

Method

The research on the intergenerational parallelism of deviance to be reported in Parts II and III should be evaluated against a consideration of the adequacy of the procedures that have been followed in extant intergenerational research. Presented, in turn, is a critical review of the methodological characteristics of others' research on intergenerational parallelism and a description of the research procedures employed in the present study to address what are perceived as methodological limitations in current research.

Methodological Limitations of Current Research

The research to be reported in subsequent chapters tries to assess and explain the degree to which parent's deviant behavior in late childhood or early adolescence predicts similar behavior in their own children at comparable ages. A large literature exists that attempts to accomplish these same objectives. A number of observers have presented what they see as desirable characteristics of such research, implying that a good deal of research on (what is here called) intergenerational parallelism fails to approximate these ideal conditions. Thus, It has been argued that the design of intergenerational research should properly meet three criteria (Cairns, Cairns, Xie, Leung & Hearne, 1998):

First, such investigations presuppose the study of people who are observed at approximately the same age (or developmental stage) in two or more successive generations. The information from each generation should be

longitudinal to use time and sequence as primary variables and to clarify the potential mediational processes. Second, the longitudinal information should be prospective rather than retrospective. Given the limitations of retrospective reports—forgetting, distorting, and interpreting the past in light of the present—there are few alternatives to the prospective longitudinal study. Third, the information should be multilevel and not restricted to a single measurement source or domain. As Radke-Yarrow, Campbell, and Burton (1968) emphasized, independence of assessment provides a buffer against confounding in interpretation, whether the analyses are contemporary, retrospective, or prospective. (p. 1163)

Similarly, Van Ijzendoorn (1992) suggested with regard to studies of the intergenerational transmission of parenting that

...longitudinal studies should be carried out measuring parenting with comparable instruments at comparable times across the lifespan. Furthermore, contextual factors should be taken into account because the transmission may be stronger or weaker depending upon the influence of these contextual accounts on two or three generations. (p. 97)

More recently, Smith and Farrington (2004) concurred that the

...true nature of intergenerational continuities in behavior can be established more clearly using independent information about youth behavior in two successive generations assessed prospectively at roughly the same ages... (p. 231)

Taken together, these descriptions of desirable methodologies include the following: observation of the different generations at comparable developmental stages; use of prospective longitudinal designs; specification of mediating causal mechanisms, intergenerationally stable causes of the phenomenon of interest in each generation and (by implication) moderators of intergenerational parallelism; and independent sources of data for the separate generations as the more salient features of ideal research designs for the investigation of intergenerational parallelism.

Comparable Developmental Stages

Several researchers have noted the virtues of controlling on developmental stage, and the biases that might be introduced by failing to do so, in studies of intergenerational parallelism. For example, Cairns and associates (1998) observed:

Early parenting studies have added significantly to the understanding of socialization processes and the various ways that parents and young children interact (Parke & Buriel, 1997). However, there are difficulties inherent in invoking parent–child comparisons to explain continuity across generations. Large differences exist between adults and children in the organization, form, and functions of their social and cognitive patterns. Ignoring these disparities may invite superficial evaluations of parent–child similarities and their functions. More important, because of the dynamic and ephemeral nature of interactional adaptations, it cannot be assumed that similarities observed in early transactions will be maintained across successive periods of developmental reorganization.

Given these concerns, the rigorous assessment of intergenerational continuity seems to require the study of child–child comparisons across generations (see de Beer, 1958; Cairns, Elder, & Costello, 1996; Cairns, Gariepy, & Hood, 1990; Elder, Caspi, & Downey, 1986; Hood & Cairns, 1988; Magnusson & Cairns, 1996). The term *intergenerational development* refers to the relations between the ontogeny of behavior and cognition in children and the ontogeny of the same characteristics in their parents when they, in their own time, were children. To determine whether mother–child similarities persist over time, parent-to-child comparisons can be embedded in a long-term child-to-child comparisons across generations. When integrated over time, such linked longitudinal studies may provide a picture of the relations between the childhood of the parents and the childhood of the children and what the mediators of these effects are. Such studies also seem critical in the identification of possible mechanisms of cross-generational continuity and change (Burgess & Youngblade, 1988; Caspi & Elder, 1988; Furstenberg, Brooks-Gunn, & Morgan, 1987; Serbin, 1996; Werner & Smith, 1982). (p.1162)

The failure to control on stage in the life course across generations would be to ignore that individuals might have different life-course-related needs that lead to biased reporting. Thus, according to generation stake theory (Acock & Bengtson 1980, p. 512): “Each generation has an investment in the generational bond. But, for youth, the ‘stake’ is more toward maximizing a sense of separate identity; for parents, the investment pays off in maximizing continuity.” The young adult children would be motivated to perceive relationships with parents as more conflictual than they actually are, whereas the parents might be more motivated to view the relationships in more positive terms that belie the realistic picture of the relationship. In support of this perspective, Aquilino (1999) cited studies of parents and children at different stages of the life cycle, including pairs of parents and adolescents (Noller, Seth-Smith, Bouma, & Schweitzer, 1992), parents and college students (Thompson, Clark, & Gunn, 1985), and

middle-aged children and their parents (Bond & Harvey, 1991). As another example, Van Ijzendoorn (1992), citing a study (Hanson & Mullis, 1986) that evaluates parenting and child-rearing attitudes of female college students and their parents, observed that by comparing attitudes of two generations at different phases of the lifespan, it is not evident that the birth of a baby would or would not change the students' attitudes to children and to child-rearing in one or another direction, which could have been ascertained had the female college students been tested at the same developmental stage as their parents.

As these illustrations suggest, studies of intergenerational parallelism vary according to whether the developmental stages of the representatives of the separate generations are comparable. Data might be provided by or about representatives of the two generations at different or comparable developmental stages. In the former case, for example, adults might provide information about themselves as adults and about their children when they were preadolescent, adolescent subjects might provide information about themselves during adolescence and about their parents' attitudes and behaviors, or adult subjects might provide information about their own attitudes and behaviors and their adolescent children might provide data about their attitudes and behaviors. In the latter case, where the data referred to comparable developmental stages of the representatives of the successive generations, subjects might report about the attitudes and behaviors of their young adult children and about their own attitudes and behaviors when they were young adults, or self-reports about adolescent subjects in one generation might be compared with the self-reports of adolescents in the next generation (i.e., their children) at the same developmental stage. For the above-noted reasons, controlling on developmental stage is the much preferred design feature in studies of intergenerational parallelism.

Prospective Longitudinal Studies

An important limitation of studies of intergenerational parallelism in deviant behavior is the correlational nature of these investigations—that is, the failure to establish temporal priority among parental deviance, putative intervening circumstances, and deviance of the children (Downey & Coyne, 1990). Cairns and his associates (1998, p. 1162) noted that “only a handful of empirical researchers have assessed,

prospectively, the behavioral and cognitive development of children relative to the development of their parents when they, in their own time, were children (but see Eron & Huesmann, 1987; Furstenberg et al., 1987; Serbin et al., 1991)."

When studying intergenerational continuities, many research designs do not permit disentangling the influence of parents on children from the influence of children on parents. This is less of an issue in circumstances such as the present study in which measures on the parental generation were taken well before parenthood was reached. Thus, if causal processes are implicated at all, temporal priority of the baseline measures of the first-generation subjects have been clearly been established to be temporally prior to second-generation outcomes. This is not to say that more proximal processes might not involve reciprocal influences. As others have noted, particularly during late adolescence and early adulthood, relationships tend to be increasingly egalitarian (Vollebergh, Iedema, & Raaijmakers, 2001). In any case, particularly where the specification of intervening processes is an issue, prospective longitudinal designs facilitate less equivocal interpretations of observed instances of intergenerational parallelism.

Mediators, Common Antecedents, Moderators

Research protocols that aim at a more complete understanding of instances of intergenerational parallelism must permit the specification and measurement of hypothetical mediating, moderating, and common antecedent variables. Any complete understanding of intergenerational parallelism must focus on intervening mechanisms of intergenerational transmission. As Van Ijzendoorn (1992) observed in connection with a review of studies of intergenerational transmission of parenting:

Remarkably little is known about the mechanism of intergenerational transmission of parenting. Learning to be a parent and to acquire a certain parenting style may be the outcome of modeling, coaching, or other cognitive processes, and we are not able to derive from the studies reviewed here which (combination of) learning process(es) is most supported by the empirical evidence. Most studies are restricted to just showing that a relation between infant and adult characteristics exists, and do not give insight into the causal mechanism. (p. 95)

Specification of intervening mechanisms provides insight into some of the circumstances that moderate instances of interpretational parallelism. Any variable that is observed to mediate the intergenerational transmission could be assumed to be a moderator of that relationship. Thus, if the intergenerational continuity of poverty is mediated by the influence of poverty on the educational level of the next generation, then the degree of continuity depends on the level of education of the second-generation subject. Under conditions whereby the second-generation subject gains a higher level of education, intergenerational continuity of poverty is less likely to be observed. Where poverty in one generation decreases a parent's commitment to educational level in the second generation, first-generation parental commitment to the educational aspirations of the second generation will modulate the intergenerational transmission such that under conditions of high educational aspirations by the first-generation parent, intergenerational continuity of poverty is less likely to be observed.

Although many instances of intergenerational parallelism might be explained by consequences of first-generation behavior or characteristics (and, by extension, moderators of the parallelism), these same or other instances might be explained, in part, by intergenerational continuities in factors that within each generation are significant causes of the phenomenon of interest (i.e., the construct that manifests intergenerational parallelism). Intergenerational parallelisms might be explained at many different levels. Thus, intergenerational continuities might result from continuities in social context or by intergenerational communication. As Vollebergh et al. (2001) observed with regard to intergenerational congruity in cultural orientation:

Comparable orientations in both parents and their children may result from sharing the same environment or sharing comparable social status. Explanations for the intergenerational transmission of attitudes are therefore on at least two levels of analysis: on the level of the intergenerational transmission of social status and social positions from parents to their children (Glass, Bengtson, & Dunham, 1986) and on the level of direct transmission of cultural orientations of parents to those of their children through communication within the family (Acock & Bengtson, 1980; Beck & Jennings, 1975; Moen, Erickson, & Dempster-McClain, 1997; Petit, Clawson, Dodge, & Bates, 1996). Past empirical studies have suggested that both explanations are valid and complementary (Glass et al., 1986; Moen et al., 1997; Vollebergh, Iedema, & Raaijmakers, 1999). (pp. 1185–1186)

Clearly, then, the research design that attempts to account for, rather than merely describe the existence of, intergenerational parallelism requires

the specification of intervening processes, variables that moderate the presence and magnitude of intergenerational parallelism, and intergenerational continuities that have causal impact on the outcome of interest in each generation.

Independent Sources of Data

Studies of intergenerational parallelism vary according to whether a representative of one generation provides data about both generations or information on each generation is provided by a representative of that generation. In the former instance, for example, youth, adolescent, or adult subjects might provide data about themselves and about their parents or grandparents or adult subjects might provide information about themselves and about their children. Responses by the person regarding comparable attitudes or behaviors of the two generational representatives are taken to be an indication of intergenerational continuity or parallelism. In the latter instance, data are provided by a representative of each of the generations. Thus, parents might provide responses about their own behavior and their children might provide data about their own behavior. Where the separate responses indicate comparable behaviors or attitudes, intergenerational continuity or parallelism is said to have occurred.

Studies of intergenerational transmission too frequently use reports by one or the other generation of the phenomenon that is examined across generations. Thus, for example, findings indicating that children of depressed parents demonstrate poorer psychological functioning than children of parents who are not depressed are often based on parents' reports of the children's functioning rather than on the children's self-reports or independent observations of the children. Thus, it is possible that the effect of parental depression on children's psychological functioning might be the spurious result of the tendency of the depressed parents to bias their perceptions in a negative direction (Avison, 1999; Rickard, Forehand, Wells, Griest, & McMahon, 1981). As another example, Velleman and Orford (1990) reported data for themselves as adults and for their parents with drinking problems.

The importance of having independent reporters for each generation is apparent when contrasting methods in which a single reporter was used with those in which in separate representatives of the multiple generations reported. Thus, Chassin and her associates (1998), examining the

continuity of parenting across generations, observed such continuity when the mothers offered data on their own parenting behavior assessed in adulthood. However, intergenerational continuity was not observed when the children's perceptions of maternal support were used.

Studies in which the same subject provides data about both generations, whether the data are provided about the different generations at different developmental stages, are vulnerable to criticism on numerous methodological grounds. Validity might be compromised by problems associated with method variance whereby associations between measures of the two generations are artificially inflated due to, for example, common underlying personality traits. As Simons, Whitbeck, Conger, and Wu (1991) observed:

In the issue of intergenerational transmission of harsh parenting, it may be that aggressive individuals tend to see others, including their parents, as displaying high rates of aggressive behavior (Straus et al., 1980) thereby producing an artifactual relationship between descriptions of their own behavior and that of their parents. (p. 160)

In support of this assertion, although studies based on adolescents' perceptions of congruence reveal intergenerational continuity regarding values dealing with educational goals, career, and major life concerns, the relatively few studies dealing with congruence based on reports from parents and their adolescent children observe relatively little congruence (Gecas & Seff, 1990).

Another problem with collecting data on the two generations from a common source is that the relationship might be artifactually reduced if we are dealing with socially undesirable data. The cross-generational data are further threatened when a representative of only one of the generations provides data about both generations due to retrospective distortion. As Oliver (1993) observed with regard to the intergenerational transmission of child abuse:

Omission, confusion, irrationality, distortion, and most bizarre of all, idealization in accounts of cruel, rejecting, or neglectful grandparents (G1) by parents (G2) with rearing difficulties and/or ill-treated children (G3) are universal findings. (p. 1320)

The work of Aquilino (1999), alluded to earlier in connection with the need to control on stage in the life course, is relevant also in regard to requiring that each representative of successive generations provide data on

their own situation rather than having one or the other representative provide data for both. It apparently does matter who provides self-report data on such matters as relationship quality contact and interaction patterns. Systematic differences exist in the perspectives of parents and their adult children regarding the nature and quality of their relationships. In general, the parents are more likely to provide a happier view of the intergenerational relationships than a sample of adult children. This might be due, in part, to the two generations having different stakes in the relationship—that is, different psychological needs of the two generations at different points in the life cycle (Acock & Bengtson 1980). Thus, young adult children might exaggerate conflictual relations with parents in order to facilitate their need for separation from the family of procreation. The older parents, on the other hand, might experience a need for continuity of preexisting relationships and, therefore, might need to perceive a more positive relationship with the adult children. As Aquilino (1999) concluded:

...theoretical and empirical scholarship on intergenerational relations would benefit from more attention to the issue of divergent perspectives in families. I believe there is a strong case for making the collection of equivalent data from both generations a standard practice in designing research on parents and adult children. (p. 869)

A number of studies can be mentioned that conform to some of the features of an ideal study design but not to the others. Thus, although very few studies exist that permit assessments of antisocial behavior across “two generations of children at comparable ages, employing independent reporters and comparable measurements” (Smith & Farrington, 2004, p. 231), Smith and Farrington (2004) cite a number of studies that have these desirable characteristics:

The Concordia Longitudinal Risk Project followed female subjects from childhood over a span of 20 years, and assessed a subsample of 89 Canadian females and their children at ages 5 to 13 (Serbin et al., 1998) Here a weak correlation between aggression in successive generations was found, although this was largely mediated by the mother’s educational attainment.

Farrington (1993) utilized Cambridge Study data to assess bullying and antisocial behavior in a sample of 411 London males from the age of 8, and in their resident children aged 3–15 when the men were ages 32. This study found continuity in bullying, with 30% of men who had been identified as bullies, at age 14 reporting that their children were bullies, compared with 17% of men who had not been bullies. Huesmann and his colleagues (1984b) assessed aggression in over 600 subjects followed from age 8 to age 30, and

found that in the subjects at age 8 predicted aggression in the subject's children 22 years later when they were aged 8. (p. 231)

Other studies of intergenerational parallelism (whether or not focused on antisocial behavior) might be cited as well, in which data are provided by these representatives of the several generations at comparable developmental stages (Blee & Tickamyer, 1986; Harburg, Gleiberman, DiFranceisco, Schork, & Weissfeld, 1990; Lefkowitz, Huesmann, & Eron, 1978).

Although these studies display a number of desirable design features, they are limited in other ways. For example, Smith and Farrington's (2004) study of continuities in antisocial behavior and parenting across three generations uses data on only 411 inner-city (London) males. Data from the earlier point in time come from first-generation mothers, whereas data at the later point in time come predominately from second-generation fathers; that is, data on the index child's (second generation) conduct problems were provided by the child's mothers, whereas data on the children of those subjects (grown up) were provided by the fathers. In Capaldi and Clark's (1998) investigation, antisocial behavior was measured by different indicators for the parents as opposed to their 10-year-old boys. Other studies failed to address the issue of explaining observed instances of intergenerational parallelism in terms of intervening processes, moderating variables, and intergenerationally stable effects that have causal implications for the variable of interest in each generation.

The present research design meets these criteria. The subjects in each generation are observed at approximately the same developmental stage (early adolescence) in two successive generations. The information for the first generation is longitudinal in nature, the subjects having been assessed several times between early adolescence and the fourth decade of life. The second-generation subjects have been or will be interviewed at different points in the life course. The longitudinal information is prospective rather than retrospective. Information on the respective generations is provided by the separate representatives of the generations rather than being provided for both generations by the representative of one of the generations. The sample size is adequate to estimate reasonably complex models. Analyses take into account moderators, intervening processes, and intergenerationally continuous factors that have common causal effects within each generation on the object of the investigation of intergenerational parallelism. We now turn to a description of the multi-generation prospective longitudinal study that provides the data for the investigation of intergenerational parallelism of deviance.

A Multigeneration Prospective Study

Data Collection

The data used in this study were collected from a first-generation (G1) panel starting in 1971 (G1T1) from a target population made up of the seventh-graders ($N = 9,056$) enrolled in a random half of all the junior high schools ($N = 18$) in the Houston Independent School District. These adolescents were surveyed again in 1972 (G1T2) and in 1973 (G1T3). The survey instrument consisted of 209 items and was identical in all three waves with the exception that waves two and three provide (for the most part) self-reported deviant behaviors for the previous year, rather than the previous month, as in the first wave. Since then, the subjects have been surveyed through personal interviews (including the T1–T3 variables) using greatly expanded instruments in the 1980s (G1T4) and again in latter part of the 1990s (G1T5), which was the final G1 data wave with a total of 5,467 subjects.

At G1T5, the adult respondents were asked about the number, ages, sex, and addresses of their biological, step, adopted, and foster children. The parent's permission was then sought to interview their children (G2), who would be contacted to participate in a second-generation study (G2T1). If the child subject was younger than the age that their parent was when he or she was first interviewed, then that child was not interviewed until they were at least 12 years old. At the end of the G2T1 data collection wave, a total of 7,519 second-generation subjects had been interviewed. It was from these parents and their children that the sample for this study was drawn.

The selection criteria needed for the G1 subjects to be included in the sample were that they provided data in both the Time 1 (T1) and Time 2 (T2) data collection waves and had a least one child interviewed at G2T1. The selection criteria for the G2 subjects were that they provided data at Time 1, they were biologically related to the parent subject, and they were one to 2 years younger or older in age than the parent subject at the time he or she was interviewed. These criteria produced an initial sample size of $N = 2,721$.

The sample was then aggregated, so that G1 subjects who had multiple children in the sample had their children's responses averaged within their respective families. This means that the variable values were first added together for all children within any given family and then were

divided by that same number of children in each family independently. This produced a one-to-one correlation of one G1 subject to an averaged G2 subject for each intergenerational relationship, for a total sample size of $N = 1,658$.

Missing Data

Ignorable missing data are usually a product of two types of mechanism: missing completely at random (MCAR) and missing at random (MAR). Data are MCAR when a subject's nonresponse to a question is not dependent on any other measured or observed variable related to the subject, study, or the question itself. If a subject's nonresponse to a given question is contingent on subject characteristics or a previous response, but not dependent on the question itself, then the data are considered MAR (Enders & Bandalos, 2001; Rubin, 1976). It should be evident that MCAR is the stronger assumption, because data that are MCAR are also MAR.

Missing data in the variables reported here are assumed to be the less restrictive MAR type. However, given the nature of the variables, it is possible that the subjects' responses might not even meet MAR assumptions. There are several methods for addressing missing data. Such methods include theory-based direct maximum likelihood (ML) or full information maximum likelihood (FIML), listwise and pairwise deletion, and different forms of multiple imputation. In general, the majority of recent research into the efficiency of missing-data methods has shown that direct ML techniques outperform all other methods (Enders, 2001; Little & Rubin, 1987). One drawback of the direct ML method is that it assumes multivariate normality similar to all ML estimation methods. However, little is known about how these methods work in the presence of nonnormal data and/or clustered data such as used for the study. If it follows other ML estimation techniques, then parameters will be increasingly biased as the degree of nonnormality and clustering increase.

An alternative form of imputation, and the one used for this study, called the similar response pattern method has been implemented in PRELIS 2, which is a preprocessor for the LISREL program (Joreskog & Sorbom 1994a). The method attempts to impute real values from another case with similar observed values by using a minimization routine based on a set of matching variables. If the routine cannot find a case with

complete data using the matching variables, then the missing value for that variable is not imputed into the case and remains missing. A study by Brown (1994) found that compared to listwise and pairwise deletion, mean imputation, and hot-deck imputation, similar response pattern imputation produced the least bias overall with regard to structural and measurement model parameters. However, he did find that there was some positive bias in the error estimates, indicating that Type 1 error rates would be larger than normal.

Although there is no statistical theory that would support this method over direct missing-data methods, the fact that it imputes values from similar cases is attractive because of the clustered nature of the second-generation data. If it is plausible that children from the same family would have more similar responses to each other than to children from other families, then possibly imputing a value from a respondent's sibling does have some validity. As suggested in the PRELIS manual, a large number of matching variables were used, including subject identification numbers, that were not otherwise used to select the G1 or G2 subjects or used in any of the model estimations as moderators, indicators, or other variables.

Variable Construction

All variables, except for the binary moderators, are cumulative indexes made up of a number of binary items. The items were coded such that as the value of the variable increased the negative effect of the variable increased. Specifically, the first-generation deviant behavior variables were taken from a combination of the Time 1 and Time 2 surveys. As mentioned earlier, the items that were used to construct these indicators were based on two different time references. Time 1 questions asked whether each subject preformed the deviant behavior within the last month, except for the alcohol use question, which asked if it had happened within the last 2 weeks. The same questions were asked at Time 2, but the time reference was within the last year, except for the alcohol use question, which again asked if it had happened within the last 2 weeks.

The Time 2 data were the primary responses used for G1 variable construction. If G1 subjects had a missing value at Time 2, then the Time 1 response was substituted in its place. If, for any given item, the value was missing at both Time 2 and Time 1, then that response was coded as a

missing value. The effect of this scheme minimized missing data at Time 2 and best replicated deviant behavior ever engaged in by the G1 subject despite the fact that the questions only covered behavior within the last 2 years of the subject's life. Given that the G1 variables were indeed simple additions of similar items with less than seven categories, all of them were treated as true ordinal-scale variables

The survey questions for the G2 subjects specifically asked whether they had ever performed the deviant behavior in their lives up to the point of the survey. Thus, only the G2T1 survey responses were used to construct their indicator variables. Because in many instances there was more than one G2 subject from a family in the sample, after the variable indicators were constructed they were then added together with their siblings and then divided by the total number of their siblings within each family. This averaging produced more values for each G2 variable than the SEM software would classify as ordinal, so the G2 indicators were treated as continuous such that when used with the ordinal indicators of the G1 subjects, polyserial correlations were adopted in model estimation.

For both G1 and G2, the items used to construct the disposition to deviance and negative self-feeling variables were not time referenced. In general, missing values for these items were very low, so only Time 2 responses were used to construct the G1 variables. As with the deviant behavior items, only the G2T1 survey responses were used to construct the G2 variables.

Variable Nonnormality

As previously mentioned, the scale of the G1 variables were treated as ordinal and the G2 variables were treated as continuous. There is some ambiguity and debate about how to classify variables measured on an ordinal scale when there are only three to five categories, as is the case with many of the G1 variables. Given that these variables were cumulative indexes composed of face valid, similar items, with relatively high reliability coefficients, it is plausible that the values between categories were equidistant. It is also plausible that the relationship between the categorical measured variable and the underlying theoretical variable that it was supposed to measure had a linear relationship. However, given the nature of deviant behavior in general, both the G1 and G2 variables measuring the behavior were, in a univariate sense, both heavily skewed and kurtotic.

Another way to investigate the degree of nonnormality is to estimate the bivariate polyserial correlations between all of the indicators in any model to be estimated. PRELIS 2 has a unique routine that allows the evaluation of the degree of nonnormality of any given polyserial correlation by a root mean square error of approximation (RMSEA) score (Joreskog & Sorbom, 1994a). All of the variables used in the models and their corresponding polyserial correlations with the other variables in any specific model fell within the acceptable range.

Statistical Methods and Latent Variable Model Estimation

SPSS 11.1 and LISREL 8.54 (Joreskog & Sorbom, 1994b) were used for variable recoding, calculation of univariate statistics, data imputation, bivariate normality evaluation, and any needed secondary analyses. The software used for the estimation of the latent variable models was Mplus 3.12 (Muthen & Muthen, 2004). The output from Mplus includes univariate and multivariate statistics, model fit, and residual information that was used to assess the hypotheses and theories that motivated this study.

Structural equation modeling specifies hypothesized relationships between observed variables and their latent constructs and the structural relationships among the latent constructs in the model. The measurement model describes the hypothesized relationship between the observed variables and the unobserved constructs that are presumed to underlie the indicators. The relationship between indicator and construct is expressed in terms of factor loadings and error terms for both variable and construct. The structural model reflects the hypothesized causal relationships between latent constructs as regression coefficients. For ordinal models, the interpretation of the estimates/coefficients for paths to a categorical outcome in Mplus, such as paths from predictors to an observed categorical dependent variable, are probit regression coefficients. The test statistic for the indicators is the estimated parameter divided by its standard error. The asymptotic distribution of Est./S.E (Estimate divided by Standard Error) can be treated as standard normal with a z -distribution (Muthén, & Satorra, 1995).

The models computed with Mplus used the WLSMV estimator, which produces weighted least square parameter estimates utilizing a diagonal weight matrix with robust standard errors and a mean and

variance modified chi-square test statistic. The models were evaluated in terms of the strength of the indicator regressions, magnitude of the structural parameter(s), and the goodness of fit. The three fit statistics used were the chi-square value, Steiger's RMSEA as described by Steiger and Lind (1980), and the Comparative Fit Index (CFI) as described by Bentler (1990). The chi-square value was the primary evaluator, but given its susceptibility to bias, if it was found to be too high given sufficient sample size and simple model structure, then the other two fit statistics were used.

Multigroup Analysis

Binary moderators were used to divide the sample into two subgroups. These subgroups were then compared for invariance across the models of interest. In general, two types of comparison can be employed to test for invariance, model form, and parameter congruence (Bollen, 1989). Given the relative simplicity of the models, it was assumed that the form of the models would be identical. Thus, only the model parameters were tested for invariance.

The strategy of the multigroup analysis was to first estimate the baseline models for each subgroup separately to see if the separate models fit the data. This is an important first step, because if the models do not fit the data for one of the subgroups, then it would be unlikely that the two subgroups would be equivalent on a significant number of the model parameters, which would negate the need for further tests of invariance. If it was found that the models fit both of the subgroups via inspection of their respective fit indexes, then the measurement parameters were tested for invariance by placing across group equality constraints on the factor loadings for each group and estimated as a stacked model. The p -value of the chi-square statistic was inspected and if it was found to be significant, then the invariance of the measurement model was supported. When the measurement model was found to be invariant, the third step of constraining the structural parameter to equivalence across the subgroups was performed. The difference in chi-square statistic between the constrained measurement parameters and the fully constrained model and its respective p -value was calculated to test whether the structural parameter was invariant across the two subgroups.



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