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Strategies for Strengthening Causal Inferences in Cross Cultural Research The Consilience Approach

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ABSTRACT True experiments cannot be conducted in cross cultural research because it is impossible to assign participants to different cultures randomly. Cross cultural studies are therefore regarded as quasi-experimental research, and threats that jeopardize the validity of causal inferences in cross cultural research are reviewed. Borrowing from evolutionary biology and epidemiology, the consilience approach is advocated for strengthening the validity of cross cultural causal inferences. This approach holds that causal inferences in cross cultural research are most convincing when supported by diverse evidence based on a sound theoretical basis, multiple sources of data, different research methods, and explicit refutation of alternative interpretations. Three broad strategies for strengthening cross cultural causal inferences are proposed under the consilience framework, including the systematic contrast of cultural groups, the inclusion of covariates to rule out alternative explanations, and the use of multiple research methods, such as cross cultural experimentation. Future developments of cross cultural research methods are discussed.

KEY WORDS • causal inferences • consilience • cross cultural studies • culture • research methodology

As globalization and multiculturalism now epitomize our *Zeitgeist*, cross cultural research, once a peripheral area, is now prominent in most subfields of psychology (e.g. for recent reviews, see Greenfield et al., 2003; Lehman et al., 2004) and organization and manage-

ment studies (Smith, 2001; Triandis, 2001). Several decades of research have documented a myriad of cultural differences across diverse cultural and ethnic groups, but unambiguous explanations of these differences prove to be elusive and controversial,

making cross cultural psychology and organizational studies difficult areas for developing and testing causal theories (e.g. Gelfand et al., 2002; Van de Vijver and Leung, 1997).

Two major reasons may explain why the interpretation of cultural differences is so challenging. First, culture is a fuzzy concept that includes many facets. Campbell (1986) uses the term 'molar' to describe a complex treatment, and culture is probably the most 'molar' treatment that one can imagine! The complex nature of culture makes it difficult to delineate its causal role (Brockner, 2003; van de Vijver and Leung, 1997). Second, true experiments, the most rigorous way to test causal claims, are impossible in cross cultural research because we simply cannot assign people randomly to different cultural groups.

This article first examines the major threats to causal inferences in cross cultural research. A framework for tackling these threats, the consilience approach, is then proposed. Finally, we review a wide range of research strategies under the consilience approach that can be deployed to strengthen causal inferences in cross cultural research. We note that we primarily rely on the literature in cross cultural psychology in our analysis because of its long tradition, but our arguments are applicable to much of cross cultural management research. We return to this issue in the conclusion.

Culture as a Cause

Culture and Causal Relationships

Causal inferences are prominent only in some approaches to the study of culture, notably cross cultural psychology (Greenfield, 2000). In contrast, cultural psychology views culture as an inseparable, holistic construct. Proponents of cultural psychology de-emphasize causality and prediction, and focus on explicating the underlying meaning of cultural phenomena (e.g. Greenfield, 2000; Shweder and Sullivan, 1993).

Our focus is the causal approach, because

this is the dominant approach in psychology and management. To delineate the role of culture as a causal agent, we need to explicate (1) what a causal relationship is, (2) what culture is, and (3) in what way culture is a cause. The concept of causality is complex and involves multiple meanings (for a review from a psychological perspective, see Cook and Campbell, 1979, Ch. 1). For our purposes, we follow the definition of Shadish et al. (2002), which is based on John Stuart Mill: 'a causal relationship exists if (1) the cause preceded the effect, (2) the cause was related to the effect, and (3) we can find no plausible alternative explanation for the effect other than the cause' (p. 6).

We now turn to the second question of what culture is. Over 50 years ago, Kroeber and Kluckhohn (1952) offered the following definition: 'Culture consists of patterns, explicit and implicit, of and for behavior acquired and transmitted by symbols, constituting the distinctive achievements of human groups, including their embodiment in artifacts' (p. 181).

Contemporary definitions of culture tend to be less encompassing. Triandis (1972) distinguishes between physical elements of culture, such as buildings and transportation networks, and subjective elements, such as values and norms. In cultural psychology, more emphasis is placed on culture as the interpretation of meanings, which can be traced to Geertz (1973), who views culture as 'an historically transmitted pattern of meanings in symbols' (p. 89). After a thorough review, Smith and Bond (1998) come up with a broad definition: 'a culture is a relatively organized system of shared meanings' (p. 39). For our purposes, we mainly focus on subjective elements of culture, such as values, beliefs, attitudes, norms, roles, affects, cognitions, meanings, and mental processes.

Finally, conceptualizing culture as a causal agent is too broad and uninformative. It is widely agreed that culture needs to be 'unpacked' into specific elements, which are

then used to explain observed cultural differences (Matsumoto and Yoo, 2006; Whiting, 1976). In this approach, a specific cultural attribute is regarded as the cause of an observed cultural difference, and such a relationship is falsifiable and not tautological. A good example is given by Nisbett and his colleagues, who argue that specific differences in cognitive styles between East Asians and European Americans are responsible for diverse cultural differences in cognitive processes (Nisbett, 2003). For instance, Koreans displayed less surprise and more hindsight bias than European Americans, which is consistent with the holistic reasoning style of Koreans (Choi and Nisbett, 2000).

We should note that our focus on unpacking culture does not mean the denigration of broad cultural dispositions, such as individualism–collectivism. We favor causal explanations that are specific and can elucidate the psychological processes underlying observed cultural differences. A broad construct such as individualism–collectivism may function as a distal variable in a broad theoretical framework, whose influence on psychological outcomes are through more proximal, specific variables.

Ascertaining the Causal Role of Culture

The first requirement of Shadish et al. (2002) for demonstrating a causal relationship is that culture as a cause should precede an effect. One may argue, as many cultural psychologists would, that because people are immersed in culture and define the culture that they collectively share, culture cannot *precede* an effect. This challenge is overcome by a broadened view of causality. In a review of different philosophical stances on causality, Cook and Campbell (1979: 35–6) conclude that some causes have instantaneous effects. Thus an important way to demonstrate causality is to show that a change in one variable leads to a corresponding change in another. Cultures do change (e.g. Ingle-

hart and Baker, 2000), and Cook and Campbell's logic suggests that an important way to ascertain the causal role of culture is to assess the effects of culture change. Weber's well-known assertion that Protestantism led to the rise of capitalism is an obvious example of the application of this logic. We also note that the requirement that causes precede effects is not fulfilled in many other areas of research. For instance, applications of 'causal modeling' techniques, such as path analysis, typically involve data collected concurrently and are rarely based on longitudinal data. This type of causal modeling amounts to no more than fitting a hypothesized set of relationships to a correlational data set.

The second requirement, that a cause is related to an effect, is relatively easy to demonstrate. It is the third requirement that is challenging; namely, that no other alternative explanation can explain the effect. Typically, causes in physical sciences are sufficient to explain an effect (e.g. heating provides a necessary and sufficient condition for boiling). However, culture is likely to be one of many causes of an effect, and as will be discussed later, the ruling out of rival hypotheses is indeed a daunting task in cross cultural research. The ascription of a causal role to a specific cultural element has to wrestle with a wide range of validity threats, which is the topic of the next section.

Cross Cultural Research as Quasi-experiments

The impossibility of true experiments is not unique to cross cultural research (e.g. see Rutter et al., 2001, for similar problems in psychotherapy research). Even in medical research, the lack of experimental evidence explains why it took decades to ascertain the cause of some diseases. For instance, peptic ulcers were long regarded as being caused by excessive acid, but Marshall and Warren (1984) found a strong association between the presence of a strain of bacteria and the

occurrence of peptic ulcers. After more than a decade of mostly non-experimental research, the causal role of this strain of bacteria was confirmed, which has revolutionized ulcer treatment and won the authors a Nobel Prize.

A wide range of methodological and statistical strategies have been developed for causal inferences when experimentation is not feasible. In perhaps the most influential work of this tradition, Campbell and Stanley (1963) present an in-depth analysis of drawing causal inferences from quasi-experiments, in which participants are not randomly assigned to different experimental conditions. Cross cultural studies may be regarded as quasi-experiments, because cultural membership cannot be randomly assigned.

Cross cultural studies face additional problems that rarely occur in monocultural studies with a similar design. The core of these problems is bias, a generic term for any systematic source of distortion that challenges the validity of cross cultural comparisons (van de Vijver and Leung, 1997). Bias can arise from different sources. A construct may be conceptualized differently across cultures, resulting in *construct bias*. For instance, depression is associated with somatic complaints in all cultures, but with psychological complaints only in some (van de Vijver and Tanaka-Matsumi, 2008). As another example, continuance commitment in western models focuses on the alleged costs associated with leaving or altering one's involvement with an organization, implying a perceived need to stay. Wasti (2002; and see van de Vijver and Fischer, 2008) argues that such a definition for continuance commitment is too narrow in a Turkish context. In more collectivistic contexts, loyalty and trust are important and strongly associated with paternalistic management practices. Therefore, employers are more likely to give jobs to trusted family members or friends, involving these individuals into relationships of dependency and obligation. This practice, in turn,

leads to efforts on the part of the recipients to maintain 'face' and credibility, and attempts to return the favor. These normative pressures therefore become part of continuance commitment, involving both financial and rational considerations (such as investments and benefits as found in western contexts) as well as social costs (loss of face and credibility).

The methods used may be not be equivalent across cultures, a problem known as *method bias*. For instance, cross cultural differences may be influenced by social desirability (van Hemert et al., 2002). A final source of bias may reside in the measurement tools on which cultural differences are based, and is known as *item bias* or *differential item functioning* (van de Vijver and Leung, 1997). These problems may arise from the translation or adaptation of a measurement instrument for application in a foreign culture. The various sources of bias and non-equivalence are discussed in detail in the next section on validity threats.

Threats to Causal Inferences in Cross Cultural Research

In their classic analysis of quasi-experimentation, Cook and Campbell (1979) provide a list of validity threats and the correspondent strategies to alleviate them. An update is provided by Shadish et al. (2002), but we do not repeat their advice here. Instead, we focus on validity threats that are common, if not unique, in cross culture research. Because our focus is on causal inferences, we do not discuss issues associated with external validity.

Threats to Statistical Conclusion Validity

Inadequate statistical testing When means from two cultures are compared, statistical tests are always performed. However, when internal consistencies, correlations and regression weights are compared across cultures, there is a tendency to rely on

visual inspection, but not formal tests, of cross cultural differences. Similarly, it is common to report a cultural difference if a correlation or beta weight is significant in one culture but not in the other. However, two correlations or beta weights may be statistically similar, even if one is significant and the other is not.

Inadequate testing for structural equivalence

It is routine to assess the adequacy of measures used for each culture with regard to their internal consistencies, but it is less common to assess whether an instrument measures the same psychological construct across cultures (structural equivalence; see van de Vijver and Leung, 1997). Internal consistencies are inadequate markers of construct equivalence, and more complex statistical techniques, such as exploratory and confirmatory factor analyses, provide stronger evidence for cultural invariance of constructs.

Inadequate testing for scalar equivalence

Measures from different cultures are directly comparable only if they show scalar, or true score, equivalence. The so-called 'nonarbitrary metrics' (Blanton and Jaccard, 2006), measures that are absolute and likely to show scalar equivalence across cultures, are rare in psychology. It is regrettable that researchers often take observed cultural differences as real, without any attempt to ascertain scalar equivalence. Unlike natural sciences, even if a cross cultural study employs measures based on interval scales, such as money, scalar equivalence is not guaranteed. For instance, an identical sum of money may have different meanings in different societies, depending on the affluence of a society. Few would agree that an American taxi driver who donates 100 dollars is more charitable than a taxi driver in India who donates 80 dollars.

We should point out that cultural invariance in factor structures does not constitute evidence for scalar equivalence. One can add

a constant to the data of one cultural group and create massive cultural differences, but it does not affect factor similarity across cultures.

Confirmatory factor analysis and item response theory are able to provide statistical justification for scalar equivalence (van de Vijver, 2002; van de Vijver and Leung, 1997).

Threats to Internal Validity

Selection Selection is often used to describe the situation in which a difference between two groups is caused by some other systematic differences between the groups, but not by the difference in the experimental treatment received (Larzelere et al., 2004; Rosenbaum, 2002; Shadish et al., 2002). In a cross cultural study, participants may differ in many aspects other than the specific aspect hypothesized to be the cause of a cultural difference, and the potential effects of these other aspects need to be ruled out. For instance, Leung et al. (1998) found that an authoritarian parenting style was related to children's academic performance positively in Hong Kong, but negatively in Australia and the USA. However, it turned out that parental education was much lower in Hong Kong than in the two English-speaking countries, and that parental authoritarianism also showed a positive relationship with children's academic performance for Australian and American parents with lower education. Thus the cultural difference observed is explainable by differences in parental education across cultures instead of cultural differences in values.

In general, random samples from different cultures may lead to incomparable samples because cultures may show drastic differences in some demographic variables, such as education level. Samples from different cultures that are matched on a set of demographic variables may create equivalent samples, but these matched samples may not be representative of their cultural groups. Internal validity is then achieved at the

expense of external validity. If the demographic profiles of the cultures in a cross cultural study are not too dissimilar, the distinction between matched and random samples is not crucial and could be overcome by statistical adjustment, such as by assigning different weights to respondents with different demographic profiles.

We also note that the use of convenience samples is not ideal because it is difficult to assess the generalization of the results based on convenience samples to the larger population. As is discussed later, the measurement of potential confounding characteristics can help reduce the problems of convenience sampling by a statistical evaluation of their influence. In general, a rigorous consideration of sampling issues increases our sensitivity to cultural differences in background variables and their potential confounding effects (Betancourt and Lopez, 1993).

Cultural differences in response style

Different cultural groups may show different response styles in answering a questionnaire. For instance, Hui and Triandis (1989) found that in the USA, Hispanics showed a stronger tendency to choose extreme responses than did Caucasian Americans. Cultural differences may arise from different response styles rather than from differences in a specific cultural element. Some recent research has shown that some response styles are related to some cultural characteristics systematically. Smith (2004) showed with seven large-scale multicultural data sets that power distance is positively associated with the acquiescence tendency across cultures. Van Hemert et al. (2002) found a strong negative relationship between the affluence level of a country and its score on social desirability. These studies show that the response to a questionnaire item may capture more than the reaction to the content of the item. When cultural groups are compared, cultural differences in response styles must be taken into account.

Threats to Construct Validity

Non-equivalent construct definition

Constructs may be conceptualized differently across cultures, and comparing non-equivalent constructs is misleading. For instance, lay conceptions of intelligence vary drastically across cultures, especially social aspects of intelligence (e.g. Sternberg, 2004). A good example is that obedience may be regarded as part of intelligence in Africa (Serpell, 1993), but not in the West. A valid comparison of intelligence across cultures must be based on an equivalent definition of intelligence across the cultures concerned.

Non-equivalent operational definition

A construct may be defined similarly across cultures, but its operational definition may show cultural differences. For instance, interrupting someone in a conversation typically conveys rudeness in the USA, but 'conversational overlaps' – talking while the other person is talking – are common in Brazil (Graham, 1985). The use of interruption as an operationalization of rudeness is likely to be culturally non-equivalent across Brazil and the USA. Interruption probably conveys much less rudeness in Brazil than in the USA, and the use of this non-equivalent operationalization will lead to misleading results.

Cross-level issues Cross cultural studies often employ both individual- and culture-level concepts. Individual-level constructs need no explanation, and country-level constructs may be based on country-level indicators, such as GNP, or on aggregated individual-level characteristics, such as country mean scores on personality dimensions (McCrae et al., 2005). There is no necessary connection between these two levels, and at least two issues threaten the validity of the use of individual- and country-level variables in cross cultural research. The first threat is the indiscriminate application of cross-level aggrega-

tion, such as the application of a country characteristic to all individuals in that country (the ecological fallacy). For instance, a collectivist country has a sizeable number of individualists, and it is inaccurate to assume that all members are equally collectivistic. The second threat involves a possible shift of meaning after (dis)aggregation of individual-level measures. Hofstede (1980) provides a lucid explanation for why the aggregation of individual-level measures to form a country-level construct may shift its meaning, and vice versa. Standard statistical procedures are now available for examining multilevel structural equivalence (Muthén, 1994; van de Vijver and Poortinga, 2002).

Item non-equivalence Translation problems may cause erroneous interpretations of cultural differences. A classic example of this problem is the claim made by Bloom (1981) that Chinese are less likely to engage in counterfactual reasoning because a distinctive counterfactual marker is absent in the Chinese language. Bloom's claim was refuted by subsequent evidence showing that counterfactual reasoning does not depend on a distinctive counterfactual marker in the Chinese language. His results are partly a product of sub-optimal translation of the English materials into Chinese (Au, 1983; Liu, 1985).

Even if a translation is accurate, different shades in meaning may lead to unexpected differences; Hambleton (1994) provides an interesting illustration of this problem. An item assessing educational achievement asks children to choose from a list of places where a bird that has webbed feet lives. This item is straightforward in English and the correct answer is in the sea. However, the item is problematic if used in Sweden because the translation of webbed feet in Swedish is 'swimming feet', which makes it easy to identify the correct answer.

Differential familiarity with research materials and settings The materials and research settings used may vary unintentionally across cultures despite a conscious effort to avoid such variations. A classic example is provided by Serpell (1979), who found that British children were better at reproducing a pattern by drawing than were Zambian children. One may be tempted to conclude that the British children were better at recognizing and reproducing a pattern. However, when iron wire was used for reproducing the pattern, Zambian children outperformed the British children. A plausible explanation is that British children were more familiar with drawing, whereas Zambian children were more familiar with the use of iron wire, which in fact was a popular pastime for them. Thus cultural differences may be produced by differential familiarity with research materials across cultures.

Reactivity to the research setting Participants may react to the research setting based on their interpretation of the situation. Rosenzweig (1933) suggests that participants may behave in a way so as to provide what they think the experimenter expects of them. The clues that convey the experimenter's expectations are labeled as 'demand characteristics', which may vary across cultures. An observed cultural difference may reflect cultural differences in demand characteristics rather than cultural differences in a specific cause.

Experimenter expectancies It is well known that experimenters may unintentionally exert influence on participants to obtain the empirical results they anticipate, a phenomenon known as the *experimenter expectancy effect* (Rosenthal, 1968). In a cross cultural study, an observed cultural difference may be caused by differences in experimenter expectancy effect across cultures. The study by Smith (2004), which showed that power distance is related to acquiescence across cul-

tures, suggests that participants from high power distance cultures may be more susceptible to the influence of experimenter expectancies.

The Consilience Approach

Insights from Evolutionary Biology and Epidemiological Research

A wide range of threats that may compromise causal inferences in cross cultural research are reviewed above. Cross cultural psychology is not the only discipline that lacks experimental evidence, and it is instructive to consider how other fields grapple with causal inferences in the absence of experimental data. An obvious discipline to draw insight from is Darwin's evolution theory, which is not amenable to experimental evaluation. In evolutionary psychology, Caporael (2001) notes that researchers generally follow the methods of evolutionary biologists, namely, William Whewell's consilience of inductions (Ruse, 1989). In essence, while no single piece of evidence can prove natural selection, evidence from diverse sources provides the basis for formulating consilience arguments that are hard to dismiss. Indeed, evolutionary biologists have amassed a wide range of evidence to support the causal role of natural selection and rule out many competing explanations.

Epidemiological research is another discipline in which experimentation is infrequent because of ethical considerations. Researchers cannot simply assign patients randomly to treatment and control groups. Larzelere et al. (2004) describe four criteria that epidemiologists rely on to draw causal inferences from non-experimental data (see also Rothman and Greenland, 1998). The first two criteria are less emphasized by evolutionary biologists. First, the strength of an association between a cause and an effect can be used to rule out plausible alternatives that are unable to give rise to an association of a similar

magnitude. In fact, the emphasis on effect size by epidemiological researchers perhaps explains why they often conduct sensitivity analysis to assess the potential effect of hidden bias, a topic that is explained in more detail below. Interestingly, effect size arguments have rarely been used in psychological and management research to rule out alternative explanations, although some journals now require information on effect sizes.

Second, causes should precede effects, and this temporal sequence can be used to rule out some alternative explanations. Epidemiological researchers take great pains to examine the temporal sequence of causes and effects, while most cross cultural research is based on concurrent effects of culture on its consequences. In an earlier section, we discussed the importance of studying culture change as a way to affirm a causal theory. Longitudinal studies that track cultural changes over a relatively long period of time can address the temporal relationships between causes and effects. A good example is the longitudinal survey of values across many societies orchestrated by Inglehart and his associates. Change in a specific value over time can be related to change in a target variable, thus supporting the causal role of the value (e.g. Granato et al., 1996; Inglehart and Baker, 2000).

The last two criteria are in line with the consilience approach of evolutionary biologists. The third criterion is consistence, which refers to a replicable effect across different populations and in different circumstances. The fourth criterion is coherence, which refers to the absence of conflicting evidence for an asserted causal relationship.

The Consilience Approach for Psychological Research on Culture

We have reviewed how evolutionary biologists and epidemiologists cope with the challenge of drawing causal inferences in the absence of experimental evidence. Based on

a synthesis of their approaches with established cross cultural research methods (e.g. Gelfand et al., 2002; Matsumoto and Yoo, 2006; van de Vijver and Leung, 1997), we develop the consilience framework for substantiating causal inferences in cross cultural research. Our consilience framework borrows heavily from the notion of consilience in evolutionary biology, but some features are unique because human groups, not flora and fauna, are being studied in cross cultural psychology and organization studies. To meet the requirements for establishing a causal relationship, we distinguish four kinds of consilience. First, *contextual consilience* requires that diverse evidence is collected from a wide range of cultural contexts and cultural groups. The convergence of results obtained in diverse cultural settings provides a powerful way to substantiate the relationship between a cause and an effect.

Second, *methodological consilience* requires the demonstration of a causal relationship with diverse methods, such as surveys, experimentation, and longitudinal studies. Methods that demonstrate the temporal relationships between causes and effects are especially valuable. This notion is consistent with the practice of multiple operationalism or triangulation; that is, the verification of a finding with different methods (Crano and Brewer, 2002: 10–11). These first two aspects of consilience provide support for the first and second requirements of a causal relationship; namely, that a cause is related to and precedes an effect.

Third, the notion of *predictive consilience* resembles the dominant strategy that evolutionary biologists use to substantiate evolutionary theory. Diverse predictions based on a causal theory are evaluated, and the confirmation of these predictions provides strong evidence for this theory. We note that in natural sciences, causal inferences are typically based on complex and detailed theories that yield precise and in some cases counter-intuitive predictions. If these com-

plex predictions are confirmed, it is hard to generate alternative explanations for the results. For instance, if birds are indeed direct descendents of dinosaurs, an intermediate creature between birds and dinosaurs should have existed in a specific time period with specific features resembling both birds and dinosaurs.

In cross cultural research, however, theories are less precise, and complex patterns are rarely hypothesized. Causal claims are usually susceptible to many alternative explanations even if the predictions are borne out. The attainment of predictive consilience hinges on the development of sophisticated theories, and on the derivation of diverse but precise and complex predictions from them. While such theories are rare in cross cultural psychological and organization studies, a recent attempt by van de Vliert and his colleagues to develop a climatic theory of social behavior provides a good illustration. As an application of their theorizing, van de Vliert et al. (2004a) argue that climate and wealth interact to affect cooperative behavior. In wealthy societies, altruistic behaviors involve less self-sacrifice and may be viewed as a form of self-identity, and hence self-serving motivation should be related to altruistic behavior positively. In poor societies, however, altruistic behaviors are more taxing on the individuals, and self-serving motivation should be related to altruistic behaviors negatively. They further argue that a difficult environment, represented by an uncomfortably hot or cold climate, would accentuate the effects of wealth on the relationship between self-serving motivation and altruistic motivation. The complex three-way interaction between wealth, climate and self-serving motivation was borne out in a country-level analysis of secondary data.

Van de Vliert and his colleagues have actually made a number of complex predictions of climatic effects, which are generally supported (e.g. van de Vliert et al., 2004b). It is not our intention to evaluate their provoca-

tive theorizing; we want to point out that because of the complex nature of their theorizing and predictions, they can be easily falsified. However, if van de Vliert and his colleagues are able to amass supportive evidence from diverse social behaviors, it is hard to dismiss their theorizing.

Finally, *exclusive consilience* requires that no alternative explanation is able to explain the evidence for a given causal explanation. A working assumption underlying exclusive consilience is that we may take a causal relationship as valid, but a wide range of alternative explanations should be evaluated. The emergence of conflicting evidence will lead to the revision of the causal relationship. This refinement process is similar to the view of Popper (1959) that science progresses through a falsification process.

The highest level of consilience is achieved if all four kinds are substantiated, which requires extensive evidence from diverse sources. In cross cultural research, no area comes close to the depth and breadth of empirical evidence in support of evolution theory. Perhaps the extensive research on individualism–collectivism (IND–COL) provides a case in which considerable consilience for its causal role has been achieved. After an extensive literature review, Oyserman et al. (2002) conclude that, ‘IND and COL do influence basic psychological processes. However, the empirical basis for this conclusion is not as firm as might be desired’ (p. 43). In their view, despite the voluminous literature on IND–COL, many empirical gaps exist, making it hard to be definite in some critical and controversial issues. In its current state of development, the literature on IND–COL leans towards the gathering of confirmatory evidence, and an explicit focus on exclusive consilience would help settle many controversial issues.

Research Strategies for the Consilience Approach

Under the consilience framework, various research strategies can be grouped into three broad categories for bolstering the validity of causal inferences in cross cultural studies (see Table 1 for a schematic presentation). These strategies may be regarded as the ‘translation’ of consilience into research practices. An optimal choice of strategy has to be based on an analysis of a particular cross cultural difference and the confounding factors that may jeopardize the causal inferences involved. The first category, *systematic contrast strategies*, primarily aims at contextual and predictive consilience by a strategic choice of diverse cultural contexts. The second category, *covariate strategies*, primarily aims at exclusive consilience and relies on the measurement of confounding variables and the use of statistical techniques to rule out rival hypotheses. The final category, *multimethod strategies*, aims at methodological and predictive consilience and involves the deployment of diverse research methods. We highlight experimental strategies in this category that provide a novel way to bolster methodological and predictive consilience by simulating the effects of culture experimentally.

Systematic Contrast Strategies

Multiple Contrast Strategy

In this strategy, three cultural groups or more constitute at least three levels of an experimental condition. Ideally, the sampled cultures should have low, moderate, and high values on a relevant cultural dimension. The key issue is whether the expected cross cultural difference emerges in the predicted order. The availability of at least three cultures reduces the interpretational ambiguity associated with two-group comparisons. Within-culture variation is usually not dealt with in multiple contrasts and is regarded as

Table 1 A typology of methodological strategies under the consilience framework

| Strategy | Systematic Contrast | Covariate | Multimethod |
|---------------------------------|--|---|--|
| Consilience targeted | Contextual and predictive | Exclusive | Methodological and predictive |
| Orientation | Correlational | Correlational | Correlational or experimental |
| Related concepts | Cross cultural comparisons; cultural differences | Contextual variables, Confounding variables, bias, sensitivity analysis | Multi-method multi-trait analysis; experimental ethnography, experimental anthropology |
| Level of analysis | Individual or culture | Individual | Individual |
| Culture as independent variable | Categorical – cultural groups | Cultural elements – continuous, measured | Cultural groups – categorical; cultural elements – measured or manipulated |
| Alternative explanations | Usually no explicit ruling out of alternative explanations | Planned ruling out of alternative explanations | Usually no explicit ruling out of alternative explanations |
| Strength of causal inference | Low | Moderate | Moderate to high |
| Ecological validity | High | High | Low for laboratory studies and high for field studies |

error in the same way as in an experiment. No attempt is usually made to rule out any rival hypotheses in this strategy.

The strength of the multiple contrast strategy is its high ecological validity, but the weakness is its lack of control for confounding variables. The selected cultural groups may differ on many non-focal aspects and it is hard to rule out their influence. Another major limitation is that this strategy provides no direct evidence on the causal mechanism hypothesized.

Temporal Contrast Strategy

In the *temporal contrast strategy*, a single group is studied over time, and temporal change in

the cultural characterization of this group is related to change in the dependent variable. For example, individuals are likely to exhibit cultural change if they are subjected to a new cultural environment for some natural reasons, such as immigration. The change in their cultural characterization as a result of this new cultural experience makes it possible to test whether there is a link between the cultural change and a correspondent change in a focal dependent variable. Studies employing this strategy are in many ways similar to interrupted time-series studies (Cook and Campbell, 1979).

Many studies on sojourners and migrants employ the temporal contrast strategy and

compare their behaviors in their original culture and in a host culture. A good demonstration is provided by Heine et al. (1999), who argue that an interdependent self-construal is associated with lower self-esteem. Heine and Lehman (1997, cited in Heine et al., 1999) compared the self-esteem of Japanese exchange students in Canada assessed a few days after their arrival with their self-esteem assessed seven months later. As expected, contact with Canadian culture, which emphasizes independence, was associated with an increase in self-esteem. In contrast, a group of Canadian English teachers showed a decline in self-esteem when their self-esteem prior to their departure for Japan was compared with their self-esteem seven months after their arrival in Japan. This trend suggests that contact with a culture emphasizing interdependence was associated with a decline in self-esteem for the Canadians.

A variant of this strategy involves the contrast of cohorts that differ in their duration of exposure to a cultural environment, such as first- and second-generation immigrants. A good example is given by Chao (2001), who explored cultural differences in the consequences of different parental styles. Leung et al. (1998) reported that an authoritative parenting style was associated with children's academic performance positively in the USA and Australia, but not in Hong Kong. Chao (2001) went one step further to explore the reason for this cultural difference by contrasting three cultural groups: European Americans, first-generation, and second-generation Chinese Americans. As expected, the effect of parental authoritativeness on school grades was strongest for European Americans, followed by second-generation Chinese Americans, and then by first-generation Chinese Americans.

Other variants of the temporal contrast strategy are possible, such as the inclusion of a control group for benchmarking (see Shadish et al., 2002, Ch. 6.). The major

strength of this strategy is that the use of a single group or similar cohort groups alleviates the validity threats associated with non-equivalence. For instance, selection effects are unlikely to be a threat because background variables are identical for a single group and are likely to be similar for cohort groups. Cultural differences in response style are likely to be non-existent or small. However, a major threat to this type of study is that because longitudinal studies involve a relatively long period of time, many variables other than the hypothesized cause may have changed as well, leading to a variety of alternative explanations for the observed change. A contrast of cohort groups runs into a similar problem in that these groups may have been exposed to a variety of different environmental characteristics other than the hypothesized cause, making a firm causal inference difficult (see Shadish et al., 2002, Ch. 6, for a detailed discussion).

Strengthening the Systematic Contrast Strategies

The adequate sampling of cultures can bolster a causal inference based on a systematic contrast strategy. These methods can be applied in a single study or in analyses of secondary data. Three sampling schemes of cultures are possible. In *random replication*, the focus is not to rule out the effects of specific confounding variables, but to rely on the logic that the effects of major confounding variables can be ruled out if a converging pattern is found across a sufficiently large random sample of cultures. The resources needed for a truly random sample of cultures are usually prohibitively high, but a collective research effort or meta-analyses can result in a sample of diverse cultures that approximates a random sample. Good examples include the hypothesized connection between collectivism and conformity based on Asch's line judgment task (Bond and Smith, 1996), and between situationalism and external attribution (Choi et al., 1999). The hypothe-

ses proposed by the authors of these two major literature reviews are persuasive because a variety of cultural groups show the predicted differences, thus making alternative explanations unlikely.

A variant of this strategy is to consider whether the ranking of a group of cultures on a given cultural dimension corresponds to their ranking on a dependent variable. As an example, Graham et al. (1994) found that across eight countries, the higher the collectivism of a group, the stronger the preference for a negotiation style that is characterized by cooperativeness and willingness to attend to the other party's needs. Confounding variables that deviate significantly from the rank order of these eight countries based on individualism–collectivism can be confidently ruled out (Campbell, 1986). The persuasiveness of this approach increases with the number of countries involved, because it is unlikely that alternative explanations can generate a ranking similar to the predicted ranking across a large set of cultures.

A related, though less demanding method is to replicate the findings across diverse pairs of cultural groups. The logic behind *diverse sampling* is that each pair of cultural groups may differ on attributes other than the attribute hypothesized to be the cause of a cultural difference. If a similar pattern emerges across very diverse cultural groups, the hypothesized cause is likely to be the only consistent difference that is common to the diverse pairs involved.

In the third method, termed *systematic sampling*, cultures are systematically selected for inclusion in order to rule out specific confounding variables. Two or more cultural groups are selected by matching them on the confounding variables, and if the hypothesized cultural difference still emerges, the alternative explanations based on the confounding variables are refuted. A variant of this approach is the inclusion of the confounding variable in the design as an independent variable. For instance, collectivism is

found to relate to the preference for non-confrontational conflict resolution methods, but many studies confounded individualism–collectivism and masculinity–femininity, and it is unclear whether cultural differences in the preference for non-confrontational conflict resolution methods can be attributed to cultural differences in masculinity–femininity. To resolve this ambiguity, Leung et al. (1992) selected four cultural groups that differed systematically in individualism–collectivism and masculinity–femininity. Their results showed that, as predicted, the preference for non-confrontational methods varied with individualism–collectivism, but not with masculinity–femininity.

Systematic sampling can be extended to the study of existing groups that vary on a cultural dimension of interest in a single culture. For instance, Erez and Earley (1987) contrasted the behaviors of Israeli kibbutz and non-kibbutz members in experimental studies because these two groups vary in individualism–collectivism. Their results are generally consistent with the cross cultural results based on the individualism–collectivism framework. A more recent study by Kurman (2001) contrasted four groups in Israel: religious kibbutz members, non-religious kibbutz members, religious urban Israelis, and non-religious urban Israelis. Kibbutz membership provides the variation in individualism–collectivism, whereas religiosity provides the variation in the emphasis on modesty, which forms the basis of an alternative explanation to individualism–collectivism. Kurman was able to show with this four-group design that self-enhancement was related to a modesty norm, but not to individualism–collectivism. An advantage of monocultural studies is that they avoid many confounding variables that plague cross cultural comparisons because of the common cultural background of the participants.

Covariate Strategies

Simple Covariate Approach

The second type of strategy involves the use of covariates to strengthen the causal inferences made by ruling out alternative explanations. In the *simple covariate approach*, culture is conceptualized and measured as an individual-differences variable, and cultural differences in a dependent variable are attributed to cultural differences in this individual-differences variable (Brockner, 2003). If the effect of culture is controlled for statistically, the observed cultural difference should become smaller or disappear altogether. A good example is given by Earley (1989), who investigated the effects of individualism–collectivism on social loafing (the phenomenon that people exert less effort when they work in a group than when they work alone). American participants were found to show more social loafing than Chinese participants. Furthermore, when individualism–collectivism scores of participants were included as covariates, the difference in social loafing between these two groups disappeared.

Culture may influence a relationship rather than the extent to which a certain characteristic or behavior is displayed, and the covariate approach can also be used to ascertain such effects. A good example is provided by Brockner et al. (2001), who examined the effects of power distance and participation in decision-making on organizational commitment. In three studies, Brockner et al. showed that participation in decision making was more positively related to work attitudes and behaviors in low power distance societies (Germany and the USA) than in high power distance societies (Hong Kong, Mexico, and mainland China). Furthermore, Brockner et al. showed that power distance beliefs were responsible for the magnitude of the positive effects of participation on work attitudes and behaviors.

Complex Covariate Approaches

In a complex form of the covariate approach, variables based on other plausible hypotheses are included together with a cultural variable hypothesized to be the cause of a cultural difference. In a study described before, Chen et al. (1998) evaluated several facets of individualism–collectivism, and identified only one facet, collective primacy, that was able to explain cultural differences in in-group favoritism. The other facets of individualism–collectivism were dismissed as causes of the observed cultural difference.

The analysis in this type of study may go beyond a simple covariate analysis, and involve causal modeling. For instance, Farkas et al. (1990) evaluated a few explanations for differences in academic grades among students of different ethnicity. The results of their causal modeling showed that teachers' judgment of work habits of students was the most important factor in accounting for ethnic differences in academic grades. Recent development in propensity score analysis, which employs sophisticated procedures to evaluate the effects of confounds (e.g. Larzelere et al., 2004; McCaffrey et al., 2004), is relevant for this type of analysis, but this more complex approach has rarely been attempted in cross cultural research.

Monocultural Extension

An interesting extension of this approach is to test culturally derived hypotheses within a single culture. The central idea is that participants from a single culture who vary along a cultural attribute show variation in a focal dependent variable that is consistent the relevant cross cultural results. For instance, Brockner et al. (2000) found that the effects of procedural justice were stronger for Chinese (high in interdependent self-construal) than for Americans (high in independent self-construal), and that the cultural difference disappeared when cultural differences in self-construal were controlled for. More relevant

to our discussion, the self-construal of a group of American participants was measured, and the results derived from this mono-cultural group resembled the cross cultural results. Those with an interdependent self-construal showed a stronger effect of procedural justice than those with an independent self-construal. The convergence of the cross cultural and mono-cultural results provides support to the cultural explanation of the cross cultural differences documented.

Strengthening the Covariate Strategies

Matching An effective way to rule out the influence of covariates unrelated to a causal theory is to match different cultural groups on these covariates. Matching has a significant advantage over statistical control in that matching makes fewer statistical assumptions. Procedures for statistical control, such as regression or analysis of covariance, typically assume that the relationships of the covariates with the dependent variables are linear and identical across cultural groups. Matching does not require these two assumptions, which may explain its popularity in epidemiological research. Another difference between these two methods lies in their realm of applicability. Covariate strategies can be implemented even when cross cultural differences in the covariates are large. In fact, if the values of the covariates are non-overlapping across cultures, matching is not possible, but a covariate approach can still be used. Nonetheless, the two methods are similar in that the cross cultural equivalence of the measures used must be assessed. When matching samples from different cultures, the equivalence of the matching criteria used needs to be ascertained.

It is interesting to note that matching is often done at the individual level in epidemiological research (Rosenbaum, 2002). Each participant from the treatment group is matched with another participant from the control group, so that these two participants

are similar with regard to the covariates. In contrast, matching in cross cultural research is usually done at the group level. We note that individual matching is harder to do than matching at the group level, but more stringent statistical tests can be applied to analyzing matched pairs. Individual matching seems to offer a new way for controlling confounds in cross cultural research.

Omitted variables The major weakness of covariate strategies is that researchers may have inadvertently left out some confounding variables that may mask or even reverse a predicted relationship. The effects of these omitted variables are known as hidden bias in epidemiology (Rosenbaum, 2002). Many variables other than culture may impact a given phenomenon (Cohen, 2001), and to argue for a particular cultural cause, the impact of all other variables needs to be ruled out. Consider the following hypothetical example. Assume that we want to show that collectivism is related to conformity in two cultures, A and B. Assume that culture A is more collectivistic than culture B, and that collectivism is indeed related to conformity in both cultures. In normal circumstances, participants from culture A would show a higher level of conformity than participants from culture B (Bond and Smith, 1996). We now assume that an omitted variable, urbanization, has a significant impact on conformity in that people from urban settings show less conformity than those from rural settings (e.g. Park and Gallimore, 1975). If there are significantly more people in urban settings in culture A than in culture B, the effect of the setting could nullify or even reverse the effect of collectivism. A researcher who ignores urbanization will report no difference in conformity between these two cultures, or even a difference in the opposite direction.

Omitted variables may be responsible for some puzzling inconsistencies in cross cultural findings, but sound theoretical analysis is needed to pinpoint the relevant omitted

variables. In a study described before, Leung et al. (1998) found that consistent with a cultural analysis based on power distance, an authoritarian parenting style was related to better academic results among children in Hong Kong, but not in the USA and Australia. However, Leung et al. also found that this difference was explainable by a cultural difference in parental education. Thus prior knowledge about the relationship between parental education and parenting style is needed to avoid relegating parental education to the status of an omitted variable.

In epidemiological research, the influence of omitted variables is often assessed by sensitivity analysis. The logic of this type of analysis is that if we are able to estimate the magnitude of a hidden bias that is needed to produce the effect observed, we know whether or not these omitted variables are important (see Rosenbaum, 2002, Ch. 4, for analytic procedures for this purpose). A study is 'sensitive' if a small degree of hidden bias is able to alter the results obtained. Results of sensitive studies are more likely to be influenced by omitted covariates, and hence are more open to alternative interpretations. Another way to put it is that a relationship is 'robust' if the absence or presence of confounding variables has only a small impact on the relationship. To the best of our knowledge, sensitivity analysis has not been attempted in cross cultural research, and an important future direction is to develop procedures for sensitivity analysis in cross cultural research.

Multimethod Strategies

The use of diverse methodologies is an important way to reduce the confounding influence of cultural differences in reactions to research situations, procedures, and materials. The merits of multimethod approaches in cross cultural research are well-known (e.g. van de Vijver and Leung, 1997), and we do not provide a detailed discussion here.

Instead, we highlight two relatively more novel developments in this area: multiple dependent variables and experimental strategies.

Multiple Dependent Variables

The use of diverse dependent variables in a single study is important because different dependent variables may be associated with different confounding variables. If a similar pattern of results emerges across diverse dependent variables, a single confounding variable is unlikely to constitute an adequate explanation for the configuration of the results.

In epidemiological research, the use of diverse dependent variables is common, and the notion of *specificity* is often used to boost causal inferences. If a hypothesized cause is shown to relate to a specific phenomenon, but not to another related but conceptually different phenomenon, the causal inference is strong because this specific pattern of results can rule out many alternative explanations. For instance, Trichopoulos et al. (1983) showed that coronary mortality was higher after an earthquake, but no increase in cancer-related mortality was found in the same period. The specificity of the effect of the earthquake provides strong support for the causal effect of acute stress on fatal heart attack.

Experimental Strategies

Although true experiments are not possible in cross cultural research, there have been some novel applications of experimentation to strengthen cross cultural causal inferences. There is a long tradition of experimental work in cultural research, and Cole et al. (1971) label monocultural studies of cultural phenomena that employ both qualitative and experimental methodologies as 'experimental anthropology'. More recently, Cohen et al. (1996) coined the term 'experimental ethnography' to refer to an approach that involves the manipulation of some variables

Table 2 Two types of experimental strategies

| Type | Idiographic experiments | Ecological experiments |
|------------------------|--------------------------------------|--|
| Target of manipulation | Person | Environment |
| Manipulation methods | Priming, explicit instructions | Explicit instructions, formation of artificial groups, systematic change in physical or social environment |
| Duration | Usually short | Short to moderate |
| Dependent variables | Social behavior, cognitive processes | Social behaviors, cognitive processes, individual differences variables, and psychological adjustment |

to weaken or accentuate a cultural effect in order to support a causal inference. The central idea is that while culture cannot be manipulated, the effect of culture can be demonstrated by experimentally creating a specific situation for a predicted effect to emerge. In demonstrating a 'culture of honor' prevalent in the South of the USA, Cohen et al. created a situation in which a participant was insulted by a confederate. Compared to Northerners, Southerners were more likely to feel a threat to their masculine reputation, show more anger as measured by a rise in cortisol level, and display more aggressive behaviors. In general, this approach first identifies a cultural element as the cause of an observed effect. An experiment is then designed to show that an experimentally created variation in some variables related to this cultural element shows a predicted effect on a dependent variable.

Morris et al. (2004) provided an interesting example in which a cultural difference was suppressed by a manipulated variable. Chinese typically prefer mediation more and adjudication less than do Americans, and Morris et al. argued that the preference of Chinese for mediation is based on their perception that mediation can resolve a conflict more effectively than adjudication. In line with this argument, when no information was provided about the other disputant,

Morris et al. were able to replicate the finding that Chinese preferred mediation more and adjudication less than Americans. However, in an experimental condition in which the other disputant was described as low in agreeableness and high in emotionality, cultural differences vanished, and both Chinese and American participants preferred adjudication to mediation. These experimental results support the role of the perceived efficacy of different conflict procedures as a cause for cultural differences in procedural preference.

Ecological vs. idiographic experimental strategies Two types of experimental strategies can be identified (see Table 2). The studies by Cohen et al. (1996) and Morris et al. (2004) described above involve the manipulation of situational variables to demonstrate some predicted effects, and such experiments may be termed *ecological experiments*. These types of studies attempt to change some aspect of the social environment and observe how people behave in this contrived environment as compared to their behavior in normal circumstances.

Another way to demonstrate the causal effect of a cultural element is to create a change in this element experimentally, and to compare the behaviors of participants in this experimental condition with those in

other conditions. A good example of an *idiographic experiment* comes from priming studies by Hong et al. (2000). Chinese are more likely to make external attributions for observed events than are Americans, and this difference is attributed to individualism–collectivism, but the evidence available is correlational in nature (Choi et al., 1999). Hong et al. presented two sets of priming materials to Hong Kong Chinese. One set contained icons characteristic of the US culture, whereas the other set contained icons characteristic of the Chinese culture. Chinese participants reported a higher level of internal attribution when they were primed with US icons than with Chinese icons. The control condition, in which there was no priming manipulation, yielded a pattern that was between the two experimental conditions. Priming techniques are now quite popular for demonstrating the causal effects of cultural elements (e.g. Haberstroh et al., 2002).

The study by Hong et al. (2000) involved a non-specific manipulation of the salience of cultural knowledge, but other studies manipulated more specific cultural elements. Trafimow et al. (1991) asked respondents to think about either what they had in common with or what made them different from their family and friends. Results showed that when University of Illinois students with either Chinese or European names were asked to think about what they had in common with their family and friends, the percentage of social self-descriptions increased (e.g. ‘I am a Roman Catholic’; ‘I am from a certain city’). This experiment showed that for both individualistic and collectivistic participants, the focus on interdependence led to more social self-descriptions. These findings corroborate previous cross cultural findings that people who are oriented toward interdependent self-construal tend to report more social self-descriptions (e.g. Cousins, 1989). More importantly, these experimental results support the causal role of independent–interdependent self-construal in influencing the

nature of self-description. Another example of priming a specific aspect of individualism–collectivism, namely, independence vs. interdependence, was given by Oishi et al. (2000, Study 3), who demonstrated a causal effect of viewing the self as an interdependent entity on the making of external attributions.

Priming studies are still nascent and a definitive evaluation of their usefulness is premature. But, it is interesting to reflect on their potential from a causal perspective. The main advantage of priming studies is that they bring cultural factors under experimental control. Although the initial findings are intriguing, however, the constructs that can be primed may be exhausted quickly. Another point to note is that priming studies are usually conducted with bicultural participants, and thus bear some conceptual similarity to the studies of bilinguals. In these studies, bilingual respondents are asked to respond to a linguistically equivalent instrument in two languages (e.g. Ralston et al., 1995). A working assumption is that by answering items in a specific language, a specific cultural frame associated with that language is activated, leading to different responses to the two language versions. We now know that many such studies reported inconsistent differences between different language versions. A major weakness of such studies is that it is hard to ascertain what exactly is being activated by the language of a questionnaire. Priming studies seem to share a similar weakness because priming manipulations are transient and sometimes fuzzy. In future applications, it is important to probe the processes activated by priming procedures and develop independent manipulation checks to identify the cultural elements that are responsible for the effects observed.

Direct vs. indirect experimental strategies

Direct strategies attempt to demonstrate the impact of a cultural construct directly through its experimental manipula-

tion, such as the cross cultural study on conflict by Morris et al. (2004) described earlier. Indirect strategies involve the experimental demonstration of an effect that is consistent with the predictions of a cultural theory, but the relevant cultural elements are not (and usually cannot be) manipulated. A good example is provided by the study of Cohen et al. (1996) on the effects of an insult on Southerners in the USA.

Field experiments A natural experiment makes use of different cultural contexts that vary in a theoretically meaningful way in order to test a given proposition. For example, Scribner and Cole (1981) were interested in the influence of writing skills on cognitive development, but writing skills and schooling tend to be confounded in most societies. However, the Vai in Liberia had a system for teaching writing skills in their indigenous language outside school. Thus Scribner and Cole were able to compare the cognitive performance of three groups of Liberian children: unschooled and illiterate in their indigenous language, unschooled and literate in their indigenous language, and schooled. They found that schooling had a more pervasive influence on cognitive test scores than unschooled literacy.

Another example comes from Shebani et al.'s (2005) test of Baddeley's phonological loop model, which posits that memory span varies across languages according to the articulation time needed for a given set of items. In other words, cultural differences in the memory span for a set of stimuli are caused by cultural differences in the articulation time needed for the stimuli. The Arabic language offers a unique advantage for testing Baddeley's model because there are two ways, differing in length, to pronounce each digit. Thus word pairs in Arabic that are conceptually identical but are of different length allow for a stricter test of the phonological loop model than has been done previously. In support of the model, memory span is

larger for stimuli with a shorter articulation time.

Evaluation of Experimental Strategies

Experimental strategies can be classified by three dimensions: ecological vs. idiographic, general (molar) vs. specific manipulation of a cultural element, and direct vs. indirect demonstration of a cultural effect. In general, experiments that involve narrowly defined cultural constructs and provide a direct demonstration of their causal effects are most persuasive in establishing a causal cultural theory. An example is provided by Oishi et al. (2000), who demonstrated that priming a view of the self as an interdependent entity led to more external attributions. Interdependent self-construal is a narrowly defined cultural construct, and the experiment demonstrated explicitly its causal effect on external attributions.

We note that experiments are not a panacea for ascertaining causal claims in cross cultural research, and there are at least two major limitations. First, some cultural variables are not amenable to experimental manipulation, which limits the scope of attitudes and behaviors that can be examined in experiments. For instance, it is not easy to induce a benevolent value or a universalistic orientation in a one-hour laboratory experiment. Second, experiments are good at capturing transient effects, but the effects of some cultural variables may take a long time to surface. For instance, the effects of modernization on attitudes and behaviors take years to show (Inglehart and Baker, 2000).

Conclusions

Future Development in Cross Cultural Research Methodology

In reviewing the range of methodological strategies available for probing cultural elements as causal agents, we note three important broad directions for future research.

First, the quality and quantity of cross cultural research have improved dramatically in the past decades, and many researchers routinely examine cultural variables for evaluating and extending their theoretical frameworks. The popularity of cross cultural research has increased the sophistication of the methodologies used, and we hope our consilience framework will encourage the reliance on multiple sources of evidence, and the inclusion of diverse measures to ascertain a causal explanation and refute alternative explanations in future research endeavors. Broad-brushed descriptions of cultural differences and general statements about the effects of culture should eventually be replaced by specific loci of cultural differences and well-defined causal processes associated with the differences.

Second, we expect to see the study of more novel cultural variables in the future. In the past decade, culture has been conceptualized in some novel ways, such as knowledge structure (Hong et al., 2000; Söderberg and Holden, 2002), general beliefs or social axioms (Leung and Bond, 2004) and cognitive styles (Nisbett, 2003), and the search for innovative ways to delineate culture will continue. In addition, traditional cross cultural research relies mostly on paper-and-pencil and behavioral responses as dependent variables, but some novel measures have recently been used, such as reaction time and perceptual reactions (e.g. Kobayashi and Greenwald, 2003; Nisbett, 2003).

Third, some recently developed statistical techniques are valuable to cross cultural research in ascertaining complex relationships. Multilevel modeling is a prominent example, which has already been widely used in many areas of research (e.g. Muthén, 1994; Raudenbush and Bryk, 2002). As cross cultural researchers are better trained in methodology, we expect the use of sophisticated statistical techniques to surge.

Implications for Cross Cultural Management Research

The consilience approach described in this article is illustrated with examples mostly from cross cultural psychology. Psychology is a behavioral science and therefore the research strategies proposed earlier should be applicable to behavioral research under the rubric of cross cultural management research, such as cross cultural studies on organizational behavior. There are, however, at least two major areas of cross cultural management research that our analysis may not be completely relevant. First, firm-level issues are often studied in cross cultural management studies, such as human resource practices across firms from different cultural backgrounds (e.g. Aycan, 2005). Some unique research methods are needed to tackle this type of cross cultural research, which are beyond the scope of the present article.

Second, intercultural interactions, such as negotiation across cultural boundaries and workplace diversity, are a major focus of cross cultural management research. However, in a recent review on the research in cross cultural organizational behavior, Gelfand et al. (2007) lament that intercultural research has largely been ignored. Leung (2008) notes that because this type of research involves at least two cultural groups, on top of cultural dynamics, intergroup dynamics and identity issues are also important. Some unique research methods are probably needed for intercultural research, but we do not know much about the issues involved because of the dearth of research in this area. Future research is desperately needed to develop research methods that can address the specific methodological difficulties of this line of work.

To sum up, because the field of cross cultural management is relatively nascent, it is hard to discern any methods and data analytic techniques that are uniquely associated with this field of enquiry. However,

high-quality cross cultural management research demands both sound theories and high-quality research methods. We propose the consilience approach as a guiding framework for cross cultural research with a causal emphasis, and provide a comprehensive review of the strategies for bolstering causal claims. In our view, despite the absence of true experiments, studies that are carefully conceptualized, designed and analyzed can go a long way toward establishing causal links. We hope that our consilience approach will help leapfrog cross cultural research from a mostly descriptive stage to a causal stage, in which sophisticated causal theories are being developed and refined.

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Résumé

Stratégies visant à renforcer les inférences causales dans la recherche interculturelle : une approche fondée sur la coïncidence (Kwok Leung and Fons J. R. van de Vijver)

La recherche interculturelle n'autorise pas de vraies expériences car il est impossible d'affecter de manière aléatoire des participants à des cultures différentes. Les études interculturelles sont donc perçues comme de la recherche quasi expérimentale et les menaces qui compromettent la validité d'inférences causales dans la recherche interculturelle sont passées en revue. S'appuyant sur la biologie de l'évolution et l'épidémiologie, l'approche fondée sur la coïncidence est préconisée afin de renforcer la validité des inférences causales interculturelles. Cette approche prétend que les inférences causales, dans la recherche interculturelle, sont d'autant plus convaincantes qu'elles sont soutenues par des preuves qui s'appuient sur une base théorique solide, des sources de données multiples, des méthodes de recherche différentes et une réfutation explicite d'interprétations alternatives. Trois grandes stratégies visant à renforcer les inférences causales interculturelles sont proposées dans ce cadre de coïncidence, dont le contraste systématique des groupes culturels, l'inclusion de covariables pour exclure les explications alternatives et l'utilisation de multiples méthodes de recherche, comme l'expérimentation interculturelle. Les méthodes de recherche interculturelles et leurs développements futurs sont aussi étudiés.

摘要

跨文化研究中强化因果推理的策略：知识融通的方法

Kwok Leung and Fons J. R. van de Vijver

在跨文化研究中，由于无法随机地把参与者分派到不同文化中，所以不能进行真实的实验。跨文化研究因此被认为是准实验研究，并有可能破坏跨文化研究中因果推理的效度，本文对此进行了文献回顾。借鉴进化生物学和流行病学的研究，我们提出用知识融通的方法来强化跨文化因果推理的效度。本方法认为，借助于可靠的理论基础，多渠道的数据，不同的研究方法以及对其它解释的明确驳斥，跨文化研究中的因果推理才能最令人信服。在知识融通框架下，我们提出三种用以强化跨文化因果推理的广泛策略：系统对比文化组群；包含相关变量以排除其它解释；使用多种研究方法，比如跨文化实验法。本文也讨论了跨文化研究的未来发展。