Strategy Selection in Causal Reasoning: When Beliefs and Covariation Collide

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Abstract The present study investigated how people combine covariation information (Cheng & Novick, 1990, 1992) with pre-existing beliefs (White, 1989) when evaluating causal hypotheses. Three experiments, using both within- and between-subjects designs, found that the use of covariation information and beliefs interacted, such that the effects of covariation were larger when people assessed hypotheses about believable than about unbelievable causal candidates. In Experiment 2, this interaction was observed when participants made judgments in stages (e.g., first evaluating covariation information about a causal candidate and then evaluating the believability of a candidate), as well as when the information was presented simultaneously. Experiment 3 demonstrated that this pattern was also reflected in participants' metacognitive judgments: Participants indicated that they weighed covariation information more heavily for believable than unbelievable candidates. Finally, Experiments 1 and 2 demonstrated the presence of individual differences in the use of covariation- and belief-based cues. That is, individuals who tended to base their causality judgments primarily on belief were less likely to make use of covariation information and vice versa. The findings were most consistent with White's (1989) causal power theory, which suggests that covariation information is more likely to be considered relevant to believable than unbelievable causes.

One encounters many situations in which one must assess the importance of potential causal candidates in order to determine the likelihood that putative causes are responsible for observed effects: People become ill, cars malfunction, and the stock market crashes. Moreover, the importance of making accurate causal attributions is not merely of academic importance. For example, if one ingests a noxious substance prior to becoming ill, making a causal link between the noxious substance and subsequent illness could prove imperative for individual and species survival. The goal of the present paper is to investigate how people combine and weigh information derived from various sources when making causal attributions. In particular, we examined how people combine information derived from empirical observations (i.e., the degree to which the effect is observed to vary with a putative cause) in light of their pre-existing beliefs.

Consider the case in which the individual has become ill after ingesting the noxious substance. She could conclude that the substance was responsible for the illness based on her previous experience with that substance: If illness occurred on two previous occasions where the substance was eaten, but rarely occurred otherwise, she would have evidence that the substance covaried with the illness and this evidence would support a causal link. Alternatively, she could consult her knowledge or beliefs regarding the substance in question: If the substance were something believed to have the capacity to produce illness, such as a mushroom, the reasoner might consider that grounds to infer a causal relationship. Finally, it is possible that both of these sources of information are brought to bear on the problem at hand. If so, how might she weigh and evaluate the importance of the two types of information? Although there is much evidence to suggest that each of the cues, covariation and beliefs, is instrumental in evaluating causal hypotheses when considered in isolation, to date there is no firm evidence to suggest how they might be combined.

COVARIATION-BASED THEORIES

Some researchers claim that humans are "intuitive statisticians" (Peterson & Beach, 1967) who make causality judgments using a normative strategy wherein the degree of covariation between a putative cause and its observed effect is computed. This model of causation is a product of the Humean tradition of radical empiricism (Hume, 1739/1978), which posits that humans and other animals rely primarily on observable empirical cues to understand and explain causal sequences. These observations can be summarized into three main rules of cause and effect relations: (a) Causes must precede effects, (b) causes and effects must be related both temporally and spatially, and (c) there must be consistency in the cause-effect relation such that they repeatedly co-occur in regular succession (i.e., that the cause and the effect covary).

A modern successor to the Humean philosophy is the contingency model (Jenkins & Ward, 1965; Rescorla, 1968; Salmon, 1965). This model is based on the assumption that an event that exhibits a regularity of association with an effect (i.e., covaries with that effect) is more likely to be identified as a cause of that effect than is an event that does not exhibit a regularity of association. Proponents of the contingency model argue that causal knowledge is derived from observable events and is thus acquired from sensory input (Cheng, 1997).

One such contingency-based model of causation is the probabilistic contrast model (Cheng & Novick, 1990). According to this model, the reasoner considers two pieces of information when deriving a covariation estimate: the probability of the effect occurring when the cause is present [P(e/i)] and the probability of the effect occurring when the cause is absent [P(e/~i)]. Following this logic, causal roles are defined empirically by using the following contingency rule:

$$\Delta P_i = P(e/i) - P(e/\sim i) \quad (1)$$

If the computation of ΔP_i results in a positive value, then the candidate cause (i) should be judged to be a facilitatory factor in producing the effect (e). If, on the other hand, the computation of ΔP_i results in a negative value, the potential cause (i) should be judged to be an inhibitory factor. Under conditions in which ΔP_i is zero, the candidate cause (i) should be judged as a noncausal factor with respect to the observed effect (e).

Proponents of this and other contingency-based models claim that humans and other animals are sensitive to the contingency (ΔP_i) between the cause and effect, and then use this information as part of a causal model or schema to ascertain a measure of causal strength or likelihood (Cheng, 1997; Cheng & Novick, 1990, 1992). Indeed, some researchers claim that the use of covariation-based cues is primary in that "the presence of covariation overrides other evidence that may call causation into question" (Koslowski, Okagaki, Lorenz, & Umbach, 1989, p. 1316).

Evidence for normative covariation-based theories. The ΔP index is regarded by virtually all researchers in the field as a normatively appropriate index of the contingency between two binary variables (Kao & Wasserman, 1993). Further, it has been proposed that the contingency-based model of causation appears to capture (at least in part) our everyday notion of what a cause and effect sequence might behave like (Spellman, 1996). For example, many individuals argue that smoking causes lung cancer, which is based primarily on the finding that the probability of getting lung cancer if one smokes is greater than the probability of getting lung cancer if one does not smoke (i.e., ΔP_i is positive), even though smoking is neither necessary nor sufficient for getting lung cancer. In this way, the fact that the two events covary is enough for people to support a causal relation, although the fact that the two events covary does not, of course, guarantee a causal link.

Laboratory evidence suggests that the ΔP index is both descriptively as well as normatively appropriate. Several studies have suggested that the majority of reasoners make causality judgments that conform to the ΔP contingency rule. This is the case regardless of whether the information is presented in a 2 × 2 contingency table (Allan & Jenkins, 1980), or is presented in a free-operant paradigm where participants observe the putative causes and subsequent effects continuously in time (Wasserman, Chatlosh, & Neunaber, 1983), or when the information is summarized in sentences (Cheng & Novick, 1990).

Limitations of covariation-based theories. Although there is much evidence to suggest that reasoners are sensitive to ΔP information, all covariation-based models face a fundamental problem in that covariation does not necessarily imply causation. Take for example the regular succession of day and night, where day is perfectly contiguous with night ($\Delta P_i = 1$), and appears temporally prior to night. Despite this perfect covariation, reasoners know that these two events are not causally linked. Although sequences such as this exhibit similar observable statistical characteristics to their valid causal counterparts, they lack the critical connection implied by a truly causal relationship (Cheng, 1997).

In addition, there are several other research findings that challenge traditional contingency-based theories. Specifically, several studies have demonstrated that participants will confidently infer a strong causal link between two events after observing only a single positive instance of a cause and effect co-occurring (Beasly, 1968; Boyle, 1960; Michotte, 1963). These reasoners cannot be seen as using some derivative of ΔP to inform their judgments because ΔP requires at least two observations and must include information about the probability of the effect occurring in the absence of the cause [P(e/~i)]. Moreover, although ΔP assumes equal weighting for all four cells of a 2 × 2 contingency table, participants appear to weight them differentially (Downing, Sternberg, & Ross, 1985; Schustack & Sternberg, 1981; Wasserman, Dorner, & Kao, 1990).

Clearly, these findings indicate that participants must make use of other sources of information when faced with assessing causal hypotheses of this sort. Indeed, this is not a new idea. In their now classic review of causality judgments, Einhorn and Hogarth (1986) made reference to several cues to causality such as temporal order (Siegler & Liebert, 1974; Tversky & Kahneman, 1980), contiguity in time and space (Bullock, Gelman, & Baillargeon, 1982; Michotte, 1963), and similarity between cause and effect (Shultz & Ravinsky, 1977; Tversky, 1977) in which attributions are made even though they may conflict with covariation-based cues. In this way, covariation may be viewed merely as one of many cues to causality (White, 1992).

CONCEPT-BASED THEORIES

Whereas covariation-based theories suggest that reasoners use empirical observation to evaluate causal hypotheses, concept-based theories have evolved to incorporate the role of acquired knowledge in the evaluation of causal relations. There have been several specific proposals concerning the nature and origin of this causal knowledge, but most can be traced to Kant's (1781/1965) model of generative transmission. This view posits that causes not only covary with effects, but actually produce those effects. The term generative transmission refers to the transmission of energy from the cause to the effect such that the cause, through the nature of its properties, acts on the object resulting in a causal outcome (White, 1995). In the previous example involving the ingestion of a mushroom, generative transmission would refer to the transfer of energy (presumably in the form of poisonous chemicals) from the mushroom to the intestinal system, thus producing an aversive reaction.

Following Kant's concept-based view utilizing the notion of generative transmission, several theorists have proposed that causation be defined in terms of specific intrinsic properties of objects (Harre & Madden, 1975; Madden & Humber, 1974; White, 1989). For example, the notion of generative transmission forms the core of the causal powers theory that was first proposed in philosophy by Harre and Madden (1975) and later applied to psychologically relevant questions by White (1989). Harre and Madden (1975) posited that causal powers are stable properties of objects whose power to produce an effect is based on the "chemical, physical, or genetic natures of the entities involved" (p. 5). However, this power only produces an effect under the appropriate enabling conditions (White, 1995).

Based on these tenets of the causal powers theory (White, 1989), causal roles are defined conceptually, rather than through empirical associations. The assessment of causal hypotheses, therefore, is thought to be mainly a matter of seeking some object believed to possess the power to produce the effect in question and then determining if the appropriate releasing conditions are present to enable the power of the object to exert the effect (White, 1989). For example, consider that the person in the mushroom example had eaten a large meal consisting of several different courses. Of the many items consumed, mushrooms might be singled out because they are believed to have the potential to produce illness (given the enabling condition that they are ingested), whereas the putative role of lettuce or carrots might be overlooked because they are not commonly believed to be linked to illness.

Evidence for concept-based theories. Evidence in support of concept-based theories has come from a variety of studies and research applications. These findings demonstrate that when asked to evaluate the utility of various types of cues to test a causal hypothesis (e.g., temporal and spatial contiguity), reasoners reliably indicate that information regarding the causal mechanism will be the most informative (Shultz, 1982; Shultz, Fisher, Pratt, & Rulf, 1986). Moreover, reasoners do not spontaneously seek out covariation information between potential causal candidates and effects when they were provided with the opportunity. Rather, it has been found that individuals prefer to gather further information regarding the specific target events in question to test hypotheses about possible underlying mechanisms (Ahn, Kalish, Medin, & Gelman, 1995; White, 1989). Taken together, these research findings suggest a strong propensity for reasoners to prefer information regarding a potential causal mechanism rather than covariation information when testing causal hypotheses.

COMBINING COVARIATION WITH BELIEFS

How do people combine covariation information with their pre-existing knowledge and beliefs about causal relationships? The preceding evidence suggests that people seek information regarding the mechanisms of causation when they are unknown, and that the need for this type of information may take priority over the need to inquire about covariation. Further, several studies have demonstrated that prior knowledge may guide the interpretation of covariation information (e.g., Waldmann, 1996; White, 1989, 1995). For example, prior knowledge may be used to determine which of two contingent events is the cause and which is the effect (Waldmann, 1996). That is, we know that "effects can be achieved by manipulating causes but causes cannot be accomplished by manipulating their effects" (p. 52).

However, there is little evidence to tell us how people combine covariation information with already existing beliefs about the putative cause and effect. In the case of the mushrooms, for example, the reasoner is in a position to evaluate two pieces of information. One is derived from observation and concerns the number of observed occasions in which ingesting mushrooms produced illness. The second is a set of beliefs regarding the types of food likely to produce illness after consumption. How are these two elements weighed and evaluated?

The only study we are aware of that addressed this question was conducted by White (1995). In this study, participants assigned causal roles, such as simple cause, enabling condition, or constant condition, to various factors in a causal scenario. White found support for his causal powers theory in that the choice of causal roles was largely determined by the causal power of the agent to produce the outcome in question. Even when a candidate cause covaried perfectly with the effect, it was not identified as a cause unless it was perceived to have the causal power to produce the effect. However, White did not provide complete information required for the computation of ΔP for all the putative causal agents, nor was ΔP manipulated orthogonally with causal power. Thus, we still do not know how attention to, and use of, these cues varies across values of the other cue.

Experiment 1

Given that reasoners often make these attributions in information-rich settings where they have access to a variety of cues, it is relevant to know how these various cues to causality are evaluated in combination. The goal of this and the following experiments was to examine how reasoners integrate covariation information with prior beliefs when making causal attributions. To do so, we provided participants with scenarios describing an effect and a putative cause. The cause was either a believable or an unbelievable candidate, and reasoners were told that the cause either varied or did not covary with the effect. Several possible relationships between belief-based and covariation-based cues were considered.

According to the probabilistic contrast model, individuals evaluate causal hypotheses using the normative ΔP rule (Cheng and Novick, 1990, 1992). This model proposes that the strength of a causal judgment will vary with the size of ΔP, and further, that reasoners will make use of this covariation information regardless of the other information that is provided.

In contrast, according to the mechanism-based models (e.g., Ahn et al., 1995; Shultz, 1982; Shultz et al., 1986) reasoners base their causal attributions on generative transmission, whereby reasoners make use of a priori knowledge to determine the existence of a causal link between the putative cause and its effect. Thus, participants should make use of belief-based cues, regardless of what other information is available. Extrapolating from the evidence regarding preferences in causal cues can derive an even stronger prediction. Shultz (1982) found that people prefer to use mechanism-based cues relative to other types of cues, and chose to use these other cues (such as covariation) only if mechanism-based cues were not available. To the extent that reasoners' actual causal judgments reflect these types of cue preferences, we would expect that reasoners would rely only on belief-based cues in our study, and ignore or downplay the covariation information.

Both the belief-based and covariation-based models predict main effects of the relevant causal cue. A third possibility was suggested by White's (1989) causal powers theory. This theory assumes that individuals' preconceived understanding or beliefs about the operation of specific causal powers in the environment is present prior to the use of any normative empirical cues (White, 1989, 1992). White (1989) further proposes that "people will only use covariation information if at all, when an event or condition passes some initial test of plausibility as a potential cause of the effect in question" (p. 435). In other words, pre-existing beliefs can be used to restrict the set of candidates about which covariation information is considered. In the mushroom case, for example, the reasoner might consult long-term memory for evidence regarding the covariation of mushrooms and illness, because mushrooms are believed a priori to have the capacity to produce illness; similar information may not be sought for other substances, such as carrots or lettuce, which are not commonly believed to produce illness. Thus, it is possible that covariation information will only be used in conjunction with believable, and not unbelievable, causal candidates. This suggests an overadditive interaction between beliefs and covariation, whereby covariation information ought to only influence judgments for candidates that are believable causes and have no effect for unbelievable causal candidates.

METHOD

Participants. One hundred and fifty first-year psychology students from the University of Saskatchewan with a mean age of 19.8 (range 17-37) completed the study in partial fulfillment of a course requirement.

Design. This was a 2 × 3 within-subjects design with two levels of the belief manipulation (low and high) and the three levels of the covariation manipulation (ΔP's = 0, .5, and 1).

Materials. Six story scenarios were constructed about the following content areas: depleted fish populations, car start failures, productivity slowdown, disease epidemic, car accidents, and allergic reactions (see Appendix). Each scenario presented a relationship between a cause and an effect that was either believable or unbelievable. In the high-belief scenarios, the covariate was something that is commonly believed to have the causal power to produce the effect in question, provided that the appropriate enabling conditions are present. For example, severed brake lines are commonly believed to be a plausible precursor to a car accident. In the low-belief scenarios, the covariate was something that is not commonly believed to have the causal power to produce the effect in question. For example, doing homework is not commonly believed to be a plausible precursor to an allergic reaction.

The believability of potential causal candidates was determined in a pilot test. Thirty-six scenarios were presented to 64 other participants. They were presented with candidate causes and effects in the absence of covariation information and were asked to judge the plausibility of the link between the putative causal candidate and the effect using a 9-point Likert scale which ranged from 1 (highly

TABLE 1
Mean Believability Ratings and Standard Deviations (*SD*) of the Putative Causes Used in Experiment 1

Belief	Scenario	Putative Causes	Mean	*SD*
High	Depleted Fish	Insecticides	8.1	1.1
High	Car Accident	Severed Brake Lines	8.0	1.6
High	Productivity Slowdown	Illness	7.8	.8
Low	Epidemic	Green Eyes	2.1	1.5
Low	Car Start	Flat Tire	1.4	1.2
Low	Allergic Reaction	Homework	1.5	.8

implausible) to 9 (highly plausible). The causal candidates were chosen a priori to be plausible, implausible, or neutral (i.e., an unknown candidate). For example, individuals were asked to judge the plausibility that insecticides, PCP, boronium, chloroprinate, aeration, and algae could cause depleted fish populations. Similar judgments were made for the remaining scenarios. The three causal candidates given the highest plausibility ratings (M = 8.0) and the three causal candidates given the lowest plausibility ratings (M = 1.7) were chosen as the high- and low-belief candidates, respectively, for the following experiments. The mean believability ratings for these six causal candidates are presented in Table 1.

For the current experiment, participants were presented with scenarios describing the candidate cause and effect; in addition, the scenarios indicated that each putative cause was either perfectly contingent with the effect (ΔP_i = 1), moderately contingent with the effect (ΔP_i = .5), or noncontingent with the effect (ΔP_i = 0). Covariation information was presented in a discrete format which specified the number of times the effect occurred in the presence of the cause [P(e/i)], and the number of times the effect occurred in the absence of the cause [P(e/~i)]. This format was chosen because several researchers (e.g., Kao and Wassermann, 1993; Ward & Jenkins, 1965) have found that participants typically make more accurate contingency judgments when presented covariation information in a discrete format, as compared with a continuous format, in which causes and effects are observed continuously in time. The marginal totals (i.e., total number of observations where the cause is present or absent) were set at 10 for all levels of ΔP. The value of ΔP was manipulated by changing the probability P(e/~i), as illustrated in Table 2.

Procedure. Each scenario was presented on a separate page. Across the six scenarios, levels of ΔP were crossed with belief, such that one problem was presented to each participant in each belief × ΔP cell. Moreover, across participants, the assignment of belief and ΔP conditions was counterbalanced such that the believable and unbelievable causal candidates appeared equally often in all covariation contingencies. The six scenarios were presented in a different random order for each participant. Participants were asked to respond to the scenarios using a 9-point Likert scale that ranged from 1 (definitely has nothing to do with it) to 9 (definitely has something to do with it) with 5 (may or may not have anything to do with it) as the midpoint.

All participants were tested in a single session. The materials for this experiment were included in a set of unrelated experiments on other topics. Instructions were all written, so the experimenter gave only a brief introduction informing participants to complete the questions in the order in which they appeared and to work at their own pace. Participants were encouraged to ask any questions they might have at any time during the experiment.

TABLE 2
Event Frequencies Used for the Computation of the ΔP Values Used in Experiments 1, 2, and 3

Frequencies				ΔP Computations			
ie	i~e	~ie	~i~e	P(e/i)	–	P(e/~i)	ΔP
10	0	0	10	10/10	–	0/10	1
10	0	5	5	10/10	–	5/10	.5
10	0	10	0	10/10	–	10/10	0

Note. (ie) represents the number of times the cause and effect co-occurred; (i~e) represents the number of times the cause occurred in the absence of the effect; (~ie) represents the number of times the effect occurred in the absence of the cause; (~i~e) represents the number of times the effect was absent when the cause was absent.

RESULTS AND DISCUSSION

The alpha level for all statistical tests was set at .05 (two-tailed) for the three experiments unless otherwise stated. Effect size estimates were computed using partial η^2 throughout.

Relationships between beliefs and ΔP. Figure 1 presents the mean causality judgments for the two belief conditions (low belief and high belief) and the three ΔP conditions (0, .5, and 1). This first series of analyses tested the differences in causal judgments as a function of belief and covariation. The mean likelihood ratings were analyzed using a 2 × 3 (belief × ΔP) repeated measures ANOVA. Consistent with Cheng and Novick's (1990) probabilistic contrast model, the analysis revealed a significant main effect of ΔP, $F(2,298)$ = 276.49, MSE = 4.96, η^2 = .65. The pattern of data was such that increments in ΔP from 0 to .5 and 1 resulted in mean increased likelihood judgments of 2.6, 5.2, and 6.8, respectively. Similarly, consistent with mechanism-based models (e.g., Ahn et al., 1995; Shultz, 1982), there was a significant main effect of belief, $F(1,149)$ = 213.56, MSE = 5.88, η^2 = .59. Here, causality judgments were greater for scenarios with highly believable causal candidates (M = 6.0) than scenarios with unbelievable causal candidates (M = 3.7). However, the effect of ΔP interacted with belief $F(2,298)$ = 8.69, MSE = 3.07, η^2 = .06. Consequently, simple effects analyses

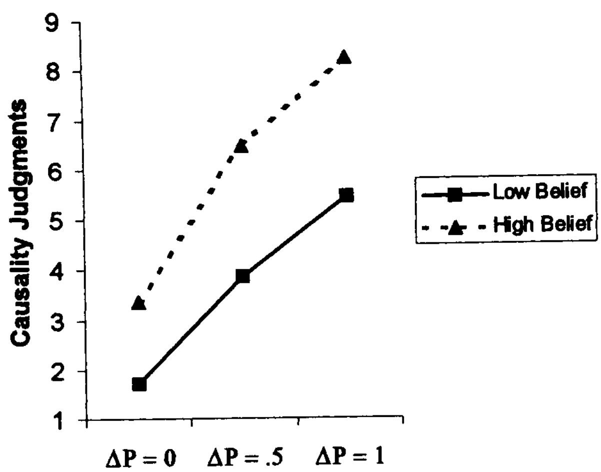

Figure 1. Mean causality judgments for the two belief conditions (low belief and high belief) and the three ΔP conditions (0, .5, and 1) in Experiment 1.

were performed comparing the ΔP effect for each level of belief. These follow-up analyses revealed that each increment in ΔP produced a significant increase in likelihood ratings for both believable and unbelievable causes (all $ts > 6.52$); however, the effect of covariation was larger for believable candidates (M difference = 2.4) than for unbelievable candidates (M difference = 1.9). Therefore, these data are most consistent with White's (1989) causal power theory as it clearly stipulates an interactive relationship between beliefs and ΔP. However, in contrast to a strict interpretation of White's theory, ΔP had an effect for unbelievable candidates, albeit a smaller effect than for believable ones.

These findings could also be interpreted to reflect a confirmation bias (see Evans, 1989 for an overview) whereby individuals tend to seek out information that is consistent with, or confirms their initial hypothesis. That is, when reasoners believe the candidate is a likely cause of the effect, other evidence that also supports such a link, such as covariation, may be attended to. In contrast, when the candidate is unbelievable, evidence supporting a causal link may be downplayed because it fails to confirm the initial belief.

Participants' interpretations of contingency relations. There are potentially several ways to interpret the observed relationship between ΔP and perceived causality. For example, our findings appeared to suggest that the higher the degree of covariation between the putative cause and its effect, the higher the perceived causality. In contrast, an argument could be made that the degree of covariation does not necessarily predict the strength of the causal relationship. That is, any degree of covariation should be enough to provide evidence for a causal link between cause and effect; indeed, causal relationships often have small correlations (e.g., smoking and lung cancer). Based on this logic, the relationship between perceived causal efficacy and perceived covariation would be binary: When ΔP exceeds 0, a causal relation should be perceived regardless of the value of ΔP. By extension, higher degrees of covariation would not necessarily imply a stronger relationship.

Our data, however, suggests otherwise: Every increment of ΔP resulted in a significant difference in reported causality judgment (smallest mean difference = 1.6). That is, higher values of covariation did appear to imply a stronger causal link. Indeed, when individual responses were analyzed, they too reflected this overall trend: Ninety percent of the participants in this experiment increased their judgment of causality when ΔP increased. More telling is the number of participants who increased their causality judgments as a function of increases in ΔP from .5 to 1: Seventy-eight percent of the participants made higher causality judgments for a ΔP of 1 than .5. Based on this data, it is clear that participants' judgments of causal likelihood are influenced by covariation information in a nonbinary fashion.

Another possible ambiguity in the relationship between ΔP and cause concerns the interpretation of the $\Delta P_i = 0$ condition. Cheng (1997) has argued that when $\Delta P_i = 0$ is computed by setting $P(e/\sim i) = 1$ (as was the case in our study), the resultant contingency is uninterpretable. Cheng (p. 368) provides a personal example of this situation with a scenario involving allergic reactions. She suspected that she had some food allergies so she went to the doctor to have a scratch test. The doctor made a grid of scratches on her back and put multiple samples of food on the various scratched spots. After a few minutes, the doctor observed a hive on every spot [i.e., P(hives/food) = 1]. Based on this information, the doctor could have mistakenly inferred that she was allergic to every food tested. However, it turned out that she was allergic to the scratches because she received a hive every time the doctor made a scratch on her back even when no food was present [i.e., P(hives/ ~ food) = 1]. Therefore $\Delta P_{food} = 0$. However, Cheng argued that the test for the allergy to foods was uninterpretable because one does not know whether or not the food has the causal power to produce the allergic reactions given that the scratches masked the effect. Because this is analogous to how we computed $\Delta P_i = 0$ in the present series of experiments, this condition is potentially uninterpretable according to this reasoning.

However, within the set of events defined by the problem space (i.e., this set of scratches), we would argue that the evidence is unambiguous. Thus, the fact that the $\Delta P_{food} = 0$ provides conclusive evidence that these reactions were definitely caused by something other than food allergies. The ambiguity arises because there was a confounding variable (i.e., food confounded with scratches) in the experiment, not because $\Delta P_i = 0$ is uninterpretable. However, given these concerns, it is relevant to know how participants interpret scenarios when the covariation information is noncontingent ($\Delta P_i = 0$).

Our participants clearly saw no ambiguity. When the causal candidate was not believable, 75% of the participants reported that this candidate "definitely had nothing to do with the effect" (1 on the 9-point scale) whereas only 6% stated that the cause "may or may not have anything to do with the effect" (5 on the 9-point scale). Note that a causal likelihood judgment of 5 would most closely represent a judgment that the causal relationship was uninterpretable. When the candidate was believable, the modal response was still 1: Thirty-three percent of the participants reported that this candidate "definitely had nothing to do with the effect" and only 11% stated that the cause "may or may not have anything to do with the effect." Taken together, these data clearly demonstrate that individuals did not interpret a $\Delta P_i = 0$ as uninterpretable and instead interpreted it as evidence that the causal candidate did not produce the effect.

Individual differences in cue weighting. The means analysis leads us to expect a modal pattern of responding whereby individuals combine belief-based and covariation-based cues. To discover the extent to which the overall pattern of means typified individual responses, two difference scores were computed for each participant. The "belief-difference" score was computed by taking the difference between causality judgments for the low-belief and the high-belief items, averaged across the three ΔP contingencies (0, .5, and 1). The "ΔP-difference" score was computed by determining the average increment in causality judgments as ΔP increased from 0 to .5 and .5 to 1, averaged across the low- and high-belief items.

Figure 2 plots the participants' ΔP-difference scores against their belief-difference scores. As suggested by the scatterplot, there was a significant negative correlation, $r(148) = -.480, p < .001$, between the belief-difference scores and the ΔP-difference scores.[1] In other words, individual participants do not appear to have been combining belief-based and covariation-based cues in a straightforward manner. Instead, individuals whose judgments differed as a function of ΔP tended to differ less as a function of beliefs and vice versa. This suggests that the means analysis may have presented a misleading picture. The large main effects of belief and covariation appear to have been produced by different subpopulations of participants, some of whom used

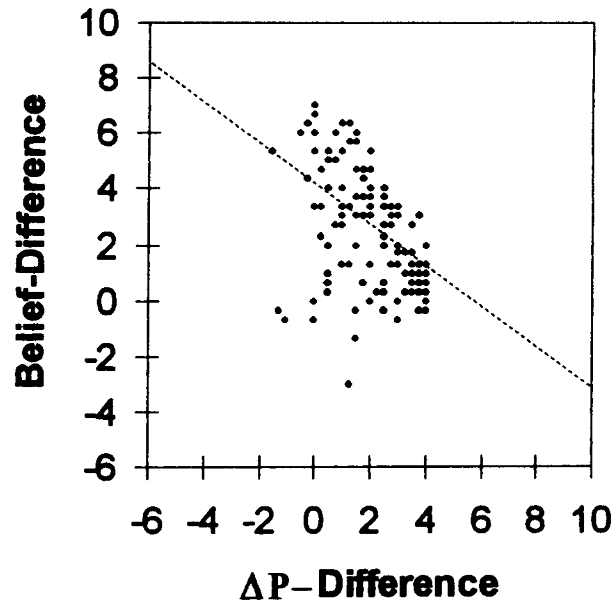

Figure 2. Individual differences in causality judgments as a function of ΔP (composite of 0, .5 and 1) and belief (low belief and high belief) in Experiment 1.

belief-based cues disproportionately, and others of whom used covariation-based cues disproportionately.

Experiment 2

Experiment 2 had two goals. The first was to investigate further the individual differences discovered in Experiment 1. Before drawing theoretical inferences from those findings, it was first necessary to determine the extent to which they reflected stable properties of the reasoner as opposed to situationally determined task demands. Thus, we manipulated the manner in which covariation and belief-based information was presented to participants, and looked at how changes in the task demands affected the disproportionate use of causal cues.[2] This method also allowed us to pursue our second goal, which was to look more closely at how the two sources of information are combined to make a global judgment.

We hypothesized that if the importance attached to covaration-based and belief-based cues was situationally determined, it should be possible to influence how they are used by manipulating the salience of the cues. To test this hypothesis, we included three types of problems. The "original" problems were identical to those used in Experiment 1, and served as a control condition. For the other two conditions, participants were asked to make their judgments in stages.

For the "belief to ΔP" condition, participants first made judgments based on the perceived causal power (believabil-

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[1] The two constructed difference scores (belief-difference and ΔP-difference) are not completely independent. That is, they both contain two common terms: high belief/high covariation and low belief/low covariation. Therefore, the nature of the relationship between these two derived difference scores comes from the remaining two terms: high belief/low covariation and low belief/high covariation. However, these scores are nonetheless informative in that they tell us that individuals' judgments differ more as a function of either belief or covariation information relative to these baselines (i.e., high belief/high covariation and low belief/low covariation).

[2] We would like to thank Peter White for suggesting that manipulating the instructions might be a useful way to investigate individual differences.

ity) of the candidate cause without any covariation information present. They were then provided with covariation information and asked to reassess their judgments. According to causal powers theory (White, 1989), this sequence more closely parallels the natural sequence of causal evaluation than does the "original" presentation. That is, reasoners are assumed to assess the believability of a candidate, and to then evaluate covariation-based cues in light of this believability judgment. Presenting the belief information first should therefore increase the probability that reasoners attend to both sources of information, and thereby reduce the tendency for reasoners to rely on one or the other of the cues. Thus, the negative correlation between use of beliefs and the use of covariation information should be reduced relative to the "original" presentation condition.

A third condition was included as an additional control. Specifically, if as expected, there was a reduction in the correlation in the "belief to ΔP" condition relative to the "original" condition, it might be possible to attribute this effect to the use of a sequential evaluation paradigm per se. Thus, the fact that the cues are evaluated in stages might lead reasoners to attend to both belief-based and covariation-based cues, regardless of the order in which they were presented. To rule out this possibility, participants in the "ΔP to belief" condition made their judgments in the reverse sequence. They first made a causal likelihood judgment based only on covariation information; they were then asked to reassess their judgment based on the believability of the candidate. Therefore, if successful cue integration depends on the evaluation of belief-based cues before covariation-based cues, one would expect to observe the negative correlation between participants' beliefs and covariation utilization in the "ΔP to belief" condition.

METHOD

Participants. Two hundred and eighty-two first-year psychology students from the University of Saskatchewan with a mean age of 20.8 (range 17-48) completed the study in partial fulfillment of a course requirement.

Design. This was a 2 × 2 × 3 mixed design with beliefs (low and high) and covariation (ΔP's = 0 and 1) as within-subject variables; problem format ("original," "belief to ΔP," and "ΔP to belief") was a between-subject variable.

Materials. This experiment used four of the six scenarios from Experiment 1; these were presented on separate pages in a small booklet. The four story scenarios concerned the following content areas: depleted fish populations, car start failures, car accidents, and allergic reactions. As in Experiment 1, each scenario contained causal links that were either believable or unbelievable. In addition, each putative cause was either perfectly contingent with the effect ($\Delta P_i = 1$), or noncontingent with the effect ($\Delta P_i = 0$). Scenarios and ΔP were fully counterbalanced such that each scenario appeared equally often under each covariation contingency.

The critical manipulation in this experiment was the order in which ΔP and belief information were presented. The "original" problem format was identical to that utilized in Experiment 1: Causally relevant information was presented simultaneously in a paragraph format, and participants made only a single judgment based on both sources of information. In the two modified problem formats, however, two judgments were required per problem.

For the belief to ΔP problem format, participants were first presented with the causal candidate hypothesized to be responsible for the effect. The candidate was either a believable or an unbelievable cause. At this point, participants judged the likelihood that the putative causal candidate was responsible for the observed effect using the same 9-point Likert scale utilized in Experiment 1. Note that this initial likelihood judgment is based solely on knowledge of the believability of the candidate cause in the absence of any empirical cues. Immediately following the initial judgment, covariation information was presented in the same frequency format as in Experiment 1, followed by another Likert scale. Participants were asked to reassess their likelihood judgment based on this new evidence.

For the ΔP to belief problem format, participants were first presented with covariation information for an unknown causal candidate; they were then asked to make a causality judgment using the 9-point Likert scale. Therefore, the initial likelihood judgment in this problem format condition was based solely on the covariation information in the absence of any preconceived knowledge of causal candidacy. Immediately following the initial judgment, the nature of the causal candidate was made known. For example, the second part of the "fish scenario" revealed that the causal candidate was "insecticides." This was followed by another Likert scale where participants were asked to reassess their likelihood judgment based on this new evidence.

Procedure. The procedure was otherwise identical to that of Experiment 1.

RESULTS AND DISCUSSION

Individual differences in cue weighting. This first series of analyses examined individual differences for each of the three presentation formats. As was the case in Experiment 1, two scores were computed for each participant based on their final causality judgments: the degree to which judgments differed as a function of ΔP (ΔP-difference) and beliefs (belief-difference). Figure 3 plots each participant's ΔP-difference score against their belief-difference score for each of the three problem formats.

The scatterplots in Figure 3 support the predictions derived from causal powers theory (White, 1989). First, the findings of Experiment 1 were replicated in that a reliable

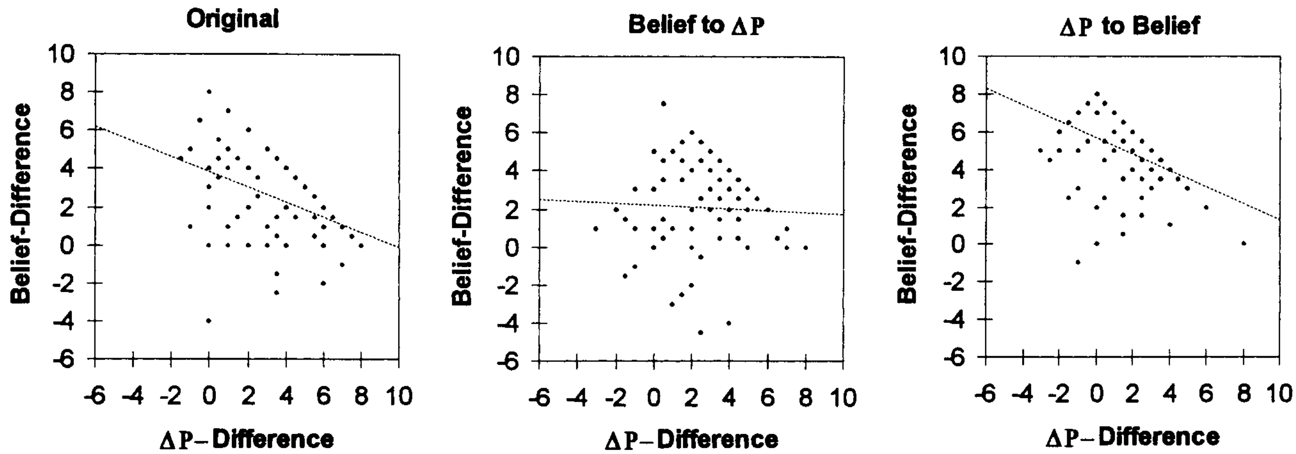

Figure 3. Individual differences in causality judgments as a function of ΔP and beliefs for each of the three problem formats in Experiment 2.

negative correlation emerged for the "original" problem format $r(93) = -.46, p < .001$. This reflected a tendency on behalf of participants to disproportionately attend to either belief-based or covariation-based cues. A similar tendency was observed in the ΔP to belief condition $r(91) = -.43$, $p < .001$. Thus, presenting each cue separately did not affect the disproportionate weighting of the cues. However, the best-fit line in the belief to ΔP condition is flat $r(95) = -.05$, $p = .66$ despite adequate power to detect a correlation ($1 - \beta$ for $(r > .3) = .83$). As predicted, therefore, presenting the belief-information first reduced any systematic tendency to weight one cue more heavily than the other.

These analyses were corroborated by performing a one-way ANOVA on the slopes of the three best-fit lines. These slopes differed as a function of the problem format manipulation $F(2, 276) = 5.61$, $MSE = 4.21$. Single degree of freedom comparisons indicated that the slope of the belief to ΔP problem format differed reliably from the slope of both the "original" condition $F(1, 186) = 7.95$, $MSE = 4.34$ and the ΔP to belief condition, $F(1, 184) = 8.33$, $MSE = 4.25$. However, the "original" and ΔP to belief condition did not reliably differ from each other ($F < 1$).

Judgment updating. The next series of analyses tested the changes in causal judgments between initial and final judgments for the two sequential presentation conditions (the belief to ΔP and ΔP to belief condition). Figure 4 presents the initial causality judgments, and the final causality judgments as a function of the belief conditions (low belief and high belief), and ΔP conditions (0, and 1), for the three problem formats.

The initial judgments were examined first in order to determine that these judgments reflected the effects of beliefs (in the belief to ΔP condition) and covariation (in the ΔP to belief condition). Indeed, the initial judgments for the belief to ΔP condition (which were based only on beliefs) revealed a reliable main effect of belief $F(1, 95) = 250.71$, $MSE = 3.60$, $\eta^2 = .73$; the initial judgments for the ΔP to belief condition (which were based only on ΔP information) revealed a reliable main effect of ΔP, $F(1, 91) = 109.20$, $MSE = 8.14$, $\eta^2 = .55$.[3] In addition, the magnitude of these effects were highly similar: The average difference between low and high belief was almost identical to the average difference between $\Delta P = 0$ and $\Delta P = 1$ ($Ms = 3.1$).

Although the belief- and covariation-based cues produced effects of equivalent magnitudes when presented in isolation, the effect of these cues when used to update judgments were not equivalent (final judgments minus initial judgments). That is, the initial judgments changed more when updated on the basis of belief information ($M = 2.2$) than on the basis of covariation information ($M = 1.4$), $t\ (186) = 5.28$, $SE = .14$. That is, people appeared to be utilizing a conservative belief updating strategy according to which they were reluctant to change their beliefs in light of new covariation information. Moreover, the effect of belief was primarily negative. That is, an unbelievable candidate reduced judgments more than a believable candidate added to judgments. This finding is consistent with the belief-effects observed in the deductive reasoning literature (see Evans, Newstead, Allen, & Pollard, 1994; Torrens, Thompson, & Cramer, 1999).

The final likelihood judgments reflected a recency effect, in keeping with general principles of belief updating (see

[3] The initial judgments for the "ΔP to belief" condition (judgments based only on ΔP information) also revealed a reliable main effect of believability $F(1, 91) = 14.98$, $MSE = 1.96$, $\eta^2 = .14$. This suggests that the initial judgments for this condition may not represent a pure measure of the effects of covariation. Therefore, participants who produced a belief effect on their initial judgments were separated from those who did not produce a belief effect. This analysis revealed that there were no systematic differences between these two groups in terms of the influence of the addition of belief information (all $Fs < 1.68$). Therefore, to retain power comparable to the other groups, the full sample was retained for the remainder of the analyses.

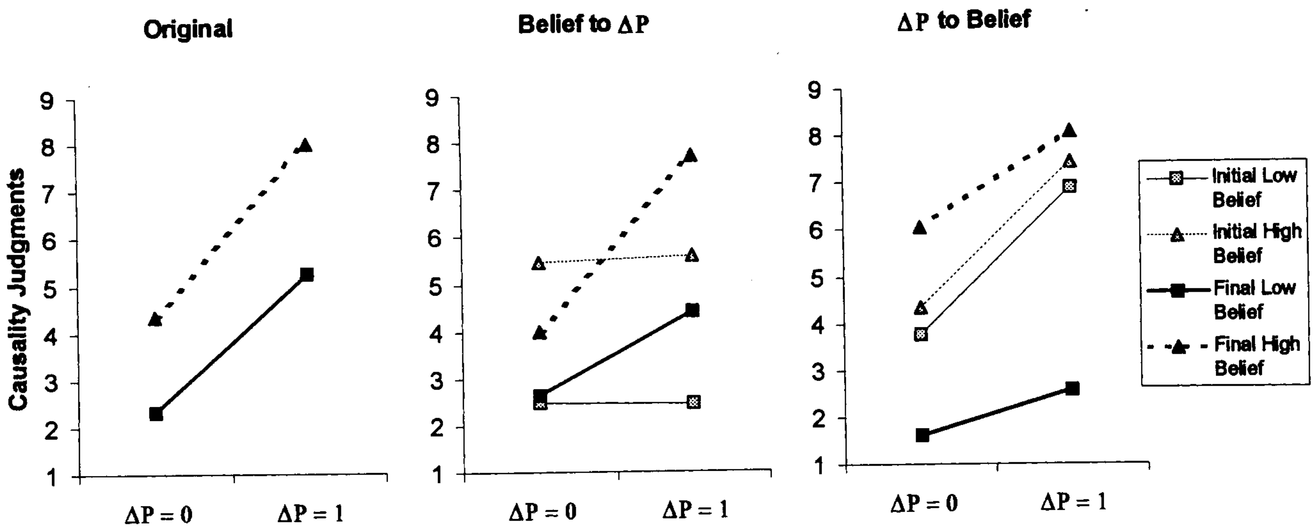

Figure 4. Initial causality judgments and the final causality judgments for the two belief conditions (low belief and high belief), and the two ΔP conditions (0, and 1) for the three problem formats in Experiment 2.

Hogarth & Einhorn, 1992). Specifically, the effects of belief and covariation were largest in those conditions where those cues had been presented most recently. In other words, the ΔP effect was larger for final judgments in the belief to ΔP condition (M = 2.8) than the ΔP to belief condition (M = 1.49), t (186) = 3.91, SE = .32. Similarly, the effect of belief was larger for final judgments in the "ΔP to belief" condition (M = 5.0) than the belief to ΔP condition (M = 2.3), t (186) = 8.41, SE = .31. According to Hogarth and Einhorn (1992), these types of recency effects are to be expected when the task is relatively simple and the judgment is made in stages.

Relationships between beliefs, ΔP, and order of cue presentation. The final series of analyses examined effects of beliefs, covariation, and the problem format manipulation in the final causal judgments. Theses scores appear in Figure 4. The final likelihood ratings (the second of the two judgments) were analyzed using a 2 × 2 × 3 (belief × ΔP × problem format) mixed ANOVA.

As was the case in Experiment 1, there were main effects of belief $F(1,279)$ = 609.44, MSE = 4.80, η^2 = .69; ΔP, $F(1,279)$ = 320.26, MSE = 5.56, η^2 = .53, and a reliable belief × ΔP interaction $F(1,279)$ = 36.19, MSE = 3.02, η^2 = .12 according to which the effects of covariation were larger for believable candidates (M difference = 3.1) than unbelievable candidates (M difference = 1.9). The three-way interaction among beliefs, ΔP, and problem format was also significant $F(2,279)$ = 3.28, MSE = 3.02, η^2 = .02. Separate belief × ΔP ANOVAs computed for each presentation condition revealed that all three two-way interactions were significant (all $Fs > 4.66$, $p < .05$). However, it appeared that the effect size of the belief × ΔP interaction was larger in the belief to ΔP condition (η^2 = .25) than either the "original" problem format (η^2 = .05) or the "ΔP to belief" problem format (η^2 = .07).

The three-way interaction is most consistent with White's (1989) causal power theory when one considers the effect of the problem format manipulation on the effects of cue integration. That is, the overadditive interaction is proposed to occur only when participants are actually attending to both cues. Therefore, by encouraging participants to attend to both cues in the order prescribed by White (1989), cue integrating should be increased.

Conclusions. These findings are consistent with White's (1989) assumption that covariation information is evaluated in light of one's pre-existing beliefs. Specifically, it appears that the order of cue presentation is crucial to the successful integration of causally relevant cues. That is, merely requiring individuals to attend separately to each cue, as was the case in the "ΔP to belief" condition, was not enough to promote successful cue integration. Instead, participants in the "ΔP to belief", as well as the "original" condition, systematically used one cue or the other. In contrast, when the cues were evaluated first in terms of beliefs and then ΔP, the disproportionate weighting trend disappeared. In addition, the interaction between beliefs and ΔP was observed for all instruction conditions. The form of the interaction was such that the effects of covariation were larger for believable than unbelievable candidates, suggesting that covariation information is weighed more when the candidate is believable. Finally, the interaction between covariation and beliefs was strongest when the systematic tendency toward disproportionate cue use was eliminated.

Experiment 3

Experiment 3 had three main goals. The first was to extend and replicate the findings of Experiments 1 and 2 using a different experimental design. In particular, we were concerned about the possibility that individuals' causality judgments may have been influenced by their judgments of previous problems. For example, it is possible that the causality judgment given to a perfectly contingent problem (ΔP_i = 1) may have been affected to some degree if the participant previously responded to a noncontingent (ΔP_i = 0) scenario. These carry-over effects would be especially problematic if they were asymmetrical; that is if judgments in one condition were more influenced by preceding events than judgments in another condition. To rule out the possibility that carry-over effects influenced our earlier findings, Experiment 3 tested the effects of covariation and beliefs using a between-subjects design.

A second purpose of Experiment 3 was to determine whether the interaction between covariation and beliefs extends to participants' metacognitive judgments of the usefulness of covariation information. That is, do reasoners perceive that covariation information is more useful to them when they are making judgments about believable as opposed to unbelievable causal candidates? This was addressed by including a question that asked participants to rate how they weighed covariation information for the scenario they had received.

A third purpose of the present experiment was to investigate the potential mechanisms whereby beliefs influence causal judgments. In other words, when a reasoner believes that an item in question has the causal power to produce an effect, what type of information informs that belief? We investigated two nonmutually exclusive possibilities. The first was directly related to the concept of generative transmission (Kant, 1781/1965; White, 1989) and concerned the extent to which the reasoner could identify a causal mechanism that linked the cause and effect. Based on this view, we predicted that the ability to identify such a mechanism would be related to both the believability of the cause-effect link, as well as the reasoner's causal likelihood judgment. The second possibility was derived from Cheng's power pc theory (1997), the successor to Cheng and Novick's (1990) probabilistic contrast model. It stipulates that causal power varies as a function of the presence of independently occurring alternative causes for a given effect. That is, Cheng (1997) argued that causal power could only be derived if sufficient information is available to separate the causal power of the candidate from that of alternative causes. Further, Cheng (1997) argued, "the contrast for a candidate cause with a given power is reduced, in the extreme to zero, when alternative independent causes of the same effect are present" (p. 382). According to this view, we predicted that the causal likelihood judgments would be inversely related to an individual's ability to generate alternative causes and the plausibility of those alternatives for the event in question. Therefore, one can think of causal power arising from at least two nonmutually exclusive qualities: (a) generative transmission (White, 1989), and (b) number and strength of alternative causes (Cheng, 1997).

METHOD

Participants. Three hundred and twenty first-year psychology students from the University of Saskatchewan with a mean age of 20.1 (range 17-52) completed the study in partial fulfilment of a course requirement.

Design. This was a 2 × 2 between-subjects design with two levels of the belief manipulation (low and high) and two levels of the covariation manipulation (ΔP = 0 and 1).

Materials. The items for this experiment consisted of the same four scenarios utilized in Experiment 2. Belief and covariation information were presented in the same manner as they were both in Experiment 1 and the "original" problem format in Experiment 2.

In addition to making causal likelihood judgments, participants were asked to make five additional judgments: (a) Participants were asked to evaluate how plausible a given candidate was as a cause of the effect. These plausibility estimates were recorded using a 9-point Likert scale which ranged from 1 (not plausible) to 9 (extremely plausible). (b) Participants were asked whether they could think of a causal mechanism which would enable the causal candidate to exert its influence to produce the effect. (c) Participants were asked to record any alternative causes which they felt could be responsible for the observed effect, and to rate the plausibility of those alternatives using the plausibility scale described in (a) above. (d) Participants were asked to indicate how heavily they weighed both the covariation information and the belief information using a 9-point Likert scale which ranged from 1 (not at all) to 9 (extremely heavily). To do this, participants were asked to consider both sources of information and determined how heavily they influenced their final causal judgment.

Procedure. The procedure was identical to that of Experiment 1 and Experiment 2 with the exception that believability and covariation were manipulated between-subjects. Each of the 320 participants randomly received one problem in total from one of the four belief × ΔP cells.

RESULTS AND DISCUSSION

Relationships between beliefs, and ⊥P. Figure 5 presents the mean causality judgments as a function of the two belief conditions (low belief and high belief), and the two ΔP conditions (0, and 1). The first series of analyses tested the relative differences in causal judgments as a function of belief and covariation. The mean likelihood ratings were analyzed using a 2 × 2 (belief × ΔP) between-subjects ANOVA. There were main effects of belief, $F(1,316) = 186.00$, $MSE = 3.77$, $\eta^2 = .37$; ΔP $F(1,316) = 82.28$, $MSE = 3.77$, $\eta^2 = .21$; and a

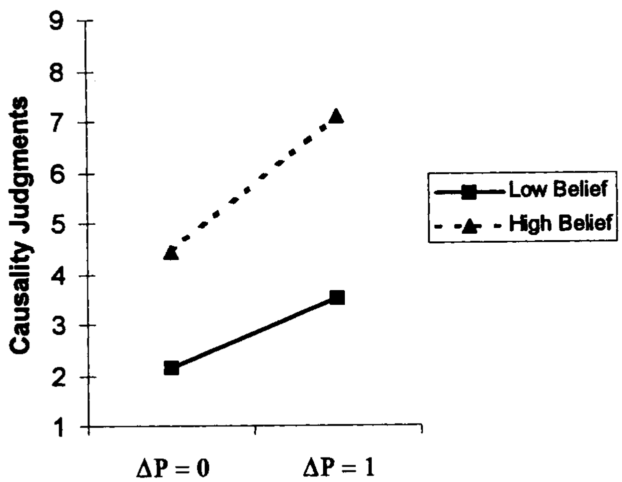

Figure 5. Mean causality judgments for the two belief conditions (low belief and high belief) and the two ΔP conditions (0, and 1) in Experiment 3.

belief × ΔP interaction $F(1,316) = 7.92$, $MSE = 3.77$, $\eta^2 = .02$, whereby the effects of covariation were larger in the high belief condition (M difference = 2.7) than in the low belief condition (M difference = 1.4).

These findings replicated those of Experiment 1 and Experiment 2 using a between-subject design. Specifically, a reliable over-additive interaction was discovered according to which the effects of covariation were larger when comparing the causality judgments given for the high-belief items than the low-belief items. Therefore, these data are most consistent with White's (1989) causal power theory if one relaxes the criterion of selectivity to allow covariation information to be weighed more heavily for, as opposed to used exclusively for, believable candidates.

Mechanism and/or alternatives. Several analyses explored the relationship between causal likelihood ratings, the believability of the cause-effect relationship, and the reasoner's ability to provide a mechanism whereby the cause could be linked to the effect. First, participants were more likely to generate a mechanism for scenarios categorized as believable (34%) than scenarios categorized as unbelievable (21%), $\chi^2 = 6.62$, $p = .01$.[4] Moreover, causal likelihood ratings were higher for candidates when a mechanism was reported ($M = 5.2$) than when no mechanism was reported ($M = 3.9$), $t(318) = 3.70$, $SE = .33$. These data are consistent with causal powers theory (White, 1989) and others that refer to the role of mechanisms in causal induction (e.g., Ahn et al, 1995; Shultz, 1982; Shultz et al., 1986; Waldmann, 1996). However, the presence of a mechanism per se only mattered when the candidate was an unbelievable cause, $t(159) = 3.37$, $SE = .36$; when the candidate was believable then judgments did not differ according to whether a mechanism could be generated or not, $t(157) = .92$, $SE = .41$.

For believable candidates, therefore, it appears that it is the belief that a cause and effect are related that is important for determining causal likelihood judgments. That is, so long as reasoners believe that the candidate has the power to produce the effect, they will judge it to be a likely cause of that effect, regardless of whether they are able to identify a specific mechanism by which the effect is produced. The ability to identify a mechanism, however, clearly plays a role in moderating the effects of disbelief. When the candidate is unbelievable, being able to ascertain a mechanism capable of transmitting the effect lent plausibility to the candidate.

The next set of analyses revealed that neither the strength nor the availability of alternative causes was related to either believability or likelihood. First, participants were no more likely to generate an alternative for unbelievable scenarios (20%) than believable scenarios (18%), $\chi^2 = .52$, $p = .47$. Moreover, causal likelihood ratings did not vary as a function of whether alternative causes were generated ($M = 4.2$) or not generated ($M = 4.3$), $t(318) = .58$, $SE = .31$. Further, for those participants who generated alternative causes, the number of alternatives generated (range = 1 to 4) was not related to whether the candidate was unbelievable ($M = 1.8$) or believable ($M = 2.2$), $t(116) = 2.04$, $SE = .17$, nor to the likelihood judgment the candidate received $r(116) = -.02$, $p = .80$. Finally, the combined plausibility ratings of the alternative causes did not vary as a function of whether the candidate was unbelievable ($M = 11.4$) or believable ($M = 12.5$), $t(115) = .93$, $SE = 1.18$, nor were these plausibility ratings related to likelihood judgments $r(115) = -.03$, $p = .73$. Thus, neither the presence, number, nor strength of alternative causes was related to a candidate's believability or causal likelihood judgments. These data are not consistent with a quantitative interpretation of causal power in which causal power is related to the presence of alternative causes (Cheng, 1997).

Subjective weighting of covariation and belief information. The final set of analyses assessed participants' responses to the covariation, and the belief-weighting question. Consistent with our previous observations regarding the interactive effects of beliefs and ΔP, individuals reported that they weighed covariation information more heavily when the candidate was highly believable ($M = 6.2$) than when it was of low belief ($M = 4.9$), $t(318) = 5.28$, $SE = .23$. Although none of the reviewed theories provided predictions regarding participants' metacognitive judgments, this pattern of data is most consistent with the behavioural outcomes predicted by White's (1989) causal power theory. In addition, individuals judged belief information to be more useful when the candidate was perfectly contingent ($M = 5.4$) than

[4] We originally intended to use plausibility as an indication of how individual participants judged believability. However, plausibility judgments were found not to be a pure measure of believability as they were also sensitive to the covariation manipulation.

when it was noncontingent ($M = 4.8$), $t(318) = 2.55$, $SE = .24$.

General Discussion

The goal of the present experiments was to investigate how individuals integrate covariation information with belief-based information when assessing causal hypotheses about known candidates. Consistent with previous work, it was found that causal judgments varied as a function of the degree of covariation (Cheng & Novick, 1990, 1992) as well as the believability (White, 1995) of the causal candidate. In addition, several novel findings were obtained.

COVARIATION AND BELIEFS: THEORETICAL VIEWS AND THE CURRENT DATA

All of these findings are broadly consistent with both mechanism-based and covariation-based approaches, and if this were the totality of our observations, it would be a simple matter to suggest how to incorporate the role of beliefs into covariation theories and vice versa. For example, the findings summarized above could easily be incorporated into a covariation-based theory by assuming that (a) information from other sources (such as beliefs) is combined additively with covariation information, and (b) as most theorists acknowledge (e.g., Cheng, 1997), covariation does not guarantee causation. This set of assumptions would accommodate the fact that belief-based cues contributed to causal judgments, as well as the observation that even high values of ΔP did not guarantee a causal attribution when the candidate was unbelievable.

However, it was also observed that the effect of covariation information was larger for believable than unbelievable candidates. Thus, our findings suggest these models must do more than just acknowledge the importance of belief-based information. Instead, they must specify how and when covariation-based cues are used, and how the use of these cues depends on other information that is available. Our data provide some initial constraints on such a theory-building process.

FOCAL SETS AND THE COMPUTATION OF COVARIATION

One explanation for the over-additive interaction between covariation and belief can be derived from Cheng and Novick's (1990) probabilistic contrast model. Cheng and Novick (1990) argued that participants compute covariation with respect to a target population of events, which they termed a "focal set." This target population may extend beyond the boundaries of the events defined by the experimenter in the problem scenario, and include extra information extracted from long-term memory by the participant. One reason, therefore, that causal judgments in the low belief/high covariation condition were low, is that participants may have redefined the relevant focal set to include instances where the effect failed to occur when the cause was present. When covariation is computed within this larger universe of events, it is possible that cause and effect no longer covary perfectly, and consequently, the cause may be downgraded from a plausible to an implausible candidate.

One problem with the focal set analysis is that it can be applied post hoc to account for almost any pattern of observations that is inconsistent with the covariation-based view. That is, whenever participants fail to behave in a manner predicted by the probabilistic contrast model (Cheng & Novick, 1990), a possible explanation might be that participants are using a different focal set than the one assumed by the researcher. One possible contribution of our findings, therefore, might be to delimit circumstances under which reasoners either expand their focal sets beyond the range of stimuli provided, or restrict themselves to the events discussed in the problem. Specifically, on the basis of our findings, one would predict that participants are more likely to consider an expanded focal set of events when dealing with unbelievable than with believable causes.

This hypothesis is also consistent with evidence that suggests that individuals use their knowledge to guide the selection of events to be used in the computation of contingency (Waldmann & Hagmayer, 1995). The perception of a causal mechanism is thought to guide the process of focal set selection such that only relevant variables are included in the focal set. A potential flaw in this explanation, however, is the failure to find a relationship between the number and strength of alternative causes and either the believability or judged likelihood of the cause. That is, when faced with an unbelievable causal cue (e.g., homework as a cause of allergic reactions), one should expand the focal set to include information about the number of times the cause is not related to the effect (e.g., where doing homework fails to produce an allergic reaction). However, it seems reasonable to assume that this expanded focal set would include candidates that are known to covary with the observed effect. Thus, one would expect that more alternative candidates would be derived for an effect when the given candidate is of low belief. However, the results of Experiments 3 demonstrated that this is not the case. Therefore, if unbelievable candidates cause reasoners to redefine their focal sets, they must be redefined in a very restricted manner to incorporate information only about the events under consideration.

CAUSAL POWERS THEORY

Causal powers theory (White, 1989) provides an elegant explanation for the overall pattern of findings. According to this view, covariation information is assessed in light of the reasoner's pre-existing beliefs about the candidates' capacity or power to produce the effect in question. These beliefs are assumed to restrict the set of candidates about which covariation information is considered, such that people should only seek out covariation information about believable and not unbelievable candidates. This reasoning implies that covariation information should be only relevant to considering causal hypotheses about believable candidates. Our findings were consistent with a relaxed version of this view: The effect of covariation information was larger when reasoners were assessing believable rather than unbelievable candidates. Moreover, reasoners indicated that they weighed covariation information more heavily when assessing believable rather than unbelievable candidates.

It should be noted, however, that although the interaction between beliefs and covariation was robust, effects of ΔP were still found for low-belief candidates. That is, although covariation information is weighed more heavily for high- than for low-belief items, people do not appear to disregard covariation information completely for low-belief items as suggested by White (1989). There are at least two possible mechanisms that might account for these findings.

One possibility is that individual reasoners weigh covariation information as a linear function of the believability of the candidate cause. Thus, as the belief in the candidate increases, the weight assigned to the covariation information increases also. Alternatively, it is possible that individual reasoners set a threshold for what constitutes a believable or unbelievable candidate: For items that exceed this threshold, covariation information will be used, but for items below the threshold, covariation information will be ignored. As the actual believability of the items increases, so does the probability that any given individual will employ covariation information. The number of individuals using covariation information should therefore increase as a function of the believability of the candidate.

INDIVIDUAL DIFFERENCES IN THE USE OF COVARIATION-BASED AND BELIEF-BASED CUES

Another challenge for current theories (e.g., Cheng, 1997; Cheng & Novick, 1990; White, 1989) is to account for individual differences in the use of covariation and belief information. The analysis of the data in Experiments 1 and 2 revealed that the obtained means did not reflect modal responses. Rather, it appears that our sample of participants is made up of at least two groups of individuals: believers and empiricists. These groups of individuals were observed to preferentially weight one causal cue at the expense of the other. Therefore, one must be cautious about proposing theories that suggest that effects of belief and covariation are combined at the level of the individual.

BELIEFS, ALTERNATIVES, AND MECHANISMS

In these experiments, we have examined how people's preexisting beliefs affect their judgments of causal likelihood. In Experiment 3 we also addressed the question of what those beliefs may constitute. The evidence obtained in Experiment 3 demonstrated that although mechanisms were important to both beliefs and causal judgments, the effects of belief were not reducible to people's understanding of a mechanism. These findings are consistent with White's (1995) view regarding the link among causal powers, mechanisms, and beliefs. White (1995) suggested that causal beliefs subsume the notion of causal mechanism, but also include other concepts such as causal power, releasing condition, and liability.

For example, I possess a causal belief about the relationship between antibiotics and recovering from bacterial infection, even though I would be hard put to specify a causal mechanism whereby antibiotics achieve this effect. Specifically, I believe that antibiotics have the causal power to kill infections under the enabling conditions that (a) the antibiotics are ingested at regular intervals, (b) the bacteria is not antibiotic resistant, and (c) it is a bacterial and not a viral infection. I also believe that bacteria have a liability to being affected by antibiotics. Thus, although I believe there is a mechanism to explain how antibiotics kill bacteria, my knowledge of biochemistry affords me only the fuzziest model of exactly how the cause is transmitted to the effect. In sum, it is possible that people's beliefs reflect their understanding of a set of causal roles and belief in a mechanism, rather than their ability to specify what that mechanism is.

An alternative interpretation of what constitutes a belief is that it may be nothing more than knowledge of covariation information (e.g., Cheng & Lien, 1995). In other words, a candidate is deemed believable or unbelievable based on the experience of past contingencies. Candidates that are seen as believable are those that are known to have a high contingency with the effect; candidates that are unbelievable are those that are known to show little regularity of association with the effect. For example, doing homework might be perceived as an unbelievable cause of allergies because, in the reasoner's experience, doing homework has not exhibited a regularity of association with allergies.

Although this is a possibility, and one that cannot be discounted on the basis of our data, there are nonetheless several reasons why it is unlikely that beliefs reflect only beliefs about covariation. First, it is clear that people hold causal beliefs in the absence of any kind of covariation information. That is, people are known to form a causal link based on the observation of only a single episode; these beliefs cannot be founded on covariation information because, in order to compute covariation, the reasoner must be exposed to multiple episodes, some of which include information about the effect in the absence of the cause.

In addition, people hold causal beliefs about things for which no covariation information could possibly be available. For example, many people erroneously believe there is a relationship between criminal behaviour and the lunar cycle, in defiance of the fact that these two events do not covary in the world. It is clear, however, that people who believe this relationship exists also believe that there is a correlation between the cause and effect. There are two possible explanations for this. First, the belief that two events are causally related produces the belief that they covary. In other words, the fact that one believes that a full moon causes criminal behaviour may lead people to believe that full moons and criminal behavior will covary to some extent. Moreover, when people believe that two events ought to covary, they may be biased to detect a covariation between them, even though none exists. This phenomenon is the well-known illusory correlation effect (Chapman & Chapman, 1980).

Finally, people have accurate causal beliefs about many phenomena with which they have not had prior experience and do not therefore possess the information needed to compute a covariation estimate. For example, I am quite certain that swallowing a cup full of thumb tacks will cause great distress to my intestinal system, even though I have had no experience, either direct or indirect, with thumb tacks in that capacity. Thus, my belief in the causal power of thumbtacks cannot be based on knowledge of a covariation relation; instead, it concerns my beliefs about the properties of thumbtacks (sharp) and intestinal systems (soft).

The finding that beliefs in causal power were not related to the number of alternative causes generated nor to the plausibility of those alternative causes poses further problems for covariation-based accounts of causal beliefs. These findings may be seen as quite surprising when one considers the abundance of empirical research which has demonstrated a reliable impact of alternative causes on people's reasoning judgments (e.g., Baker, Mercier, Vallee-Tourangeau, Frank, & Pan, 1993; Chapman & Robbins, 1990; Cummins, Lubart, Alksnis, and Rist, 1991; Dickinson, Shanks, & Evenden, 1984; Shanks, 1986; Wasserman, 1990). In such studies it is typically observed that one's reasoning judgments vary systematically as a function of the presence or absence of alternative causes. There are at least two explanations that can potentially account for this discrepancy. First, causal judgments and the believability of the candidate may be independently influenced by perceived alternative causes. That is, alternative causes can influence causal judgments without affecting the believability of the causal candidates. For example, it could be argued that both smoking cigarettes and ingesting asbestos fumes are believable causes of lung cancer. The fact that lung cancer can be caused by many factors does not diminish the believability of either smoking or asbestos as probable causes. In such cases one would not expect the number or plausibility of alternative causes to affect the believability of such candidates[5].

Secondly, it is possible that people do not have an accurate memory-based representation of alternative causes. That is, causal beliefs may not be partially composed of stored representations of alternative causes. Therefore, it is only when such alternatives are made explicit to the reasoner that they are utilized to inform judgments of causality. This possibility needs to be qualified by the lack of an observed effect of generated alternative causes and causal judgments in the present study. Perhaps the number of alternatives generated by participants is not an accurate reflection of the number of known alternative causes. These possibilities need to be empirically dissociated in future studies before any firm conclusions are drawn about the relationship between the nature of causal beliefs and knowledge of alternative causes.

Conclusions

The present series of experiments has demonstrated that the use of covariation-based information is dependent on the knowledge one brings to the causal situation. When assessing causal hypotheses about known objects, individuals appeared to make use of empirical cues primarily under conditions in which they hold a priori beliefs of causal candidacy. Although this strategy may not be normatively correct in the strict sense, it is nonetheless practical. That is, when faced with an infinite number of potential causal candidates for every given effect encountered in the natural environment, it makes sense to restrict the set of candidates about which covariation information is assessed. Therefore, the practical strategy then is to import conceptual knowledge concerning the causal candidate in question to determine its plausibility before the decision is made to make use of any empirical cues.

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Appendix
Causal Scenarios Used in the Current Experiments

The six scenarios used are presented below. Note that in the experiments, all scenarios were presented equally often using all the covariation contingencies; however, only the perfectly contingent (ΔP_i = 1) versions are illustrated here. All six scenarios were used in Experiment 1. Only the depleted fish, car start failures, car accident, and allergic reaction scenarios were used in Experiment 2 and 3.

Depleted Fish
Imagine you are a biologist who is trying to determine the cause of a recent decrease in the population of fish in Canada's lakes. You have a hypothesis that the decrease in fish may be due to the recent introduction of insecticides designed to halt the birth of mosquito larvae. In order to test this theory, you decide to investigate 10 lakes with insecticides and 10 lakes without insecticides. A thorough investigation revealed the following information: of the 10 lakes that contained insecticides, 10 had depleted fish populations; of the 10 lakes that did not contain insecticides, 0 had depleted fish populations.

Productivity Slowdown
Imagine you are a CEO for a major car manufacturing plant who is trying to determine the cause of a recent decrease in productivity in your factories' output. You have a hypothesis that the decrease in productivity could be due to a recent illness of the factory employees. To test this theory, you examine 10 factories that had ill employees, and 10 factories that did not have ill employees. A thorough investigation revealed the following information: of the 10 factories that had ill employees, 10 were experiencing declines in productivity; of the 10 factories which did not have ill employees, 0 were experiencing declines in productivity.

Disease Epidemic
Imagine you are a virologist who is trying to determine the cause of a recent viral disease outbreak in small African villages. You have a hypothesis that the recent outbreak may be due to the tribes-people having green eyes. To test this theory, you examine 10 villages whose tribes-people had green eyes and 10 villages whose tribes-people did not have green eyes. A thorough investigation revealed the following information: of the 10 villages with green-eyed tribes-people, 10 were experiencing the viral disease outbreak; of the 10 villages without green-eyed tribes-people, 0 were experiencing the viral disease outbreak.

Car Start Failures
Imagine you are a tow-truck driver who is trying to determine the cause of a recent surge in car start failures. You have a hypothesis that the car start failures may be due to flat tires. To test this theory, you examine 10 cars that had flat tires and 10 cars that did not have flat tires. A thorough investigation revealed the following information: of the 10 cars that had flat tires, 10 failed to start; of the 10 cars that did not have flat tires, 0 failed start.

Car Accidents
Imagine you are a police officer who is trying to determine the cause of a recent surge in accidents on the Trans-Canada Highway. You have a hypothesis that the accidents may be due to severed brake lines. To test this theory, you examine 10 cars that had severed brake lines and 10 cars that did not have severed brake lines. A thorough investigation revealed the following information: of the 10 cars that had severed brake lines, 10 were involved in an accident; of the 10 cars that did not have severed brake lines, 0 were involved in an accident.

Allergic Reaction
Imagine you are a doctor who is trying to determine the cause of a recent surge of allergic reactions in children. You have a hypothesis that the allergic reactions may be due to doing homework. To test this theory, you examine 10 children who were doing homework prior to admission and 10 children who

were not doing homework prior to admission. A thorough investigation revealed the following information: of the 10 children who were doing homework prior to admission, 10 were displaying signs of an allergic reaction; of the 10 children who were not doing homework prior to admission, 0 were displaying signs of an allergic reaction.

Sommaire

Des études antérieures sur les jugements applicables à la causalité humaine ont examiné, de façon indépendante, l'influence de l'information sur la covariation (Cheng et Novick, 1990) et les croyances préexistantes (White, 1995). Dans l'actuelle série d'expériences, nous avons examiné comment les gens combinent ces deux sources d'information en évaluant les hypothèses causales. Dans l'expérience 1, les participants devaient poser un jugement causal selon les scénarios décrivant un résultat et une cause présumée, vraisemblable ou non, et variait selon l'intensité de sa covariation avec l'effet (ΔP = 0, 0,5 et 1). Deux principales conclusions sont apparues. D'abord, malgré la présence d'effets à la fois attribuables aux croyances et à la covariation, on a constaté une importante interaction de sorte que les effets de la covariation étaient plus prononcés pendant l'évaluation des causes hypothétiques vraisemblables que pour les causes invraisemblables. En second lieu, on a découvert des différences individuelles, révélant que les personnes tendant à baser leur jugement de causalité surtout sur les croyances utilisaient moins l'information sur la covariation, et vice versa. La deuxième expérience portait sur ces différences individuelles en manipulant l'ordre dans lequel les participants recevaient les signaux pertinents aux causes. En se basant sur les principes de la théorie des pouvoirs de causalité (1989), on a émis l'hypothèse que les personnes devraient être plus susceptibles d'intégrer les deux signaux quand l'information sur les croyances est présentée avant celle qui est liée à la covariation. Pour tester cette hypothèse, on a demandé aux participants (a) de poser un jugement de causalité après avoir reçu les deux types d'information, (b) de poser un jugement de causalité après n'avoir reçu que l'information sur les croyances puis un autre après avoir reçu celle sur la covariation, ou (c) de se prononcer après avoir reçu seulement l'information sur la covariation, puis une autre fois après avoir reçu l'information sur les croyances. Les résultats ont montré que les différences précédemment observées dans les signaux semblaient attribuables au format de la présentation de ceux-ci: l'échange systématique de signaux était nettement réduit quand on demandait aux participants d'évaluer la vraisemblabilité des causes initialement seulement selon leurs croyances, et ensuite après avoir reçu l'information sur la covariation. Si, d'autre part, les participants posaient un jugement initial de causalité en se fondant à la fois sur les croyances et la covariation, ou se prononçaient d'abord selon cette dernière puis une autre fois après des signaux basés sur les croyances, des échanges systématiques recommençaient à se manifester. Une troisième expérience a été conçue pour examiner davantage les mécanismes potentiels selon lesquels les croyances influencent les jugements de causalité. Autrement dit, quand un raisonneur croit une cause assez vraisemblable pour produire un effet donné, sur quel type d'information fonde-t-il son jugement? Nous avons examiné deux possibilités non mutuellement exclusives. La première était directement liée au concept de transmission générative (Kant 1781/1965; White 1989) et visait à déterminer à quel point le raisonneur pouvait, par un mécanisme de causalité, relier une cause à un effet. La deuxième possibilité découlait de la théorie « power pc » de Cheng (1997), qui stipule que le pouvoir causal varie en fonction de la présence d'autres causes indépendantes pour un effet donné. Les résultats ont révélé que c'est la croyance en un mécanisme causal, et non celle en d'éventuelles autres causes, qui influe sur la vraisemblance de causes déterminées. La troisième expérience a aussi démontré que l'interaction entre les croyances et la covariation se reflétait dans les jugements métacognitifs des participants, lesquels ont indiqué accorder une plus grande pondération à l'information sur la covariation pour les causes vraisemblables que pour les invraisemblables. Ces conclusions nous semblent plus appropriées à la théorie du pouvoir causal de White (1989), qui stipule que l'information sur la covariation est examinée sous l'angle des croyances préexistantes.