Unification and Scientific Realism Revisited[1]

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1. Introduction

Section 2 will begin by formulating Reichenbach's principle of common cause in a more general way than is usual but in a way that makes the idea behind it a lot clearer. The way that Salmon has pushed the principle into the services of scientific realism will be explained in terms of an example. van Fraassen objects, Salmon modifies his stand and van Fraassen rejoins - all in section 2. (See van Fraassen 1980, chapter 2).

In this episode I think van Fraassen right in claiming - against Salmon that there is no categorical imperative for common cause explanation, and I add my own examples in section 3. The first example is the explanation of the correlation between the equilibrium positions of two objects on a balance in terms of their property of "mass" and the law of moments. The second example is the correlation between independent measurements of "mass" - one using a balance and the second using springs. The explanation of the correlation here is that both are measurements of the <u>same</u> property, *viz.*, "mass". No common causes are invoked in these explanations, but *contra* van Fraassen I think these examples still favour the realist on an intuitive level. For how else can we accept an identity statement about properties without believing in the existence of those properties?

The problem is to find an argument founded on something more precise than raw intuition (given that Salmon's valiant attempt has failed). An attempt at this is made in section 4 by arguing that the identification of properties across diverse experimental domains (used to explain the higher-order correlations of the last example) <u>precludes</u> the maximization of empirical adequacy. If this is right then the anti-realist cannot straightforwardly account for the ubiquitous use of this kind of unification in science. The realist does better, though his account remains intuitive (and this is admittedly a remaining weakness).

2. van Fraassen's Argument Against Realism

Smart (1963 and 1985) and Salmon (1978) have both argued for scientific realism by pleading that the regularities uncovered in

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experimental science would remain nothing more than enigmatic cosmic coincidences were we not to believe in some sort of reality behind the phenomena - were we not to believe in theoretical properties. Salmon has made a worthy attempt to make this argument precise in terms of Reichenbach's principle of common cause.

The idea is that the principle of common cause legislates that we must always explain any statistically significant correlation observed between two event-variables pertaining to space-like separated spaciotemporal regions by postulating the existence of a common cause located in the intersection of their backwards-light-cones (the theory of special relativity dictates that such correlations cannot be due to direct causal action across a space-like interval). As a categorical imperative, Reichenbach's principle then provides a justification for positing a theoretical entity even when none is observable. So, scientific explanation would be impossible without unobservable entities, and the aim of science is to provide explanation. "Therefore, the aim of science can only be served if it is true that there are unobservable entities." (van Fraassen 1980, p.26).

The force of the argument is best illustrated by a concrete example, which will also serve to introduce the formal structure of Reichenbach's explanatory schema. Suppose that we observe the behaviour of two neighboring geysers, call them a and b (the example is from Reichenbach 1956). Let A(t) be the variable denoting the height of the geyser a and B(t) the height of b at time t. For simplicity, suppose that we observe the heights of the geysers at regular time intervals t = 1, 2, ..., N. We then have two sequences of data A(1), A(2), ..., A(N) and B(1), B(2), ..., B(N). While there is no autocorrelation found within each sequence (i.e., no equation of the form $A(t)=a_0+a_1A(t-\delta)$ or $B(t)=b_0+b_1B(t-\delta)$ fits the data with a significantly positive correlation), there is a significant cross-correlation between the variables A and B . That is, some equation

$$A(t)=c_0+c_1B(t) \qquad \text{..........(1)}$$

provides a good fit with the data, and relative to this equation the correlation coefficient is significantly greater than zero. Because these statistical terms will arise again I will take time out now to explain their meaning. (My reference is Spiegel 1963, chapter 14).

The recorded data might look like that in Figure 1. From such a scatter diagram it is intuitively apparent that a linear functional relationship, as in equation (1), gives a reasonably good fit to the data. But which line gives the best fit? The question is answered by the method of least squares as the line that minimizes the sum of the squares of the deviations of the line from the data points., i.e., the line defined by the parameters c_0 and c_1 such that

$$\sum_t [A(t) - (c_0+c_1B(t))]^2$$

is minimum. The next question is: How good is this best fit? Formally, this question can be answered in terms of the correlation coefficient r defined as:

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$$r = \sqrt{\frac{\text{Explained Variation}}{\text{Total Variation}}} \qquad \text{..........(2)}$$

where

$$\text{Explained Variation} = \sum_t [f(B(t))-\bar{A}]^2 \qquad \text{.........(3)}$$

and

$$\text{Total Variation} = \sum_t [A(t)-\bar{A}]^2 \qquad \text{.........(4)}$$

where f(B(t)) is the theoretical value for A(t) predicted by the theoretical equation (1), and $\bar{A}$ is the average value of A(t) as computed from the data.

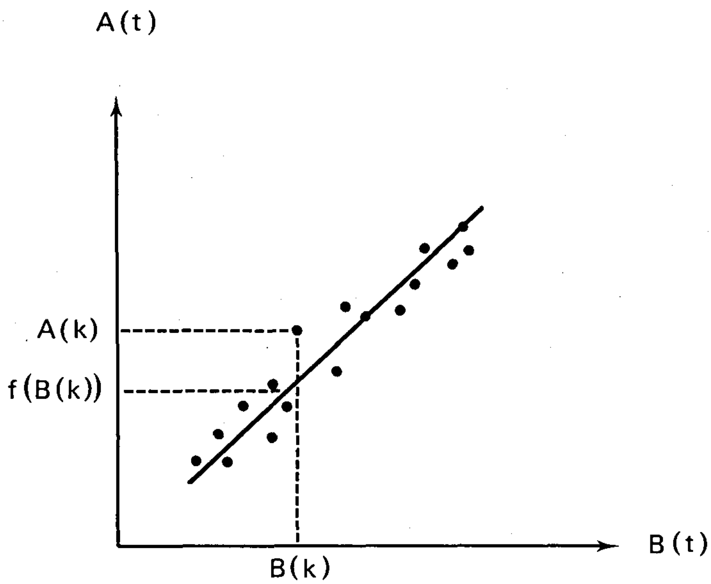

Figure 1. A typical scatter diagram.

Each data point is indexed by some value of t. The observed value A(k), for t=k, is generally different from the theoretical value f(B(k)), and both different from the average value $\bar{A}$.

The correlation coefficient r is a measure of how well the best theoretical equation of the form A(t)=f(B(t)) fits the data. The Total Variation is a theory-independent feature of the data, while the

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Explained Variation depends on the function f (Note that nothing depends on f being linear as in this example). The greater the value of r the better the fit. When r is a maximum value of 1, the fit is perfect in that all data points lie on the theoretical curve, and when is at its minimum value of 0 none of the variation in A is "explained" because no *functional dependence of A on B is postulated (e.g., put* c_1*=0 in* equation (1)).

Strictly speaking, it is always possible to find a curve that passes through every data point thereby obtaining a correlation of 1, but only at the expense of introducing as many parameters into the curve as there are data points. Since these parameters must be calculated from the data itself, the high correlation obtained is of little significance. It is normal practice, therefore, to fix the functional form of the curve to be fitted (e.g., by equation (1)) and limit the number of parameters to be estimated to a number far below the number of data points (N). So, given the functional form as in equation (1), the problem of "estimating" the parameters c_1 is solved by the method of least squares (see Spiegel 1963). The correlation coefficient is a measure of that fit.[2]

Suppose that the best fitting curve for the data in Figure 1 is simply A(t)=B(t) with a correlation coefficient of r=.9, say. It does not automatically follow that this (albeit high) correlation is statistically significant (for instance it might be that N=2). There may be no justification in thinking that A and B are "really" functionally connected <u>just</u> on the basis of a high correlation. Intuitively, we must also require that the number of observed instances is large. Quantitatively this is captured in terms of the statistic

$$t = r\sqrt{N-2} / \sqrt{1-r^2} \quad \ldots\ldots\ldots\ldots(5)$$

The higher the value of t, the more significant is the observed correlation between A and B (Spiegel 1963, p. 247). So, given r=.9 over a large number of instances, N, of data points we are confident that we have observed a "cosmic correlation" which can not be written off as due to accidental fluctuations between two stochastically independent variables.

We have a functional dependence between two variables that needs to be explained. But how should it be explained? Reichenbach's principle of common cause says that any such correlation should be explained by introducing a third variable $C(t-\delta)$ which is functionally related to A(t) and functionally related to B(t) such that the statistically inferred relation between A and B deductively follows from these.

In our example, we explain why both geysers spout at approximately the same height by hypothesizing that the height of each geyser at time t is determined by the water pressure at time $t-\delta$ in the underground reservoir connecting them. That is, there is some property $C(t-\delta)$ of their common reservoir such that

$$A(t) = C(t-\delta) \quad \text{and} \quad B(t) = C(t-\delta) \quad \ldots\ldots\ldots(6).$$

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Equations (6) form the explanans, from which it follows deductively that A(t) = B(t), thereby explaining that statistical fact. (Of course the equations (6) must be construed probabilistically in the sense of allowing an imperfect correlation between A and C, and B and C, for otherwise the imperfect correlation observed between A and B would serve to falsify the premises of the explanation - and we don't want that).

The point about this explanation is that it postulates the existence of theoretical properties and processes, namely the pressure of the water and the processes that determine the height of the geysers. In fact, if a good case could be made for considering pressure to be unobservable, then this would be an example in which Reichenbach's principle persuades us to believe in unobservable properties. Surely, the argument would go, if we don't believe in theoretical posits such as pressure then the regularity observed between the heights of the two geysers remains unexplained - nothing more than a "cosmic coincidence". Pressure may not be unobservable, but given that unobservables exist (and van Fraassen grants this) it is implausible that they never act as common causes.

van Fraassen objects that Reichenbach's explanatory principle "is not a principle that guides twentieth-century science" (van Fraassen 1980, p. 28), and demonstrates this by using the example of spin correlations in quantum mechanics. In this van Fraassen is undoubtedly right (see also his 1982), and I will add some examples later. So quantum mechanics is either a non-explanatory theory or else it explains without the need to introduce new entities (hidden variables). Either way the realist must rethink his case, because with its categorical imperative gone the common cause principle does not force us to believe in anything.

Salmon wants to liberalize Reichenbach's schema to accommodate quantum mechanical phenomena. In effect, Salmon's schema is as follows:

<u>Premise 1</u> $A = a_0 + a_1 B + a_2 C$

<u>Premise 2</u> $B = b_0 + b_1 C$

<u>Conclusion</u> $A = (a_0 - a_2 b_0 / b_1) + (a_1 + a_2 / b_1) B$

As can be checked, the conclusion follows deductively from the two premises as before, and the parameters in the premises can be chosen to account for any observed correlation between A and B. Let us grant that this weakened schema can accommodate the observed correlations of quantum physics. But, says van Fraassen, "weakening the principle in various ways ... will remove the force of the realist arguments. For any weakening is an agreement to leave some sorts of 'cosmic coincidences' unexplained. But that is to admit the tenability of the nominalist/empiricist point of view, for the demand for explanation ceases then to be a scientific 'categorical imperative'." (1980, p. 30).

What does van Fraassen mean here? Let me reconstruct his argument in full. van Fraassen sees the force of Reichenbach's schema (obtained as a special case of Salmon's by putting $a_1 = 0$) as consisting in the fact that if we fix the "hidden" variable C at a particular constant value the variables A and B will vary independently of each other - there is

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no correlation between A and B conditional on a fixed value of C. (It might appear from premise 2 that fixing C will fix B, and a fixed B and C will then fix A by premise 1. But the equations in premises 1 and 2 give the theoretical values of A and B - the observed values of A and B will vary even when C is fixed. This "residual" variation of variables is called the Unexplained Variation exactly because it is not predicted by the theoretical equations).

So, van Fraassen sees the explanatory utility of introducing the variable C as "removing" the correlation between A and B in this sense, and thereby explaining it. But in Salmon's schema there remains a correlation between A and B even when C is fixed, namely a functional dependence of the form $A = a^* + a_1B$. (This functional dependence of A on B is only observed when B has some unexplained variation, for otherwise B will be fixed when C is fixed). So, concludes van Fraassen, Salmon's explanatory schema allows this residual correlation (for fear of an infinite regress) to go unexplained.

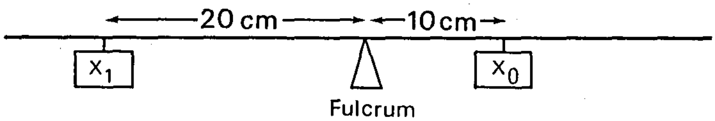
The idea that to explain a correlation is to "remove" it has to be wrong. For it is easy to see that Reichenbach's schema would never be explanatory either for it posits correlations between A and C and between B and C, which are unexplained in terms of the schema, again threatening an infinite regress. But van Fraassen agrees that common causes are sometimes explanatory. We need not labor this point, for it has to be agreed that modern physics does not recognize the need for such hidden variables anyway. Though compatible with quantum phenomena, any insistence on Salmon's explanatory schema as a categorical imperative would brand most modern physicists as instrumentalists. Given that one of the motives of a philosophy of science is to account for scientific practice, this would leave the realist in a damaged position.

Fortunately for the realist, Salmon's explanatory schema does not even apply universally within classical physics as I will show in the next section. This is damaging to van Fraassen's position because there is no intuitive argument against realism in these classical examples, yet van Fraassen's argument should apply were it sound. So, its not sound.

But van Fraassen has left the realist with the following un-met challenge: How do we make the intuitive argument for theoretical entities from the explanation of cosmic correlations precise, once we know that Salmon's formulation of Smart's argument does not work.

3. An Example from Classical Mechanics

If we hang two weights on a balance, we find that the ratio of their distances from the fulcrum at equilibrium is a constant. That is, if we hang object x_0 at 10cm from the fulcrum and find that x_1 balances at 20cm, then if we repeat the experiment with x_0 at 15cm x_1 will then balance at 30cm. The explanation is that x_0 is twice as heavy as x_1.



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Suppose that we perform N trials of this experiment, each time varying the distance of x_0 (denoted by the variable $D(x_0)$) from the fulcrum and recording the equilibrium position of x_1 (denoted by $D(x_1)$) to obtain a scatter diagram similar to that in figure 1. For large enough N we will get a high and statistically significant correlation between $D(x_0)$ and $D(x_1)$ relative to a theoretical postulate of the form

$$D(x_1) = m_b(x_1).D(x_0) \qquad \ldots\ldots\ldots\ldots(7)$$

where $m_b(x_1)$ is a constant parameter so labelled for reasons that will be obvious later. The constant $m_b(x_1)$ will be determined from the data by the method of least squares so as to maximize the fit of equation (7) to the data (m_b will be close to 1/2 in this example). But as is well known, this correlation between $D(x_0)$ and $D(x_1)$ is not explained by introducing a third variable. Its explanation, though, <u>does have ontological import</u> and this is what is important for the realist.

The explanation of this phenomenon involves the net cancellation of the moments of force about the fulcrum where the forces involved are those of gravity and are proportional to the <u>masses</u> of the hanging objects. In fact, theory tells us that the parameter $m_b(x_1)$ provides a <u>measurement</u> of the mass of x_1 (as a proportion of the mass x_0). Surely, the realist will say, if we are to take this explanation seriously, we must believe that objects have an unobservable property called mass, for otherwise the regularity observed between $D(x_1)$ and $D(x_0)$ would remain a mystery. Of course, this just restates Smart's intuitive argument, and does not make it precise.

This example at least gives the realist some hope that, while not all scientific explanations of correlation require the postulation of a common cause, they do require the postulation of <u>some</u> theoretical entity or property - "mass" in this example. So there is hope that all good explanations of cosmic correlations will have some ontological import - that some sort of categorical imperative can be returned to a revised realist principle.

Note also that this is a counter-example to van Fraassen's premise in his argument against Salmon that the strength of common cause explanation lies in the "removal" of the correlation when we conditionalize on C. For there is no third variable introduced into the explanation to conditionalize on - certainly $m_b(x_1)$ will not do for it is constant anyway.

Secondly, this explanation has an advantage over the common cause variety in that it does not explain observed regularities in terms of unobserved regularities, thereby avoiding a possible infinite regress in the demand for explanation.

This is not to say that the demand for explanation stops. Indeed, the parameter m_b <u>treated as variable</u> over different masses will be correlated with other determinations of "mass", and this higher-order regularity requires explanation as well. This will be my next example. It will serve to illustrate yet another way of explaining correlations in science, but more importantly it will be used to formulate the new argument for realism explained in the next section.

First, suppose that we use the experiment of the previous example for M different objects $x_1, x_2, \ldots, x_M$ each paired with the "unit mass" x_0 on the balance. Again each experiment has N trials where the value of $D(x_0)$ is varied for each trial. For each of the M experiments we obtain a scatter diagram as in figure 1, and in each case we fit a curve by the method of least squares to obtain the M theoretical equations.

$$\begin{aligned} D(x_1) &= m_b(x_1).D(x_0) \\ D(x_2) &= m_b(x_2).D(x_0) \\ &\vdots \\ D(x_M) &= m_b(x_M).D(x_0) \end{aligned} \qquad \ldots\ldots\ldots\ldots(8)$$

Now suppose we now make an independent measurement of "mass" of x_1 using the following procedure. Collect together N different springs. For each spring hang x_0 on it and record the extension of the spring from its natural position as $S(x_0)$. Remove x_0 and place x_1 on the spring recording the amount it stretches the (same) spring as $S(x_1)$. Repeat this for all N springs. Again when we record the results on a scatter diagram we obtain a correlation.

Repeat this procedure for $x_2, x_3, \ldots, x_M$, and let the best fitting lines be given by

$$\begin{aligned} S(x_1) &= m_s(x_1).S(x_0) \\ S(x_2) &= m_s(x_2).S(x_0) \\ &\vdots \\ S(x_M) &= m_s(x_M).S(x_0). \end{aligned} \qquad \ldots\ldots\ldots\ldots(9)$$

Now plot the M data points $(m_b(x_i), m_s(x_i))$ for $i=1, \ldots, M$ on a new scatter diagram, and we will again obtain something like that in figure 1. We have thereby exhibited a higher-order correlation which will be statistically significant if M is large enough.

The best fitting line will be given by

$$m_s(x_i) = (1-\epsilon)m_b(x_i) + \delta \qquad \ldots\ldots\ldots(10)$$

where ϵ and δ will be fairly small but measurably non-zero. How, then, does modern science explain this correlation?

So as not to prejudge the answer to this question let me refer to m_s as 'spring-mass' and m_b as 'balance-mass'. The explanation as to why m_s and m_b are approximately equal is that they both measure the same thing, namely "mass".

Does this explanation have any ontological import? Well, not in the sense of requiring any additional theoretical entity in its explanation, but it certainly requires a realist attitude towards theoretical properties. How could we take seriously the assertion that spring-mass and balance-mass are identical if we don't believe in either?

4. A New Argument against Constructive Empiricism

The example of higher-order correlations just discussed illustrated the work of unification in science. The realist assertion that spring-mass just is balance-mass enables us to rewrite equations (8) and (9) in a unified form as

$$\begin{aligned} D(x_1) &= m(x_1).D(x_0) \\ &\vdots \\ D(x_M) &= m(x_M).D(x_0) \\ S(x_1) &= m(x_1).S(x_0) \\ &\vdots \\ S(x_M) &= m(x_M).S(x_0) \end{aligned} \qquad \ldots\ldots\ldots(11)$$

For this theory new values of the parameters $m(x_i)$ are obtained by the method of least squares this time using the spring and balance data collectively. Equation (10) does not arise except in the trivial form of $m(x_i) = m(x_i)$.

Before considering what argument there is here for realism, some questions are worth asking about the notion of empirical adequacy in van Fraassen's sense. In particular, how do we compare the empirical fit of the un-unified theory with the unified theory? The un-unified theory consists in the conjunction of equations (8) (call this T_b) with (9) (call this T_s) together with (10) (call this T′), i.e., $T_b \& T_s \& T'$. The unified theory consists of equations (11) (call this T*). Does T* fit the facts better than $T_b \& T_s \& T'$?

I want to argue that the answer to this question is in the negative. The unified theory has worse fit than the un-unified theory. First, it is vitally important to remember that T′ does not follow deductively from $T_b \& T_s$ - it is logically independent of them. And, more obviously, T_b and T_s are logically independent of each other. Now, for simplicity, suppose that $m(x_i) = m_b(x_i)$ for all i (as might be expected from the data of T_b are intrinsically more precise than those of T_s and so will dominate the least squares determination of $m(x_i)$).[3] So, to compare $T_b \& T_s \& T'$ with T* we need only compare $T_s \& T'$ with the corresponding equations in (11). The argument is that a theory logically equivalent to T* can be obtained in two steps, and each step decreases empirical fit, so T* is empirically less fit.

The first step is to replace T′ by $m_s(x_i) = m_b(x_i)$, where the right hand side, remember, is numerically equal to $m(x_i)$. This step decreases closeness of fit because it is different from (10), which is the closest fitting line. The next step is to readjust the parameters $m_s(x_i)$ to equal $m_b(x_i)$. Again this reduces the empirical fit of the equations. So the unified theory has less empirical fit than the un-unified theory.

It might be objected that the argument of the previous paragraph depends on the order of the steps taken. Suppose we first adjust m_s to equal m_b. Then we plot the points (m_s', m_b) on a scatter diagram and find that the fit is perfect. But this comparison is surely illegitimate for we have now changed the data on the domain of T′ from

(m_s,m_b) to (m_s',m_b). And if we are to compare two theories within a certain domain, we must make the comparison against the same set of data.

For van Fraassen empirical adequacy encompasses past, present and future empirical fit of the theory. The completion of the argument is straightforward. So, given that actual fit with the data is the best indicator of empirical adequacy we have, an anti-realist methodology of science should recommend the use of un-unified theories in science. But scientists actually opt for unified theories as the expense of empirical fit. So, the anti-realist's theory of science does not accord with actual scientific practice. The realist on the other hand has a rationale for this practice. The unified theory provides a better explanation of the data, and this advantage is well worth some sacrifices. The notion of "better explanation" is distinctly realist. The best explanation deepens our understanding of the underlying reality behind the phenomena even if it worsens our description of it.

Some might say that this argument for realism is not new, citing perhaps Earman (1978) or Friedman (1981). Certainly, many of the ideas come from those papers, but neither of these authors recognizes that the anti-realist has more than the conjunction $T_b\&T_s$ available to him. In particular, the anti-realist has empirical justification for the higher-order theory T', and this might make a difference. That it does not requires a new argument, which is what has been attempted here.

5. Conclusions

This paper has tried to make two points against anti-realists such as van Fraassen. The first is that the fact that quantum mechanical phenomena cannot be made to conform to a common cause mode of explanation does not undercut the strength of the realist position. For examples from classical physics are common in which correlations are not explained in this way and there is no anti-realist intuition at work in these examples. In fact, it is clear from the examples given that quantum mechanical explanations of correlations share many features with those in classical physics namely, the postulation of theoretical properties and their identification across different domains of experimental enquiry. There is every reason to believe that the argument of section 4 applies to quantum mechanical examples as well (though this has not been argued here).

Secondly, that empirical adequacy cannot account for practice of using unified theories in science. Empirical adequacy is in fact traded off for other <u>explanatory</u> virtues, and only the realist has a plausible rationale for this practice. The argument given here goes beyond others in the literature in allowing that the anti-realist has some justification for asserting more than the mere conjunction of the subtheories. But his position fails in spite of that.

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Notes

[1]Sincere thanks go to Professor Peter Finch and especially to Professor Cliff Hooker for useful feedback on the paper. As always, the final responsibility for errors and oversights are mine.

[2]In the case of dichotomous variables A and B taking on values 0 and 1, the correlation coefficient r reduces to the expression

$$\frac{P(A=1 \;\&\; B=1) - P(A=1) \;.\; P(B=1)}{\sqrt{P(A=1)P(A=0)}\;\sqrt{P(B=1)P(B=0)}}$$

where P denotes the relative frequency in the data.

This has the same qualitative properties as the more common measures of correlation in the philosophical literature, such as the covariance [P(A=1 & B=1) - P(A=1). P(B=1)], [P(A=1/B=1) - P(A=1)], or the regression coefficient [P(A=1/B=1) - P(A=1/B=0))]. If one is positive so are all the others, if one is negative then so are all the others, and if one is zero then they all are.

[3]This assumption simplifies the argument but is not essential to it.

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