Evidence-Based Science

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1. Introduction

How should we go about giving accounts of scientific modal relations, such as laws, chances and counterfactuals? In this paper, I take a *functionalist* approach. What laws, chances and counterfactuals are, and how they should be evaluated, are determined by the roles these modal relations play in our lives and scientific theorizing. [1] I'll suggest that these modal relations are primarily evidential devices that help us reason about the world. While they ultimately allow us to control the world as well, their evidential function is the more basic.

Why take this functionalist approach? Here are two reasons, put very briefly. Firstly, any account of what a 'higher-level' modal relation is needs to be *justified*. Why evaluate counterfactuals, for example, in Lewis' terms rather than some others? The most straightforward way of justifying an account is to show that the relation, thus reduced, is fit to play its role. So, for non-fundamental relations at least, we need to begin with a specification of what the role of a modal relation is, and then consider what plays that role. Secondly, functionalism of this kind is motivated by a form of naturalism regarding metaphysics. According to this form of naturalism, we should only use the posits and standards of science to answer metaphysical questions. We don't use intuition or a distinctive metaphysical methodology to consider what modal relation are, or how they play their role. Functionalism gives us a way of pursuing metaphysical questions about what modal relations are, while respecting naturalism. For any modal relation (whether higher-level or not) we can ask why we theorize about the world using that relation. For a naturalist, this question is answered scientifically, by considering the role of that relation in our lives.

The functionalist approach I'll adopt shares features in common with recent Humean accounts of modal relations (Hicks 2018, Dorst 2019, Jaag and Loew 2020). These accounts not only reduce scientific modal relations to the non-modal, but, more so than Lewis (1983, 1994), explicitly *justify*

[1] My general approach is heavily informed by Price (2013) and Ismael (2017), who also focus on the function of scientific modal relations.

such reductions by showing how the relations (thus reduced) will play the appropriate role.[2] But while the Humean and I are both concerned with function, I reject the further Humean commitment that scientific modal relations reduce to *non-modal* relations. In keeping with naturalism, scientific modal relations should be used in explaining how physically fundamental modal relations are fit to play their roles (Fernandes 2020b)—suggesting a non-Humean analysis of such relations. Nor do I think there are other naturalist motivations to reduce the modal to the non-modal. My focus today, however, will be on cases where the Humean and the non-Humean can largely agree: using physically fundamental dynamical laws and objective probabilities to give accounts of 'higher-level' modal relations, including chances and counterfactuals.

Here's some justification for this approach to higher-level modal relations. Distinguish between 'physically fundamental' modal relations, including the laws and chances of fundamental physics, and 'higher-level' modal relations, including counterfactuals, causation, and the laws and regularities of non-fundamental sciences. This hierarchy of modal relations is well-motivated by scientific reductionism of a relatively weak kind. Take fundamental physics to be the science that aims to scientifically explain the regularities of higher-level sciences. If one is convinced by Russell's arguments (1912−13) that causal relations don't feature in fundamental physics, and Cartwright's arguments (1979) that causal relations are needed in higher-level sciences, then a natural thought is that causal relations are regularities of the higher-level sciences, not of fundamental physics.[3] Similar arguments are plausible in the case of counterfactuals. If so, causal and counterfactual relations can be accounted for *scientifically* using the laws and probabilities of fundamental physics—in keeping with naturalism.

While the focus of the paper isn't on free will *per se*, let me note some implications. Firstly, by thinking of modal relations as primarily evidential devices, the approach suggests that laws and causes are not the kind of things that force, compel, necessitate or metaphysically govern. The approach treats laws and causes are distinct. Moreover, neither is *necessarily* temporally directed as an intrinsic feature of what is is to be a cause or a law. Insofar as arguments against the compatibility of free will and determinism are based on intuitions about laws and causes that compel, the approach

[2] I use 'reduction' as a neutral term to refer to whatever metaphysical dependency relation the Humean adopts.
[3] There is room for disagreement. Woodward (2007, p. 103), for example, is skeptical that there is any illuminating account of causation using fundamental laws to be had.

supports compatibilism. One doesn't have to be Humean to 'deflate' laws and causes of their metaphysical oomph. Secondly, as with Lewis' (1981), Loewer's (2007, 2012), and Albert's (2000, 2015) accounts, the approach distinguishes sharply between what counterfactuals are true, and what causal relations hold. There are things that would be different (were an agent to act in a given way), but which the agent *cannot* control. The past may counterfactually depend on what the agent does, for example, even if the agent does not control the past. For this reason, the approach can diffuse arguments for incompatibilism such as van Inwagen's Consequence Argument (ref!), which rely on intuitions that confuse counterfactual dependence and control. Thirdly, and more speculatively, a thought I've pursued elsewhere (2016) is that our freedom itself might be accounted for in partly evidential terms. We will only deliberative on what we will do, and take ourselves to be free to decide, if various epistemic conditions are met—such as our being ignorant of what we will do. Our conception of ourselves as free may therefore depend on whether the world is evidentially like. I discuss some of these ideas briefly in Section 5.

The paper proceeds as follows. In Section 2 I outline the role of laws and probabilities in evidential reasoning. In Section 3, I use this outline to develop two accounts of how laws and probabilities allow us to reason about the past. In Section 4, I use these accounts to develop an evidential account of chance. Finally, in Section 5, I develop an evidential account of counterfactuals, according to which counterfactual reasoning models idealised cases of evidential reasoning. For those familiar with Albert and Loewer's 'Mentaculus' account of laws and chances, Sections 3−4 present an alternative to that vision. While one could adopt the evidential account of counterfactuals while still adopting the Mentaculus, the alternative is worth exploring in its own right, and motivates the evidential function of counterfactuals (Section 5).

2. The Evidential Role of Laws

In order to develop accounts of higher-level modal relations, I'll begin with some assumptions about the function of laws. Laws are primarily evidential devices. They allow us to reason from the state of a system at one time to its or another's state at some time, given background assumptions. Ecological laws, for example, allow us to reason about what the prey population is like at a given time, given the predator population and other assumptions. While laws play other roles, in explanation, causation and counterfactuals, I take their evidential role to be basic.

In the case of fundamental physics, this rough characterisation can be made more precise. The dynamical laws of fundamental physics allow us to reason from the *complete* state of an isolated system at one time to something about its state at another time. If the laws are deterministic, they allow us to reason from the complete state of an isolated system at one time to its complete state at *any* other time. If the laws are indeterministic, they may imply well-defined transition probabilities in one temporal direction only. This is true of collapse versions of Quantum Mechanics such as GRW. For simplicity, I will assume the indeterministic laws we're concerned with are temporally asymmetric in this way.

There are various other features that make fundamental physical laws useful for evidential reasoning. Some of these features may be essential to the evidential role of laws, while some may be merely desirable. Without settling that issue, here are some features that are, at least, important. Other features may also be required, such computational tractability.

a) Fundamental laws allow us to reason not only about the universe as a whole, but also subsystems of the universe that are 'quasi-isolated' with respect to some variables (Elga 2007). This condition is plausible, given that our direct epistemic engagements with the world are with subsystems.

b) Fundamental laws allow us to reason about the behaviour of systems using macroscopic characterisations, and absent detailed knowledge of their *microstates*—even though microscopic information is required to make the most precise predictions. This condition is plausible, given our direct epistemic engagements with the world are typically macroscopic.

c) Fundamental laws allow us to reason about the behavior of systems, absent complete detail of their *macrostates*. Even for a (quasi-isolated) subsystem, knowledge of its full state is not required to predict the behaviour of parts of it (Elga 2007). This condition is plausible, given we often don't have detailed knowledge of complete macrostates, but still can reason about the behaviour of parts of systems.

If fundamental dynamical laws are to allow us to reason about macrostates, absent detailed knowledge of microstates, there must be a way of relating macroscopic and microscopic information. One way is to employ a probability distribution that specifies a probability measure over microstates consistent with a given macrostate at a single time. Even though this probability distribution has implications for states at other times, it applies, in the first instance, to states at a single time. In the case of deterministic laws, a probability distribution with at least *some* particular features is required to derive subsequent macroscopic behaviour from macroscopic information. In the case of indeterministic laws, a probability distribution may not be required. Indeterministic dynamical laws, such as those of GRW, specify 'transition probabilities'—probabilities for later microstates given earlier microstates. In the case of GRW, *any* probability distribution will lead to (roughly) the same probabilities for later *macrostates* after a short time (Albert 2000, Ch. 7).[4] If so, the particular probability assignment is irrelevant to deriving the subsequent macroscopic behaviour, and so may not be part of the objective content of science. To allow for this view, I will not assume an objective probability measure in the case of indeterministic laws.

In the case of deterministic laws, a probability distribution is required. But there is debate over whether one should employ a *particular* probability distribution, typically given by the Lebesgue measure (Albert 2000, Ch. 3; 2015, Ch. 1; Loewer 2007), or simply require that it satisfy certain minimal conditions, such as being continuous with the Lebesgue measure (Maudlin 2007b)—perhaps features so minimal that the probability distribution is not part of the objective content of science. My own stance is that the probability distribution *is* a matter for science, and that considerations of simplicity suggest a particular distribution, given by the Lebesgue measure. The probabilities are just as objective as the dynamical laws, given that both are required for scientific derivations of macroscopic behaviour. However, while I will assume objective probabilities in what follows, it will not matter to the structure of the accounts if one takes the probabilities to be less objective than the laws. The consequence will simply be that chances and counterfactuals are less objective than the laws. I will also assume a particular probability distribution given by the standard Lebesgue measure (while noting alternatives). Albert and Loewer call this postulate the 'Statistical Postulate', when applied to the initial macrostate of the universe. Because I will apply the postulate to other times, typically to the full phase space of a system, I will use a different name: the 'Lebesgue Postulate'. I remain neutral on whether the probability distribution is a law. I will, however, hereafter use 'law' to refer only to fundamental dynamical laws.

[4] If this condition is not satisfied, a probability distribution will also be required.

There are some further details needed to understand the full the role of laws and probabilities in evidential reasoning. I will allow these to emerge in the accounts of how we reason about the past (Section 3), chances (Section 4), and counterfactuals (Section 5).

3. Reasoning about the Past

In this section I develop two accounts of how laws and probabilities allow us to reason about the past. These accounts form the basis of the accounts of chances and counterfactuals (Sections 4−5).

Section 2 outlined the evidential role of laws. But more needs to be said to explain how laws allow us to reason about the past—events earlier than our 'current' location in time. In the case of indeterministic laws (with well-defined probabilities in one temporal direction only), laws don't directly define transition probabilities for earlier events given later events. So, they don't directly determine how we should reason about the past. In the case of deterministic laws, there are other problems (Albert 2000, Ch 6). When reasoning towards the *future* macrostates of an isolated system, deterministic dynamical laws and the Lebesgue Postulate applied to the system's present macrostate allow one to accurately derive future macrostates. Albert calls this future-directed method 'prediction'. But prediction will not generally work when reasoning about past macrostates—what Albert calls 'retrodiction'. Retrodiction will always lead one to infer that isolated systems at non-maximal entropy were *higher* entropy in the past, since higher entropy states are extremely probable, given a present macrostate at non-maximal entropy, dynamical laws and the Lebesgue Postulate.[5] Yet all our evidence suggests that the entropy of isolated systems increases only towards the future in the vast majority of cases—a generalisation captured in the Second Law of Thermodynamics. So, we must use another method to reason about the past.

I will give two accounts of how we reason about the past. The first, the *Method of Forwards Evolution*, expects the *forwards* evolution of a system in time to be probable. This method aims to be one we would recognise ourselves as employing. According to the method, we reason in such a way that we expect the forwards evolution of an isolated system in time to be probable—where the probabilities are derived either from the dynamical laws (in the case of indeterministic laws), or from the dynamical laws and the Lebesgue Postulate (in the case of deterministic laws). When reasoning

[5] High entropy states are equilibrium states those whose macrostates occupy a large volume of the phase space of the system at a given time. They can, roughly, be thought of as highly disordered states.

about the past, we reason to a past state that implies the evolution of the system towards its known future states was probable. When reasoning towards the future, we reason to a future state that is probable, given known past states. The method has an in-built temporal asymmetry, since it requires the forwards evolution of a system to be probable, but not its backwards evolution.

To lay out the method in more formal terms, I will assume the relevant laws and probabilities are *known*.[6] The *Method of Forwards Evolution* requires one to have (other things being equal) higher credences in states of systems in the past or future that imply that an isolated system's forwards evolution was *probable*, and lower credences in states of systems that imply that the isolated system's forwards evolution was *improbable*—where (other things being equal) the credences are *proportional* to the relevant probabilities. The relevant probabilities (P) are given either by transition probabilities (in the case of indeterministic laws) or the Lebesgue measure applied to the full phase space of the system at the earliest time (in the case of deterministic laws). In more formal terms, one should have (other things being equal) proportionally higher credences in states such that P(later(U+K)|earlier(U+K)) is higher, where 'earlierU+K' includes *all* information about the state of the system at the earliest time (*t*), where that information is known (K) or unknown and of interest (U), and 'laterU+K' includes *all* information about the system *after t* that is known (K) or is unknown and of interest (U). That is,

$$Cr\ (U|K) \propto P(\text{later}(U+K)|\text{earlier}(U+K))$$

While the method is simpler to envisage when one has complete macroscopic information about the system at a time, the general requirement is that all and any (even incomplete) known information is included. This information is always organised temporally.

Before considering further details, it will be helpful to see how the method applies in some simple cases. Assume one knows the present macrostate of an isolated system at non-maximal entropy, knows that the system is in the middle or a long period of isolation, and has no other information about its past or future states. When reasoning about the future state of the system, ones credences

[6] A more general formulation would consider credences over the relevant laws and probabilities (Lewis 1986). Alternatively, the method could be taken to 'build in' features of how we *should* reason using laws and probabilities (an idealisation) even if the laws and probabilities are not known.

should be determined by P(laterU|earlierK), where 'laterU' is its unknown future state, and 'earlierK' is its known present state.

$$Cr(laterU|earlierK) = P(laterU|earlierK)$$

One's credences are determined as in the method of prediction, leading one to infer the entropy of the system will be higher in the future.

When reasoning about the unknown past state of the system ('earlierU') given knowledge of its later macrostate ('laterK'), the relevant credence, Cr(earlierU|laterK), must be calculated *indirectly* using the probabilities for its present state, given unknown past states (P(laterK|earlierU)), and 'prior' credences concerning past states—more on which in a moment. Using Bayes' theorem:

$$Cr(earlierU|laterK) = P(laterK|earlierU).Cr(earlierU)/Cr(laterK)$$

Comparing two unknown past states, U1 and U2, and taking credences for later states given earlier states to be determined by the probabilities P (as above), we can derive:

$$\frac{Cr(earlierU1|laterK)}{Cr\ (earlierU2|laterK)} = \frac{P(laterK|earlierU1).Cr(earlierU1)}{P(laterK|earlierU2).Cr(earlierU2)}$$

While I'm making use of Bayes' theorem, unlike the general form of Bayesianism, the 'likelihoods', P(laterK|earlierU), are necessarily objective and are determined by laws and a probability distribution applied to the total phase space of the system conditionalised on earlier states (in the case of deterministic laws) or probabilities from the dynamical laws given earlier states (in the case of indeterministic laws). By conditionalising on earlier states, the method builds in a temporal asymmetry that is not built into Bayesianism as such.

Provided the 'prior' credences for U1 and U2 are not too dissimilar (what was implied earlier by 'other things being equal'), one will tend to have higher credences in past states that imply future known states are probable. So one will infer that the entropy of an isolated systems at non-maximal entropy was *lower* in the past, since only a lower entropy past state would make the system's forwards evolution towards its present mid-entropy state, P(laterK|earlierU), probable. A high entropy part would imply a *very* improbable forwards evolution. If so, the *Method of Forwards Evolution* will allow one to reason that the entropy of an isolated system at non-maximal entropy was lower in the past. The method also allows one to reason that the entropy of the whole universe was lower in the past, since only a lower entropy past state implies the universe's current mid-entropy state is probable. The method even allows one to reason to the particular state the universe begun in, what Albert calls the 'Past Hypothesis' (2000), given sufficiently knowledge of its later states. Unlike on Albert's approach, the Past Hypothesis is not an input to reasoning, but as a state one might reason to.

It's time to address the important business of the priors. Provided the priors are not extreme (not 1 or 0), the method requires one to *increase* one's credences in past states on which future evolutions are probable. But, without some further restriction on the priors, one might still never have particularly high credences in low entropy past states. In particular, if one were to set one's credences in past states by the Lebesgue Postulate, low entropy past states would have such low priors that we would not never infer to them. Nor can we ignore the priors—to do so is effectively to take any past states to be equally probable, which implies inconsistencies. One might adopt a particular credence distribution, such as taking all past *macrostates* to be equally probable. But I'm inclined to think the more realistic option is the standard Bayesian approach: for all 'reasonable' priors, the contributions on the priors will be swamped by appropriate updating, where reasonable priors are those that don't take low entropy past states to be highly improbable.[7]

The *Method of Forwards Evolution* is a form of reasoning we might recognise ourselves as employing. I suspect this is partly due to its in-built temporal asymmetry. Insofar as we think of the past as 'fixed', it feels unnatural to think of there being objective probabilities for earlier events conditional on later events. The *Method of Forwards Evolution* only makes use of objective probabilities for later events given earlier events. While there is nothing to prevent one using past-directed objective probabilities in the case of deterministic laws, I suspect our familiarity with indeterministic temporally asymmetric macrolaws, and other temporal asymmetric phenomena, makes past-directed probabilities seem unnatural.

[7] I confess there is something unsatisfactory in all this. But I take myself to be in good company. The same kind of problem arises if one uses subjective Bayesianism to explain how we reason *to* the Past Hypothesis (refs!)—one's initial credence in the Past Hypothesis cannot be too low, or one will never be led to infer to it.

Why is the *Method of Forwards Evolution* temporally asymmetric? In the case of indeterministic laws, the answer is relatively straightforward—the dynamical probabilities are well-defined in one temporal direction only, suggesting our reasoning will be different towards the past and future. But in the case of deterministic laws past states do have well-defined probabilities conditional on information about later states—so this answer cannot be given. One might use the 'Past Hypothesis' (the posit that the universe begun in a particular low entropy state) to explains the method's temporal asymmetry (Albert 2000). However, one doesn't need a posit about the *particular* state the universe begun in to explain the asymmetry. All one needs is a more minimal posit that the universe started out in *a* low-entropy state—sufficiently low such that its evolution to our present time implies that we are still in the middle of a long entropic upgrade.[8] Given the universe *begun* in a low-entropy state, and isn't currently heading towards a future lower entropy state, its evolution implies the *Method of Forwards Evolution* is reliable, but its temporal inverse would not be. According to this explanation, the *Method of Forwards Evolution* is reliable because it is aligned to the entropic gradient of the universe. In addition to being more minimal, this entropic explanation allows one to also derive temporal asymmetries in worlds that start out in a different low-entropy macrostate—unlike the Past Hypothesis—making the explanation more general. For these reasons, I prefer this entropic explanation over the Past Hypothesis.

By using this entropic explanation of the *Method of Forwards Evolution*, there is a kind of circularity. The *Method of Forwards Evolution* allows one to reason that the entropy of the universe was low in the past and rises towards the future. The fact that the entropy of the universe was low in the past and rises towards the future explains *why* the *Method of Forwards Evolution* is temporally directed. One direction tracks explanation, the other tracks how we reason. I don't take the circularity to be vicious—it is to be expected when we use posits we reason *to* in order to explain *how* we reason.

While the temporal asymmetry of the *Method of Forwards Evolution* is acceptable, there are sometimes advantages to having a method that does not build in a temporal asymmetry. One such method is the *Symmetric Method*. The *Symmetric Method* is not likely to be a method we would recognise ourselves as employing (at least when directed towards the past). But it can still determine what inferences are

[8] The proposal is similar to Reichenbach's (1956) posit of a long entropic upgrade.

justified or reliable, and so explain our ability to reason accurately, if our methods of reasoning are sensitive to inferences licensed by the *Symmetric Method*.

Take the dynamical laws and the Lebesgue Postulate, applied to full phase space an isolated system at any time, to define probabilities (Prob).[9] Conditionalise on what is known about states of the system at any time (K). According to the *Symmetric Method*, one's conditional credences in unknown states, U, should be determined by Prob(U|K):

$$Cr(U|K) = Prob(U|K)$$

One would expect such a method to fail when reasoning about the past, by leading one to infer that the entropy of an isolated system at non-maximal entropy was higher in the past. However, if one *knows* a system begun (or was isolated) in a low entropy state, this information is part of K. If one conditionalises on this low entropy initial state, one will not be led to infer the system was at higher entropy. I suggest that other methods, including the *Method of Forwards Evolution*, allow one to infer to low entropy past states. When such states are conditionalised on, the *Symmetric Method* will then be reliable in determining what other inferences are licensed.

Contrast this approach with Albert's (2000, 2015) and Loewer's (2007). Albert and Loewer argue that our reliable inferences to the past are those licensed by statistical-mechanical probabilities, given by the Statistical Postulate (the Lebesgue Postulates applied to the initial macrostate of the universe), the dynamical laws, and the Past Hypothesis (PH): Prob(U|K.PH). The Past Hypothesis features as a postulate in our reasoning. It is the ultimate 'ready state'—an initial constrained state such that, when reasoning about the past using present information, we always reason to an unknown state at a time between two known states—what Albert calls 'measurement'. According to Albert, it is the fact that there is a Past Hypothesis, but no Future Hypothesis, that ultimately explains why we can measure the past and not the future.

As Albert is aware, if the Past Hypothesis is to explain temporal asymmetries in how we reason, we must have knowledge of the Past Hypothesis. So he gestures at a broadly (subjective) Bayesian

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[9] The *Symmetric Method* only applies in cases where the laws are deterministic. If one uses an alternative accounts of probabilities in deterministic settings, such as a typicality approach, the *Symmetric Method* may not be available.

account, whereby evolution, experience and explicit study have led us to accept the Past Hypothesis, at least implicitly (2000, pp. 118−9; 2015, pp. 16−7, 39). The success of the Past Hypothesis in allowing us to reliably predict future observations (and in render other beliefs of ours compatible) accounts for why we believe it (2000, p. 119). Our epistemic access to the Past Hypothesis is similar to our access to the dynamical laws—not surprising since Albert counts the Past Hypothesis as a law. The question of how we reason to the Past Hypothesis is, in fact, folded into the question of how we reason to the whole 'Mentaculus' package of the Past Hypothesis, the dynamical laws and the Statistical Postulate (2015, pp. 16−7, 39). For this reason, there are substantially different accounts to be offered concerning our reasoning towards the Past Hypothesis and our reasoning towards other past states.

By contrast, I suggest that the Past Hypothesis is a contingent state that we reason *to* using the *Method of Forwards Evolution*, just as we reason to other past states. It is also a state we might reason *from* using the *Symmetric Method*, just as we reason from other past (and future) states. The question of how we reason to the Past Hypothesis does not need to be packaged in with the question of how we reason to dynamical laws and a probability postulate. We can separate out a method for how we reason to the Past Hypothesis (and other past states), once laws and probabilities are in play. An explicit story of this kind can answer skeptical worries about how we reason to the Past Hypothesis (refs!), by showing this as simply part of the ordinary ways we reason about the past. It also allows one to give an account of how specifically *modal* relations allow us to reason evidentially—something the functionalist is concerned with, and that will be used in developing accounts of chances and counterfactuals (Sections 4−5).

One might respond that it is precisely because we don't know the *full* Past Hypothesis that it has some role to play in our reasoning beyond that of a known contingent state. However, even if one takes the Past Hypothesis to be an 'ideal' we're heading towards, this does not distinguish its role from other contingent states. Other contingent unknown states are also 'ideals' in this sense. I also argued that the full Past Hypothesis was not required to explain the temporal asymmetry of the *Method of Forwards Evolution*: just an appropriate entropy gradient. A similar argument applies in the case of other temporal asymmetries in our reasoning. If lawhood is to be characterised using the evidential function of laws, then the Past Hypothesis is not a law. By its function, it is merely a contingent state. If so, this is a reason to reject Albert's and Loewer's 'Best Systems' Humean account of laws. Even if the Past Hypothesis helps systematize patterns in the Humean mosaic, by its evidential function, it is not a law.

In this section I've offered two accounts of how we reason—the *Method of Forwards Evolution* and the *Symmetric Method*. The two methods are complementary; neither rules out the other, and each has its advantages. The *Symmetric Method* has no temporal asymmetry; the *Method of Forwards Evolution* is available regardless of whether the laws are deterministic or indeterministic. I've also argued that the Past Hypothesis functions as a contingent state—not a postulate of how we reason. These results will be used to develop accounts of chance and counterfactuals and to make sense of their temporal asymmetries.

4. Chances

Chances are objective probabilities that apply in the single case and that are 'worldly': they are not mere recommendations for what we should believe but are features of the world. Roughly put, chances are as 'worldly' as the fundamental dynamical laws. But even though the above is and is regularly accepted as a *necessary* condition on chance, arguably it is not a *definition* of chance. For example, even though transition probabilities and probability distributions are part of fundamental physics (and presumably as objective and worldly as the laws), it is often argued there are further criteria that probabilities must satisfy in order to be chances—see, for example, Ismael (2011) and examples below. In this section, I show how objective probabilities and laws *are* enough to account for what chances are. I will do so by considering the role of probabilities and laws evidential reasoning.

According to the 'evidential account of chance', dynamical laws, an appropriate probability distribution (in the case of deterministic laws) or transition probabilities (in the case of indeterministic laws), and other information about contingent states define conditional *chances*: chances for contingent states (A) conditional on other contingent states (B): Prob(A|B) or P(A|B). In the case of deterministic laws, one uses the *Symmetric Method*, Prob(A|B), and there are no restrictions on what information is included in B—so chances are well-defined towards both the past and future. I'll call such chances 'deterministic chances'. In the case of indeterministic laws, P(A|B), B must include information only about states *before* the states included in A—so chances are only defined towards the future. I'll call such chances 'future-directed chances'. While these chances are temporally asymmetric, their temporal asymmetry arises only from an asymmetry in the transition probabilities—it is not built into the nature of chance that chances are temporally asymmetric.

Chances, as just defined, guide credence in a straightforward way.[10] In the case of deterministic chance, one considers probabilities for states that are unknown (U) conditional on states that are known (K): Prob(U|K). Using the *Symmetric Method* (Section 3):

$$\text{Credence}(U|K) = \text{Prob}(U|K)$$

In the case future-directed chances, the relevant chances are of the form P(later(U+K)|earlier(U+K)). When reasoning 'towards' the future, one's credences are given simply by:

$$\text{Cr}(\text{later}U|\text{earlier}K) = P(\text{later}U|\text{earlier}K)$$

When reasoning towards the past, however, the *Method of Forwards Evolution* must be used (as formulated above, Section 3); future-directed chances can only update prior credences in past states.

According to the evidential account, the chances that determine how one should reason about other states are typically probabilities conditional on what one *knows*. In the deterministic case, if the probability of A conditional on the contingent states one knows obtain is high, then one should have a high credence in A. A quick thought might be that probabilities conditionalised on whatever states one happens to know simply can't be chances. These probabilities may seem subjective or evidential, and not objective and worldly. But that is too fast. According to the evidential account, chances are always *conditional*. Their value depends on what information is included in A and B—not on whether A and B are known. Even though the chances *relevant* to a reasoner will typically depend on what they know, the *values* of the conditional chances do not depend on what they know.

The most unusual feature of this evidential account of chance is that, beyond the minimal posits required to define the relevant probabilities (and so a temporal restriction in the case of future-

[10] In these formulations, I assume the relevant chances are known—see footnote above.

directed chances), there are no further restrictions on what information about contingent states can be conditionalised on.[11] This is what allow chances to play a direct credence-guiding role. If one characterises chance by its role in evidential reasoning, this is a strong advantage.

A more standard approach is to argue for further restrictions on what count as chances: chances are only well-defined when they are conditionalised on *all* information that is 'admissible'. Lewis (1986) and Schaffer (2007), for example, take information about all prior history (H) to be admissible, so that the chance of an event E occurring, Ch(E), is given by the objective probability of E conditional on its prior history H, P(E|H). Therefore, Ch(E) does not change its value when further information about the past (H1) is conditionalised on: Ch(E) = P(E|H) = P(E|H.H1). Under the evidential account, by contrast, there are no criteria for admissibility beyond the minimal requirements required to define probabilities. So there is no guarantee that the relevant chance value will not change on further conditionalising.

There are now-standard reasons for rejecting Lewis' particular criteria for admissibility. Firstly, by making information about the full history admissible, Lewis rules out their being any non-trivial chances if the laws are deterministic. But one needs objective probabilities to derive generalisations about macroscopic behaviour—such as the Second Law of Thermodynamics—giving us strong reason for treating such probabilities as chances (Loewer 2001; Emery 2016). Secondly, by conditionalising on past history, Lewis' account implies that chances conditional on some information about later events, 'past-directed chances', *always* take trivial values—1 if the event occurred, 0 otherwise. Lewis (1986) and Schaffer (2007) defend the rejection of past-directed chances by appeal to intuitions about the fixity of the past. However, if our focus is on the role of chance in our practical and theoretical lives, and we are naturalists, we should be suspicious of these intuition-based arguments. We do sometimes need to reason about the probability of earlier states given information about later states, and (non-trivial) past-directed chances should guide our reasoning in such cases when they are able to.

However, rather than rejecting admissibility *per se*, the more standard move is to revise Lewis' criteria for admissibility. Statistical-mechanical accounts of chance, for example, take the Past Hypothesis to

[11] See Hall (2004) for another account that denies additional restrictions.

be admissible (Albert 2000, Loewer 2007). However, while these accounts are improvement on Lewis', by allowing past-directed chances and deterministic chances to take non-trivial values, they still grant too much to notions of admissibility. While we may *know* the Past Hypothesis, and so chances conditional on it are particularly relevant, this is no reason to restrict chances to those conditional on the Past Hypothesis. As I've argued above (Section 3), the Past Hypothesis' role in evidential reasoning can be accounted for by its being a known contingent state. Moreover, even if the Past Hypothesis is crucial in explaining temporal asymmetries (something I was skeptical of, Section 3), this does not grant it a special function—its role in explanation can still be accounted for by treating it as a contingent state. We use contingent states to explain generalisations.

Altogether, if the central role of chance is to guide our evidential reasoning, we have reason to reject notions of admissibility. Instead, dynamical laws and (in deterministic cases) a probability distribution define conditional chances. Which chances are relevant to guiding our reasoning in any given case will depend on what contingent states are known.

5. Counterfactuals

In this final section, I develop an evidential account of counterfactuals. The account is evidential in the sense that it uses the role of counterfactuals in evidential reasoning to justify standards for how counterfactuals are evaluated. One of the major departures from more standard accounts will be that counterfactuals are not to be evaluated using minimal spatiotemporal departures from actuality, but by identifying 'branch points': points in time when the counterfactual antecedent had a reasonable probability of coming about.

The *Method of Forwards Evolution* (Section 3) requires the evolution of a system forwards in time to be probable. Based on this thought, the evidential account of counterfactuals requires us to reason counterfactually to states before the time of the antecedent that would lead to the antecedent being satisfied with 'reasonable' probability—thus keeping the forwards evolution of the system probable. Consider a time t, where t is simultaneous with or as close as possible earlier than the time of the antecedent (t_A). t is chosen such that the antecedent coming about and the antecedent failing to come about are both 'reasonably probable'—where the relevant probabilities are those conditional on *certain* states up to or at at t—'Prior'. Both P(A | Prior) and P(¬A | Prior) must be reasonably probable. What counts as 'reasonably probable' will depend on the systems dynamics. For most ordinary cases, as a simplification, 'reasonably probable' can be taken to imply a probability of approximately 0.5.[12] Call the state of the system at *t* the 'branch point' (see Figure 1). The following counterfactuals are then true: 'If A were to be the case, C would have a probability of P(C|Prior.A)'. 'If A were *not* to be the case, C would have a probability P(C|Prior.¬A)'. *Mutatis mutandi* for ¬C. A is counterfactually *relevant* to C just in case Prob(C|Prior.A) is not equal to Prob(C|Prior.¬A).

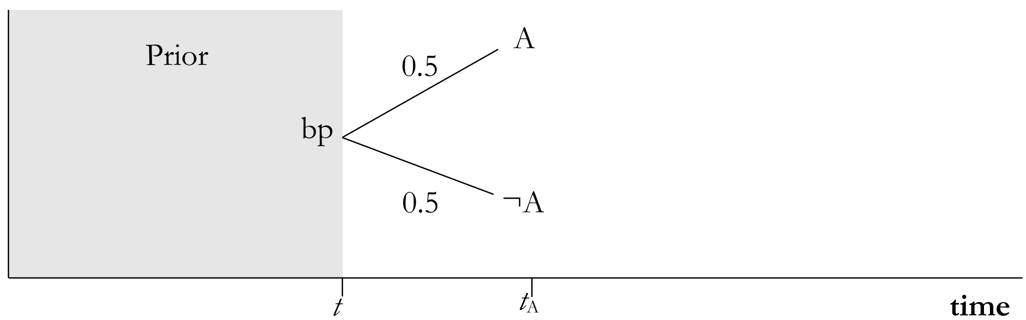

Figure 1: In evaluating counterfactuals, one considers the state of the system at a time *t* (the branch point, 'bp') at which the probability of A and the probability of not A are both reasonable, where the probabilities are conditional on (some) states up to and including the branch point (Prior).

On the evidential account, counterfactuals have probabilistic consequents. Counterfactuals with non-probabilistic consequents are approximations of these. The counterfactual 'if A were to be the case, C would be the case' can be treated as roughly true just in case P(C|Prior.A) is very high. 'C counterfactually *depends* on A' can be treated as roughly true just in case P(C|Prior.A) and P(¬C|Prior.¬A) are both sufficiently high.[13] What counts as 'very high' and 'sufficiently high' will depend on the dynamics of the system and other features. I won't attempt to settle these standards, as I take counterfactual dependence to be an approximation of the more precise relation of counterfactual relevance. But I will sometimes talk of 'dependence' for grammatical ease.

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[12] More precisely, one 'rewinds' to a branch point as far back such that the probability of A and ¬A are both as high as possible, provided there are not later temporal points for which the probability of A or ¬A is closer to 0.5. In cases where states never have a probability as high as 0.5, lesser probabilities will therefore suffice, right down to a limiting case where A (or ¬A) is very improbable. I suspect there may be limits on how far back in time one is willing to place the branch point. There may also be problematic cases where potential branch points further in the past cannot be reached, because of intermediate states which take the probability of A further from 0.5. I take the vagueness on these points to reflect the fact that counterfactuals are idealised approximations of evidential reasoning (discussed below).

[13] One could adopt more standard Lewisian semantics where 'If A were to be the case C would be the case' is true if A and C are both true of the actual world. Because I take the major role of counterfactuals to concern counterfactual relevance, and the more precise formulations to be probabilistic, I don't adopt that assumption here.

According the evidential account, 'Prior' includes only *certain* states. There are different ways the account could be precessified. I will commit to the following here. Prior includes the full state of the universe *up to and at t* (if the laws are indeterministic) or the full *macrostate* of the universe *up to and at t* (if the laws are deterministic).[14] By including the full state (or full macrostate) up to and at *t* in Prior, later states will not counterfactually depend on states prior to the branch point (in the case of indeterministic laws), or will not depend on macrostates prior to the branch point (in the case of deterministic laws).

The evidential account takes counterfactuals to model certain idealised cases of evidential reasoning. The above way of precessifying the account takes information at the time of the branch point and prior to be 'accessible' and therefore unchanged in counterfactual scenarios, in a way information about later states is not. In the case of deterministic laws, the account also takes macroscopic information to be accessible in a way microscopic information is not. These idealisations are approximations. Regarding the limitation to macrostates, there may be cases where we have knowledge of microstates in the *past* of or at *t* that are relevant. Microstates could be included in Prior, provided they still allowed *some* Prior to be identified.[15] Below I'll consider why Prior is limited to information at or prior to the branch point.

A nearby alternative would be to take Prior to include only the full state of the universe *at t* (indeterministic laws) or the full macrostate of the universe *at t* (deterministic laws), and (perhaps) further relevant known past states (such as the Past Hypothesis)—something much closer to Albert (2002, Ch. 6) and Loewer's (2007) statistical mechanical accounts. This alternative might seem preferable: it would allow (macroscopic) counterfactuals consequents prior to the branch point to have non-trivial values. However, (macroscopic) antecedents will still never be counterfactually relevant to states prior to the branch point, since the state at *t* largely screens off any such probabilistic dependency.[16] So it will do no harm to help ourselves to an idealisation that holds the (macroscopic) past of the branch point 'fixed'.

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14 One could also restrict Prior to the macrostate in the case of indeterministic laws.
15 One would then face choices about *which* microstates are to be included. See Kutach (2002) for an example of how one could hold some microstates fixed while still using deterministic chances. My preference would be to include microscopic information that is known.
16 See Loewer (20007). There may only be dependence when a macroscopic event in the past is correlated with a *microscopic* event at *t*.

It is worth keeping in mind that holding the present fixed *or* holding the past and present fixed are both idealizations, given we never know the full state at *t*. More generally, the vagueness of what is included in 'Prior' reflects that fact that counterfactual reasoning is an idealisation—designed to capture something that holds by and large of our evidential reasoning, but nothing more precise than that.

In other work (Fernandes 2020a, 2020c, Forth.a) I use cases of time travel to motivate more subjective alternatives, which take Prior to include only information *known* or *directly available*. These subjective alternatives do better in contexts involving backwards causation, time travel and causal loops. So they may provide a more general account of counterfactuals. The justification for the evidential account of counterfactuals I defend here is tethered to the justification for the *Method of Forwards Evolution*—in contexts where that method fails or is ill-defined, the evidential account is no longer ideal. But it remains defensible as an idealisation that holds of the actual world, given its contingent features.

Having laid out the evidential account, and defended some of its features, I will spend the remainder of this section considering what justifies the evidential account, and how it differs from others.

The evidential account is primarily justified in evidential term. The method involves evaluating counterfactuals using probabilities and the *Method of Forwards Evolution* because such a method is a reliable evidential guide. In cases where the *Method of Forwards Evolution* is reliable, the evidential account models how one *should* reason about a counterfactual consequent (C), given an antecedent (A)—whether or not the antecedent or consequent is known. In cases where one of these states is known, the account allows one to reason *from* A to C (or vice versa). In cases where *neither* state is known, the method delivers hypothetical information—what one would learn, on learning A or C—and so can direct further information gathering. In cases where both A and C are known, the method allows one to ascertain the evidential relation between them, and the probabilistic structure underlying that relation—which will be helpful in relevantly similar cases where at least one state is not known.

In fact, it is only in cases where A and C are both known that we *need* counterfactual reasoning at all. In cases where at least one state is not known, we can simply reason using chances (Section 3). Only in the case where A and C are both known do we have to make assumptions about what information we'd have access to in relevantly similar circumstances—and hence an idealisation features in counterfactual reasoning that is absent from probability and chance-based reasoning. On this point I take issue with Dorst (Forth.). Dorst and I agree that laws play an evidential role in evaluating counterfactuals. But, *contra* Dorst, cases where A or C are unknown are insufficient to justify standards for how counterfactuals are evaluated.

With an evidential justification in mind, we can address an outstanding piece of business: whether future states should ever be included in Prior. Some accounts of counterfactuals adopt the feature 'hindsight': chancy events in the future of an antecedent are sometimes held fixed when evaluating counterfactuals (Maudlin 2007a, Ch. 1; Dorst Forth.). These accounts motivate hindsight as follows. Say you don't bet on Heads (¬B), then a chancy coin is tossed (T), and it lands Heads (H). It seems that following counterfactual is true: 'if you had bet on Heads, you would have won'. So it seems chancy events such as the coin landing heads (H) should be held 'fixed' in counterfactual scenarios, even if they are not implied by the state of the system at (or prior to) the time of the antecedent and the dynamics. Maudlin, for example, holds fixed events in the future of the antecedent fixed, provided they are not 'infected' by changes implied by the antecedent—a requirement that is meant to keep events in the causal future of the antecedents unfixed.

The evidential account does not hold events in the future of the antecedent or the branch point fixed. The justification for this feature is that events in the future of the branch point aren't included in the idealized evidential situation counterfactual reasoning aims to capture. Consider the coin toss case. In this case, the branch point is likely the time of your deliberation, t_D,—your bet is probabilistically 'unsettled' at that point.[17] At t_D, furthermore, there is no evidence that settles the coin landing heads (H). If there were, it would not be a chancy coin toss. Given heads (H) is no more probable than tails given the evidence available at t_D, insofar as counterfactuals are guide your evidential reasoning at t_D, H should not be held fixed. If one is interested in choosing B or not, or interested in figuring out what one can reason to concerning the outcome of similar coin tosses, one

[17] I discuss below why our deliberations are typically branch points.

can do no better than assigning equal probability to H and ¬H. We may have intuitions to the contrary that lead us to regret our failed bets. I suspect this is because, in non-chancy cases, such outcomes can be known in advance, and we adopt this expectation even in the chancy case.

There will still be cases where we hold H fixed when reasoning evidentially. For example, to take a case from Dorst (Forth.), we might hold fixed the chancy event of a car's anti-lock braking system failing to engage when working out what observed tire tracks should lead us to expect about the car's earlier speed. But, *contra* Dorst, this doesn't justify hindsight. Probabilities already allow us to reason evidentially in such cases. In cases such as this, we simply use our knowledge about what happened at any time to constrain our reasoning about the unknown. We do not need to hold other parts of the unknown fixed. If counterfactuals are to be defended as something distinct from mere probabilities, they must serve some further function.

The justification for the evidential account is evidential. But considerations of causation, agency and control have a role to play. Part of our reason for being particularly interested in probabilistic structure exhibiting these branch points is that they are especially relevant for us as *agents* who deliberate and decide. At the time at which we deliberate, our decisions and subsequent actions are often unsettled by probabilities determined by present and previous states (Prior). Our deliberations typically take place *at* branch points with respect to our decisions and actions.

Why do our deliberations typically take place at branch points? I suspect that the evidential 'underdetermination' of our decisions arises from the complexity of decision-making: we respond to a variety of evidence and reasons in nuanced and often unique ways, sometimes by reflecting on and revising the decision-process itself. [18] Furthermore, good evidence of our deliberations often being branch points comes from norms on deliberation. It would seem to make little sense to deliberate if we were already certain of our decisions and actions. While sometimes there may be evidence which we don't have (or ignore), typically our decisions and actions won't be determined by macroscopic evidence in the world at the time of deliberation.

[18] For more on the evidential underdetermination of decisions, see Fernandes (2016, 2017). My account draws on Price (2012), but differs in not being concerned with evidential relations that hold merely from the deliberator's *perspective*. I also draw on Ismael (2012), but differ from her in not assuming agents are always free to thwart evidence of how they'll decide.

That said, I don't take it to be a *necessary* feature of decision-making that our deliberations are branch points. We might sometimes be reliable responders, such that our decisions are evidentially determined by previous states (Fernandes Forth.b). In such cases, the relevant branch point are earlier than the states we reliably respond to. Because I don't rule out such cases, a different response will have to made to why we don't count as *influencing* the past in such cases. I develop the full account elsewhere (2017). The core idea, based on Price (1991), is that available evidence at the time of deliberation screens off correlations towards the past *even if the deliberation is not at a branch point*—implying our decisions won't raise the objective probability of past states, given our available evidence while deliberating. Causal relations are defined as evidential relations we could use in (reasonable) deliberation to raise the probability of outcomes we seek, given available evidence while deliberating. So there are no causal relations between states we might decide on now and previous states. While this 'deliberative' account of causation is apt to seem anti-realist, what should be clear from the above is that to tie causation to evidence is not to be anti-realist about causation—but to link causation to the most fundamental relations of science—dynamical laws and probabilities.

A final feature of the evidential account of counterfactuals to consider is its temporal asymmetry. The evidential account builds in a temporal asymmetry by requiring the branch point to be *prior* in time to the antecedent. This may seem to illicitly build in a temporal asymmetry 'by hand'. I have three complementary responses. Firstly, the account I defend does not reduce causal relations to counterfactual relations. So, even if counterfactuals are evaluated in temporally asymmetric terms, this does not prevent us providing a substantial account of why causation is temporally asymmetric at our world (2017, and above).

Secondly, even though the account is temporally asymmetric, it does not rule out backwards counterfactual dependence. In fact, it allows for more substantial forms of backwards counterfactual dependence than more standard accounts. The evidential account allows for the past to depend counterfactually on the present, whenever the consequent is *after* the branch point and *before* the antecedent. If the consequent is after the branch point it may well counterfactually depend on the antecedent, implying backwards counterfactual dependence. The account does not rule out backwards counterfactual dependence 'by hand'.

In fact, the evidential account allows much more scope for backwards counterfactual dependence than other accounts. Typically accounts of counterfactuals aim to minimise the 'transition period'—the time between when the state of the counterfactual world diverges from that of the actual world and the antecedent. They do so in order to minimise the scope for backwards counterfactual dependence, and so backwards causation. Remaining problem cases are typically dealt with by arguing that counterfactual dependence during the transition period is insufficiently 'robust' to count as causal (Lewis refs!). For example, Lewis (1979) minimises the transition period by maximizing the spatiotemporal area of perfect match between the actual world and the counterfactual world. Statistical-mechanical accounts minimise the transition period by requiring the macrostate of the counterfactual world to *match* that of the actual world at the time of the antecedent (outside the area of the antecedent) (Albert 2000; Loewer 2007, 2012), or, more rarely, by minimising such changes.[19] The evidential account does not make that stipulation.

Conversely, there are two positive features of the evidential account that allow for longer transition periods. Firstly, the branch point one 'rewinds' to is determined by features of the probability structure. One rewinds to a choice point on which the forwards evolution is reasonably probable. This is in contrast to having the choice point set by context, as in altered-states recipe approaches (Maudlin 2007a). It is also in contrast to Lewis' (1979) account, and most statistical-mechanical accounts (Albert 2000; Loewer 2007, 2012), which limit the transition period by limiting the spatiotemporal differences allowed between the actual and counterfactual world at the time of the antecedent—even if that implies a highly improbable forwards evolution.

The second feature of the evidential account that allows for long transition periods is that the account holds fixed both the fundamental dynamical laws as well as the expectation of regular macroscopic behaviour. This is in contrast Lewis' account (1979) which employs 'miracles'—areas of counterfactual worlds that violate the fundamental laws of our own. It is also in contrast to statistical-mechanical accounts that restrict the changes allowed at the time of the antecedent to the area of the antecedent (Albert 2000; Loewer 2007, 2012). This restriction rules out the usual macroscopic correlations we would expect between the antecedent and events *at the time of the antecedent*. While it is a positive feature of statistical mechanical accounts that they avoid violations to

[19] See Kutach (2002) and an alternative interpretation of Albert (2000) presented in Fernandes (Forth b.).

the fundamental laws (Dorr 2016), I suggest that, if counterfactuals are to be justified based on their evidential role, violations to macroscopic behaviour should also be avoided. We should allow for longer transition periods.

There is second way in which the evidential account does not illicitly put in a temporal asymmetry 'by hand': its temporal asymmetry is justified in evidential terms. The asymmetry reflects an asymmetry in probabilistic structure that is relevant, given our goals as reasoners and agents. Counterfactuals model an epistemic situation we often find ourselves in. We reason about a time *t* when we face an unknown future possibility A (that could come about with reasonable probability, given what we have evidential access to now) and are interested in what else we would reason to were we to learn A. A might be an action we're contemplating, or some other state. While there are states in the past we don't know of, we expect these either to be recoverable by the *Method of Forwards Evolution* from present states or not recoverable at all—we don't expect there to be past states we'll 'later' learn of that have further implications (beyond what is derivable from the present). This 'later' matters, because we are systems that *gather* and record information in one temporal direction and not the other (Reichenbach 1956). In so far as counterfactual reasoning guides our expectations about what we'll 'later' learn in that temporal direction, branch points will be prior to antecedents.

Contrast this evidential justification with more standard approaches. Altered-states recipe approaches (Maudlin 2007a, Ch.1) are similar to the evidential account, in that they involve rewinding the universe to a time at or prior to the antecedent, and using the laws to determine whether the consequent occurs.[20] But these approaches don't allow one to evaluate counterfactuals where the consequent is prior to the antecedent. While they do allow some changes to the time of the antecedent outside the area of the antecedent, these are all specified by 'context'—so any backwards counterfactual dependency is a feature of context, rather than a discovery. Moreover, when temporal asymmetries are considered, they are justified by a metaphysical intrinsic asymmetry in time itself (Maudlin 2007a).

[20] Maudlin uses a cauchy surface, but that difference does not affect the arguments here.

Another contrast is Loewer's (2007) statistical-mechanical account. Loewer also uses probabilities derived from the statistical postulate, the dynamical laws and the full macrostate of the universe at a time to evaluate counterfactuals.[21] Loewer's account and the evidential account are in fact equivalent in cases where *t* is the time of the antecedent. It might seem, however, that Loewer's account does better at explaining temporal asymmetries, since Prior only includes the present state. But appearances are misleading. Loewer restricts the antecedents he considers to decisions, and stipulates that these are less than macroscopic and that have a 'reasonable' probability of coming or failing to come about conditional on the macrostate *at or prior to* the time of the antecedent (Loewer 2007). The assumption that antecedents are probabilistically independent of past states builds in an unexplained asymmetry. Moreover, requiring antecedents to be less than macroscopic and probabilistically independent of states in the present restricts the scope of Loewer's account (as he is aware). The evidential account aims at a more general approach to counterfactuals. For this reason, it defends the use of temporally asymmetric requirement, and does so in evidential terms.

6. Conclusion

In this paper, I've used evidential function to provide a unified account of a range of scientific modal relations. I began by considering the evidential role of laws and probabilities, and how they allow us to reason about the past. I then used the evidential role of chances and counterfactuals to develop standards for how these should be evaluated, using laws, probabilities and contingent states. The accounts I've offered are naturalistic on two fronts: they explain temporal asymmetries in these relations in scientific terms, and they don't go beyond the bounds of science in explaining what scientific modal relations are, and how they are fit to play their roles.

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[21] Statistical-mechanical probabilities also require conditionalising on the Past Hypothesis. The same results are implied, on the evidential account, by using the *Method of Forwards Evolution*.

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