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Do Two Research Cultures Imply Two Scientific Paradigms?

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In *A Tale of Two Cultures*, Gary Goertz and James Mahoney (2012) seek to describe two distinct social science cultures: "quantitative" and "qualitative." Cultural differences such as these often provide an excuse for groups to retreat to separate sides, to wallow in in-group solidarity, and to prepare for a long and stubborn dispute instead of seeking compromise and agreement. Goertz and Mahoney profess no desire to start a civil war in the social sciences. They are, they tell us, merely anthropologists who have encountered the "Quant" and the "Qual" tribes and who want to catalog their distinctive characteristics. In doing so, they want to make sure that both groups get the respect they deserve for their cultural uniqueness.

After a few preliminary chapters, Goertz and Mahoney march through 14 chapters that present contrasting observations on how the Quants and the Quals search for causal relationships, explicate and measure concepts, and undertake research design. The chapters are all relatively short. Each one briskly and pithily illustrates, as in a 19th-century travel book cataloging the oddities of remarkably different groups in far off lands, the differences between the Quants and the Quals.

Because they find the Quants and the Quals to be different, Goertz and Mahoney conclude "there is no set of principles that unifies all social scientific work" (chap. 17, p. 220). The two cultures are different, and each should be allowed to thrive on its own terms, although "there is room for dialogue between the quantitative and qualitative paradigms" (p. 220). And "If we allow for some division of labor and the possibility of mixing the two cultures, we arrive at a pluralistic vision of social science" (p. 226).

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I am very sympathetic to taking qualitative research seriously. As a quantitative researcher, I wrote a review of King, Keohane, and Verba's (1994) *Designing Social Inquiry*, which lamented that the best that volume could seem to do for qualitative researchers was to recommend that they not be "small-*N*" researchers.[1] By contrast, I believe that quantitative researchers have often underrated the importance and capabilities of qualitative work. I applaud the fact that over the past 15 years, qualitative researchers have formed a section of the American Political Science Association, established an ongoing annual training program, and made a start on codifying "good practice" for qualitative research. Gaining an identity is probably important for progress in any subfield, and by treating qualitative and quantitative researchers as equals with different cultures, Goertz and Mahoney provide qualitative researchers with added respectability.

Yet I have also coedited a book with David Collier, *Rethinking Social Inquiry* (2004) whose subtitle is *Diverse Tools, Shared Standards*. The message of that book is that cultures (the tools used by researchers) may be different, but scientific paradigms should be the same. I believe that Goertz and Mahoney have too readily accepted the notion that different cultures mean different paradigms. I believe that their book would have been even better if they had tried to develop a common framework for thinking about the supposedly different paradigms of the Quants and Quals.

The authors seem curiously pleased with cataloging difference after difference without asking how deep these differences go. Once in a while they note that it is possible to translate ideas from the language of one culture to that of the other, but they do not seem to think that this is generally a good idea. I am distressed by this, and I think that opportunities for developing a unified paradigm are lost by bifurcating instead of unifying social science methodology.

To the extent that there are differences between the Quants and the Quals, I believe that many of them are simply differences in language and tools and not in their fundamental paradigms for doing research.[2] Language differences can be important, but they can be transcended through careful translation. Different tools are valuable because each group can learn new techniques from the other, but tools are ways of doing things—not the things themselves. Moreover, scientific discourse and practice usually strive for a level of universality that transcends different languages and that encompasses diverse tools. In the end, although it is interesting to find out that the Quants and the Quals use different languages and often proceed by using different tools in their research, I am not sure that scientific discourse is helped very much by these observations.

Instead of voluminously detailing all the differences, bridging them might be a better agenda item for methodologists. *A Tale of Two Cultures*, with its short, snappy chapters that observe that the "Quals do it this way" and the "Quants do it this way," is provocative but ultimately not as helpful as it might have been if the authors had resolutely taken C. P. Snow's (1959/1998) route and tried to bridge the two cultures.

Positive Contributions

Later on, I provide some of this bridging work, but before offering my synthesis of the two perspectives, let me say what I liked about *A Tale of Two Cultures* and what I found confusing. My likes:

An emphasis on the importance of studying "causes of effects" as well as the "effects of causes.". One major point of entry here is the Neyman–Rubin–Holland (NRH) framework of causal inference (Holland, 1986; Neyman, 1923/1990; Rubin, 1974), which has made tremendous inroads in the social sciences over the past three decades. This framework demands that researchers focus on inferring the effects of causes, not the causes of effects. According to NRH, researchers should set up experiments to manipulate putative causes such as political messages, economic incentives, trust in others, or educational curriculums, and then they should measure the impacts of these treatments, preferably by randomly assigning treatments to only some observable units so that there are "controls" as well as "treated" units.

The NRH framework suggests that trying to identify the causes of effects is doomed to failure. Efforts such as attempting to determine the causes of the cold war, the reasons why someone voted for Barack Obama, or the genesis of the Tea Party cannot succeed because once an effect is observed, the cause must already have happened, so that it can neither be manipulated nor randomly assigned.

Yet policy makers and diagnosticians usually care most about explaining the causes of effects: Why is someone ill? Why is that person poor? Why did that nation declare war? For this practical reason alone we cannot abandon attempts to explain the causes of effects, but there are also methodological reasons for studying the causes of effects. The largest body of empirical data comes from natural and human history that informs cosmology, astronomy, geology, evolution, and, of course, our understanding of social and political phenomena. This history sometimes contains natural experiments that can be used to study the effects of causes, but the vast bulk of it is nonexperimental observational data. Nevertheless, science has certainly advanced in at least some of these fields, so that there must be useful ways to study these topics without experimentation. It would be foolish to forsake these methods under the banner of "Effects of Causes; Not Causes of Effects."

Criticism of linear additive regression models and average effects. Partly because it provided a method for elaborating the causes of effects, the "regression model" written as a linear additive equation reigned supreme in much of social science for many years despite the efforts of the best methodologists to warn researchers that they must consider nonlinear models and interaction effects (e.g., Achen, 2002, 2005; Franzese & Kam, 2007).[3] It has recently been replaced with the equally limited notion of simply estimating "average effects" in an experiment using the NRH "model" for causal effects. In both cases, sophisticated practitioners of the craft have known that each "model" has its limitations, and a great deal of effort has been devoted to getting beyond their deficiencies: Lazarsfeld's elaboration analysis for simple tables, path analysis and structural equation modeling for regression, nonlinear regression models, definitions of average effects adjusted for heterogeneity in responses or variability in participant behavior (e.g., compliance vs. noncompliance), and examining how average effects vary with mediating variables. But actual practice often lagged behind these developments, partly because of the seductiveness of the simple probabilistic definition of causation described by these methods that made it relatively easy to simply run an additive regression or to estimate average effects in an experiment and to consider these actions a solution to the quest for causal effects. Sadly, it is not anachronistic to criticize the uncritical use of linear additive models and average effects for estimating causal impacts, even though the best quantitative research usually does much better than this.

Beginning the study of causal inference by thinking about deterministic notions of causality such as necessary and sufficient and INUS conditions. Several years ago (Brady, 2008), I argued that to understand causality, one had to start by understanding the logic of deterministic causal mechanisms. I argued that it was a mistake to jump immediately to a probabilistic definition of causation in which "X causing Y" is defined as a change in the probability of Y when X occurs compared to the case when it does not occur. There are several reasons for this. First, whether or not it is ontologically (i.e., metaphysically) true that causation is deterministic, our everyday intuitions are about deterministic chains of causality (as in the proverb "for the want of a nail").[4] Second, an examination of causal chains alerts researchers to the complexity of causal mechanisms—the possibility that some events are necessary, others are sufficient, and still others are INUS.[5] Third, a thorough examination of deterministic causality shows that there are many ways that what appears to be probabilistic causation can result from estimating regression equations or setting up experiments that omit or ignore some of the important variables, interactions, or conjunctures that exist in a truly deterministic phenomenon (see Brady, 2008, pp. 226-230). Researchers who know this will probably be more likely to be unsatisfied with merely recording average effects from experiments. At the very least they will want to elaborate on the processes that led to the result. They might even go so far as to incorporate nonlinearities and interaction effects, and in the best of circumstances they might employ the more general framework that I outline below.

Points of Concern

The following approaches and discussions in Goertz and Mahoney's book seem more questionable to me:

Quickly jumping away from considering interpretive approaches. It seems odd that "anthropologists" would so quickly turn away from studying the tribe of interpretivists who may be much more interesting and different than qualitative and quantitative researchers are from one another. It would be valuable to know a bit more about them. Indeed, if there is a candidate for a group with a different culture and paradigm, the interpretivists—vis-à-vis all of the other methodologies discussed here—might well be it.[6]

A somewhat facile notion of culture and of scientific methodology. Are quantitative and qualitative researchers really two cultures? And even if they are, does this mean that they represent different paradigms for doing research? It is hard to think of two other social science concepts that have as many guises and manifestations as "culture" and "paradigm," so any discussion of them is a dense thicket of interpretation and disputation about essentially contested concepts. Nevertheless, it seems quite evident that even if the Quants and the Quals are different cultures, they do not necessarily represent different paradigms for doing research. Hence, the authors have to do more than just prove that there are two cultures; they must also show that there are two distinct research paradigms. I am doubtful about both claims, but I am especially doubtful about the second.

Claiming in chapters 6 and 9 that the quantitative approach uses only a counterfactual definition of causality but not counterfactuals themselves. Chapter 6 considers Hume's famous definition of causation in which he switches from Definition 1, which focuses on "constant conjunction," to Definition 2, which seems to be a "counterfactual" definition. Goertz and Mahoney argue that in the NRH framework, "Hume's constant conjunction definition 1 is doing the heavily lifting even though the starting point is his counterfactual definition 2" (p. 78). This makes no sense to me since the NRH approach relies very heavily on counterfactuals both in its definition of causality *and* in its solution to the problem of estimating causal effects by one of two strategies. One is to find a "matching" case without the treatment in the "closest possible world," and the other is to randomize treatment and control cases to create a "counterfactual world" in the control cases (see Brady, 2008, pp. 249-267). The authors go on to claim that for qualitative researchers, Hume's Definition 1 about constant conjunction represents a claim about causal sufficiency, whereas Definition 2 is a claim about a necessary condition. The discussion here seems forced to me, and I am not at all sure that it does much to illuminate causation or differences across paradigms. Chapter 9 deepens my confusion with the claim that counterfactuals are "not commonly used within the quantitative tradition" (p. 115). To the contrary, the whole point of the experimental method is to create directly comparable "factual worlds" (the treatment cases) and "counterfactual worlds" (the control cases).

A forced discussion of how the two groups differ in their consideration of concepts, coding, and measurement. This part of the book (chaps. 10-13) seemed quite odd, with a great deal of caricature of the quantitative paradigm. We are told in chapter 10 that "[w]ithin the quantitative culture, discussions and debates about concepts focus on issues of data and measurement, and less on semantics and meaning" (128). Chapter 11 repeats this claim by saying that "[f]or qualitative scholars, the relationship between a concept and data is one of *semantics*, i.e., meaning. . . . For quantitative scholars, by contrast, the relationship between variable and indicator concerns the *measurement* of the variable" (p. 140). These claims just ring false to me. Quantitative researchers have spent a great deal of time on the semantics and meaning of party identification, ideology, ethnic identity, social capital, and even physical capital (see the controversies on capital among Cambridge economists).

In chapter 10 Goertz and Mahoney compare the pattern of errors in the coding of democracies as estimated by quantitative researchers (Figure 10.2) and by qualitative researchers (Figure 10.3). They claim that the error for cases in the qualitative tradition is typically less at the extremes because they are easier to code (Figure 10.3), whereas the error for cases in the quantitative tradition is typically greatest at the extremes (Figure 10.2) because of peculiarities in the statistical methods used to score them. Yet the figures really consider two quite different things. Figure 10.2 from the quantitative analysis assigns a continuous democracy score to units much like a specific temperature to a day. The error bars indicate that just as it is hard to get exact temperatures at the extremes because there are so few other extreme cases for comparison (which causes the measuring device to be uncertain about the exact temperature), so it is hard to get exact democracy scores for these extreme cases. Nevertheless, these extreme cases are quite clearly at each end of the continuum, so that there is no question about their classification as “very cold” or “very hot” or “very undemocratic” or “very democratic.” Figure 10.3 from the qualitative analysis summarizes a process whereby units are binned into broad categories of democracy similar to binning days into the categories of “very cold,” “cold,” “hot,” and “very hot.” It is very easy for two different rating entities to identify the extreme cases in this case, just as it was easy for the quantitative researcher to know that some cases were at the extremes, so there is no disagreement here.[7] The difference is that the quantitative method is trying to go beyond simply identifying extreme cases to actually assigning a numerical score to each case—and this turns out to be highly problematic at the extremes.

Not enough tough-mindedness in the discussion of differences. As the examples cited above might suggest, the book time and again identifies a supposed difference, but it does not wrestle the difference to the ground and see whether it is real or simply caricature, and whether it is important or unimportant. Nor, as noted, does it distinguish between cultural differences and more profound paradigm differences. There is far too little effort to develop some intellectual machinery to determine how deep the differences go, with a resulting feeling that the book has failed to go beyond being an excellent tourist guide to “Travels in the Lands of the Quants and Quals.”

A Common Framework for Quants and Quals

Goertz and Mahoney point out many differences between the two cultures, but I believe that a large number of them can be traced back to differences in how the Quants and the Quals typically describe causation. More important, with a little bit of work we can develop a common framework—a common paradigm—that will allow the two sides to communicate better and to build on one another’s insights. This common framework also suggests that qualitative researchers and model-based quantitative researchers have a great deal in common, and to the degree there is a cultural split, it may be between these two groups and experimentalists who eschew modeling.

The biggest difference between the two cultures is that some quantitative researchers are typically happy with, and resigned to, measuring average treatment effects because they have recipes (e.g., experimentation with random assignment) that produce good estimates of these effects. Some qualitative—as well as a good many quantitative researchers—want more. They want more because they typically care about the causes of effects, and to get at the causes of effects, they must concern themselves with describing causal mechanisms. Some quantitative researchers (especially experimentalists in the NRH camp) are dismissive of what qualitative researchers want. They argue that given the probabilistic nature of social phenomena, the best we can do is to estimate some kind of average effect. Here I think that the qualitative researchers provide a useful challenge to these quantitative researchers. I think that we should care about more than average effects, and, in fact, as we will see, there is a quantitative tradition that cares about much more than just the average effects of a manipulated variable.

Quantitative researchers often begin their courses with a discussion of average causal effects within the context of a regression equation such as the following,

$$Y_i = a + b\,X_i + e_i, \tag{1}$$

where Y is the outcome or dependent variable, X is the treatment or independent variable, a and b are constants (the intercept and effect coefficient respectively), e is an error term with expected value zero, and i indexes different observations or cases. In the simplest case where X is zero (not treated) or one (treated), then the concern is with estimating b, which is the expected impact of the treatment. If we use the conditional expectation terminology where $E(Y_i|X_i = 1)$ is the average value of the outcome variable for those getting the treatment and $E(Y_i|X_i = 0)$ is the average value of the outcome variable for those not getting the treatment, then the impact of the treatment can be obtained by some simple algebra of expectations (remembering that the expectation of a constant is that constant and that the expectation of e_i is zero by assumption):

$$E(Y_i|\mathrm{X}_i = 1) - E(Y_i|X_i = 0) = a + b - a = b. \tag{2}$$

If the cases have been randomly assigned to treatment and control groups, then there are good reasons to believe that this parameter b measures the "causal impact" of the treatment. Even if b is not a constant—even if cases react differently to the treatment—this setup with random assignment still yields the average treatment effect *E(b)* for the case where there is "heterogeneity" in the population. If there hasn't been random assignment, then it is possible that the treatment is correlated with some omitted variable that affects Y_i so that the quantity b is measuring the causal impact not only of the treatment but of some omitted variable as well. This is the classic case of "specification bias."

Qualitative researchers, because they sometimes focus on singular causation and the causes of effects, are typically interested in much more than this. They want to know if a treatment or condition was necessary or sufficient for causation. They want to think about the possibilities of conjunctural causation and equifinality (which are closely related to INUS conditions).

Goertz and Mahoney argue that one of the big differences between quantitative and qualitative researchers is that the former use equations like the one above, which is based on probability and statistical theory, whereas qualitative researchers use set theory and logic to advance theories using a notation such as the following,

$$Y = X*Z + Q*R, \tag{3}$$

where Y is the outcome; X, Z, Q, and R are events or conditions; * stands for the logical "And"; and + stands for the logical "Or." Hence this equation means that Y occurs when X and Z occur or when Q and R occur. Goertz and Mahoney believe that writing down the logic of the process that produces an effect in this way amounts to describing the putative mechanism behind the causal effects. By doing this, a researcher might be able to go beyond merely "accounting" equations like Equations 1 and 2 above, which obscure and ignore the mechanisms and pathways by which outcomes occur.

To what extent are these claims by Goertz and Mahoney true? To really learn about how Equation 3 might represent mechanisms and pathways, it must be improved still further. As it stands, Equation 3 says nothing about the exact functional form of how X, Z, Q, and R operate. And as it stands, it is hard to see how Equation 3 is related to Equation 1. To get a sense of how to fill in these gaps, consider Figure 2.2a in the Goertz and Mahoney book (p. 27), which is the same as Figure 1a in their article. This figure is a scatterplot of a putative dependent variable (Y) and a putative independent variable (X) where both variables are scored to go from zero to one. In this figure, all the observations are scattered *below* a 45 degree line drawn from the origin (0,0) to the top of the graph at (1,1). According to Goertz and Mahoney, this figure describes a scatterplot of data for a situation where X (assumed to be always nonnegative) is a necessary condition for Y in the sense that Y cannot have a value bigger than the value of X. Since X is only a necessary condition, there must be something more than just X that "causes" Y. One plausible model is that there is at least one other factor Z (also assumed to be nonnegative) that is also necessary for Y. Together, X and Z are then sufficient for Y:

$$Y = X*Z. \tag{4}$$

This equation is one piece of Equation 3 above, and we will show how it can be turned into an equation like Equation 1. One way to do this is to write,

$$Y = a + b\,X^{*}Z + e, \tag{5}$$

so that X has an impact on Y, but only in the presence of some other factor Z. If $Z = 0$, then no amount of X can increase the value of Y. In the simplest case where X is dichotomous and Z is dichotomous, it is obvious that the effect $bX^{*}Y$ is nonzero if and only if $X = 1$ and $Z = 1$—hence the equation reproduces the logical "and" for a necessary condition.

This is not the only way to write out a model in which both X and Z are necessary conditions. The following also produce the same result for dichotomous variables,

$$Y = a + b\,(X^{\eta 1} {*} Z^{\eta 2}) + e, \tag{6}$$

$$Y = a + b\,Min(X^{\eta 1}, Z^{\eta 2}) + e, \tag{7}$$

where $\eta 1$ and $\eta 2$ are fixed parameters. In fact, the following very general equation produces Equations 5 through 7 as special cases:[8]

$$Y = a + \varphi\,[\alpha\,X^{\gamma\delta 1} + (1\text{-}\alpha)\,Z^{\gamma\delta 2}]^{(1/\gamma)} \tag{8}$$

For this equation, we assume that α is in (0,1), φ is a fixed parameter, $\delta 1$ and $\delta 2$ are fixed parameters, and γ is in the range (–infinity, 0). This equation is a slight generalization of the constant elasticity of substitution production function that is used in economics. Perhaps the greatest advantage of considering Equation 8 is that it can be used for situations where X and Z are continuous positive variables.

Using this equation also moves us beyond thinking of X and Z as necessary conditions—now we can consider the properties of the "production function" that might create the outcome Y. Economists have developed many different functional forms for production functions that have different implications for how X and Z produce Y.

We can also consider the case of a sufficient condition which Goertz and Mahoney argue is represented by Figure 2.2b in their book and Figure 1b in their article. This figure presents a graph like Figure 1a, where both the putative dependent variable (Y) and the putative independent variable (X) are scored from zero to one, but in this case the points are all scattered *above* a 45 degree line. If X is truly a sufficient condition for Y in the sense that Y will always have a value at least as big as X, then X alone can have an impact on Y so that we could have,

$$Y = a + b\,X + e. \tag{9}$$

This would produce a scatterplot around the line ($a + bX$), but a glance at Figure 1b in the article suggests that Goertz and Mahoney are considering a case where there is some other factor Z, which is also sufficient to cause Y (otherwise Y would not sometimes be much bigger than X) so that we must have,

$$Y = a + b\,X + c\,Z + e. \tag{10}$$

In the dichotomous case, it is obvious that there is a causal effect if $X = 1$ or if $Z = 1$. In the case where X and Z are continuous, this is the linear additive model.

Just as with the case of necessary conditions, there are many other possibilities for functional forms including these two:

$$Y = a + b\,X^{\eta 1} + c\,Z^{\eta 2} + e \tag{11}$$

$$Y = a + b\,Max(X^{\eta 1}, Z^{\eta 2}) + e \tag{12}$$

Moreover, it is intriguing to note that the general Equation 8 can represent Equation 11 if we set $\gamma = 1, \delta 1 = \eta 1, \delta 2 = \eta 2, b = \alpha\varphi$, and $c = (1-\alpha)\varphi$. It can also be shown that Equation 8 leads to Equation 12 if γ equals infinity. In fact, if $\gamma > 0$, then X and Z can be considered "sufficient" for Y in the sense that an increase in either one leads to an increase in Y no matter what the value of Z.

This derivation shows that Equation 8 can be used to represent both cases where we think that X might be a necessary condition and where we think X might be a sufficient condition—the test for the kind of condition depends on the parameter γ. If it is in the range (–infinity, 0) then we have necessary conditions; if it is in the range (0, +infinity) then we have sufficient conditions. But estimating the functional form provides a great deal more nuance because it tells us by how much an increase in X might increase Y. It also links the discussion of necessary and sufficient conditions to the discussion of factors that are complements to one another (necessary for one another), such as milk and a container for slaking thirst, and those that are substitutes for one another (sufficient alone), such as milk and water for drinking.[9] There is a very large literature on this topic that can inform the discussion of causality and functional forms (e.g., Samuelson, 1974), and I think that the use of the terms *complements* and *substitutes* (which refer to continuous possibilities) is a better approach when talking about continuous variables than using the terms *necessary* and *sufficient*, which are dichotomous concepts.

Conclusions

This excursion into functional forms and modeling demonstrates one way that we can unify the approaches of the Quants and the Quals, and in doing so, we can learn a great deal about how to think about causality. To begin with, the framework developed above suggests that with their interest in mechanisms and in necessary and sufficient conditions, qualitative researchers are fundamentally in the modeling tradition.

Indeed, if there is a cultural split, it is more likely between (a) the experimentalists with their NRH recipe for measuring causal effects and (b) quantitative and qualitative researchers who share the practice of using modeling to decipher causal effects. In fact, I believe that all these groups could benefit from more thinking about models (mathematical or otherwise) as a way to advance the scientific enterprise of developing theoretical explanations for phenomena. More important, I believe that there is a unified paradigm for thinking about causal inference and that ultimately scientific research is not about cookbook recipes for inferring causality. It is about developing models of social and political phenomena. There may or may not be two cultures, but there is one paradigm.

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Notes

1. See chapter 3 of Brady and Collier (2004, 2010).
2. I am not sure that these differences amount to cultural differences, but rather than argue that point, I want to focus on whether cultural differences necessarily imply different epistemological paradigms.

3. And as we discuss in more detail later, there is a substantial tradition of quantitative research that has taken this warning to heart (see, e.g., Franzese, Kam, & Jamal, 2001).
4. “For want of a nail the shoe was lost; For want of a shoe the horse was lost; For want of a horse the rider was lost; For want of a rider the message was lost; For want of a message the battle was lost; For want of a battle the kingdom was lost; And all for the want of a horseshoe nail.”
5. An INUS cause may be an insufficient (I) but necessary (N) part of a condition, which is itself unnecessary (U) but exclusively sufficient (S) for the effect.
6. Perhaps not surprisingly, I would argue that intepretivists may be a third culture, but they do not really have a different paradigm. Rather, they emphasize the importance of description, of ideas, of social construction of institutions and practices, of meaning, and of the interpretations that people place on ideas. I think that all of this can be comfortably fit within a unifying paradigm for social science research.
7. There may be more than this going on in each case (including the fact that there are different numbers of cases in each part of the scales), but the bottom line is that one should be very wary of these pictures, and a complete analysis would have delved much more deeply into what each one is doing. (There also could have been much more description of exactly what each picture represents.)
8. Equation 6 follows from Equation 8 by setting $\gamma = 0$, setting $\eta 1 = \alpha\ \delta 1$ and $\eta 2 = (1-\alpha)\ \delta 2$ and letting $b = \varphi$. Then Equation 4 follows by setting $\eta 1 = \eta 2 = 1$. Equation 6 follows by letting $\gamma = -$infinity, $\eta 1 = \delta 1$ and $\eta 2 = \delta 2$, and $b = \varphi$
9. Strictly speaking, milk and water both require a container so that they are substitutes for one another (sufficient alone) if a container (a complement and a necessary condition) is available. This discussion helps to emphasize even more the complexity of causal thinking.

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