l*-

level chance of an event *e* may be positive provided that *e* is not, in the circumstances, ruled out by the laws of that level.

The objector might still insist that the chance-possibility connection is captured by *RP*, which rules out deterministic chances because the events in question are not compossible with the circumstances given *all the laws*, rather than *RP** which allows them because the events they concern are compossible with the circumstances plus the *higher-level laws*.

The dispute is then one over the precise content of the chance-possibility platitude. And one might wonder whether this content is rich enough to allow us to adjudicate between *RP* and *RP** as candidates for *the* correct precisification of the chance-possibility connection.

Indeed, these aren't the only two contenders. Schaffer (*op cit*. p. 124) takes the *RP* to be a relatively uncontroversial strengthening of Bigelow, Collins and Pargetter's *Basic Chance Principle* ([1993], p. 459):

(*BCP*) If $Ch_{tw}(p) > 0$, then there exists a world w' such that (i) $Ch_{tw'}(p) = Ch_{tw}(p)$; (ii) w' matches *w* in occurrent history up until time *t*; (iii) *p* is true at w'.

But there is evidently no inconsistency between *BCP* and non-trivial deterministic chances. Suppose that *w* is microphysically deterministic, and that w' is not (the *BCP* doesn't require *w* and w' to agree in laws). And suppose that both worlds are Mendelian, that Jim and Jill are crossed at time *t* in both worlds, and that p_{rg} is the proposition that Tom (a resulting pea plant) has round, green peas. Then $Ch_{tw'l_h}(p_{rg}) = Ch_{twl_h}(p_{rg}) = 0.1875$. Since *w* is microphysically deterministic, it might be that Tom is determined to have wrinkled, yellow peas in *w*. It is compatible with this that Tom turns out to have round, green peas in w' (since w' is *not* microphyscally deterministic). Therefore, the *BCP* is compatible with the existence of a positive chance for p_{rg} in w, even though p_{rg} is incompatible with the microphysical laws plus microphysical history of w.

Nevertheless, suppose *BCP* to be somehow ruled out as a candidate precisification of the chance-possibility connection, and take just *RP* and RP^* as the relevant alternatives. There are the following grounds to favour RP^* (which is compatible with non-trivial deterministic chances) over *RP* (which is not).

If, as it seems one must when discussing the content of *platitudes* about chance, one takes seriously the layperson's talk about chances for ordinary macroscopic events–such as the 50% chance of the coin landing heads–or the scientist's talk about chances for special scientific events–such as the 35% chance of the hurricane making landfall in southern Florida–then there is pressure to conclude that chances are not the sorts of things that must (as the *RP* requires) indicate compossibility with the circumstances given the *fundamental* laws. The layperson and the meteorologist may not know, or have any opinion about, whether these events are compossible with the circumstances given the *fundamental* laws. Yet any doubt about this does not lead to doubt about the correctness of these chance assignments.

What the layperson *does* know is that (as required by the RP^*) heads is not ruled out by the macro-level law ascribing a 0.5 chance to coin flips. What the meteorologist knows is that (again, as required by the RP^*) the south Florida landfall is not ruled out by her model, a model that encodes (at least approximations of) the probabilistic functional laws of her discipline.

There is another reason to favour the RP^* over the *RP*. Change the earlier example so that the crossing of the pea plants now occurs in a micro-indeterministic world, w_i. And suppose that the Mendelian laws still hold at w_i, so that $Ch_{tw_il_h}(\sim p_{rg}) = 0.8125$. Suppose, moreover, that both p_{rg} and $\sim p_{rg}$ are compossible with the circumstances plus the fundamental laws of w_i, so that this chance is consistent with the original *RP*.

But suppose that $\sim p_{rg}$ is only compossible with the fundamental laws in virtue of the possibility of an extremely unlikely quantum event,[16] so that the chance assigned by the fundamental laws to $\sim p_{rg}$ is exceedingly small, e.g. $Ch_{tw_il_f}(\sim p_{rg}) = 0.1 \times 10^{-1,000,000}$. Surely it is bizarre in the extreme to maintain that it is this immensely unlikely and seemingly irrelevant possibility that grounds the positive value of $Ch_{tw_il_h}(\sim p_{rg})$. Remove the astronomically small quantum uncertainty, and suddenly the value of $Ch_{tw_il_h}(\sim p_{rg})$ switches from 0.8125 to 0 (despite the continued existence of the 0.8125 probability projected by the Mendelian laws)! This is hardly plausible.

The l_h chance of 0.8125 sanctioned by the RP^* clearly remains even if the fundamental dynamics of the world turn out to be deterministic. The elimination of the very slight quantum uncertainty has only the effect of changing the value of the l_f chance from $0.1 \times 10^{-1,000,000}$ to 0, again in accordance with the RP^*.

So the RP^* looks like a better chance-possibility principle than *RP*, and since the former sanctions non-trivial chances in deterministic worlds with probabilistic special scientific laws, it seems that non-trivial deterministic chances are compatible with the chance-possibility connection.

5 Chance and Causation

Schaffer claims that non-trivial deterministic chances would violate the connections from chance to rational credence, possibility and lawhood. In the previous section it was shown that recognition of the level-relativity of chances makes it clear that this is not so. Indeed it was argued that in deterministic worlds with probabilistic special scientific laws *only* a non-trivial function can underwrite the chance-credence and chance-law connections.

The aim of this section is to reinforce the conclusion that there are deterministic chances by considering one of the three remaining platitudes that Schaffer identifies: namely that connecting chance to causation. Schaffer says that he sees no incompatibility between the chance-causation platitude and a non-trivial deterministic chance function (he says the same about the final two–chance-futurity and chance-intrinsicness–platitudes). But I shall argue that there *is* an incompatibility with *trivial* deterministic chance functions.

The argument is independent of considerations concerning the level-relativity of chances. Consequently, in what follows, the level index to the chance function shall be suppressed for simplicity. The argument is also ancillary to the main case for deterministic chance made in §§3-4 above, which goes through even if one rejects the claim that there is a platitudinious connection from chance to causation (or if one rejects the admittedly contentious precisification of that platitude that I suggest below).

Schaffer (*op. cit.*, p. 126) claims that the platitude about the connection from chance to causation is as follows:

> [C]hances should live within the causal transitions they impact. That is, if a given chance is to explain the transition from cause to effect, that chance must concern some event targeted within the time interval from when the cause occurs, to when the effect occurs. Otherwise that chance cannot impact the transition from cause into effect–it would be left outside of the action.

He states (ibid.) that this platitude may be codified as the *Causal Transition Constraint* (*CTC*):

(CTC): If $Ch_{tw}(p_e)$ plays a role in the causal relation between c and d, then $t_e \in [t_c, t_d]$.[17]

Schaffer (ibid. p. 132) says that he sees 'no problem' about non-trivial deterministic chances fitting the *CTC*.[18]

I am sceptical about the status of this 'platitude'. The phrase 'chances should live within the causal transitions they impact' seems very obscure, as does the notion of a chance's being able to 'explain the transition from cause to effect'. Even the notion of a chance's 'playing a role in a causal relation' which appears in the alleged precisification of the chance-causation platitude, *CTC*, is very opaque.

In so far as I can make sense of these statements, they just seem *false* (and can therefore hardly be platitudinous). Consider the following example.[19] Suppose that Napoleon is on his way to Waterloo. I am a general in the Prussian army and set an ambush. My men attack Napoleon's troops, inflicting heavy losses (e). Napoleon reaches Waterloo with his army severely depleted and unable to withstand Wellington's charge (c). Wellington is victorious (d).

Let $Ch_{t_e - \varepsilon\, w_@}(p_e)$ be the chance, just before I attacked, of my infliciting heavy losses on Napoleon. It seems that this chance helps explain why I did in fact inflict heavy losses upon Napoleon (e). This, in turn, helps explain why Wellington's charge (c) resulted in his victory (d). There does not *seem* to be any failure of transitivity here, and so (insofar as I can make sense of these notions) it seems that the chance of e helps explain (and 'impacts' upon) the causal transition from c to d. Nevertheless, $t_e \notin [t_c, t_d]$. Thus, insofar as the *CTC* is meaningful, it seems to be false.

In any case, it strikes me as implausible to hold that the claim 'chances [...] live within the causal transitions they impact' is the obvious candidate for *the* platitude connecting causation and chance. I think many more people have thought the following to be a platitude: 'causes (tend to) raise the chance of their effects'.[20]

A natural way to be precise about the relevant chance-raising relation is to cash it out in terms of an inequality between two conditional chances:

$$Ch_{t_c-\varepsilon\, w}(p_e|p_c) > Ch_{t_c-\varepsilon\, w}(p_e|{\sim}p_c) \tag{1}$$

This inequality says that, just before *c* occurred, the chance of *e* conditional upon the occurrence of *c* was greater than the chance of *e* conditional upon the non-occurrence of *c*.

The result is the following *Chance-Causation Constraint* (*CCC*):

> (*CCC*) If *c* is a cause of *e* then (*ceteris paribus*) *c* raises the chance of *e* in the sense that (1) obtains.[21]

Since chances obey the probability calculus (see Lewis [1986b], esp. p. 98) we have, by the axiom of conditional probability:

$$Ch_{tw}(p|q) = \frac{Ch_{tw}(p.q)}{Ch_{tw}(q)}, \qquad Ch_{tw}(q) > 0 \tag{2}$$

Where $Ch_{tw}(q) = 0$, the axiom of conditional probability leaves $Ch_{tw}(p|q)$ undefined.

Suppose that *w* is deterministic, and *c* is a cause of *e*. Then, by *CCC*, it is the case (*ceteris paribus*) that *c* raises the chance of *e* in the sense that (1) holds. But (1) will hold only if both terms are well-defined. By (2) (plus the Complementation Theorem) both terms will be well-defined only if $1 > Ch_{t_c-\varepsilon\, w}(p_c) > 0$. But, by the assumption that there are only trivial chances in deterministic worlds, it follows that $Ch_{t_c - \varepsilon\, w}(p_c)$ is equal to 1 or 0. Contradiction! Conclusion: a chance function that outputs only trivial chances in deterministic worlds is incompatible with the connection between chance and causation captured by the *CCC*.[22]

Note that, although *CCC* says only that causes raise the chance of their effects *ceteris paribus*, a chance function that outputted *only* trivial values in deterministic worlds would mean that causes *never* raise the chance of their effects in those worlds. By *CCC*, this is unacceptable.

6 Conclusion

The probabilistic special scientific laws of deterministic worlds, when taken together with the initial histories of those worlds, entail non-trivial probabilities for the events that they concern. In addition to being *lawfully projected*, these non-trivial probabilities *guide rational credence* and indicate *compossibility* with the special scientific laws plus circumstances. They therefore play the chance role and should be considered chances.

Indeed, not only are there non-trivial deterministic probability functions (deterministic probability functions that output at least some non-trivial values) that play the chance role, there are such functions that play it better than any *trivial* deterministic probability function. In deterministic worlds with probabilistic special scientific laws, *only* a non-trivial probability function can underwrite the connections from chance to lawhood and rational credence (as was argued in §§4.1&4.2). And if the argument of §5 is found compelling there is reason to think that only a non-trivial function can underwrite the connection from chance to causation.

The conclusion that there exist non-trivial deterministic chances is not in tension with the claim that there also exist trivial chances (trivial probabilities that *also* play the chance role) for the very same propositions at the very same times and worlds (outputted by the very same chance function). Any appearance of tension dissolves once it is recognized that chance is a function of *four* arguments: a proposition, a time, a world, and *a level*.

The *illusion* of tension between trivial and non-trivial deterministic chances has been the reason for opposition to the latter. This is in evidence in the passage quoted from Lewis in the introduction to this paper. It is also in evidence in Schaffer's arguments (reviewed in §2) that sought to derive a contradiction from the supposition of non-trivial deterministic chances.

Chance *can* be reconciled with determinism, and disparate chances *can* be reconciled with one another. The reconciliation is effected by the level-relativity of chance. Lewis is correct in observing (in the passage quoted in the introduction) that it is false that *the* chance of a fair coin landing heads can be both zero or one *and* also 50%. But it is only false because the supposition of uniqueness is false. It can be the case both that a coin has *a* chance zero or one and *a* chance 50%: the chances being chances of different levels. Once the illusion of tension between disparate chances is dispelled through recognition of chance's level-relativity, there remains no obstacle to accepting that the non-trivial, chance-role-playing probabilities projected by the special scientific laws of deterministic worlds are indeed deterministic chances.

Appendix: Times, Levels and Chance Setups

Hájek ([2003a], [2003b], [2007]) has convincingly argued that chances must be relativised to chance setups. This seems to be implicitly accepted by both Lewis and Schaffer. Both relativize chances to *times*, but as Hoefer observes ([2007], pp. 564-5):[23]

> For Lewis, a non-trivial time-indexed objective probability $Ch_{tw}(p)$ is, in effect, the chance of p occurring given the instantiation of a big setup: the entire history of the world up to time t.[24]

This suggests that the following analysis might be adopted:[25]

$$Ch_{tw}(p) =_{def} Ch_w(p|H_{tw})$$

Chances, on this analysis, are not fundamentally time-relative. Rather, the chance of p at t and w is just the chance for p that results from conditioning upon the history of w through t. This analysis has its merits: if p concerns what is past by t then H_{tw} entails p and $Ch_w(p|H_{tw}) = 1$; moreover, since the big chance setup H_{tw} changes as the value of t changes, so will the chance distribution that results from conditioning upon it. In short, conditioning the chance function on history through time t reproduces the properties of a time-indexed chance function.

I argue in the paper that chances are *level*-relative. In §3.2, it was suggested that the relationship of realisation grounds the distinction between higher- and lower-levels: two sciences can be regarded as (at least partially) characterising distinct levels when various distributions of the properties of concern to one *realise* those of concern to the other.

With this in mind, define the *history* of world w at level l up until time t as the distribution of l-level properties throughout w up until t. The proposition giving this distribution can be written 'H_{twl}'. I think that the following analysis might then be adopted:

$$Ch_{twl}(p) =_{def} Ch_w(p|H_{twl})$$

Chances, on this analysis, are not fundamentally level-relative. The l-level chance of p at t and w is just the chance for p that results by conditioning upon the l-level history of w through t.[26] This analysis has its merits, foremost among which is that if the l-level history together with the laws of w don't entail p, then conditioning upon H_{twl} won't trivialise the chance of p. Conditioning the chance function on l-level initial history therefore reproduces this important property of a level-indexed chance function.

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[1] This point is emphasized by Loewer ([2001], see esp. p. 610).

[2] My concern in this paper is with what Schaffer (*op cit*., p. 120) calls *posterior* deterministic chances: chances concerning events occurring *after* the first moment of the universe. Schaffer's case against *initial* deterministic chances–concerning events occurring *at* the first moment of a deterministic universe–is somewhat different, and I won't defend such chances here.

[3] In a footnote, Schaffer (*op*. *cit*., p. 115n.) states that he is assuming chance to be a function of only three arguments: propositions, worlds and times. He says (ibid.) that 'whether determinism is compatible with a "chance" function that is relativized to [...] further inputs should be considered a separate question not addressed in the main text.'

Perhaps Schaffer is correct that, *on the assumption that the chance function has only three arguments*, it follows that there are no deterministic chances. But what I am concerned with is whether there are deterministic chances, not whether there are deterministic chances *on this assumption*. Consequently, I regard evidence that there is a fourth argument (such as a *level*) as relevant to my topic. Such evidence will be presented in the course of this paper.

Schaffer suggests that he has some justification for making the three-argument assumption. He says (ibid.) that in his paper the assumption 'will be defended indirectly [...]

insofar as the role of chance [...] will prove explicable in ways that require no further inputs.' But I shall argue below that Schaffer's explication of the role of chance is flawed, and that the flaws can only be remedied by explicitly relativizing chance to a fourth input.

I am far from being the first to argue that chance is relative to more than just propositions, times, and worlds. Hájek ([2003a], [2003b], [2007]), for example, has argued vigorously (and in my view convincingly) that chance must be relativised to a chance setup or reference class. In the appendix, I discuss the relation between my claim that chance is level-relative, and Hájek's that chance is setup-relative.

[4] Lewis (ibid., pp. 91-2) advances arguments for this time- and world-relativity. The issue of whether chance is *fundamentally* time-relative shall be taken up in the appendix.

[5] Schaffer (ibid., p. 121n.) explicitly restricts his attention to deterministic chances pertaining to propositions concerning individual, momentary event occurrences. I shall follow him in this.

[6] The *RP* is a strengthened version of Bigelow, Collins, and Pargetter's *Basic Chance Principle* ([1993], p. 459).

[7] Or at least in *some* such worlds. There might be deterministic worlds lacking probabilistic special scientific laws. I do not argue that there are non-trivial chances in these worlds. My thesis is merely that there are *some* deterministic worlds with non-trivial chances. In particular, there are non-trivial chances in those deterministic worlds that ours might have turned out to be (if it had turned out to be Newtonian) or might still turn out to be (if it turns out to be Bohmian): worlds that are deterministic at the microphysical level but at which there are probabilistic special scientific laws.

[8] This condition creates a difficulty for the view that the Albert package is the Best System for some not-too-distant Newtonian world. The problem is that, in an earlier paper, Lewis restricts candidates for best systemhood to those systems whose axioms refer only to

perfectly natural properties (Lewis [1983], pp. 368-9). Lewis holds that the perfectly natural properties are those to be discovered by fundamental physics (ibid., pp. 365, 368). And, as Schaffer (ibid., p. 130) points out:

> The Albert package contains predicates such as ‘low entropy’ that refer to properties that are not perfectly natural–in microphysical vocabulary, that property is infinitely disjunctive. Hence the Albert package is *not even in the running* for the Lewis laws. It is ineligible from the start.

I think that Lewis’s restriction is unduly severe (he does not mention it in his final statement of his Best System Analysis [1994]). His ([1983], p. 367) justification for it was that the simplicity of a system is relative to the vocabulary in which it is expressed and that, by employing a very unnatural predicate, we might make a strong system very (syntactically) simple indeed. The concern was that, if just *any* predicates are allowed, it is difficult to see how simplicity is a virtue of a system, and how it can put any constraints upon a system’s acceptability.

But it does not follow that a restriction to perfectly natural predicates is required. Naturalness, as Lewis recognises (ibid. p. 368), admits of degrees. And the fact that, as Lewis also recognises (ibid.), our language ‘has words mostly for not-too-unnatural properties’ suggests that we are fairly adept at distinguishing reasonably natural properties from unnatural ones. Simplicity will not be a vacuous criterion so long as we require that the predicates employed in the axioms of the candidate systems refer to *reasonably* natural properties. Requiring *reasonable* naturalness may introduce a certain amount of indeterminacy or subjectivity into the Best System Analysis but, as Lewis ([1994], p. 479) is aware, there is already indeterminacy and subjectivity in the analysis as a result of its employment of the notions of simplicity, strength and balance.

Relaxing a little the requirement of perfect naturalness of the predicates employed in the axioms is perhaps reasonable if the consequence of maintaining it in its strictest form is the exclusion of certain systems, such as the Albert package, which yield great gains in strength.

[9] If a Humean analysis of laws is correct, one might wonder whether laws are genuinely confirmable by their instances and, indeed, whether they can really support counterfactuals and underwrite explanations and predictions. But there is no *special* problem for special scientific laws here. The problem, if there is one, is with Humeanism and not with special scientific laws.

[10] In the case of pea crosses, genotypes are reliably reflected in phenotypes.

[11] More generally, it is the key to the reconciliation of divergent chances projected by the laws of different levels. In the special case of interest to us, where the world in question is micro-deterministic, it allows a reconciliation of the trivial chances projected by the microphysical laws with non-trivial chances projected by higher-level laws.

Note that the reconciliation thus effected between chance and determinism is a relatively weak form of reconciliation: specifically, it is *not* claimed that determinism at a level *l* can be reconciled with non-trivial objective *l-level chances*. This point has as a corollary the observation (footnote 7 above) that there may not exist non-trivial objective chances in *thoroughly* deterministic worlds: that is, worlds in which the laws of *all* levels are deterministic.

[12] Lewis himself ([1994]) recognises that if a Humean view of laws is correct the laws are not fully admissible. He consequently acknowledges that the *RPP* is not a strictly valid reformulation of the *PP*. However, Lewis's reason for taking the laws to be strictly

inadmissible centres around the problem of *undermining futures* and is somewhat different from that which is of concern here. It shall be set aside in what follows.

[13] This is analogous to the manner in which the chances of p_{rg} at times later than t constitute inadmissible additional information relevant to whether p_{rg} is true.

[14] Even this formulation is problematic if a Humean view of laws is correct (and not just because of the problem of undermining futures). On a Humean view the laws of all levels supervene upon history. Consequently, for some level l' ($\neq l$), the initial history H_{tw} may constrain what the l' laws are. If so, someone who knew H_{tw} would have information about what the l' laws and therefore the l' chances are, and would thereby have information about whether p is true that is not just information about the value of $Ch_{twl}(p)$. The proposition H_{tw} would therefore not be fully admissible relative to $Ch_{twl}(p)$, and so even (RPP^*) would not be a valid reformulation of (PP).

The solution to this problem of inadmissibility might be to restrict the historical proposition to be a proposition (H_{twl}) giving information only about the history of w at level l. Such propositions are discussed further in the appendix.

[15] Hoefer ([2007], pp592-3) argues that in fundamentally *indeterministic* worlds reasonable credence needn't track the fundamental level chances in cases of conflict.

[16] This might be Railton's ([1978], p. 224) fantastic event in which 'all the naturally unstable nuclides on earth [...] commenced spontaneous nuclear fission in rapid succession'.

[17] t_c, t_d, and t_e here denote the times at which the subscripted events occur.

[18] At least non-trivial deterministic chances of the *posterior* sort with which this paper is concerned (see footnote 2 above).

[19] The example is due to John Hawthorne, who gave it at a seminar in Oxford on Schaffer's paper. I have embellished it somewhat.

[20] This has been a principal motivation for those (such as Good [1961a], [1961b], [1962]; Reichenbach [1971]; Suppes [1970]; Lewis [1986d]; Menzies [1989]; Eells [1991]; and Kvart [2004]) who have attempted to develop probabilistic analyses of causation. Even those who don't seek a probabilistic analysis tend to agree. For instance, Mellor ([1995], esp. p. 67) takes the tendency for causes to raise the chance of their effects to follow from the 'connotations of causation'.

[21] The level-relativity of chance makes for further subtleties in the precisification of the chance-causation connection. In particular there is the question of *which level's* chance of *e* must (*ceteris paribus*) be raised by *c* if *c* is to be a cause of *e*. Such subtleties are rather similar to those arising from the time-relativity of causation (inequality (1) incorporates the implicit assumption of one possible–and somewhat vague–answer to the question of *which time's* chances are relevant to whether *c* is a cause of *e*).

[22] Caveat: Hájek ([2003a], [2003b], [2007]) has–in my view rather convincingly–challenged the validity of the axiom of conditional probability. Since the above demonstration of incompatibility between the *CCC* and a trivial deterministic chance function depends upon that axiom, it will (at least in its present form) not be found entirely convincing by those who endorse Hájek's arguments.

[23] I have modified Hoefer's notation to render it consistent with my own.

[24] This observation applies just as well to Schaffer as it does to Lewis.

[25] Eagle ([unpublished]) gives such an analysis.

[26] Hoefer (*op cit.*, esp. pp. 562-5) too allows for the existence of well-defined chances conditional upon chance setups rather smaller than the complete initial history of the world. He explores some of the consequences of this, but I shall not enter into a discussion of these here.