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# Response to Rudmin's Book Review of *Immigrant Youth in Cultural Transition*

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We thank the book review editor for the opportunity to respond to the review (Rudmin, 2008) of our book on immigrant children's acculturation and adaptation (Berry, Phinney, Sam, & Vedder, 2006). We have a number of concerns about the accuracy of some of the reporting of our findings and of some of the criticisms made in the review. We outline these concerns in three sections.

## General

The reviewer had previously made a number of criticisms regarding our approach and previous findings (see Rudmin & Ahmadzadeh, 2001). This earlier review establishes that he had already formed a critical position on our work. Moreover, two of us responded to the article (see Berry & Sam, 2003); we are surprised that he did not reference our published response in the current review. Many of the points that we raise here were available to the reviewer in our earlier response, but he did not take these into account in his latest review. In addition, we make reference to some of the reviewer's earlier criticisms (Rudmin, 2003) in our book (see Berry et al., 2006, p. 220).

## Reporting of Our Findings

The percentages reported in the third paragraph of the review are not those reported in the book. We believe that a reviewer should report our figures accurately and then note that he or she has chosen to recalculate the percentages to take into account the cases that were not included. This reviewer appears to want to make the distinction between *valid percentage* (which we report) and his own way of reporting. He prefers to use suggestive wording to stress that we did not admit particular cases to the analysis. Later in the review, he is

more explicit when he “shows” that we left out particular cases, even stating that we selected data.

There are various places where the reviewer substitutes his terms for the names of our variables. For example, *psychological adaptation* is variously called *mental health* and *psychological well-being*, and *sociocultural adaptation* is referred to as *school well-being* and *social well-being*, making it confusing for the reader to follow the details of our presentation.

We acknowledge the error in Table 8.2, namely, listing a Turkish sample in the United States. There was no such sample, as a reading of the sample descriptions in chapter 2 (Table 2.4) clearly shows.

## Critique

The reviewer is critical of our use of the term *immigrant paradox* as it is used in the current literature. We did not coin this term, nor do we subscribe to it, neither conceptually nor empirically, in our overall findings. We do try, however, to define the concept on the basis of the different ways in which it has been used in the literature.

Based on research in sociology, epidemiology, and public health, findings in the United States show that despite their numerous risk factors (e.g., poverty, low socioeconomic status, racial/ethnic minority status), foreign-born immigrants do better than U.S.-born peers on an array of indices, ranging from health to education to criminal behavior. Moreover, research has also shown that despite their low socioeconomic status, many U.S.-born Latinos have better health and well-being than do their White peers (Hayes-Bautista, 2004). This pattern—that immigrants (and/or U.S.-born Latinos) have unexpected positive outcomes and that these positive outcomes deteriorate over time and generation—has sometimes been termed the *immigrant paradox*, or the *Hispanic/Latino paradox*, or in studies of infant mortality, the *epidemiological paradox* (Nguyen, 2006).

Although research findings that suggest an immigrant paradox has been traced to the early 20th century (Frisbie, 1993) and was first reported in the late 1960s and early 1970s (Teller & Clyburn, 1974)—starting with findings regarding birth outcomes of Mexican immigrants to the United States (e.g., low infant mortality and high birth weight of children)—it is only in recent years that the paradox has been systematically examined (Palloni & Morenoff, 2001) and has started to gain popularity. Today, the paradoxical patterns extend across outcomes (including physical and mental health, psychosocial adjustment, and academic performance), ages, ethnic groups, and to a lesser extent, national boundaries (Nguyen, 2006).

Furthermore, the reviewer confuses *integration attitude* (which reflects only a preference for a way of acculturating) with *integration profile membership* (which reflects attitudes plus identities plus behaviors). He appears to think that the concepts refer to the same phenomena, just because part of the name is the same. A careful reading shows that they are not.

The reviewer questions the use of scales with “double-barreled” items to assess the four acculturation attitudes of integration, assimilation, separation, and marginalization, claiming that they do not meet usual criteria for evaluating scales. Campbell (1957) has distinguished between two forms of validity: external validity, which is high when a concept and its operationalization closely match the reality of the phenomenon being examined; and internal

validity, which is high when there is statistical consistency in response patterns. In our earlier reply (Berry & Sam, 2003), we noted that acculturation is a process that lies at the intersection of two cultural ways of living. We believe that it is important to examine a phenomenon in context, in all its complexity, rather than by only creating simple scales with high internal consistency. Because acculturation is a double-barreled phenomenon, we believe that it is legitimate (indeed essential) as one way to assess it. It is especially defensible when we combine this approach with other kinds of acculturation and intercultural scales, as we did in this study.

Acculturation is a complex, confusing, ambiguous, and often-conflicting process for individuals. The reviewer asserts that total agreement on the four attitude scales should be 100% whereas they sum to 163%. He then asserts that this indicates acquiescence. To identify these statistical effects as being due solely to acquiescence is to view acculturating individuals as purely logical actors. If all people behaved in a purely logical way, then we would need only “logical” and not “psychological” research to understand them. A scholarly review would at least consider alternatives to the reviewer’s preferred interpretation.

In the earlier critique, Rudmin and Ahmadzadeh (2001) claimed that answers to one scale constrain the answers to the other three scales. This is not the case in our earlier research nor that in the present book: Respondents are free to respond to each of the four items, according to their preferences. In our view, the reviewers’ view is a narrowly logical one of human preferences, one that ignores the psychological complexity of living with two cultures. We made these two points in our earlier reply (Berry & Sam, 2003).

The reviewer also argues that the structural equation model shows that the effects of ethnic orientation and ethnic contact are more influential than those of integration. However, the model should be read as indicating that ethnic orientation and national orientation have unique effects on sociocultural adaptation, as does integration. Only ethnic orientation and integration have unique effects on psychological adaptation. National orientation, ethnic orientation, and integration have a combined and indirect effect through ethnic contacts. Given that the factor scores used in the model are not the same as the four acculturation attitudes (e.g., ethnic orientation is based on separation, ethnic identity, and family obligations scores) or profile membership, it is strange for the reviewer to claim that the findings are more in support of a unicultural model than a bicultural one.

The reviewer complains that we explicitly state that we carried out our analyses in ways that fit with the dominant theoretical framework guiding the study. Of course, we did—to evaluate our hypotheses on the basis of this framework! We clearly stated that we settled for the outcome that was interpretable within our theoretical framework, because this practice is common in psychology. For instance, it explains why exploratory factor analysis is one of the most favored statistical techniques.

## Conclusion

We stand by the major conclusions that we reached in the interpretation of our research. First, multiple measurement approaches to acculturation and adaptation are defensible. When we include acculturation attitudes, cultural identities, and intercultural behaviors in multivariate analyses, we are able to identify a number of various ways in which immigrant youth acculturate; integration, a joint orientation to both cultures, is more commonly

adopted than any other way of acculturating. Second, those who engage the acculturation process in this integrative way achieve better psychological and sociocultural outcomes. And, third, we are of the opinion that these two conclusions warrant our promotion of integrative strategies for acculturating individuals, communities, and national societies.

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