

Future Extensions of the Models

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According to the dictionary, a theory is a “plausible body of principles offered as an explanation of a phenomena” while a model is “a system of inferences presented as a mathematical description of a state of affairs” (Dishion & Patterson 1999). While in our writings we have perhaps mistakenly used the terms interchangeably, there is no question but that our general strategy has been to focus on the more mundane modeling rather than theory building.

At the level of theory building, our focus has shifted a great deal in emphasizing intra-individual levels of analyses. We now understand that predictions about rates of deviant behaviors require simultaneous information about contingencies available for both social competencies and for deviant behavior (e.g., it is the relative rate of reinforcement for antisocial behavior that is the key). We hope that the findings summarized in the present volume will stimulate renewed interest in extending the experimental studies began back in the 1970s (Woo, 1978; Devine 1971; & Atkinson 1971). Hopefully others will also engage in the onerous task of analyzing contingencies that are embedded in longitudinal data sets.

While traversing the circumplex that defines Figure 14.1, we find ourselves emerging with yet another change in focus. This one is subtle and relates to an increasing emphasis upon concepts such as selectivity (Donahoe & Palmer, 1994). At the intra-individual level, we have come to believe that the developing individual is essentially a self-maximizing organism that actively selects among responses, settings, and individuals. This is in stark contrast to the metaphor of the inert *tabula rasa* that could well have characterized our studies back in the 1960s and 1970s. Active selection is at the core of the process by which deviant peers are selected; it is also at the core of what forms of deviancy are the outcomes.

But within that broad theoretical framework, most of the development in the last few decades has been in learning how to define and measure our concepts more cleanly. Given the longitudinal nature of these data, our model tinkering has increasingly focused on developmental issues such as early, and late, onset delinquency, growth, or the lack of it, during some time intervals, the process by which young couples select each other. Always, it has been the case that what we studied was limited primarily by what it is that we knew how to measure. There have been some recent developments in assessment that lead us to believe that we may now be able to expand the models.

In the sections that follow, we briefly describe explorations that should produce three significant changes in the Oregon Models.

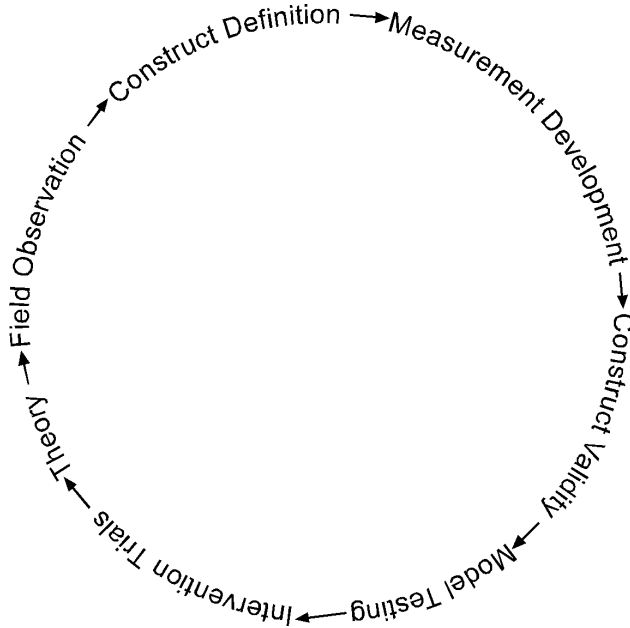


Figure 14.1. The model building process (Dishion & Patterson, 1999).

Toward More Complete Models of Child Behavior

Most of us are trained to view human behavior from a single perspective. In the 1960s one might think all of human behavior can be understood if one studies operants, by a single-minded focus upon attachment process, by the study of cognitions, by studying genes, by studying emotion, or social information processing. Most of us continue on this trajectory by sampling only a small number of journals and even being highly selective as to which articles are read.

We plan to expand the current narrow focus of the coercion model on contingencies to measures of negative emotion and social cognition. We also plan to expand the range of mechanisms invoked to explain both deviant and socially competent child behaviors. Data are now being collected in Wichita that should result in fundamental changes in our understanding of the causal mechanisms for children's aggression and social competencies as well.

As shown in Figure 14.1, the Wichita data will be used first to test the direct contribution hypotheses implied in most of the writing about attributions, negative reinforcement, and negative emotions. The theorists, in their single-minded jousting with the null hypothesis, demonstrates that their favorite variable does indeed correlate with antisocial outcomes. In demonstrating that their favored variable is better than no theory at all, they move swiftly to the implicit assumption that they have also identified *the* cause for children's aggression. We believe that a simple multivariate test, such as the one shown in Figure 14.2, will do a great deal to insert a little humility into such discussions.

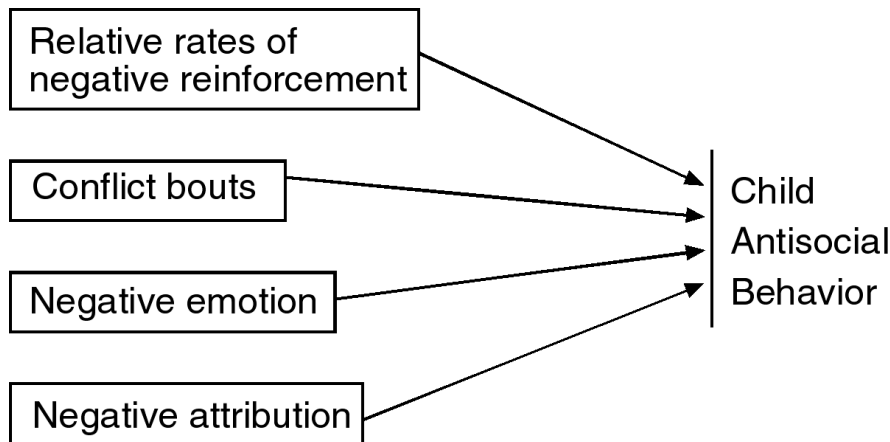


Figure 14.2. Multivariate models of antisocial behavior.

It seems odd, on the face of it, that one could study cognitions about aggression without examining them in the context of either negative emotions or contingencies being supplied for these behaviors. Alternatively why should the study of contingencies for aggression necessarily occur in a social vacuum? What contributions do negative attributions made about siblings and parents make to contingencies? Do negative attributions increase (or decrease) the functional value of successful outcomes (e.g., make them more or less reinforcing)? What contributions do negative attributions made about siblings and parents make to the process? For example, does it increase the risk for more frequent conflict bouts?

Figure 14.3 shows one hypothetical model that tests some of these hypotheses. As shown, our first best guess is that the relative rates of negative reinforcement during family conflict bouts will account for the bulk of the variance in latent constructs assessing future antisocial child behavior. In the original formulation, Snyder and Patterson (1995) hypothesized that in addition to relative rate of reinforcement, the frequency of conflict bouts should make a unique contribution analogous to the frequency of training trials. As shown in the hypothetical model, we also assume that coding facial affect for negative emotion will enhance the power of the negative reinforcement variable. Furthermore, negative attributions are thought to contribute indirectly to antisocial outcomes by enhancing the risk for increasing frequency of conflict bouts and social exchanges characterized by intense negative emotion. We should test for the possibility of a significant interaction effect for these paths (e.g., negative reinforcement by negative emotion).

The expanded coercion models must also address the fact that almost all of the micro-social analyses have focused on exchanges with a single agent. Based on the Snyder and Patterson (1995) study, we now know that with children the relative rates of reinforcement from conflict bouts with parents account for from 40% to 60% of the variance in the outcome measures of aggression used thus far. However, we

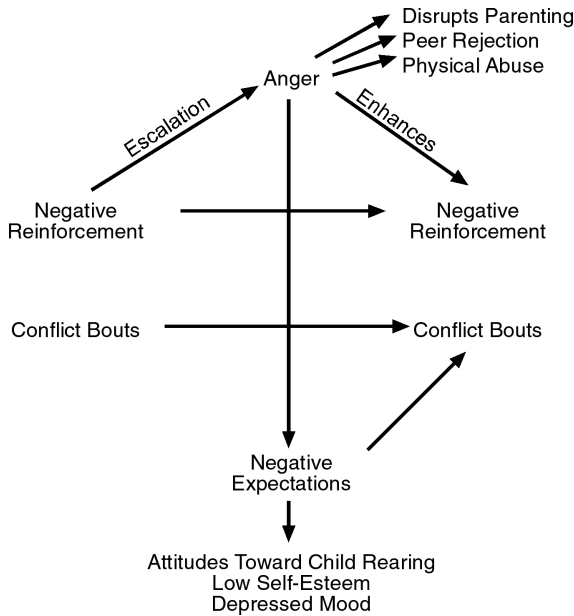


Figure 14.3. Adding thinking and feelings.

also know from Banks programmatic studies that sibling interactions contribute uniquely to the variance in addition to parent contribution. But what we need are multi-variable designs that tell us what the combined contribution would be and how much is unique to mother, to father, and to siblings. Do these contributions change as function of age/gender of child?

In that same context, it is clear from Dishion’s programmatic work that deviant peers make yet further contributions. Again in a multivariate design, is the contribution of relative rates of positive and negative reinforcement for deviancy significant above and beyond what the family contributes? The models would suggest that this must indeed be true for the young adolescent, but at what age does this contribution first become significant?

We remain deeply committed to the idea that knowing the relative rates of reinforcement provided by parents, siblings, and peers will not give us a complete picture. The best prediction about future outcomes should require an equivalent amount of information describing contingencies (and cognitions) as they relate to socially competent behavior. This will require that we invent new assessment procedures that give us reliable estimates. As noted in chapter 4, this is an area that we are only beginning to explore.

Non-Partitioned Genes

One of the great ironies in social science is that we finally have hundreds of studies examining the contributions of heritability to myriad social behaviors in children, including aggression. The sad fact is that these studies have not furthered

out knowledge of aggression by. The generation of twin and adoption designs are narrowly focused on the question of how much (variance) can be portioned into heritability (G) or environment (E). Because of the well-known myriad methodological problems (truncated distributions of E, failure to satisfy EEA, etc.) the estimates of variance accounted for by E or by G are shaky at best. However, even if the estimates were accurate, we would remain uninformed about how G and E variables contribute to our understanding of children's aggression or social competency.

Currently, OSLC staff, such as Leve, Stoolmiller, and Reid, have submitted several different proposals designed to address the question of genetic contributions to children's aggression and social competency. The genetically informed design would start at birth with information about pregnancy complications, birth weight, birth difficulties, as well as the antisocial status of the biological parents. The contribution of the biological variables to each of the mechanisms driving the progressions that move from distressed infant, to coercive toddler, to hyperactive, and finally antisocial 6-year-old were detailed in chapter 2. The distinctive feature characterizing these studies will be the focus on relating the biological contributions to the specific mechanisms that drive aggressive behavior, rather than to fruitless efforts to estimate the percentage of variance ascribable to E or to G, to shared vs. unshared environments.

We need yet another longitudinal study that employs an at-risk sample, this time of twins. Given an initial assessment at 12 months, we would plan to replicate and extend the assessment procedures used by Shaw and earlier still by Martin and Maccoby (chapter 2). This key study would include observation data collected every 6 months so that we can plot the growth of new forms of deviant behavior. Presumably the interval from 12 to 36 months is critical do the developmental of an early-onset trajectory

Explain Changes in Societal Rates of Crime

As briefly noted in the discussion of delinquency models in chapter 7, it was to have been sociology's task to explain differences among communities in crime rates. It is sad to say that the structural variables proposed by the leaders in that field generate a mixed set of outcomes.

Divorce, unemployment and poverty do not reliability account for changes in societal rates of crime. This is a complex problem characterized by its own set of methodological issues. For example, it is clear that the FBI data sets reflect biases of one kind, while victim surveys have their own unique distortions. Nevertheless, models such as the Oregon delinquency models should be tested against the societal rates task. As things now stand, each politician and newscaster generates their own spin on why it is that current delinquency rates are decreasing. As noted in chapter 7, the Oregon models are relatively clear about which variables should be directly related to changes in rates. Rates of juvenile crime should increase when the prevalence of 9 to 10 year-old antisocial boys increases and this in turn would be predicted by increases in prevalence of households characterized by ineffective parenting practices. Currently there is no one at OSLC competently trained to address these problems.

Modern Questions about Treatment Process

By the mid-1980s, the Center had moved beyond the study of parent resistance and its impact upon therapist behaviors. In a relatively brief span of time, we initiated three major longitudinal studies and began planning a major investment in the newly developing prevention science. Now, in the year 2000, we find ourselves engaged in a new activity that will inevitably lead to further changes in the underlying models.

Several of the Oregon prevention groups are now committed to training substantial numbers of new therapists that will man small community clinics (in Norway) or treatment foster care centers in their own areas. This has led to the development of manuals for therapists to use in training families and manuals for the training of therapists. The new training regimes emphasize repeated role-playing activities as a means for carefully shaping the behavior of family members and simultaneously shaping the behavior of the therapist who is to bring this about.

In a half century of clinical trials, we had simply never specified in detail what the procedures were to be, and why they had to be this way. The mandate becomes that of evaluating the efficacy of this kind of training. The prediction must be that individuals who are given this level of micro training would be more effective than would trainees who simply read the manual and work out the details for themselves. We expect some such analyses to be coming out of the Norway project by 2004.

Family as a System: Just for Fun

I have always envied the facility that systems analysts such as Arny Sameroff (1989) have in presenting their worldviews. However, like Belsky et al. (1989), I am not really impressed with the actual outputs from studies attempting to empirically apply systems ideas to family processes. It is the occasional publication by developmental writers such as Oyama (1989) and Thelen (1989) keep me convinced that I need to continue reading this literature, but at best, I find myself a casual consumer of work in this area. In that context, my latest reading of Sameroff's (1989) chapter leads me to believe that over the years, the Oregon group has drifted a long way from its early operant beginnings. In fact, it is my sense that what we are doing fit at least four of Sameroff's five core requirements for a systems perspective. I thought it might be a useful exercise to list the sense in which there might be such a fit.

1. *Wholeness and Order (Continuity in Development)*. Within the coercion models, we repeatedly stress the key role played by changes in the form of coercive and antisocial behavior (e.g., from overt to covert forms). Within the model, these changes are thought to define a second order deviancy factor that itself is quite stable over time (Patterson, 1993). In this sense, the whole is greater than the sum of the parts. Our long fascination with progressions, sequences, and current preoccupation with trajectories provide a nice fit to Sameroff's discussion of wholeness and continuities requirements (Sameroff, 1989). There is also continuity to the coercion model in the sense

that while temper tantrums at age 4 differ in form from temper tantrums at age 24 years, they constitute part of the core definition of coerciveness at both ages. Continuity is about the stability in the relations among the parts. The structural relations continue while the specific forms change over time.

2. *Self-Stabilization*. “Dynamic systems respond to contextual perturbations, either by homeostatic or homeorhetic processes. . . .” Sameroff (1989, p. 222). As detailed in chapter 6, much of the recent study of family processes is centered on the question of how it is that contexts (e.g., depression, poverty, divorce, stress, neighborhood) have an impact on family microsocial exchange). For example, we know that stress and depression increase maternal irritable exchanges, and this in turn disrupts various parenting practices.

Context alters process; in some sense, process probably also alters context. We view the family as engaged with a never-ending process of accommodating contextual changes that include the maturation of the child, the aging of parents and friends, and changes in occupation. One of the particular values of the coercion models that examine context is to have the search contain information about the central role of contingencies within this process. For example, as the child grows from what he is at age 3 to what he is at age 6, the context shifts from home to public school and enlarges to include peers. What is the impact of these changes in microsocial processes; in particular, how do the contingencies supplied by family members and peers shift during these changes in context?

3. *Self-Reorganization and Adaptation*. “Adaptive, self-organization occurs when the system encounters new constants in the environment that cannot be balanced by existing system mechanisms. Adaptation is defined locally as change that permits the system to maintain its setpoints best in new circumstances.” Sameroff (1989, p. 223). Given that the environment changes, for example when the child begins grade one he will find that his existing repertoire does not always function perfectly in each of his interactions with teachers and peers. The encounter requires some adaptive response on the part of the child.

According to the coercion models, the reorganization of existing systems to meet the new challenge will involve selective shopping. Given the new social environment the child will strive to maximize his payoffs given the constraints imposed by his prior history of learning. Presumably, he will select settings, peers, and activities that maximize his payoffs given what he brings to any particular setting.

It follows that given changes in the social environment, children will apply their selective shopping procedures with differential success. This, in turn, would imply that for some, the adaptation will include adjusting to lower overall rates of support and this, in turn, could lead to long-term depression that leads to yet more extreme methods for adapting.

As Sameroff (1989) points out, one can view coercion itself as an adaptation to a change in state provided by another family member. Here the family members' reaction determine the utility of this particular adaptation.

4. *Hierarchical Interactions.* Sameroff (1989), in an earlier critique of the systems qualities found in our studies, quite properly pointed out that we have overlooked the hierarchical relation between child and his family. At that time we had studied parents separately from siblings, and in some instances, the impact of male vs. female siblings on the identified problem child. Bank's elegant work on sibling contributions (see chapter 4) shows that the family is not just an aggregate of reinforcing agents. He demonstrates that the contributions made by siblings to negative outcome are quite unique. His work would be an important prelude to studies of the unique contributions of some dyads. The assumption here would be that an analysis at the level of dyads or triads as done by Belsky et al. (1989) would provide information above and beyond what is inherent in information about individual family members.

Our recent obsession with the growth phenomena leads, via serendipity, back to the hierarchy metaphor. As we view it in Figure 14.4, there are four major junctures that lead to an adult career as a repeated offender. The sequence of junctures defines a hierarchical structure where at each point there are two possible outcomes. In each case, the path to the left leads to yet more extreme deviancy, while the one to the right leads to normal or at least marginal adjustment. As shown, the first critical juncture rests on the resolution of the compliance problem.

Two-year-olds who achieve at least 70% compliance are likely to become normally socialized children and adults, while those who fail move toward increasing risk. At the second juncture, the coercive child will progress to the hyperactivity label (arrested socialization) or move to future extreme as an antisocial child. Those

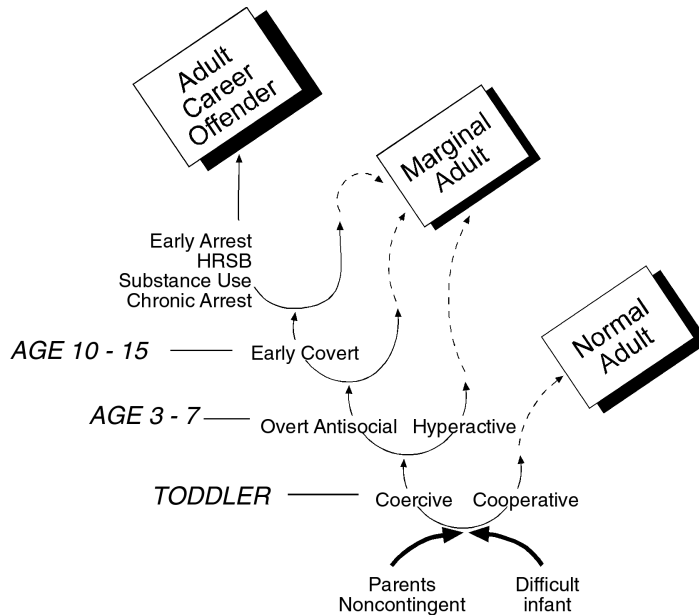


Figure 14.4. Deviancy development may be hierarchically ordered.

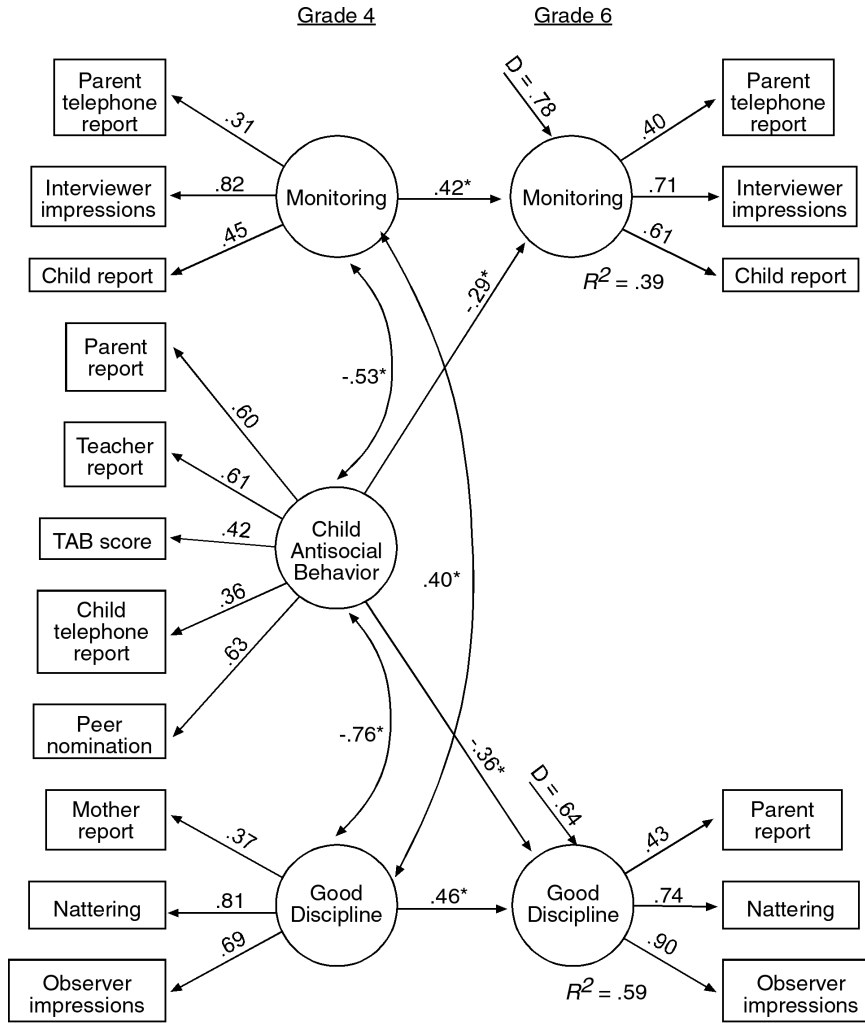
overtly antisocial children (55%) who move on to receive advanced covert training are at grave risk to become multiple offending adults $p = .48$. Conversely, those overtly antisocial boys who do not move on to covert antisocial training are at very low risk for career adult offending $p = .06$. The developmental sequence itself is hierarchically nested.

5. *Dialectical Contradiction*. Individuals come to know their world through their own activity. The contradiction lies in the fact that as one acts on the environment, they are constantly changing it; in effect, they are changing the thing that they strive to know. As Sameroff (1989) points out in his critique of our work, the coercion models provide rich examples of such contradictions. For example, the parent intends their scolding as punishment for teasing ones

There are other aspects of the Oregon models that are beginning to take on dynamic characteristics. Before we entered our growth modeling phase, we spent some time examining phenomena that seemed to fit bidirectional relationships, or the more complex positive or negative feedback loops. We studied possibilities found in both treatment process (Patterson & Chamberlain, (1988) and in family process itself (Patterson & Bank, 1989). For example the Patterson, Bank, Stoolmiller (1990) report presents a dramatic instance of bidirectional effects by first showing that both parental monitoring and discipline were highly stable when assessed at grade four and then re assessed at grade six. The stability path coefficients for the latent constructs were .66 and .77, respectively. On the other hand, as shown in Figure 14.5, part of the long-term stability in parenting practices reflects the contributions of trying to parent a problem child. The difficult child is constantly disrupting parental efforts to discipline or monitor. It may be, of course, as claimed by Rowe (1994) that the difficulty can be completely explained as a heritable trait. What is shown in Figure 14.5 is that when the latent construct for the antisocial child is introduced at T1, it accounts for a surprising amount of variance in determining how the parental monitoring and discipline unfold in the next 2 years. Having an extremely difficult child to deal with increases the likelihood that parental monitoring will be disrupted 2 years later. Notice that taking the child into account reduces our estimate of parental stability from .77 for monitoring to .42. Stability is not in the child per se. Nor is stability in the parent. It is in the dyad.

You can turn this around and ask the question “How much of the stability of child behavior is in the child, and how much of it is in the parent?” In the Patterson and Bank (1989) study, the change score for parental discipline was significantly related ($-.32 p < .05$) to the change score for child antisocial behavior.

It is also apparent that what happens within the family has an impact upon what happens on the playground at school. For example, Patterson and Bank (1989) explored the well-known relationship between being antisocial and being rejected by normal peers (see chapter 5). They found the expected (path $-.42$) relationship, but then went on to demonstrate that this relationship probably has some dynamic characteristics to it. Over a 2-year interval, the stability for the antisocial child latent construct was .65. Notice, however, in Figure 14.6 that when peer rejection at T1 is put into the model, it is shown to be massively significant

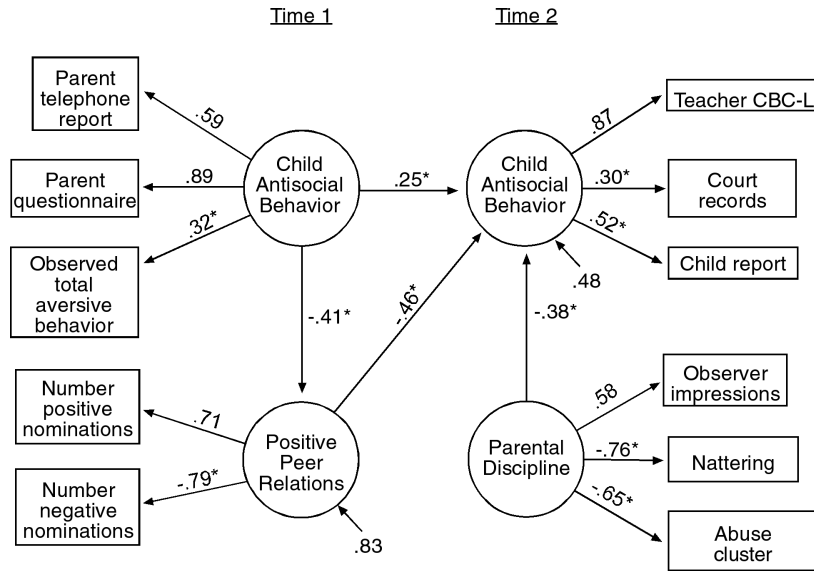


$N = 206$, $\chi^2_{(96)} = 118.97$; $p = .074$; BBN = .897; BBNN = .971; * $p < .05$

Figure 14.5. Bidirectional relations (Patterson, Bank, & Stoolmiller, 1990).

(-.46; $p = .05$) in predicting future antisocial behavior. Future parent discipline practices are significantly related to future antisocial behavior, but so is prior knowledge of rejection by peers.

One might imagine several reasons why it is that rejection by peers could function as a positive feedback loop that contributes to future maintenance or growth in antisocial behavior. In keeping with the perspective in the current volume, we assume that the mechanism will be shown to be increased risk for interaction with



$n = 99, \chi^2_{(38)} = 51.41; p = .072; RMSR = .897; *p < .05$

Figure 14.6. Rejecting peers as a feedback loop (Patterson & Bank, 1989).

other problem children who reinforce them for antisocial behavior. The new Wichita studies will permit precise tests of this assumption.

The new generation of studies will permit us to examine contributions of more than just parenting mechanisms to long-term outcomes. Obviously the picture will eventually be complicated with the contribution of multiple mechanisms from within and outside the home.