Experimentally adjudicating between different causal accounts of Bell inequality violations via statistical model selection

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Bell inequalities follow from a set of seemingly natural assumptions about how to provide a causal model of a Bell experiment. In the face of their violation, two types of causal models that modify some of these assumptions have been proposed: (i) those that are parametrically conservative and structurally radical, such as models where the parameters are conditional probability distributions (termed ‘classical causal models’) but where one posits inter-lab causal influences or superdeterminism, and (ii) those that are parametrically radical and structurally conservative, such as models where the labs are taken to be connected only by a common cause but where conditional probabilities are replaced by conditional density operators (these are termed ‘quantum causal models’). We here seek to adjudicate between these alternatives based on their predictive power. The data from a Bell experiment is divided into a training set and a test set, and for each causal model, the parameters that yield the best fit for the training set are estimated and then used to make predictions about the test set. Our main result is that the structurally radical classical causal models are disfavoured relative to the structurally conservative quantum causal model. Their lower predictive power seems to be due to the fact that, unlike the quantum causal model, they are prone to a certain type of overfitting wherein statistical fluctuations away from the no-signalling condition are mistaken for real features. Our technique shows that it is possible to witness quantumness even in a Bell experiment that does not close the locality loophole. It also overturns the notion that it is impossible to experimentally test the plausibility of superdeterminist models of Bell inequality violations.

Introduction. There is now widespread agreement that the experimental evidence in favour of nature violating Bell inequalities [1–3] is persuasive [4–6]. By contrast, there is no agreement on how to provide a causal account of such violations [7]. One of the most popular views is that Bell inequality violations imply the existence of superluminal causal influences (typically understood as action at a distance) [8, 9]. Another view is that they imply the need for “superdeterminism”, wherein the hidden variable that is a common cause of the two outcomes is also a cause of one or both of the setting variables [10–12]. A third option is that the correct causal account is one wherein there is just a common cause of the two outcomes, but wherein correlations are computed using the formalism of quantum causal models [13–16].

It is tempting to think that the only way to adjudicate between such accounts, i.e., the only way to assess the quality of the accounts of the correlations that they offer, is to consider their merits relative to some philosophical or aesthetic criteria. Recall, however, that this was the standard attitude towards the question of the merit of local hidden variable models of quantum theory until Bell showed how the question can be assessed by empirical data [1]. Motivated by Bell’s example, we undertake to show that the selection problem articulated above can also be assessed empirically.

To date, proposed causal accounts of Bell inequality violations have typically been held to the following standard: that the observations be reasonably likely given the model. In other words, a given causal model has generally been considered unobjectionable on empirical grounds as long as it does not underfit the data. From the perspective of statistical model selection, however, a more methodologically sound figure of merit when comparing models is their predictive power. The latter can be compromised not only when the model underfits the data, but for other reasons as well, such as the model overfitting the data. Overfitting occurs when the model-fitting procedure mistakes statistical fluctuations in the data for real features, a mistake which implies reduced predictive accuracy on novel data with different fluctuations. In this article, we propose to hold causal accounts to a higher standard by developing a technique for assessing the relative merit of different causal models using standard model selection tools. We then apply the technique to data obtained from a Bell-type experiment.

In our comparison of alternative causal accounts of Bell inequality violations, we will focus on the distinction between a set of accounts that are structurally radical and parametrically conservative, on the one hand, and an account that is structurally conservative and parametrically radical on the other. By “structural radicalness,” we mean that the particular causal structure that appears in a causal account is not the one that one would expect a priori to hold for a Bell scenario (more on this below). By “parametric radicalness,” we mean that the mathematical formalism by which one extracts statistical predictions from a given structure is not the conven-
tional one used in classical causal modelling, but rather
the modification thereof proposed in the recent literature on quantum causal models [14–16]. The result of our analysis is that the latter sort of account is favoured by the experimental data.

In order to be able to compare the predictive power of different causal accounts of Bell inequality violations, we must cast them into a common framework. For this purpose, we use a framework that subsumes both that of classical causal modelling [14] [18] and quantum generalizations thereof [13] [16], thereby permitting the relative predictive power of these views to be compared one to another using standard model selection techniques. Our proposal is an example of causal discovery using purely observational data. Its relation to past work on causal discovery, both classical [17] [18] and quantum [19] [20] is discussed in Appendix B. [1]

The framework of classical and quantum causal models. For classical causal models, we use Pearl’s framework [17]. The structural part of a classical causal model is a specification of the causal relations that hold among a set of systems (i.e., the causal structure) and is represented by a directed acyclic graph (DAG). Examples are given in Figs. [1(b-d)]. Each node in the DAG represents a system, which in the classical case is associated to a random variable. The directed edges into a node \( X \) from the causal parents of \( X \), denoted \( \text{Pa}(X) \), represent the potential for a direct causal influence in the intervention sense (namely, that manipulating a variable in \( \text{Pa}(X) \) while keeping all other variables fixed allows one to alter the statistical distribution of \( X \)). The parametric part of a classical causal model stipulates, for every node \( X \), the possibilities for how the statistical distribution over \( X \) can depend on a given intervention on \( \text{Pa}(X) \), that is, the possibilities for the conditional probability distribution \( P_X|\text{Pa}(X) \), termed the “do-conditional”. A causal model may stipulate a restriction on the possibilities for \( P_X|\text{Pa}(X) \) for each node \( X \), although in this article we are primarily interested in the case where there is no restriction, in which case we call the model parameter-unrestricted. Let \( V \) denote the full set of variables in the DAG. The distributions over \( V \) that are said to be compatible with the causal model are those that can be expressed as the product \( P_V = \prod_{X \in V} P_X|\text{Pa}(X) \) where \( \{P_X|\text{Pa}(X) \} \) in \( V \) are conditional distributions in the set allowed by the model, which is any conditional distribution in the case of a parameter-unrestricted model. If only a subset, \( O \), of the variables in \( V \) are observed, such that the complementary set of variables, \( V/O \), are unobserved (these are termed ‘latent variables’), then the compatible distributions on \( O \) are computed by marginalization over the latent variables, \( P_O = \sum_{V/O} P_V \). (Here, an expression such as \( \sum_Y P_{XY} \) represents the distribution \( P_X \) whose component at \( X = x \) is \( \sum_y P_{XY}(xy) \).)

Various proposals exist for how to define a quantum generalization of the notion of a causal model [14] [16]. Although there are distinctions between these, they will not be relevant for the purposes of this article. We follow Refs. [7] [21] in taking the transition from classical to quantum causal models to be a transition in the nature of the parameters that supplement the causal structure, while the causal structure is taken to be represented in the same way as in a classical causal model, namely, by a DAG. What one can infer about a system \( A \) given an intervention on its parents \( \text{Pa}(A) \) is no longer presumed to be represented by a conditional probability distribution, but is instead represented by a more exotic mathematical object, termed a conditional density operator, denoted \( \rho_A|\text{Pa}(A) \) [21]. It is a positive operator on the tensor product of the Hilbert spaces of \( A \) and \( \text{Pa}(A) \) that satisfies \( \text{Tr}_A(\rho_A|\text{Pa}(A)) = \mathbb{I}|\text{Pa}(A) \). An equivalent way of representing what one can infer about \( A \) given an intervention on \( \text{Pa}(A) \), which makes the analogy to conditional probabilities less obvious but connects better to the conventional formalism, is as a completely positive trace-preserving map from \( \text{Pa}(A) \) to \( A \). (The equivalence of these two representations is established using the Choi–Jamiołkowski isomorphism.) The parameter-unrestricted versions of quantum causal models, which are the only ones we consider here, impose no restriction on the possibilities for the conditional density operator. We will also focus here on quantum causal models wherein the only systems that can be intrinsically quantum are the latent systems, while all of the observed systems are classical, since this simplifies the analysis and is sufficient to describe the Bell scenario (see Fig. [1(c)]).

The state of causal models– We will consider a bipartite Bell experiment, depicted in Fig. [1(a)]. We refer to the two labs as ‘Alice’s’ and ‘Bob’s’. The variable corresponding to the measurement setting (resp. outcome) in Alice’s lab is denoted by \( S \) (resp. \( X \)) and the variable describing the measurement setting (resp. outcome) in Bob’s lab is denoted \( T \) (resp. \( Y \)). In the case we consider here, the outcome variables will be binary but the setting variables can take a larger number of values. The conditional probability of outcomes given settings is denoted by \( P_{XY|ST} \). The experimental data constitutes a finite sample from the distribution over \( X \) and \( Y \) for each set of values of \( S \) and \( T \), that is, a finite sample from \( P_{XY}^{s,t} := \sum_{S,T} P_{XY|ST} \delta_{S,s} \delta_{T,t} \) for each \( (s,t) \).

We begin by describing the causal model that is excluded by Bell inequality violations. As argued in Ref. [7], this is the classical causal model with the DAG of Fig. [1(b)], which describes a causal structure that fits Bell’s intuitive notion of local causality [1], namely, that there is simply a common cause of the two outcomes. The correlations that are compatible with this causal model are those of the form [17]

\[
P^\text{ccc}_{XY|ST} := \sum_{\Lambda} P_X|S_A P_Y|T_A P_\Lambda, \tag{1}
\]

for some conditional probability distributions \( P_X|S_A, P_Y|T_A, P_\Lambda \). We refer to this model as “parametrically classical and structurally common-cause”, abbreviated cCC. It is the recasting, within the framework of causal models, of what is typically called a
local hidden variable model. Distributions that are generated according to Eq. (1) satisfy Bell inequalities, and therefore the cCC model is rejected if one observes a significant violation of such an inequality. We now turn to a description of a variety of causal models that can violate Bell inequalities.

For anyone who presumes that the parameters in a causal model must be conditional probability distributions, it becomes necessary, in order to account for a Bell inequality violation, to presume a causal structure distinct from that of the DAG of Fig. 1(b). We here consider the two most prominent classes of such parametrically conservative and structurally radical proposals.

The first class consists of those models that posit that there is a causal influence from the setting or outcome variable in one lab to the setting or outcome variable in the other lab, so that there is a cause-effect relation between the labs, rather than simply a common-cause relation. We therefore refer to any such causal account as parametrically classical and structurally cause-effect, abbreviated as cCE. When the measurements in the labs are space-like separated—as they are in any experiment that seals the locality loophole [3]—these inter-lab causal influences must be superluminal. In this article, we will consider one particular representative from the class of cCE models, corresponding to assuming the DAG depicted in Fig. 1(c). We denote it by a subscript ‘0’, i.e., cCE$_0$, simply as a reminder that there are other models in the cCE class. We will refer to the cardinality of the set of values that a variable $\Lambda$ can take as simply the cardinality of $\Lambda$. We consider each possibility for the cardinality of $\Lambda$—up to the cardinality that saturates the set of achievable distributions [23]—as a distinct model. (A more detailed discussion of the cardinality of $\Lambda$ is provided in Appendix A.) We assume that the model is parameter-unrestricted, so that we allow any classical parameter values.

The compatible correlations in this case (for a fixed cardinality of $\Lambda$) are those that can be written as

$$P_{XY|ST}^{cCE_0} := \sum_{\Lambda} P_X|S_{\Lambda}P_Y|T_{\Lambda}P_{\Lambda}.$$  

(2)

for some choice of conditional probability distributions $P_X|S_{\Lambda}$, $P_Y|T_{\Lambda}$, and $P_{\Lambda}$.

A second class of causal accounts consists of those models that posit that there is a latent variable that causally influences not only the two outcomes, but also one or both of the setting variables. We refer to such models as parametrically classical and structurally superdeterministic, abbreviated as cSD. (See Ref. [7] for a justification of this causal-modelling perspective on the hypothesis of superdeterminism.) We will again consider one particular representative from this class, corresponding to assuming the DAG depicted in Fig. 1(d). We denote this model by cSD$_0$, where the subscript simply serves as a reminder that there are other representatives. Again, we consider each possibility for the cardinality of the latent variable $\Lambda$ as a distinct model, and we take the model to be parameter-unrestricted. Because the setting $S$ has a causal parent in this DAG, we must explicitly condition on $S$ to obtain the conditional $P_{XY|ST}$. That is, $P_{XY|ST}^{cSD_0} = P_{XY|S}P_{S|YT}/P_{S|T}$. Consequently, the compatible correlations in this case (for a fixed cardinality of $\Lambda$) are those that can be written as

$$P_{XY|ST}^{cSD_0} := \frac{\sum_{\Lambda} P_X|S_{\Lambda}P_Y|T_{\Lambda}P_{S|\Lambda}P_{\Lambda}}{\sum_{\Lambda} P_{S|\Lambda}P_{\Lambda}}.$$  

(3)

for some choice of conditionals $P_X|S_{\Lambda}$, $P_Y|T_{\Lambda}$, $P_{S|\Lambda}$ and $P_{\Lambda}$.

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1 In fact, as shown in Ref. [22], such influences must have infinite speed.
The final causal model we consider is one that is structurally of the common-cause form, just as CCC is, but parametrically quantum, abbreviated qCC and depicted in Fig. 1(e). Here, the latent common cause consists of the composite of the pair of quantum systems prepared in the Bell experiment, denoted AB. The associated node in the DAG is depicted differently from the others as a reminder that the parameters which make reference to it are conditional density operators rather than conditional probability distributions. The distributions over the observed variables that are deemed compatible are computed from an expression similar to Eq. (1), but where do-conditionals are replaced by conditional density operators. In the notation of Ref. [21], this expression is

$$P^{\text{qCC}}_{XY|ST} := \text{Tr}_{AB} \left( \rho_{X|SA} \rho_{Y|TB} \rho_{AB} \right)$$

for some choice of conditional density operators $\rho_{X|SA}$, $\rho_{Y|TB}$, and $\rho_{AB}$. In a more conventional notation, the compatible distributions are those whose components can be expressed as:

$$P^{\text{qCC}}_{XY|ST}(xy|st) := \text{Tr}_{AB} \left[ (E^{A}_{x}|s) \otimes (E^{B}_{y}|t) \rho_{AB} \right],$$

for some choices of $\{E^{A}_{x}|s\}$ and $\{E^{B}_{y}|t\}$, which are positive operator-valued measure on systems $A$ and $B$ respectively, and for some choice of $\rho_{AB}$, which is a density operator on the bipartite system $AB$. Note that one obtains a distinct model for every choice of Hilbert-space dimension for $A$ and $B$.

Model Selection. Each causal model we consider defines, via Eqs. (1), (4), a set of correlations that are compatible with it. Each of these sets constitutes a statistical model. The problem of causal model selection, therefore, reduces to statistical model selection. We denote the set of all possible distributions compatible with a causal model $M$ by $\mathcal{P}_{M}$.

Consider the problem of identifying which values of the parameters in a causal model best fit a given set of observed data. Quality of fit is measured by a loss function between the set of correlations realized by those parameter values (the realized statistical model) and the observed relative frequencies. The best-fit model is the one that minimizes this loss function. We will use the squared error loss function, which for our data corresponds to

$$\text{loss}(P,F) = \sum_{s,t,x,y} \left[ P_{XY|ST}(xy|st) - F(xy|st) \right]^{2},$$

where $F(xy|st)$ is the observed relative frequency of outcomes $X = x$ and $Y = y$ given settings $S = s$ and $T = t$, while $P_{XY|ST}(xy|st)$ is the probability of outcomes $X = x$ and $Y = y$ given settings $S = s$ and $T = t$ for a particular choice of parameters in the model. Here, $F$ and $P$ denote the matrices whose components are $F(xy|st)$ and $P_{XY|ST}(xy|st)$ respectively. We opt for this loss function as it is the most common choice.

The set of data that one uses to optimize the model parameters is called the training set, denoted $F_{\text{train}}$. The minimum value of the loss achieved by a model $M$ in a variation over parameter values consistent with that model is termed the training error for $M$,

$$\text{TrainingErr}_{M} = \min_{P \in \mathcal{P}_{M}} \text{loss}(P,F_{\text{train}})$$

The set of correlations defined by the best-fit model is consequently

$$\hat{P}^{M} = \arg\min_{P \in \mathcal{P}_{M}} \text{loss}(P,F_{\text{train}})$$

Returning to the problem of model selection, the reason one cannot simply select the model with the smallest training error is that this would fail to take into account overfitting. For example, in the case of a pair of models, $M$ and $M'$, where there is strict inclusion of the sets of compatible distributions, $\mathcal{P}_{M'} \subset \mathcal{P}_{M}$, such a selection criterion would always prefer the model with the largest set of compatible distributions, even though the latter may be less predictively accurate. A more appropriate criterion is to select the model that minimizes the predictive error for independent data, a quantity termed the test error [24].

Strictly speaking, the test error of a statistical model is defined as its quality of fit with the true underlying distribution. However, the latter is unknown, and so in practice one makes use of an estimate of the test error. We follow a standard approach for data-rich problems, wherein one estimates the test error of one’s statistical model using a second data set called the test set and denoted $F_{\text{test}}$. Specifically, we use what is termed the plug-in estimate of the test error [25],

$$\text{TestErr}_{M} = \text{loss}(\hat{P}^{M},F_{\text{test}}),$$

where $\hat{P}^{M}$ is as defined in Eq. (7), namely, the statistical model defined by causal model $M$ and parameter values that yield the best fit to the training set. If the test set is large, this is likely to be a good estimate of the true test error of the statistical model $\hat{P}^{M}$. Our criterion for model selection is minimization of the estimated test error. Hereafter, we will refer to the estimated test error as simply the test error.

Note that for the CCC models, we treat each possibility for the cardinality of $\Lambda$ as a separate model and find the one with the most predictive power. Thus, the test error reported for these is the minimal value in a variation over this cardinality. (We take a similar approach to cardinality in fits to the CCC causal model, to be discussed below.) For the quantum model, where the common cause is modelled as a bipartite quantum system $AB$ rather than a variable $\Lambda$, the analogue of the cardinality of $\Lambda$ is the dimension of the Hilbert space describing $AB$. Because we found that the model wherein $AB$ is a pair of qubits already outperformed the other causal models on our slate of candidates, we did...

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2 Although we also performed the data analysis using other loss functions and the conclusions remained the same.
Figure 2. Experimental diagram. Maximally polarization entangled photons pairs are created through parametric down-conversion in both paths of a Sagnac interferometer. After compensating for the drift in the fiber-optic, each photon is sent to a polarization measurement, where the choice of measurement is controlled by half-wave plates, and coincidence counts are recorded. In the dephased version of the experiment (discussed in Appendix C 3), a dephasing channel based on an LCR is implemented on one of the photons prior to measurement, while in the entangled version of the experiment, this channel is absent. PPKTP, periodically-poled potassium titanyl phosphate; PBS, polarizing beamsplitter; LCR, liquid crystal retarder; HWP, half-wave plate; QWP, quarter-wave plate; DC, dichroic mirror.

not explore qCC models with higher-dimensional common causes.

Results. In our experiment, polarization entangled photons are generated using type-II spontaneous parametric down-conversion (Fig. 2) at a rate of 22000 singles/s and 800 coincidences/s with a 3ns coincidence window. The source produces the state $\frac{1}{\sqrt{2}}(|HH\rangle + |VV\rangle)$ with 97.9 ± 0.07% fidelity. The photons are sent to different polarization analyzers, functioning as Alice’s and Bob’s labs, each one implementing one of six possible binary-outcome measurements. The data was collected for 10s for each pair of values $(s, t)$ of the measurement setting variables, and the relative frequency $F(xy|st)$ with which Alice obtains outcome $x$ in coincidence with Bob obtaining outcome $y$ was recorded. The Poissonian noise model for the photon counts is used to generate bootstrap estimates of the confidence intervals. The entire experiment was performed twice, thereby yielding a training data set and a test data set. These two data sets are the only inputs to our causal discovery algorithm.

Given that our purpose here is to adjudicate between models that can account for a violation of Bell inequalities, we leave aside cCC, and focus on adjudicating between cCE0, cSD0, and qCC. The training errors and the test errors for each are shown in Fig. 3. Recall that the selection criterion is minimization of the test error. As the difference in test error between cCE0 and qCC is approximately 5 standard deviations, and there is a similar gap between cSD0 and qCC, it follows that qCC emerges as the preferred model with high statistical confidence.

Discussion. We now address the question of why qCC tests better (i.e., achieves a lower test error) than cCE0 and cSD0. The fact that both cCE0 and cSD0 train better (i.e., achieve a lower training error) than qCC provides some insight into why this is the case. When a model trains better but tests worse than another, a likely explanation is that the first is more prone to overfitting—achieving a better fit to the training data by fitting to statistical fluctuations found therein—and this in turn implies a worse fit to the test data. It is likely, therefore, that cCE0 and cSD0 overfit the data as compared to qCC.

In a Bell experiment, there is statistical independence between an outcome variable at one wing and the setting variable at the opposite wing. This is typically termed the ‘no-signalling condition’. Classical causal models that are structurally radical and parameter-unrestricted can reproduce the no-signalling condition, but, as shown in Ref. [7] (see also Ref. [26]), they can only do so for a special class of values of the parameters (which is a set of measure zero in the full set of possible parameter values, so that they require fine-tuning in order to do so). Given that any finite sample of data exhibits statistical fluctuations away from such independence, in a structurally radical classical causal model, it is possible for the fitting procedure to mistake these fluctuations for real features, thereby yielding best-fit values of the parameters outside of the special class. In short, such models have an opportunity to overfit the data. For structurally conservative models like qCC, on the other hand, there is no possibility of such overfitting because the no-signalling condition is implied by the causal structure and therefore holds for all choices of the parameter values. This is the reason,
Figure 3. Adjudicating between different causal models based on the experimental data. Plotted are the training error (blue) and test error (red) for the cCE0, cSD0 and qCC models. Error bars denote a confidence region of one standard deviation. The qCC model has the lowest test error and is therefore preferred. The fact that the larger test error of the cCE0 and cSD0 models is accompanied by a lower training error suggests that they overfit the data relative to qCC.

we believe, that cCE0 and cSD0 overfit the data as compared to qCC.

Although we have here considered only the cCE0 representative of the cCE class of models and only the cSD0 representative of the cSD class, similar considerations apply for other representatives. More precisely, for every DAG in these classes—regardless of what pattern of inter-lab influences or superdeterministic common causes they posit—as long as the model is parameter-unrestricted, the no-signalling condition is only reproduced for a special class of parameter values \( \mathcal{P} \). Consequently, all such models are likely to be found to have less predictive power than qCC by virtue of overfitting.

Note that, relative to this account of the overfitting, one expects to obtain similar results even if the quantum source is dephased in such a way that the bipartite state it prepares is entangled. We confirmed this expectation by performing a dephased version of our experiment and verifying that although the cCC model now performs comparably to the qCC model (since it also satisfies the no-signalling condition for all parameter values), the cCE0 model still trains noticeably better and tests noticeably worse than either the cCC or qCC models, while the cSD0 model trains marginally better and tests marginally worse. This lends further support to our interpretation of the overfitting. Details are provided in Appendix C.

Conclusions. In this article, we have confined our attention to causal model types wherein no restriction is imposed on the possible values of the parameters. The fact that the classical causal models that posit inter-lab causal influences are disfavoured relative to models of the qCC type implies that one does not need to seal the locality loophole in a Bell experiment (i.e., perform measurements at space-like separation) in order for it to provide evidence of quantumness. In addition, the fact that the classical causal models that are superdeterministic are disfavoured relative to models of the qCC type overturns the claim that the loophole associated to the possibility of superdeterministic models cannot be closed.

We now consider what conclusions can be drawn from our results and our data analysis technique if one relaxes the assumption that there is no restriction on parameter values, that is, what conclusions one can draw for parameter-restricted causal models.

If the range of parameter values is restricted in such a way that the model is compatible with all and only the correlations achievable in operational quantum theory, so that in particular the no-signalling condition is satisfied for all parameter values, then one cannot hope to experimentally distinguish it from qCC via our model selection technique. The standard view of Bohmian mechanics [27] is likely to be an example of such a model (which is structurally radical by virtue of allowing inter-lab causal influences). Presumably, one can also construct superdeterministic models of the Bell experiment that are of this type.

If, on the other hand, the range of parameter values is restricted, but not such that it allows only those correlations achievable in operational quantum theory, then the model remains an empirical competitor to operational quantum theory. Many proponents of structurally radical ways out of the Bell no-go result do, in fact, endorse this type of model and indeed take its empirical inequivalence to operational quantum theory to be one of its virtues. Valentini’s subquantum-nonequilibrium version of Bohmian mechanics [28-31], which explicitly allows for the possibility of violations of the no-signalling condition in Bell experiments, is an example of such a model. Because a model with a more restricted scope of parameter values can in principle exhibit less overfitting than its parameter-unrestricted counterpart, the conclusions of our analysis need not apply to these. Nonetheless, these models can be included in the slate to which one applies the model selection technique described here. It is merely a question of specifying the range of parameter values and restricting the optimization to this range. A more detailed description of these conclusions is included in Appendix D.

The results reported here serve as a constraint on the development of alternatives to quantum theory and of interpretations of the formalism. More generally, the techniques we introduce have broad applicability in quantum foundations, as not just Bell-type experiments but all experiments seeking to adjudicate between different accounts of quantum phenomena stand to benefit from a consideration of whether a given account thereof overfits the data, rather than merely a demonstration that it does not underfit the data.
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Appendix A: Further details on the slate of causal models

In the framework for causal modelling that we are presuming, the modifiers ‘classical’ and ‘quantum’ refer only to the parametric part of a causal model. The latter is considered classical if the parameters can be specified as conditional probability distributions, while it is considered quantum if the parameters are specified as conditional density operators (or, equivalently, completely positive trace-preserving maps). The structural part of a causal model is presumed to always be stipulated by a DAG. If the causal structure mirrors the structure of the usual quantum expression for the correlations when the latter is conceptualized as a contraction of tensors, then it is deemed conservative. Otherwise, it is deemed radical. In the case of a Bell experiment, the usual quantum expression for the correlations is given in Eq. of the main text, so that an account has a conservative causal structure only if the DAG has the common-cause form of Figs. (b) or (e).

A quantum causal model of the experiment is defined in such a way that it can provide a causal account of the quantum predictions using the conservative causal structure, but where the price for the structural conservatism is that the model must be parametrically radical. A classical causal model of the same experiment, by contrast, salvages parametric conservatism by using only conditional probability distributions, but is thereby forced to be radical at the level of the causal structure.

The causal-modelling perspective on the different ways out of the Bell no-go result was initiated in Ref. [7]. That article stipulated how to recast within the framework of classical causal models the various traditional causal accounts of Bell inequality violations, all of which are parametrically conservative (i.e., parametrically classical) and structurally radical. These are the accounts that invoke interlab cause-effect relations or superdeterminism. We say a bit more about each case here.

The cCE class of causal models of the Bell experiment, wherein one posits a cause-effect influence between the labs, subsumes several possibilities for the causal structure.

In this article, we focussed on one such structure, wherein there is a causal influence from the setting $S$ to the outcome $Y$. By symmetry, of course we could just as well have considered the model wherein there is a causal influence from the setting $T$ to the outcome $X$. A distinct model within the cCE class is one wherein there are setting-to-outcome causal influences in both directions, that is, $S \rightarrow Y$ and $T \rightarrow X$. Another such model is one wherein there is a causal influence from the outcome rather than setting on one wing to the outcome on the other wing, such as $X \rightarrow Y$. More generally, every combination of influences between $S$, $X$ and $T, Y$ (e.g., every combination of arrows $X \rightarrow Y$, $S \rightarrow Y$, $S \rightarrow T$, $X \rightarrow Y$, $Y \rightarrow X$, $T \rightarrow X$, $T \rightarrow S$, $Y \rightarrow S$) that does not introduce cycles when added to the DAG of Fig. (b) constitutes an element of the cCE class of causal models. For all such models, the no-signalling condition is not implied by the causal structure. As we noted in the main text, the results of Ref. [7] imply that—assuming we are correct in our assessment that models that can violate the no-signalling condition tend to overfit the data—all models in this class that can realize the quantum correlations will be found to overfit the data and therefore to test poorly.

It is important to note that there are models in this class for which the modifications to the causal structure, radical as they are, are still underfit quantumly realizable data and consequently do not even train well on such data. An example is the classical causal model wherein the outcome at one wing (rather than the setting) has a causal influence on the outcome on the other wing, for instance, one where the DAG of Fig. (b) in the main text is supplemented by an arrow from $X$ to $Y$. For the case of ternary setting variables and binary outcomes, such a causal model was shown in Ref. [55] to imply Bell-like inequalities that are quantumly violated. As such, one expects that the performance of such models on experimental data that violates such inequalities would be comparable to the performance of the cCC model on our experimental data (see Sec. [C]). That is, insofar as such models would underfit the data, they would both train and test poorly. The same comments apply to causal structures wherein the outcome at one wing has an influence on the setting at the other wing.

We noted in the main text that structurally radical
models have the capacity to describe violations of the no-signalling condition, which ultimately is what causes them to overfit the data relative to the structurally conservative model. An example helps to illustrate this point. Within the context of a pilot-wave theory, Valentin has proposed [28–31] that obtaining the predictions of operational quantum theory might be contingent on the hidden variables being in a state of subquantum equilibrium, and that states of subquantum nonequilibrium—allowing for deviations from the predictions of operational quantum theory—might be possible in exotic scenarios. In particular, the proposal explicitly allows for violations of the no-signalling condition in subquantum nonequilibrium.

We now turn to the cSD class of causal models of the Bell experiment: classical causal models that posit that one or both of the setting variables share a common cause with the outcome variable at the opposite wing. If one considers the case where the setting $S$ shares a common cause with the outcome $Y$, then the resulting causal model is subsumed within the causal model for which the causal structure includes a three-way common cause of $S$, $X$ and $Y$. The latter is the one depicted in Fig. 1(d) and which we have focussed on in this article. Note that the superdeterminist view is sometimes described as positing a common cause $\Lambda'$ between a setting variable (say $S$) and the hidden variable $\Lambda$ in the DAG of Fig. 1(b), but without loss of generality one can absorb $\Lambda$ into the definition on $\Lambda'$, thereby obtaining the causal structure that posits a common cause of $S$, $X$ and $Y$.

To obtain a distinct representative of the cSD class, it suffices to swap the roles of the pair of parties relative to the DAG of Fig. 1(d) and have $\Lambda$ be a common cause of $T$, $X$ and $Y$. Alternatively, one could presume that $\Lambda$ influences not only the pair of outcome variables, but both setting variables as well.

As noted in Ref. [7], generic values of the parameters in such models also yield nontrivial correlations between an outcome variable and the setting variable in the opposite lab, hence a violation of the no-signalling condition. As in the cCE case, it is this possibility that makes such models prone to overfitting the data relative to the structurally conservative model.

The distributions compatible with a given classical causal model depend on the cardinalities of the latent variables. Suppose a given causal model incorporates a latent variable $\Lambda$. The set of distributions that are compatible with the classical causal model where $\Lambda$ has cardinality $d$ contains the set of distributions where $\Lambda$ has cardinality $d'$ for any $d' < d$. Even if one is considering the true causal structure, therefore, if one assumes too low a cardinality for the latent variables, one might still underfit the data. If not underfitting the data were the only standard of empirical success, it would be reasonable to presume the cardinalities of the latent variables to be those that are just sufficient to saturate the set of distributions compatible with the causal structure (these are known to be finite for classical causal models where the observed variables are discrete [23]). This choice, however, may overfit the data relative to a model with smaller cardinalities. This is why, for a given causal structure, we treat each possibility for latent cardinalities as a separate representative of its class of causal models, a separate model in the slate of candidates.

The qCC model posits the conservative causal structure, that is, a common cause acting between the labs, but it requires the parameters to be conditional density operators rather than conditional probability distributions, as noted in the main text.

For such a model, the common cause is modelled as a bipartite quantum system $AB$ rather than a variable $\Lambda$, so the analogue of the cardinality of $\Lambda$ is the dimension of the Hilbert space describing $AB$. Nonetheless, the same considerations hold: a dimension that is higher than the optimum may lead to overfitting of the data while a dimension that is lower than the optimum may lead to underfitting of the data, so it is best to treat each possibility for the dimension as a separate representative of the class of quantum causal models. As noted in the main text, however, the model wherein $A$ and $B$ are both qubits, so that $AB$ has dimension 4, already outperformed the other causal models on our slate of candidates, and so we did not need to consider any variation in this dimensionality.

1. **On whether the qCC model provides a satisfactory causal explanation**

The idea that the best causal account of Bell inequality violations is achieved by qCC, that is, a causal model with a quantum common cause, is one that has been espoused in Refs. [14, 21, 37]. As was acknowledged in those articles, however, whether such a causal model can really be deemed to achieve a realist account of the experimental data (as structurally radical classical causal models do) is one that has not yet been adequately answered. Ref. [38] argues that providing an affirmative answer to the question depends on the success of a research program that seeks to modify the notions of causation and inference to achieve a novel, nonclassical, type of realism that can underlie operational quantum theory.

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5 In terms of the framework introduced in Ref. [38], the present article is about adjudicating between different “quotiented operational causal-inferential theories” based on their power to predict the experimental data. It is not directly about adjudicating between realist representations thereof. However, the assumption that any realist representation of an operational causal-inferential theory must be diagram-preserving means that the latter must posit the same causal structure as the former for any given experiment. (See Section V.A of Ref. [38] for a defense of the assumption of diagram preservation.) Consequently, if one provides experimental evidence against a given causal structure for operational accounts of the experiment, one has provided experimental evidence against that causal structure for realist accounts as well.
Appendix B: Further details on the causal discovery algorithm

We here provide further information about the algorithm we propose for adjudicating between causal models. A classical causal discovery algorithm \[17, 18\] adjudicates between different classical causal models based on purely observational data. The algorithm we describe is also based on purely observational data, but the slate of candidate causal models is allowed to include intrinsically quantum causal models and allows a direct comparison between these and classical causal models using the same scoring criteria.

In the statistics community, in order to accommodate data-poor applications, a variety of techniques have been developed to estimate the test error while still using all of the acquired data for fitting the model parameters. Common techniques of this sort include the Akaike Information Criteria (AIC), the Bayesian Information Criteria (BIC) and Cross Validation (CV). The experiment we consider, however, is data-rich, and consequently we can simply fit the model parameters using one data set, termed the training set, and estimate the test error using a second data set, termed the test set.

A schematic of the algorithm is provided in Fig. 4. The input of the algorithm is the observed relative frequency \( F(xy|st) \) of outcomes \( X = x \) and \( Y = y \) given settings \( S = s \) and \( T = t \) for the training set and the test set. The output of the algorithm is the training error and the estimated test error, as well as their standard deviations, for each of the causal models on the slate of candidates. These allow for the determination of an ordering of the models by their relative predictive power, as well as an estimate of the statistical confidence of this ordering.

For each causal model \( M \), one minimizes the training error over the conditional probability distributions in the set \( P_M \) that are compatible with the model. As noted in the main text, we take the least squared error as our loss function. We note, however, that the data analysis was repeated for three other loss functions and the conclusions were essentially unchanged.

The set of distributions that are compatible with a generic causal model \( M \), that is, \( P_M \), is generally a non-convex set. In particular, this is the case for the causal models of interest to us here whenever the cardinality of \( \Lambda \) or the dimension of the \( AB \) system are not maximal. Consequently, our technique requires a non-convex optimization, which is generally difficult as there can be multiple local minima. Indeed, the optimization comprises almost the entire computational burden of the algorithm. To do so, we use the Nelder-Mead method, implemented via NMinimize in Mathematica. We repeated each optimization 100 times which different random seeds in order to increase the chances of finding the global minimum. Given that our loss function is an estimate of the test error, identifying a local minimum rather than the global minimum provides a worse estimate of the true test error. However, there is no reason to think that the model is more likely to get stuck in any one local minimum rather than another.

The next step is to calculate, for each model on the slate of candidates, the estimated test error for the distribution that was found to minimize the training error. This is a relatively inexpensive computation. The model that is favored by the model selection technique is the one which is estimated to have the most predictive power, that is, the one that achieves the smallest estimated test error.

Error bars for the training and test error are calculated by doing parametric bootstrap re-sampling of the initial frequency counts in the data sets (a type of Monte Carlo error estimation). The entire algorithm is repeated to find the test and training errors for the re-sampled counts. We performed 10 re-samplings and used the empirical standard deviation of this sample as our error bars.

As was noted in Section A, in order to find the most predictive model with a given causal structure, one must allow for variability in the cardinality of the latent variables therein. A brute-force optimization over the choice of these cardinalities, however, is computationally inefficient. To reduce computation time, therefore, we introduce a heuristic for this optimization, which we now describe for the case of the Bell experiment. For a given structurally radical hypothesis, such as CCE\(_0\) or CSD\(_0\), we order the causal models associated to it by the cardinality of \( \Lambda \). We then search through increasing values of cardinality for the most predictive model, and we end the search if either of the following two conditions are met: (i) for three consecutive values of the cardinality, the training error decreases while the estimated test error increases, or (ii) for three consecutive values of the cardinality, the training error and estimated test error take the same value. The first condition is reasonable since it is likely to describe a situation wherein the increasing expressive power of the models is yielding increasing degrees of overfitting, so that continuing to increase the expressive power by increasing the cardinality would only further decrease the predictive power. The second condition is reasonable since it is likely to describe a situation wherein the increasing expressive power of the models is not yielding increasing degrees of overfitting, but also not improving the fit, so that increasing the cardinality will not lead to any further increases in predictive power.\(^6\)

1. Comparison to prior work

As noted above, the fact that our model selection technique is applicable to purely observational data makes it

\(^6\) This can occur, for instance, if the cardinality has increased beyond the value that saturates the set of compatible distributions, supposing one does not know a priori what this value is.
Figure 4. A schematic of our model selection technique, where the quantities referenced are defined in Eqs. (5), (7), and (8) of the main text.

Appendix C: Further details on the experiment

1. Selection of the set of measurements

In this section, we describe the motivation behind the choice and number of measurements we implemented in our Bell experiment. We sought to perform the minimal number of measurements which could distinguish the various causal models with high statistical confidence. Minimizing the number of measurements is desirable because it limits the time required to complete the experiment and therefore also limits the error introduced by temporal drift of the experimental components. We did not attempt, however, to find provably optimal sets. Instead, we considered sets of binary-outcome projective measurements chosen in such a way as to be spread out over the Bloch sphere. For different values of \( n \), we chose \( n \) rank-1 projectors that were equally spaced along a 1-parameter family that traces a spiralling curve on the Bloch sphere (see Fig. 5). We then determined, by simulating data for an idealized version of our experiment, the minimal value of \( n \) for which our causal discovery algorithm could distinguish the slate of causal models under investigation. In this way, we settled on implementing six binary-outcome measurements at each side of our Bell experiment.

Our simulations indicated, in particular, that if instead of the six measurements along the Bloch sphere spiral, one were to implement the standard pair of measurements appearing in the Clauser-Horne-Shimony-Holt (CHSH) version of the Bell experiment, then the \( \text{cCE}_0 \), \( \text{cSD}_0 \), and \( \text{qCC} \) models had scores within one standard deviation of each other (for a quantity of data that was matched to what we could hope to achieve experimentally), such that our model selection technique could not reliably adjudicate between them. By contrast, for the
six binary-outcome measurements described in Fig. 5, the test error of the most predictive model (qCC) was separated from that of the other models (cCE₀ and cSD₀) by approximately 10 standard deviations in our simulations, thereby suggesting that this choice would be adequate for our purposes (which the experiment and our data analysis subsequently confirmed).

It is at present not clear how to anticipate by theoretical considerations rather than numerical simulations the minimum number of measurements required by our model selection technique to achieve a given level of statistical confidence.

2. Performance of the model that is parametrically classical and structurally common-cause

We noted in the main text that the cCC model, by virtue of being incapable of violating the Bell inequalities, cannot possibly do justice to our experimental data. For this reason, it was not included in the slate of candidate causal models among which we implemented model selection. Nonetheless, confirming the expectation that the cCC model is not viable as a causal account of our Bell experiment provides an additional check on our model selection technique, and so we do so here. Note that our technique does not establish the nonviability of this model in the standard way, that is, by demonstrating that our experimental data violates a Bell inequality by many standard deviations. Rather, it considers the training error and the test error of the cCC model for our data. The model is found to have a training error of 790±10, far in excess of the training error of cCE₀, cSD₀ or qCC (reported in Fig. 3 of the main text), indicating that it underfits the data relative to them, as one would expect given its inability to violate Bell inequalities. It consequently also has poor predictive power, achieving a test error of 800 ± 10, also far in excess of cCE₀, cSD₀ or qCC.

3. The dephased version of the experiment

We now describe the results of applying our model selection technique to the version of our experiment wherein the initial entangled state is completely dephased, so that it becomes a separable state.

Our primary motivation for doing so is to provide additional evidence for our interpretation of the results of the entangled version of the experiment. As noted in the main text, the fact that cCE₀, cSD₀ train better but test worse than qCC suggests that they overfit the data relative to qCC. Specifically, the statistical fluctuations away from the no-signalling condition (which are possible in any finite-run data) can be mistaken as real features by cCE₀ and cSD₀, but not by qCC, since qCC satisfies the no-signalling condition for all parameter values. But if this is indeed the case, then one expects analogous results in a dephased version of the experiment (which removes entanglement, leaving only classical correlations), where now the cCC model can do justice to the observations while also satisfying the no-signalling condition for all parameter values. Because the dephased version of the experiment is also likely to exhibit statistical fluctuations away from the no-signalling condition, one expects that cCE₀ and cSD₀ can once again mistake these fluctuations for real features, while neither qCC nor CCC can do so, thereby leading cCE₀ and cSD₀ to overfit the data relative to qCC and CCC.

The dephased version of our experiment also provides another opportunity to check our model selection technique: given that it prepares a separable state and therefore cannot violate any Bell inequalities, one expects that the cCC model should have as much predictive power as the qCC model for this experiment.

The entanglement between the two photons is removed by implementing a completely dephasing channel in the basis of eigenstates of the Pauli X operator (i.e., a noisy bit-flip channel) on one of the photons, via rapid switching between a Pauli X gate and an identity gate. This is achieved using a liquid crystal retarder (LCR) that is switched every 500ms. The bipartite state after the dephasing channel is found to have 98.3 ± 0.07% fidelity with the equal mixture of $\frac{1}{\sqrt{2}}(|HH\rangle + |VV\rangle)$ and $\frac{1}{\sqrt{2}}(|HV\rangle + |VH\rangle)$, which is a separable state, namely, $\frac{1}{2}|DD\rangle\langle DD| + \frac{1}{2}|AA\rangle\langle AA|$. The rest of the experiment proceeds as before, as does the data analysis.

The results of the experiment are presented in Fig. 6. We confirm that the cCC model has a training error and test error that are comparable (i.e., within error) to those of the qCC model. We also see that the cCE₀ model trains noticeably better and tests noticeably worse than either cCC or qCC, while the cSD₀ model trains marginally better and tests marginally worse than these, thereby confirming our expectations about their relative
Appendix D: Further details on the conclusions that can be drawn from our results

1. Some distinctions among causal models

We wish to consider in more detail what our results imply for various loopholes in Bell experiments. To prepare the ground for this discussion, it is useful to introduce some distinctions among causal models based on the scope of parameter values that they allow. We do so in a manner that is not specific to the Bell experiment, so that such distinctions can be articulated for experiments with arbitrary compositional structures.

Let the DAG corresponding to the conservative causal structure for an experiment be denoted by $G$, while structurally radical alternatives to it are denoted $G_0, G_1, \ldots$. Let the model of the experiment that is structurally conservative but parametrically quantum (i.e., parametrically radical) be denoted $qG$, and let the class of models that are parametrically classical (i.e., parametrically conservative) but structurally radical—in the sense of assuming a DAG $G_i$ that is not the conservative one—be denoted by $cG_i$.

As noted in the main text, causal models can also be distinguished by what they assume about the cardinality of the latent variables in the DAG. However, we shall not introduce additional notation for this distinction. Rather we will presume that when a given class of causal models is considered as a candidate account of some data, it is the particular element of the class that optimizes the cardinality of the latent variables that is considered.

Within a given class $cG_i$, one can distinguish different models based on the fact that one can imagine restrictions on the parameter values allowed in the model. There can be different types of restrictions on the parameters, each of which leads to a different model within the class.

First, consider what it means to assume no such restriction. For each variable $X$ corresponding to a node of $G_i$, the usual notion of a classical causal model allows any conditional probability distribution $P_X|\{\text{Pa}(X)\}$ in the set $P_X|\{\text{Pa}(X)\}$ of all such conditionals. As noted in the main text, we refer to such a classical causal model as the parameter-unrestricted model for $G_i$. In the context of the Bell experiment, $cCE_0$ is the parameter-unrestricted model for the DAG of Fig. (1c), while $cSD_0$ is the parameter-unrestricted model for the DAG of Fig. (1d).

But one can also define parameter-restricted classical causal models. For one of more variables $X$ corresponding to the nodes of $G_i$, the possibilities for $P_X|\{\text{Pa}(X)\}$ are presumed to be restricted to a set $P_X|\{\text{Pa}(X)\} \subset P_X|\{\text{Pa}(X)\}$, that is, to a strict subset of the usual possibilities. We refer to such classical causal models as parameter-restricted models for $G_i$, and denote them by $c_jG_i$, where $j$ is an index that ranges over the choice of parameter restriction. More precisely, the $j$th parameter restriction is specified by specifying the set of allowed parameter values: $S_j \equiv \{P_X|\{\text{Pa}(X)\}\} \subset P_X|\{\text{Pa}(X)\}$.

For a classical causal model based on a DAG $G_i$, and a parameter restriction described by $S_j$, that is, for the model denoted by $c_jG_i$, the set $P_{c_jG_i}$ of distributions on the set $O$ of observed variables in $G_i$ are those of the form

$$P_O = \sum_{N_{\text{Nodes}(G)/O}} \left[ \prod_{X \in \text{Nodes}(G)} P_X|\{\text{Pa}(X)\} \right]$$

(D1)

where $P_X|\{\text{Pa}(X)\} \in P_X|\{\text{Pa}(X)\}$. In the case of the classical causal model for the DAG $G_i$ that is parameter-unrestricted, $cG_i$, the set of compatible distributions is denoted $P_{cG_i}$.

Clearly, the distributions that are compatible with a parameter-restricted classical causal model $c_jG_i$, are included within those that are compatible with its parameter-unrestricted counterpart $cG_i$,

$$P_{c_jG_i} \subseteq P_{cG_i}.$$ 

The interesting restrictions on the parameters, of course, are those for which we get a strict inclusion, $P_{c_jG_i} \subset P_{cG_i}$.

Let $P_{\text{param}G}$ denote the set of distributions on the observed variables that are compatible with a parametrically quantum (hence parametrically radical) and structurally conservative model, assuming no restriction on the parameters.

Since, in this article, we are only interested in classical causal models that can fit data consistent with operational quantum theory, we limit the scope of possible parameter restrictions to those that can achieve such a fit. More precisely, we consider only those parameter-restricted classical causal models, $c_jG_i$, for which there
is sufficient parametric freedom that the compatible distributions subsume those predicted by operational quantum theory, i.e., those for which $P^{c_jG_i} \supseteq P^{qG}$.

For these, it is useful to introduce a further distinction, based on whether or not the parametric freedom is such that the compatible distributions go beyond those of operational quantum theory:

$c_jG_i$ is quantum-on-the-nose: $P^{c_jG_i} = P^{qG}$
$c_jG_i$ is quantum-extending: $P^{c_jG_i} \supset P^{qG}$

In the quantum-on-the-nose case, the restriction on the set of parameters values is such that the set of compatible distributions on the observed variables for DAG $G_i$ coincides precisely with the set of distributions compatible with a quantum causal model for DAG $G$. In the quantum-extending case, the set of compatible distributions is a strict superset of the latter.

It is useful to make explicit the connection between the distinctions introduced here and the models discussed in the main text. For a Bell experiment, the DAG $G$ corresponding to the conservative causal structure is that of Fig. 1(b) or (e), the common-cause structure, while the possibilities for DAGs $G_0, G_1, \ldots$, corresponding to radical causal structures include the various different ways of allowing cause-effect relations between the labs (with the DAG of Fig. 1(c) being one way of doing so) and the various different ways of allowing for superdeterminism (with the DAG of Fig. 1(d) being one way of doing so). It follows that the model QCC plays the role of QG, while $cCE_0$ and $cSD_0$ are examples of parameter-unrestricted $cG_i$ models.

With these distinctions in mind, we are now in a position to discuss the pertinence of our data-analysis technique to various existing attitudes towards causal accounts of Bell inequality violations.

2. Reassessment of various loopholes in Bell experiments

In discussions of Bell experiments, a “loophole” is generally taken to be a plausible reason for denying the validity of some assumption required for the derivation of the Bell inequalities (or at least the validity of this assumption for a particular experiment), such that one escapes the contradiction that exists between these assumptions and the observed violations of Bell inequalities.

Consider a Bell experiment that violates a Bell inequality, but where the measurements on the wings are not done at space-like separation (such as the one described in this article). Such an experiment is said to suffer from a locality loophole, by which it is meant that one has a plausible reason for denying the validity of Bell’s notion of local causality in such an experiment, namely, that positing a causal influence between the labs does not require those influences to be superluminal, so that such influences should be considered unobjectionable.

For a Bell experiment that fails to achieve space-like separation, a model can leverage the locality loophole, that is, make use of a cause-effect relation between the labs, in order to reproduce the observations (in particular, the Bell inequality violations). As a consequence, it is generally believed that a good Bell experiment must seal the locality loophole, that is, it must enforce space-like separations between the measurements in the laboratories.

Recall that, as noted in the introduction, not underfitting the data is a low bar, and a better figure of merit in model selection is predictive power, which can be compromised not only by underfitting but by overfitting as well. Therefore, although leveraging the locality loophole allows one to “reproduce the observations”, i.e., to pass the low bar of not underfitting the data, it is not clear a priori whether it allows one to pass the high bar of not overfitting the data. Thus, even though positing a causal influence between the labs may be unobjectionable from the perspective of relativity theory, it can remain objectionable by virtue of having poor predictive power. In this way, one can in principle provide evidence against the hypothesis of inter-lab causal influences using model selection techniques.

Note that the claim that it is critical to seal the locality loophole has not traditionally come with any caveats. For instance, no one has claimed that sealing this loophole is only important for causal models wherein the parameters are restricted but not important for causal models wherein the parameters are unrestricted. Consequently, we will take the claim that it is critical to seal the locality loophole to be overturned if it is overturned in the context of parameter-unrestricted causal models. Given that the parameter-unrestricted classical cause-effect model $cCE_0$ was found in our experiment to have less predictive power than the quantum common-cause model QCC, and given that all parameter-unrestricted models in the cCE class are likely to perform similarly to $cCE_0$ (as noted in our discussion section), our results show that one can indeed collect evidence against the hypothesis of inter-lab causal influences, without requiring the measurements to have been space-like separated.

The superdeterminism loophole consists of the fact that it is possible to devise a superdeterministic model of a Bell experiment that reproduces the statistics predicted by quantum theory. It is often claimed that this loophole
hole cannot be tested experimentally. As before, “reproducing the statistics” means merely not underfitting the data, while model selection techniques hold models to a higher bar of predictive power, including not overfitting the data.

Because the claim about the unstability of the superdeterminism loophole have not been predicated on restricting the parameter values in a superdeterminist model, we shall consider this claim to be overturned if it is overturned in the context of parameter-unrestricted models. We have here shown that the parameter-unrestricted superdeterministic model $cSD_0$ has less predictive power than the quantum common-cause model $qCC$, and we have argued (in the Discussion section) that this is likely to be the case for all parameter-unrestricted superdeterministic models. Consequently, our results provide experimental evidence against such superdeterministic models, contrary to the claim that the superdeterminist loophole cannot be tested.\(^\text{10}\)

### 3. Models that are compatible with all and only the statistics predicted by quantum theory

In a structurally radical model of the Bell experiment that is quantum-on-the-nose, the range of parameter values is so restricted that it is compatible with all and only the correlations achievable in the $qCC$ model, and consequently satisfies the no-signalling condition for all parameter values in the restricted set. In this case, one cannot hope to experimentally distinguish it from the $qCC$ model. By choosing the parameter restriction carefully, one can in principle define a quantum-on-the-nose model for any structurally radical DAG that can reproduce Bell inequality violations, such as the DAGs associated to the $cCE_0$ and $cSD_0$ models.

The standard view of Bohmian mechanics \(^\text{27}\) is likely to be an example of such a model, where the DAG is one that allows for causal influences between the setting variable in one lab and the outcome variable in the other and so is in the $cCE$ class.

We are not aware of any superdeterministic models that are quantum-on-the-nose, although in principle one could define models of this sort. Such a model would yield a novel type of superdeterministic loophole insofar as it would be predicated on restrictions in the parameter values. This loophole—unlike the one discussed above—cannot be closed by any experiment for which the correlations are consistent with operational quantum theory.\(^\text{10}\)

### 4. Models that are compatible with a superset of the statistics predicted by quantum theory

Many proponents of structurally radical ways out of the Bell no-go result take their models to be empirical competitors to quantum theory, that is, they take them to be experimentally distinguishable from it in principle. These structurally radical classical causal models are parameter-restricted but in a way that is quantum-extending.

We begin with those that are structurally radical by virtue of positing inter-lab causal influences.

We believe that Valentini’s subquantum-nonequilibrium version of Bohmian mechanics \(^\text{28–31}\), which can simulate quantum theory but also explicitly allows for violations of the no-signalling condition, is of this type.

Because such models posit a more restricted scope of parameter values than we have assumed in the optimization that finds the best-fit parameter values, and because such restrictions could in principle reduce the degree of overfitting, the data analysis we have implemented here does not adjudicate between models of this sort and $qCC$. Nonetheless, in order to be able to achieve such an adjudication using our technique, it suffices to stipulate the range of parameter values that are allowed within a given model and to repeat our analysis while restricting the optimization to this range.

Note also that although one cannot conclude, based on our analysis, that $qCC$ has more predictive power than a model of the sort just mentioned, it is still the case that our experiment provided an opportunity for finding that $qCC$ has less predictive power than these alternatives. That is, our experiment provided an opportunity for finding that nature exhibits deviations from operational quantum theory, for instance, by exhibiting the sorts of violations of the no-signalling condition that are predicted by Valentini’s non-equilibrium version of Bohmian mechanics. No evidence for such deviations was found however. This is not unexpected insofar as such deviations are thought to only arise in exotic experimental scenarios, but a precision test in a non-exotic scenario nonetheless implies the possibility for finding such deviations. (See, e.g., the discussion of the distinction articulated in Ref. \(^\text{42}\) between ‘terra-nova’ and ‘precision’ strategies for finding new physics.)

Quantum-extending models that are superdeterministic are also possible in principle. (There has certainly been at least one proposal for a superdeterministic model that is empirically distinguishable from quantum theory, namely, that of Ref. \(^\text{12}\), but it is unclear to us whether or not it subsumes all the statistics predicted by operational quantum theory.) Our technique can in principle be used to adjudicate between such quantum-extending models and $qCC$. It suffices to repeat our analysis with a fitting procedure that restricts the optimization to the…

\(^{10}\) See also Ref. \(^\text{11}\) for a complementary perspective on how to modify a Bell experiment in order to be able to test the hypothesis of a superdeterministic account of Bell inequality violations.
appropriate range of parameter values.\footnote{Note that if a given proposed superdeterminist model does not subsume the statistics predicted by operational quantum theory, then it can also be disfavoured by our data analysis technique if our experimental data conforms with the quantum predictions. It is just that it will be disfavoured in the conventional way—on the grounds that it underfits the data.}

5. Summary

The conclusions we can draw from our experimental results about the viability of various causal models can be summarized as follows:

(i) Our results provide evidence against the claim that it is possible to get around Bell’s no-go result merely by endorsing one or another type of structural radicalism (together with parametric conservatism) without explicitly articulating any restriction on the scope of parameter values. Our results therefore provide a challenge to the position of those who suggest that Bell inequality violations imply superluminal influences or superdeterminism, but do not see fit to articulate any concrete model.

(ii) Our results do not provide experimental evidence against parameter-restricted causal models that are quantum-on-the-nose (such as the standard version of Bohmian mechanics, or superdeterministic analogues thereof).

(iii) Our results also do not provide experimental evidence against parameter-restricted causal models that are quantum-extending (such as Valentini’s subquantum-nonequilibrium version of Bohmian mechanics\footnote{\textit{J.-D. Bancal, in On the Device-Independent Approach to Quantum Physics (Springer, 2014) pp. 97–105.}}\footnote{\textit{D. Rosset, N. Gisin, and E. Wolfe, Quantum Information and Computation 18, 0910 (2018).}}\footnote{\textit{T. Hastie, R. Tibshirani, and J. Friedman, “Elements of statistical learning: data mining, inference, and prediction,” (2003).}}\footnote{\textit{L. Wasserman, All of statistics: a concise course in statistical inference (Springer Science & Business Media, 2013).}}\footnote{\textit{E. G. Cavalcanti, Physical Review X 8, 021018 (2018).}}\footnote{\textit{S. Goldstein, in The Stanford Encyclopedia of Philosophy, edited by E. N. Zalta (Metaphysics Research Lab, Stanford University, 2017) summer 2017 ed.}}\footnote{\textit{M. S. Leifer and R. W. Spekkens, Phys. Rev. A 88, 52130 (2013).}}\footnote{\textit{J.-D. Bancal, in On the Device-Independent Approach to Quantum Physics (Springer, 2014) pp. 97–105.}}\footnote{\textit{D. Rosset, N. Gisin, and E. Wolfe, Quantum Information and Computation 18, 0910 (2018).}}\footnote{\textit{T. Hastie, R. Tibshirani, and J. Friedman, “Elements of statistical learning: data mining, inference, and prediction,” (2003).}}\footnote{\textit{L. Wasserman, All of statistics: a concise course in statistical inference (Springer Science & Business Media, 2013).}}\footnote{\textit{E. G. Cavalcanti, Physical Review X 8, 021018 (2018).}}\footnote{\textit{S. Goldstein, in The Stanford Encyclopedia of Philosophy, edited by E. N. Zalta (Metaphysics Research Lab, Stanford University, 2017) summer 2017 ed.}}\footnote{\textit{A. Valentini, Physics Letters A 156, 5 (1991).}}\footnote{\textit{A. Valentini, Physics Letters A 158, 1 (1991).}}\footnote{\textit{A. Valentini, Physics Letters A 297, 273 (2002).}}\footnote{\textit{A. Valentini, in Non-locality and Modality (Springer, 2002) pp. 81–103.}}\footnote{\textit{L. Hardy, arXiv preprint gr-qc/0509120 (2005).}}\footnote{\textit{O. Oreshkov, F. Costa, and C. Brukner, Nature communications 3, 1 (2012).}}\footnote{\textit{G. Chiribella, G. M. D’Ariano, P. Perinotti, and B. Valliver. Physical Review A 88, 023018 (2013).}}\footnote{\textit{L. Hardy, Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences 370, 3385 (2012).}}\footnote{\textit{R. Chaves, R. Kueng, J. B. Brask, and D. Gross, Physical review letters 114, 140403 (2015).}}.

\footnote{Note that if a given proposed superdeterminist model does not subsume the statistics predicted by operational quantum theory, then it can also be disfavoured by our data analysis technique if our experimental data conforms with the quantum predictions. It is just that it will be disfavoured in the conventional way—on the grounds that it underfits the data.}}
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