A Reply to Nina Emery

Sam Mitchell*†

This article argues against Nina Emery's view that there are compelling reasons to believe in nomological probabilities. To the degree that we can see no other way to detect the occurrence of the explanans apart from the bare fact that the explanandum happened, we may be skeptical that the proposed explanation is correct and open to the possibility that there is none. This provides us with a way to distinguish physical entities from mathematical ones, illuminating interpretations of quantum mechanics.

Emery's Argument. Emery holds that nomological probabilities—lawlike statements of the probabilities of events or propositions—play a crucial role in the way our best current scientific theories explain events and states of affairs (2017, 483). Naturalism is the view that standard scientific methodology is a good guide to successful inquiry into what the world is like (482). Hence, naturalism ought to lead us to believe that nomological probabilities exist.

Emery writes:

> **Definition of nomological probability.** If (P) is a true proposition of the form *the probability of an event of type E occurring in situation S is x*, and (F) is a true proposition of the form *the stable, long-run relative frequency with which events of type E occur in situations of type S is y*, and *x* is close to *y*, then (P) explains (F) if and only if the probability referred to in (P) is a nomological probability. (2017, 481)

Received December 2017; revised October 2018.

*To contact the author, please write to: Department of Philosophy, Mount Holyoke College, South Hadley, MA 01075.

†I am very grateful to the Philosophy Department at Mount Holyoke College for their help: Nina Emery, James Harold, and Katia Vavova. I am also grateful to two anonymous reviewers.

Philosophy of Science, 86 (October 2019) pp. 794–806. 0031-8248/2019/8604-0012$10.00
Copyright 2019 by the Philosophy of Science Association. All rights reserved.

https://doi.org/10.1086/704984 Published online by Cambridge University Press

We have observed that there is a stable long-run relative frequency of some kind of event, Emery's example being the half-life of Carbon-11. If this observation is explained by attributing a probability to the decay of the nucleus of Carbon-11, then that probability is nomological. We clearly do, in our best scientific theories, account for the data by citing the probability. This is counted an empirical success for quantum mechanics. So it appears reasonable to follow Emery it taking it to be explanatory.

Nomological probabilities are thus unobservable entities that explain some set of outcomes that we have observed. To say that nomological probabilities ought to be accepted as real is thus highly uncomfortable for many philosophical positions. It is uncomfortable for those that abandon the attempt to learn about unobservables from science (Ladyman and Ross 2007, 92) and for those that restrict the aims of science to accounts of the observable (van Fraassen 1980, 12) and for those that hold a Humean mosaic of local events is all that exists (Lewis 1986, x–xi; 1994). Emery nonetheless argues that "we should think that there are nomological probabilities, pretty much regardless of the costs . . . [to] our metaphysics" (2017, 494). She argues that in quantum mechanics, nomological probabilities are indispensable, and hence forced on us by any commitment to naturalism.

The argument for the indispensability of nomological probabilities uses an example that is representative of a broad range of patterns of reasoning in quantum mechanics:

> (1) For the vast majority of samples of carbon-11 atoms, around half of those atoms decay within 20 minutes. . . .
> (2) For any particular carbon-11 atom, the probability of that atom decaying within a particular 20 minute interval is (roughly) 1/2. . . .
>
> So (2) explains (1). (Emery 2017, 483–4)

According to Emery, nomological probabilities are indispensable because of the huge cost of abandoning the idea that 2 explains 1. For the only alternative way to explain 1 is to say:

> (3) There is some feature F, which roughly half of all carbon-11 atoms have, and which is such that all and only carbon-11 atoms that have F decay within a particular 20-minute interval. (Emery 2017, 484)

As Emery says, 3 is intolerable. There is no reason to adopt it, neither in the observations nor in the hypotheses of quantum mechanics. It amounts to an a priori commitment to hidden variables that is unmotivated by the outcomes we have observed. And in the late twentieth century, there was a particularly notorious failure of hidden variable theories. Alain Aspect (most prominently)

https://doi.org/10.1086/704984 Published online by Cambridge University Press

showed that in the correlations between spins that they predicted clashed with the predictions of quantum mechanics. (These are the EPR experiments. They are a class of experiments, first criticized by Einstein, Podolsky, and Rosen, in which quantum mechanics proposes that there are spacelike-separated correlations but no process that produces them. There is a massive literature on these. A recent treatment is Lewis [2016, 27–39].)[1]

So either 1 goes unexplained or we accept Emery's view that probabilities explain. Emery argues that there are large difficulties to saying that our theories do not explain 1. Physics does not, for example, say that explicitly. Indeed, on the face of it, it says the reverse and proposes probabilities that are embedded in more complex theories wherever a robust and stable relative frequency is observed (Emery 2017, 485). Once again, naturalism appears to force us toward nomological probabilities.

Reply to the Argument. Below, I present several arguments for the following premise:

> **Independent evidence requirement.** Consider all those cases in which F has been observed. If we know of no evidence, apart from the occurrence of F, that indicates that P happens on any of these occasions, then it is permissible not to believe that P correctly explains F.

To be explicit about the evidence we need:

> We possess evidence for P independently of F if and only if, on at least some occasions when we know that F happens, we actually possess, or can at least see how to acquire, evidence e such that both
>
> 1. e confirms that P occurs as well as F, and
> 2. Observing that $\neg e$ indicates that $\neg P$ happens even though F does.

The independent evidence requirement says that, according to naturalism, we need not accept something as the true explanation when we possess no evidence for the explanans that is independent of the explanandum.

There is some vagueness in the requirement. The idea is that the scientific theories we possess predict that we will observe e in some situation. One might insist that we actually possess independent justification for P, at one extreme. Or one might be more liberal and say we can just describe how we can get the evidence if our theories are true and we decide to do certain things. Even in the more liberal construal, though, we may be skeptical.

1. One might try to argue that Bohm (1952) held 3, and perhaps also Bell (1987). That is debatable, and in any case these views are a minority. Certainly Emery is correct when she asserts that 3 is not part of the standard way in which quantum mechanics is interpreted (2017, 484).

https://doi.org/10.1086/704984 Published online by Cambridge University Press

P might be a *potential* explanation of *F* without this independent evidence, and one might even suspect that it is the correct explanation. But one need not say that one has as yet any evidence for its being the explanation. Nobody as yet has any compelling reason to accept it as such. In particular, the view that *P* (in this example, nomological probability) is an explanation of *F* (observed relative frequency) is certainly avoidable if we lack this extra evidence. Maybe something other than *P* explains it. Maybe *F* has no explanation. Arguments favoring the independent evidence requirement are arguments favoring the view that *F* is so far unexplained when there are no independent observations confirming *P*.

This requirement might seem very odd and subject to clear counterexamples. Take a singular causal explanation. Booth pulls the trigger, and the pistol fires; call this event *A*. Lincoln is shot in the back of the head and dies the next morning; call this event *B*. We know that *A* explains *B*. What evidence do we possess such that, had it been different, Lincoln would still have died of a gunshot wound to the back of the head (in that way, at that place and time), but Booth would not have pulled the trigger? It does not seem as if there is any.[2]

But both *A* and *B* are events we observed. Suppose we had not observed *A* but had still observed *B* (Lincoln dying, by gunshot, at that place and time). Certainly Booth pulling the trigger (i.e., *A*) might be the explanation of *B*, but we are certainly not compelled to accept it as such. So the observed event in this case is *A* itself; if it had been different while Lincoln still died of that kind of wound, then *A* does not correctly explain *B*.

Perhaps the idea is that *A* is not Booth being the killer but someone firing a pistol at Lincoln. But that kind of explanation depends on a generalization for which we have evidence independently of that particular instance. We observe a corpse with a gunshot wound. We infer that there must have been a gunshot that explains that death, even though we did not witness it. But we are now depending on other observations; we are depending on the observation that all wounds of this kind are caused by gunshots. If we commonly observed this kind of wound without independently observing the gunshots that cause it, we would not find the explanation compelling. Suppose the firearm had never been invented or had been successfully banned. Suppose further that ammunition is still widely available but set off by some other method (such as ignition or a hammer). We would no longer accept gunshots as explanations for wounds.

Now the independent evidence requirement appears to have gone from being false to being trivially true. In cases in which the explanandum is true, and the explanation is compelling according to naturalism, we know of a way to get evidence such that, had it been otherwise, we would believe the explanans not to have occurred. The demand is really that we know

2. I am grateful to an anonymous reviewer for this objection.

https://doi.org/10.1086/704984 Published online by Cambridge University Press

how to acquire evidence that the explanans happened that is not simply the fact that the explanandum happened. Does not every explanation possess that feature?

No. Emery's does not. Nomological probabilities are supposed to explain frequencies even when the only way to establish what their values are is the observed relative frequencies they are supposed to explain. The argument against this is that when there is no further structure to the theory by which we can determine the value of the nomological probabilities independently of observing the relative frequencies, those nomological probabilities need not be the explanation of the observed frequencies.

The argument that nomological probabilities do not play any unavoidable explanatory role is a dilemma. Suppose first that there is no evidence establishing that $\Pr(D) = r$ except the relative frequencies m/n that we have observed in various (large) samples. These are roughly equal to r, but no other observation suggests that $\Pr(D) = r$. By the independent evidence requirement, there is no compelling reason to believe that $\Pr(D) = r$ explains our observations.

Suppose, then, that there is evidence that would leave the observations of m/n untouched however it came out but that, because of the way it did in fact turn out, confirms that $\Pr(D) = r$ as opposed to some other value for $\Pr(D)$. This evidence must confirm the value for the probability through its justification of various other empirical hypotheses, concerning various other physical entities, or features of models of scientific theories. But then, we need not suppose that it is the probability that explains the observations of the relative frequencies but rather the antics of the various physical entities—the ones the observations have independently confirmed. So in this case too, we are not required to take the nomological probability itself as explanatory.

So neither case compels us to see nomological probabilities as explanatory. In the first case, the relative frequencies might have a different explanation or no explanation, and in the second case, they are explained by the behavior of various other empirical entities or features of models. I will argue that this way of seeing things does not depart from the methodology of our best scientific theories and indeed, I argue, illuminates both the theories and the methodology. One can embrace naturalism without being forced to accept that nomological probabilities are in themselves explanatory. The next few sections spell this out and defend it.

For the Independent Evidence Requirement

Explanations Should Be Potentially Informative. Jones is puzzled by something he observes. Smith proffers an explanation, and Jones believes it. So Jones is no longer puzzled. What is the point of such an exchange?

https://doi.org/10.1086/704984 Published online by Cambridge University Press

The answer will vary according to one's theory of explanation. But I think there is one necessary condition that all such theories ought to share: the explanation must be *potentially informative about other kinds of observations*.

Before Smith's explaining, Jones takes himself to be justified in believing certain scientific hypotheses. That is, Jones expects a range of observations in the physical situations he envisages before believing the explanation. If believing the explanation makes no difference whatsoever to this, then the "explanation" makes no difference to Jones. It is irrelevant to any science he can actually practice. So Jones ought not to accept the explanation as resolving his puzzlement. The whole exchange between Smith and Jones becomes a kind of elaborate pantomime.

It is no good answering this by saying that, while Jones may be unable to see what difference the explanation makes, it still could make a difference. The argument simply repeats. For Jones to have any reason to think that the explanation could make a difference, he has to be able to see how it could make a difference. Suppose, for example, he could see what would have to be observed to test the explanation's truth independently of the explanandum, but he doubts whether any investigator will ever be exposed to the test. While that is an excellent reason for doubting the explanation, one might still argue that Jones knows that the explanation could have some relevance to the science Jones and his companions practice. But Emery's argument is that commitment to nomological probabilities is unavoidable. By introducing the test of the value of the probability that is independent of the relative frequencies they are supposed to explain, Jones has introduced empirical objects, properties, events, models, or substructures of models that can be regarded as providing the real explanation. We do not have to be committed to the nomological probabilities as the explanatory entities.

The reason scientific explanations have to be potentially informative, for empiricists, structuralists, and most realists, is that science is a practice that depends on outcomes of observation to possess justifications. No hypothesis is justified, according to this practice, if the outcomes of observation do not support it over its negation. But then we must possess some way to at least see how we might establish the hypotheses of the explanans independently of the explanandum.

Otherwise, there is no way, empirically, to distinguish between the correctness and incorrectness of the explanation. There is no way for Jones to find out either that he was right or that he was wrong in accepting Smith's account of the explanandum. There is no way to find out he was right because he cannot discover that the explanans is justified by the additional evidence. And there is no way to find out he was wrong because there is no such thing as finding out that the puzzling phenomena occur but that Smith's explanation is a lot of nonsense.

https://doi.org/10.1086/704984 Published online by Cambridge University Press

Jones will be aware of all this when he hears Smith utter the explanation. Being aware of it, and knowing that he knows of nothing that indicates that Smith is right, he will treat the explanation with considerable skepticism. He knows he has at present no reason to think it is right as opposed to wrong.

Perhaps ironically, Jones should be comforted only by those explanations that leave him more likely to be wrong, by his own lights, after he believes them. For whenever he accepts something as an explanation, he will expect future observations, or become aware of past ones, that he takes to support the explanation. And of course, they might not do so. The explanation could turn out to be wrong. That seems to me entirely appropriate; it is the cost of being informed when we accept an explanation as correct.

Threatening Circularity. Emery holds that a nomological probability explains the observed relative frequencies. We ought, therefore, to be committed to the existence of nomological probabilities. So one of the premises involved here is that when we have an explanation, we must know that the explanans is true. Anyone who denies this can allow that we possess an explanation but deny that this has any impact on what we should believe. Pace Cartwright (1983) let us agree to this.

Knowledge, though, depends on having a justification. But the justification for the belief that $\Pr(D) = r$ cannot be other than the observation that the relative frequencies of D-type events out of the total candidates is m/n and $m/n \simeq r$. For we are considering examples in which there is no other confirmation for the explanans apart from the observations in the explanandum.

This is very disconcerting. It looks circular. We should say that there is an explanation here—and say that we ought to believe the explanans—because after the explanation, we gain new knowledge that we do not possess without it. But then the additional knowledge consists in believing that the data really are explained in this instance and that we ought really to believe the explanans. This will not be consoling to anyone who doubts the explanation in terms of nomological probabilities.

Counterintuitive Examples. Convincing examples of explanations become unconvincing when one removes the independent justification for the explanans. Measles explains the presence of Koplik's spots in the throat. There is, though, some other way to establish that a patient has measles apart from the spots (most obviously, spots on the exterior skin surface a few days later). If the first physician to notice Koplik spots has no independent evidence—in this patient—that it is a case of measles, then there is no way to know that measles, and not something else, is the explanation. Preferential scattering of blue light by particles that are suspended in the atmosphere explains the blue color of the sky. We clearly have a different way of establishing

https://doi.org/10.1086/704984 Published online by Cambridge University Press

that the particles exist and that they preferentially scatter blue light. If we did not, we could not know that this was the explanation. The height of the tower explains the length of the shadow. But when the only evidence we possess for the height of the tower is the measured length of the shadow, we are obviously depending on other experiments to draw our conclusion.

"Explanations" in which there appears to be no more to the explanatory story than that the explanandum was a general observable regularity are intuitively highly suspect. They smack of Molière's *virtus dormitiva* or the "explanation" of suction by nature's *horror vacui*. One does not explain a phenomenon by translating it into Latin. Again, mutual explanations are intuitively suspect when we know of no independent evidence favoring either explanans. In the film *Idiocracy* (Judge and Cohen 2006), for example, it is explained that Brawndo has what plants crave because it contains electrolytes. It contains electrolytes because it has what plants crave.

The Costs of Violating the Requirement. On its weakest construal, the independent evidence requirement does not strictly require that we actually possess independent evidence for the explanans. It requires that we can see how it can be obtained given our accepted scientific theories. Violating it says that we have an explanation even in cases in which only wild and unfounded speculation would suggest that independent evidence will be obtained.

This conflicts with a great many positions in the philosophy of science. Consider, for example, any position that is impressed with the argument that the success of science would be a miracle if some feature—the reality of entities or the approximate correctness of scientific theories—were absent. Now consider what makes an explanation that violates the independent evidence requirement a successful one. All that is required is the continuance of the explanandum and the 'explanation' is a success. But this "success" is hardly miraculous. By contrast, if we respect the requirement, then there is a mild kind of novel prediction. Even if the explanandum continues, some other kind of observation could either support, or refute, the explanans. This latter could either succeed or fail. A history of successes, especially those that are intuitively surprising, does support the miracle argument. So many versions of realism, even those that avoid realism about entities such as Ladyman and Ross's (2007, 75), will be motivated to endorse the requirement.

Realists will be joined by empiricists, motivated by something van Fraassen (1985, 255) observed. A position appears to have an advantage if it proposes an explanation for something that another position leaves unexplained. But this advantage is empty if both positions incur the same risk of refutation. "It is but empty strutting and posturing, this display of courage not under fire. . . . What can I do except express disdain for this appearance of greater courage in embracing additional beliefs which will *ex hypothesii* never brave a more severe test?" Van Fraassen deploys the point against holding

https://doi.org/10.1086/704984 Published online by Cambridge University Press

that anything more than empirical adequacy is the aim of science, but it prohibits violations of the independent evidence principle too. If our theories give us no way to foresee how an explanans could go wrong independently of the continuance of the explanandum, then the 'explanation' is legitimately dismissed as vacuous.

Discussion. It is true that when some explanations are first proposed, the independent justification for the explanans is quite speculative. De Broglie, following on the work of Bohr, did not have any independent evidence for wave-particle duality of electrons when he suggested that it explained the emission spectrum of hydrogen. Rutherford did not have any independent evidence for the existence of a very dense atomic nucleus when he used it to explain his results on scattering. Darwin had massive independent evidence for the operation of natural selection in other cases, but he did not have any evidence for its having operated in the Galapagos islands to produce the fauna there except for the fact that it very nicely explained them. So whether we will get the independent evidence for the explanans might be unknown at the time of the explanation.

Although the explanations were first proposed without independent evidence, they were controversial. So the explanation was not accepted as correct without this independent evidence. And even the proposal came in the light of phenomena that strongly suggested that independent evidence might be forthcoming. De Broglie did not have evidence for wave-particle duality for electrons, but he had it for photons. Rutherford knew that there had to be a positive charge in the atom somewhere because atoms must contain negative electrons but are electrically neutral. And Darwin provided an enormous amount of evidence, from other contexts, in which natural selection acted to produce the same kind of results observed on the Galapagos islands. He knew what kinds of observations would refute his explanation without challenging the data it explained. These proposed explanations are speculative, but they are not shots in the dark. Other things we knew at the time suggested ways in which the explanans might be independently confirmed.

Someone might argue that natural selection is certainly an explanation for some cases of evolution. Yet natural selection was once thought to be tautologous and, hence, lack any independent evidence. So hypotheses that lack any independent evidence are sometimes held to be explanatory.

Our reply to this argument did not, however, attack the idea that explanations were compelling only with independent evidence. Instead, we attacked the idea that selection could have no evidence independently of its effect on evolution. We can observe properties that make an individual more likely to succeed, even if outrageous fortune intervenes. A comparatively fit individual may have no reproductive success, for example, because it gets eaten or struck by lightening. A comparatively unfit individual may be wildly successful

https://doi.org/10.1086/704984 Published online by Cambridge University Press

because it is, accidentally, the only one to successfully contact the gametes of another organism of the right sex and species. So fitness can be measured independently of the observed reproductive outcome. This literature, then, disputes the criticism that natural selection is unexplanatory because natural selection is not tautologous, not because it would not be a criticism if it were (Gould 1976).

Emery notes a number of examples that are not, intuitively, supportive of the independent evidence requirement (2017, 487). We accept dark energy as an explanation for the expansion of the universe (she claims), although as yet we have no evidence of its nature and no other evidence of its existence. Newton proposed a universal force applying both on earth and in the heavens, only because doing so would explain so much. Planck explained the deviations from the Raleigh-Jeans law, although he had no evidence for his law except the existence of those deviations. More and more fundamental particles explain data for which they are at first the only evidence.

These examples have a variety of answers. In the case of Planck, scientists were at first very cautious about accepting this explanation, as the independent evidence requirement says they should be. Newton did present evidence that gravitation applied in both terrestrial and celestial realms. He proposed a quantitative hypothesis, and data fit that single hypothesis in both cases (Glymour 1980, 203–26). I grant that he proposed no explanation of the law of gravitation, but then he did say as much. In the case of new observations of fundamental particles, there were successful analyses for other cases, and the apparatus for detecting them was well understood. For dark energy, we have a great deal of room for skepticism because at present we simply do not know what to say about it except that it is whatever is responsible for the phenomenon it supposedly explains. If it is really true that we have no idea how to independently confirm the existence of dark energy, then this need not be the explanation.

Nonvacuous Cases of Explanations. To review: I have made a case for the independent evidence requirement. We are not required to accept some thing as an explanation for a set of data when we know of no other observations that, had they come out differently, would refute the explanans without refuting the explanandum. So nomological probabilities need not explain relative frequencies in those cases in which the only evidence for the probability having that value is the relative frequencies themselves.

Most cases of explanations do have independent evidence for the explanans. In particular, Emery's most prominent example of nomological probabilities has independent evidence. For we possess well-confirmed hypotheses, which are amply supported by evidence other than the relative frequencies, that Carbon-11 should have a half-life of about 20 minutes. In this section, I argue that nomological probabilities need not be explanatory in these cases either.

https://doi.org/10.1086/704984 Published online by Cambridge University Press

What follows is an oversimplified explanation. Within the nucleus of a Carbon-11 atom there are protons, which are sometimes affected by force-carrying particles of short range called *W* particles. Each proton is composed of 3 quarks, 2 up, with charges of +2/3, and one down, with a charge of −1/3. The interaction with the *W* particle changes an up quark into a down quark, so the result is a neutral particle, a neutron. The positive charge the former proton possessed is carried away by a positron, and the energy is kept in balance by the emission of a neutrino. The reason the nomological probability is as it is consists in the availability of *W* particles in a nucleus of this nature, the fact that the products conserve energy and charge, and the fact that the binding energy of the resulting nucleus (Boron-11) is less than that of Carbon-11.

This is rich with independent justifications. We can produce the same kind of decay in nuclei by irradiating them. We can also observe *W* particles (with great difficulty). We can confirm the equivalence of mass and energy and use it to relate the binding energies of the different kinds of nuclei to their masses, and much else.

I do not see that it follows from this that nomological probabilities must be the explanation. What happens is that a particle that behaves in one way—which may be described in terms of either possessing the property of having a half-life or having statistically significant populations with a certain distribution of observed decays—has been replaced with other particles that behave in different nondeterministic ways. This explanation could obviously turn out to be wrong in spite of ^{11}C possessing the half-life that it does. So we can independently confirm that the half-life ought to be about 20 minutes. But then the explanation of the relative frequencies of decay that we observe can be in terms of the behavior of quarks, nucleons, *W* particles, and so on.[3]

This viewpoint has obvious points of contact with the theory of scientific explanation embraced by Philip Kitcher and Michael Friedman (Friedman 1974; Kitcher 1976). A genuine explanation is one that accounts for diverse kinds of phenomena by accounting for them as manifestations of a common set of hypotheses. The independent evidence requirement follows Kitcher and Friedman's skepticism about whether explanations that lack this kind of unification should convince us.

The independent evidence requirement allows for the possibility that relative frequencies are, as Friedman puts it, "brute" and unexplained (1974, 11). Someone might object that naturalism should not accept the potential absence of explanation in this way. Since nomological probabilities are the only (or best) explanation, we ought to believe in them.

3. Ontic Structural Realists oppose the idea that it is entities that explain and hold that structures do so instead. They are clear, though, that the structures they intend must knit together phenomena in a way the independent evidence requirement demands (Ladyman and Ross 2007, 27–28).

https://doi.org/10.1086/704984 Published online by Cambridge University Press

Against this, there is a history of practicing scientists, often empiricist, who have accepted phenomena such as gravitational attraction as unexplained regularities. The independent evidence principle does not even go that far; it says that we do not know what the correct explanation of the frequencies is and that they might lack an explanation. In addition, as van Fraassen once noted, everyone has to reject the demand for explanation somewhere (1980, 2). The early atomists thought that they could explain regularities that were then thought of as bare by introducing atoms. Eventually, of course, we got independent explanations for these things, because we could justify the hypotheses in the explanans independently of its being correct about the explanandum. But this additional step was necessary for atoms to be genuinely explanatory. When faced with a bare regularity, we can hope such a future story will turn up. But until it does turn up, we do not know why the regularity is that way, or even whether there is a reason; we just experimentally confirm that the regularity occurs. At every time, we will always reach a point when our explanations run out, and we can accept a regularity as brute, at least for the moment.

Why Does This Dispute Matter? Suppose this argument succeeds, and we do not have to believe in nomological probabilities. Well why bother? What use is it to say that we do not know the explanation of fundamental regularities, including stable relative frequencies, and perhaps they lack an explanation?

Lewis, following Ladyman and Ross (2007, 136), suggests that we face metaphysical underdetermination in the philosophy of quantum mechanics (Lewis 2016, 175). One response to underdetermination, particularly when it is metaphysical, is to shrink the realm of metaphysics so that the underdetermination does not arise.

It would be nice to have a principled way of deciding what features of our theories we need to keep and what may be legitimately ignored. The independent evidence requirement suggests one. It suggests that we can be skeptical about those features of a theory that, while they may be mathematically calculable from certain phenomena, are not independently confirmed by other observations. It suggests that we can treat nomological probabilities as mathematical entities, rather than physical entities, as van Fraassen put it (1980, 11).

The reason we may take this line consists in the argument supporting the independent evidence requirement. For it was supported by the idea that we may be agnostic about a scientific explanation unless we know how it could make some difference to the practice that constitutes science. We must know how we could at least use the things we can detect (perhaps very indirectly), to confirm that the explanation is right as opposed to wrong. So we have a way of shrinking the metaphysical demands that is motivated by something other than the need to solve the particular problems that arise in the philosophy of quantum mechanics.

https://doi.org/10.1086/704984 Published online by Cambridge University Press

That is where the interest lies in these debates, I would urge. In more prosaic examples of empirical justification outside quantum mechanics, we certainly use different sets of data to justify a hypothesis we use in an explanation by confirming it independently of the explanandum. A similar approach in more difficult cases might be productive.

REFERENCES

Bell, John. 1987. *Speakable and Unspeakable in Quantum Mechanics*. Cambridge: Cambridge University Press.
Bohm, David. 1952. "A Suggested Interpretation of the Quantum Theory in Terms of 'Hidden' Variables, I and II." *Physical Review* 8:166–93.
Cartwright, Nancy. 1983. *How the Laws of Physics Lie*. Oxford: Clarendon.
Emery, Nina. 2017. "A Naturalist's Guide to Objective Chance." *Philosophy of Science* 84 (3): 480–99.
Friedman, Michael. 1974. "Explanation and Scientific Understanding." *Journal of Philosophy* 71 (1): 5–19.
Glymour, Clark. 1980. *Theory and Evidence*. Princeton, NJ: Princeton University Press.
Gould, Steven Jay. 1976. "Darwin's Untimely Burial." *Natural History* 85: 24–30.
Judge, Mike, and Etan Cohen. 2006. *Idiocracy*. DVD. Dir. Mike Judge, Twentieth Century Fox.
Kitcher, Philip. 1976. "Explanation, Conjunction and Unification." *Journal of Philosophy* 73 (8): 207–12.
Ladyman, James, and Don Ross. 2007. *Every Thing Must Go*. Oxford: Oxford University Press.
Lewis, David. 1986. *Philosophical Papers*. Vol. 2. Oxford: Oxford University Press.
———. 1994. "Humean Supervenience Debugged." *Mind* 102 (412): 473–90.
Lewis, Peter. 2016. *Quantum Ontology*. Oxford: Oxford University Press.
van Fraassen, Bas. 1980. *The Scientific Image*. Oxford: Oxford University Press.
———. 1985. "Empiricism in the Philosophy of Science." In *Images of Science*, ed. Paul Churchland and Clifford Hooker, 245–308. Chicago: University of Chicago Press.

https://doi.org/10.1086/704984 Published online by Cambridge University Press